IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

Incentives and norms in social insurance:
applications, identification and inference

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The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market policies, studies of the functioning of the labour market, the labour market effects of educational policies and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

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Abstract

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This thesis consists of five self-contained essays.

Essay 1: (with Patrik Hesselius and Per Johansson) This essay tests if social interactions are important for work absence using a large scale randomised experiment. The treated in the experiment were exposed to less monitoring of their eligibility to collect sickness insurance benefits, which increased their non-monitored work absence. This exogenous variation is exploited in two ways. In a difference in differences analysis we exploit the variation in geographical proximity to the experiment among the non-treated. In an instrumental variables analysis we use the fact that the fraction of treated differs between immigrant networks. In both analyses we find significant, sizeable and robust social interaction effects.

Essay 2: In this essay, the effect of employer incentives in social insurance on individual wages is estimated. Several studies have documented that employer incentives, in the form of experience rating, co-insurance or deductibles, could decrease social insurance usage. Such employer incentives may, though, have unintended side effects as they give employers incentives to transfer the costs to their workers, affecting individual wages. The individual wage effects are estimated using a reform in January 1992, which introduced an employer co-insurance system into the Swedish sickness absence insurance system. The analysis based on a long population panel database, including survey information on hourly wages, gives no support to any important individual wage effects from the co-insurance reform.

Essay 3: (with Gerard J. van den Berg) Unemployment insurance systems typically include monitoring of unemployed workers and punitive sanctions if job search requirements are violated. This essay analyzes the effect of sanctions on the ensuing job quality, notably on wages and hours worked, and we examine how often a sanction leads to a change in occupation. The data cover the Swedish population over 1999-2004. We estimate duration models dealing with selection on unobservables. We

use weighted exogenous sampling maximum likelihood to deal with the fact the data register is large whereas observed punishments are rare. We also develop a theoretical job search model with monitoring of job offer rejection versus monitoring of job search effort. We find that the hourly wage and the number of hours are lower after a sanction, and that individuals move more often to a lower occupational level, incurring human capital losses.

Essay 4: This essay re-examines inference for cluster samples. Sensitivity analysis is proposed as a new method to perform inference when the number of groups is small. Based on estimations using disaggregated data, the sensitivity of the standard errors with respect to the variance of the cluster effects can be examined in order to distinguish a causal effect from random shocks. The method handles just-identified models. One important example of a just-identified model is the two groups and two time periods difference-in-differences setting. The method allows for different types of correlation over time and between groups in the cluster effects.

Essay 5: (with Geert Ridder) In this essay, identification of average treatment effects on conditional transition probabilities is considered. We show that even under random assignment only certain average treatment effects are point identified, because treated and control units drop out at different rates so that the initial comparability of treatment and controls due to randomization no longer holds. We derive sharp bounds on different average treatment effects that cannot be point identified. The bounds do not impose parametric restrictions, as e.g. proportional hazards, that would narrow the bounds or even allow for point identification. We also explore various weaker assumptions such as monotone treatment response and monotone exit rate.

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To a large extent I have written my dissertation at IFAU in Uppsala. Working at IFAU has been extremely beneficial for me. Personally and in my research I have greatly benefited from the insightful comments during seminars, all discussions during coffee breaks, the IFAU researcher's sometimes amusingly high self confidence, and that they have shown how a very high research standard can be maintained. In particular, I would like to thank Oskar, Peter S-T, Erica, Anders, Olof, Björn, Laura, P-A, Karin, Per E and Jörgen. A special thanks to Erik Grönqvist for being such character and his very careful and pedagogical reading of several of my essays, and to Louise Kennerberg whom you can always trust to have a lunch box, and to Peter F, Eva, Nikolay, Bertil and Xavier for very insightful and beneficial referee and editor comments.

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Uppsala, July 2008
Johan Vikström

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Essay 2: The Effect of Employer Incentives in Social Insurance on Individual Wages

Essay 3: Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality

Introduction

In the economics literature unemployment insurance, sickness insurance, and disability insurance/workers compensation are counted as social insurances. The effect of these insurances is undoubtedly one of the most intensively investigated topics in economics. This attention is natural: not only does social insurance constitute a big part of the government budget in many countries, but it also has significant effects on the welfare and behavior of individuals, labor unions and employer organizations. In order to design a social insurance that is efficient and fair we need to disentangle and quantify the different effects of social insurance. We need to answer questions like: how does being covered by insurance affect the wellbeing of individuals? Can certain monitoring measures prevent people from misusing the system? How do employers react faced with different insurance schemes? That is, we are interested in estimating causal relationships. The overall aim of this dissertation is to estimate different causal effects of social insurance, but also to provide new econometric methods with the aim of gaining a better understanding of causes and effects.

This dissertation consists of five self-contained essays, the two main themes of all of essays are social insurance and causal inference. Essays 4 and 5 are methodological and study identification and inference, while Essays 1, 2, and 3 focus on a certain social insurance application. The new methods developed in the two methodological essays are, however, applied to important social insurance questions. They are also from the outset inspired by questions encountered in my social insurance research. The three applied essays, on the other hand, all attempt to estimate causal effects of incentives and norms in social insurance. That is, I attempt to apply causal inference techniques in practice. In this introduction I introduce my view on social insurance and causal inference, and then introduce and interpret all five essays using that background.

Causal inference

Causal inference - or questions like: does a phenomenon have a causal effect? and: what is the cause of a phenomenon? - has intrigued researchers for a very long time. This goes all the way back to Aristotle, through for instance Hume, Mill, and Suppes. It also goes for different fields, e.g. physics, medicine, and social science. Interesting overviews of this historical development can be found in e.g. Pearl (2000) and Holland (1986). As might be expected from the intriguing topic of causal effects, there exists no single, uniformly accepted definition of causality, nor a given way to investigate causal statements. Even in the fields of economics, econometrics, and statistics, several different approaches continue to be applied. Each approach has several different focal points, and they are related to each other in several ways. This makes it difficult to give the main approaches a single name. One attempt is: the structural or econometric approach represented by e.g. Heckman (2008), the potential outcome represented by e.g. Holland (1986), and the causal diagram approach represented by e.g. Pearl (2000).[1]

This dissertation is mainly inspired by the potential outcome framework, as it is powerful for several reasons. The potential outcome framework offers a clear and easily interpretable definition of a causal effect. It makes the fundamental problem with identifying and estimating causal effects very clear, and can be used together with a long row of different identifying assumptions in order to establish important identification and non-identification results. Moreover, the approach could quite easily be communicated to individuals with limited or no background in either math or statistics. The potential outcome framework is also currently widely applied in economics.

Heckman (2008) defines three parts of causal inference. Slightly re-formulated they are: (*i*) defining interesting causal effects, (*ii*) identification of these causal effects using idealized data on the entire population, and (*iii*) estimation of the causal effects using actual data taking sampling variation into account. Needless to say any discussion of causal inference must start with a definition of a causal effect, and after that must come a discussion about which causal effects are meaningful and interesting to study. In many applications several different causal effects are meaningful. Logically then, the effect of interest must be dictated by the question that one wish to answer. The choice between different effects can often be described in terms of a trade-off between more information and fewer assumptions. In some cases relatively limited information is sufficient and one can rely on a few assumptions. In other cases many assumptions need to be imposed in order to obtain more detailed information.

In any case it should be clear that one can define interesting effects separately from the analysis of whether such effects could be identified. The distinction between the second and third parts of causal inference is less obvious, but is nevertheless important. To proceed ahead of the subsequent discussion: the definition and discussion of what a causal effect is shows that *no* causal effect can be identified without imposing an un-testable assumption. Several different assumptions can lead to identification of the same causal effect, and the same assumption can in combination with other assumptions lead to identification of different causal effects. In short, different sets of assumptions

[1] Other views on causality is found in for instance Granger (1969) on Granger causality, and the decision-analytic approach represented by e.g. Dawid (2000).

enable us to identify different causal effects. The second problem of causal inference is to logically deduce what can be identified, under a certain set of assumptions, given that we have information on the *observed* outcomes for the entire population of interest. In contrast, in the real world we never have data on the entire population of interest; instead we work with a sample of individuals. The third problem of causal inference is thus to assess what can be recovered given that we have to take sampling variation into account. How can we characterize, infer, and report the uncertainty that arises because we use a sample of observations? The next three subsections will discuss these three parts.

Causal inference is, in other words, a much deeper activity than evaluating in a certain situation whether a certain causal effect exists or not, as is the common aim of many applied policy evaluation or treatment effect studies. Such applied studies draw upon the work of established methods of causal inference, and use logic, creativity, and argumentation to favor or to deny a certain causal effect. Logic and creativity are used to choose the appropriate method to estimate the causal effect, and argumentation is used to try to defend and convince others about the assumptions that underlie that method.

Definition of causal effects

Causal effects are in my view most easily defined using the potential outcome approach, which is often referred to as the Neyman (1923)-Rubin (1974) model. The idea behind the approach was first formalized by Neyman (1923), in his discussion about potential yields in the context of farming experiments. It was later extended by Rubin (1974) into a general causal model, also applicable for non-experimental, i.e. observational data. However, since the notion of causal effects has been around for a very long time, different authors are bound to be influenced by each other, and we will observe partial similarities between different approaches. Other contributions to our understanding of causal effects are Fisher (1926), Quandt (1958), and Roy (1951), to take some examples.

Consider the following question: what is the effect of an active labor market training program on the time spent in unemployment by the unemployed? The question consists of three essential parts: the population of units of interest, here the unemployed: the cause or treatment of interest, here the training program: and the outcomes for the units in the population associated with that cause, here time spent in unemployment. At first glance this question seems to make sense, but if we re-phrase it in terms of a hypothesis, it becomes less clear. One hypothesis is that participation in training decreases the time spent in unemployment among the unemployed who participate in the training. The hypothesis suggests that the causal question is not precise enough. What we mean is: what is the effect of training in relation to the effect of not participating in training? This forms the basis of the potential outcome framework definition of causal effects, namely, what is the effect of cause A, here training, relative to cause B, here being unemployed without training.

So far no potential outcomes have been mentioned, so for the sake of illustration let us return to the training example. For every individual who participates in training we can *observe* what happens to that individual, and record the time spent in unemployment. But obviously, we cannot observe what would have happened to that individual if (s)he had not been assigned to training. That does not, however, prevent us from logically thinking about such an outcome. Furthermore, before participation in training is assigned, both the time spent in unemployment under training and the time spent in unemployment under no training are both *potentially* observed. We can then think about two different outcomes or variables, called potential outcomes, that measure the outcome under cause A, and one variable measuring the outcome under cause B. The causal effect is then defined as the difference between these two potential outcomes.

One benefit with the potential outcome framework is that it allows us to directly understand the fundamental problem of causal inference. As we can never observe both potential outcomes at the same time for a given individual, it is impossible to observe the treatment effect of any single individual. Naturally we cannot assign one thing, observe the outcome, then rewind history, change the chain of events, assign the other cause and record the outcome. This does not mean, however, that we are unable to estimate interesting causal effects. It turns out that under certain assumptions one can estimate average causal effects. That is the mean effect for the population of interest.

Identification of causal effects

The second object of causal inference is identification of interesting treatment effects using data on the entire population. One key insight is that identification of all causal effects requires that one invoke some identifying assumption. Let us return to the active labor market program training example. Say that we are interested in the average treatment effect on those who receive treatment in form of training, defined as the difference between the average of the potential outcome under treatment for those who participate in treatment and the average of the potential outcome under no treatment for those who participate in treatment. Again, note that we can never observe the latter outcome: instead we can try to infer/estimate it. One thing we can observe is the average potential outcome under no treatment for those who do not participate in training. A naive approach would be to replace the average potential outcome under no treatment for those who receive treatment, with the average outcome for those who do not participate in training. However, there is in general no reason to expect that those who participate in training would have behaved in the same way without training as those who do not participate in training. Let us say that we observe that those who participate in training leave unemployment faster than those who do not participate in training. This could then be either an effect of (*i*) a positive training effect, or (*ii*) an effect that those who participate in training are a selected subset of individuals. Formally speaking we say that two different (often more than two) economic processes give rise to the identical probability distributions of the observable random variables (here time spent in unemployment). We label such processes as observationally equivalent.

The problem addressed in the second part of causal inference is to ask what restrictions that rules out certain explanations as inadmissible. In the present case *one* identifying assumption is to assume that in expectation the potential outcome under no treatment for those who receive treatment is in expectation equal to the outcome for those who do not participate in training. If this independence assumption holds we can distinguish between our two competing explanations, and say that the causal effect or treatment effect is identified. But this is not the only identifying assumption, and in many cases it does not hold. The second part of causal inference is therefore to figure out which assumptions have identifying power, in the way that they help us render some explanations inadmissible, that is which assumptions that make an explanation not in accordance with the data. In some cases many assumptions may be required, and in other cases only a few quite weak assumptions have strong identifying power.

As an illustration, say that we have randomly assigned, by flipping a coin, training to one group of unemployed and not to another group of unemployed. Since we flipped a coin we can expect the individuals who participate in training to be similar to those who do not participate in training. The above independence assumption will then be fulfilled. In other cases we do not have experimental data; instead we possibly observe the outcomes and choices of unemployed individuals. The key answer to the identification problem then lies in investigating the process by which the treated and non-treated are selected. In the words of the potential outcome framework, we need to model the assignment mechanism.

Estimation of causal effects

So far I have discussed interesting treatment effects, and how to identify them using hypothesized data on the whole population. In practice we need to use real world data - a sample of individuals - to estimate causal effects. We need to do two things: provide an estimate of the population parameters, and assess the uncertainty of these estimates taking sampling variation into account. Our model and assumptions can tell us that certain effects are identified, but they do not tell us how to estimate them using real world data. One approach is to estimate the population parameters using their corresponding sample average. As an illustration, in a randomized training example it amounts to replacing the expected time spent in unemployment for those who participate in training with the average time spent in unemployment for those who were randomized into training. The uncertainty in the estimation is taken into account using some feasible estimator of the variance.

Social insurance

The starting point for a discussion about social insurance must be the word insurance. The intention with social insurance is to provide economic protection against one or several adverse events (here unemployment, illness and disability.) As these types of events may have very wide economic effects, social insurance is a key component of modern economies. If covered by social insurance it is easier for individuals to avoid dramatic shifts in income and thereby consumption, and hence makes it easier for individuals to make long-term plans (see e.g. Gruber (1997), Browning & Crossley (2001) and Bloemen & Stancanelli (2005) for studies on consumption smoothing). From the individual's point of view a good coverage in terms of, for instance, a high replacement rate is thus preferable, as it provides more extended protection against adverse events.

The problem is that social insurance, like any other insurance, comes with costs in form of problems with asymmetric information and moral hazard. Two insights are important: the individual has more information about his/her actions than the provider of the insurance (asymmetric information), and social insurance changes the risks faced by the individual. The asymmetric information and the changed risk imply that the individual may be tempted to behave differently from the way they would behave if they were fully exposed to the risk. Consider car insurance: a person with insurance against automobile theft may be less cautious about locking his or her car, since the negative consequences of vehicle theft are (partially) the responsibility of the insurance company. Such moral hazard problems are also present in social insurance. One can distinguish between two types of moral hazard: ex-ante moral hazard and ex-post moral hazard. Ex-ante moral hazard refers to changes in behavior prior to the negative event. For instance, if covered by unemployment insurance individuals are less eager to avoid temporary lay-offs. Ex-post moral hazard refers to changes in behavior after the negative event has occurred. For instance, if covered by sickness insurance individuals may delay their return to work. These moral hazard problems have been the key focal point of many economic studies.[2] This literature has evolved over the years, going towards more and more elaborated econometric techniques, and more extensive data

[2]For surveys of the international literature on unemployment insurance see e.g. Atkinson & Micklewright (1991) and Holmlund (1998), and for disability and sickness insurance see e.g. Barmby et al. (2002). For Sweden see e.g. Carling et al. (2001), Bennmarker et al. (2007), Johansson & Palme (1996, 2002, 2005), Henreksson & Persson (2005), and Karlström et al. (2008).

sets. The overall conclusion is that high replacement rates and longer benefit durations (better coverage) increase the length of the insurance claims and increase the incidence of such claims.

In other words a good coverage in terms of for instance a high replacement rate increases individuals' protection against adverse events. It therefore promotes efficiency as it enables individuals to smooth consumption. However, a high replacement rate also introduces inefficiencies in form of moral hazard. It gives rise to an interesting trade-off, where too high replacement rate is inefficient since there will be too many insurance claims, and too low replacement rate is inefficient since it does not provide individuals with a good insurance coverage. So that the desired replacement level has to be dictated by social welfare considerations. For an early theoretical discussion see Diamond & Mirrless (1978) and Whinston (1983). Ideally one would like to take measures to improve on this trade-off. One way to do this is to use different types of policy instruments, which alter the incentives faced by the agents in the economy and/or to improve the social work norms in the society.

Incentives

Economic incentives to counteract moral hazard can be implemented through many different policy instruments. Some examples are a time limit on the duration over which the benefits could be collected, and a replacement rate that declines with the time the individual has been collecting the benefits. Both these instruments encourage individuals who have been claiming insurance benefits for a long time to more actively search for employment, or to return to work from a sickness absence or disability period.

Two other important policy instruments are monitoring and sanctions. Monitoring, in the form of for instance job-search requirements, makes relying on benefits less attractive for those who do not comply with the insurance rules. Sanctions, in the form of reduced or entirely withdrawn benefits, should punish those who do not comply with the rules. Monitoring and sanctions are present in most social insurance systems, and their effects have been studied quite extensively, for surveys see e.g. Fredriksson & Holmlund (2004), and Van den Berg & van der Klaauw (2005, 2006). Together monitoring and sanctions may have two effects: ex-post and ex-ante effects. The ex-post effect is the effect on those who actually experience a sanction. For example, we expect those who get a sanction in unemployment insurance to increase their search effort, and/or to decrease their reservation wage, in terms of lower wages and/or lower job security. The ex-ante effects on the other hand affect all unemployed individuals, merely as the result of the threat of being monitored and sanctioned. If properly designed monitoring and sanctions therefore have the possibility to counteract the moral hazard problem even with a high replacement rate. Optimally one would like to use monitoring and sanctions to increase the activity among social insurance beneficiaries without actually punishing anyone.

The policy instruments discussed so far are all intended to correct the incentives faced by the workers. Social insurance may also have adverse affects on the incentives faced by employers and thereby affect their behavior. If their workers are covered by social insurance they have less incentives to reduce temporary lay offs, and less incentives to improve the work environment, as they take into account that their workers can collect social insurance benefits anyway. This employer moral hazard problem has received considerably less attention compared to the worker moral hazard problem. However, by now there are several empirical studies documenting how experience rating, co-insurance and other instruments can mitigate the employer moral hazard. Experience rating means that the insurance tax rate each firm pays is adjusted upwards or downwards to reflect the costs of the insurance claims made by that firm's workers in the past. Co-insurance means that the firm pays parts of the insurance benefits. To be blunt, it should be costly to avoid investing in a good work environment, and costly not to monitor absence among workers.

Norms

Another important issue is social work norms, an issue which has received increased attention within the field of economics. In recent years a number of studies have addressed questions like: how does social insurance usage or welfare usage among your friends affect your own behavior? Does your productivity at work depend on the performance of your colleagues? Such social interactions, peer effects, or norm effects have for a long time been investigated in sociology, but are now also high up on the agenda of economists. To exemplify, there are at least four reasons why an individual's sickness absence behavior may be affected by the sickness absence behavior among the individual's reference group or network. First, health spill-overs may be important, as the health of one individual likely affects the health of other individuals. Second, if your friends are absent from work, unemployed or for some other reason do not work, the value of your own absence from work increases, as you can spend time with your friends. Third, information may play a role. If sickness causes you to be absent from work, you gain knowledge about the social insurance system, which is a prerequisite for utilizing the system. If individuals share this information it is another reason for social interactions. Fourth, there may be norm effects. In a country with high work norms, there may be a stigma associated with being absent from work. It is reasonable to believe that this stigma effect is directly related to how many individuals in your reference group that are absent from work. For all these reasons we expect individual absence to be affected by others' absence.

These and other social interactions and norm mechanisms have been studied in a number of different studies, see e.g. Ichino & Maggi (2000) on work absence social interactions, Bertrand et al. (2000) and Azier & Currie (2004) on welfare, Glaeser et al. (1996) on crime, Lundborg (2006) on health behaviors, and Falk & Ichino (2006), Bandiera et al. (2008) and Hesselius et al. (2009) on social interactions at the workplace. All these studies confirm the intuitive notion that human behavior is highly affected by the behavior of other individuals. This knowledge not only gives important insights into human behavior, it also has important policy implications. First, very strong work norms may prevent individuals from misusing the insurance despite generous benefits. It may simply be the case that the work norms make the moral hazard problem small even if replacement rates are high. Second, a policy that changes the behavior of some individuals will also through social interactions change the behavior of the friends/peers of these individuals. Norm effects or social interaction effects then work as a social multiplier which increases the total effect from a policy change.

The papers in the dissertation

Essay 1: Social Interactions in Work Absence: Empirical Evidence from a Natural Experiment[3]

In the social insurance introduction in the previous section I discussed that the inter-relations between individuals in form of social interactions or social norms are a quite new feature of economics research. This new attention is a step forward since such interaction effects have important policy implications. In this essay we contribute to the understanding of social norms by investigating whether individual sickness absence behavior is affected by the sickness absence behavior among a reference group. The social insurance introduction showed that there are several important social interaction mechanisms. Identification of social interaction causal effects is, however, complicated. As an illustration take the study by Ichino & Maggi (2000). They aim to explain why absence is so much higher in the southern part of Italy compared with the northern part of Italy. One can think of a number of different reasons why this may be the case: the income is lower in south, the industry structure is different in the north, certain individuals choose to live in the north, and the norms and attitudes towards being absent from may differ between the two regions. In short, it may be due to structural differences, because different types of individuals have sorted into the two areas, or due to social interaction effects due to differences in work norms. The identification problem lies in separating the social interaction effect from the other potential explanations.

[3]Co-authored with Patrik Hesselius, IFAU-Uppsala and Department of Economics,Uppsala University, and Per Johansson, IFAU-Uppsala and Department of Economics, Uppsala University.

Our method to investigate social interactions is directly motivated by this important identification problem. To address the identification problem in one arguably more convincing way we use a large scale randomized experiment. The size and timing of the experiment make it an unique feature in Swedish social insurance policy. The experiment was carried out in the second half of 1988 in Gothenburg municipality, the second largest city in Sweden. The purpose of the experiment was to determine whether and how work absence changed when monitoring of the insurance claimants was reduced. A randomly assigned (by date of birth) treatment group was allowed to receive sickness benefits for two weeks without showing a doctor's certificate, instead of one week as usual. Everyone in Gothenburg municipality, except central government employees, was exposed to the experiment.

We exploit the experiment in two ways: in a Difference-in-Differences (DID) analysis and in an instrumental variable analysis. In both analyses we focus on those who were not directly treated by the experiment, i.e. those who still had to submit a doctor's certificate after 7 days (from now referred to as the non-treated). In the DID analysis, we exploit variation in the proximity to the experiment. To exemplify: consider the non-treated who reside within the Gothenburg municipality. In general, without any social interactions there is no reason to believe that these individuals should increase their absence more during the experiment compared to individuals who also are non-treated but live outside Gothenburg municipality. We find that absence among the non-treated increases with the proximity to the experiment. Individuals in Gothenburg MSA (Gothenburg with neighboring municipalities) increase their absence more than the individuals outside of the MSA, and the absence increases the most among the non-treated individuals living within Gothenburg municipality. This pattern of an increasing effect with proximity to the experiment supports the social interactions hypothesis.

In the instrumental variables analysis, we focus more directly on specified groups. To exemplify: take two groups of close friends. These two groups will, depending on whether their network members live outside or inside the municipality, have different proportions of treated individuals in their network. If the non-treated who have many treated individuals in their network increase their absence more compared with the non-treated who have a low fraction of treated in theirs, this is evidence of social interactions. In contrast to the DID analysis, we need to define networks in this analysis. We let immigrants with the same country of origin living within Gothenburg MSA form a network. This approach of letting immigrants from the same country of origin form networks has previously been used in e.g. Bertrand et al. (2000) and Borjas (1992,1995). Note that we do not do that because we believe that immigrants are more affected by social interactions, but because country of origin is something unlike friendship relationships, that is recorded in administrative data sets. This approach works as long as immigrants from the same country have more in common than two random persons, something we find unproblematic to assume.

In both these two analyses we find large and statistically significant social interaction effects on non-monitored work absence. A 10 percent increase in the mean absence within the network would lead to a further immediate decrease in the hazard out of work absence by about 5.7 percent on average.

Essay 2: The Effect of Employer Incentives in Social Insurance on Individual Wages

In the social insurance introduction I discussed how social insurance can affect employers' behavior. If their workers are covered by social insurance, employers have less incentives to reduce temporary lay-offs, and less incentives to improve the work environment, as the government is responsible for paying social insurance benefits. I also mentioned that a number of previous studies have shown that experience rating, co-insurance and other instruments can mitigate this employer moral hazard by introducing a direct cost for employers when the insurances are utilized by their workers. In short, it should be costly to avoid investing in a good work environment, costly not to monitor absence among the workers, and costly to have an inefficiently high number of temporary lay-offs. These employer incentives may, though, have unintended side-effects. In this essay I investigate whether employer incentives in social insurance affect individual wages.

I estimate the individual wage effects of a reform in January 1992, which introduced employer co-insurance into the Swedish sickness insurance system. The sickness insurance replaces forgone income due to temporary health problems. Prior to 1992 the benefits were financed by uniform pay-roll taxes and all benefits were paid directly by the government. The reform in January 1992 gave employers' responsibility for bearing the full cost of all absences during the first fourteen days of each absence period. As the incidence of short-term absence varies substantially, the reform introduced high costs to the employer for some workers while leaving costs unaffected for other workers. Providing employers with direct economic incentives may affect employers' behavior in various ways. Besides taking action to decrease the take-up rates through improved work environment and/or intensified monitoring, employers have other ways to avoid the direct costs introduced by the co-insurance. They may shift over the costs to the workers by adjusting individual wages, giving insurance-prone workers lower wage increases. If such wage effects are present in health-related insurances like disability insurance and sickness insurance, workers with worse health will pay the employers' direct costs through lower wages. The direct costs also provide employers with incentives to engage more in cream-skimming, by avoiding hiring workers with worse health and firing workers with declining health status.

Such side effects are important from a policy perspective, but they have almost totally been ignored in the empirical literature. Four exceptions are Anderson & Meyer (2000), Hyatt & Kralj (1995), Thomason & Pozzebon (2002), and Harcourt et al. (2007). This is unfortunate, since all the effects of different incentives should be acknowledged when designing an efficient and fair insurance system. If co-insurance and other types of employer incentives negatively affect the wages of individuals with bad health this should be taken into consideration. This essay therefore aims to fill one gap in this literature.

From a causal inference perspective the identification problem is that there are other reasons why one can expect a close relationship between wages and sickness absence. In my econometric model I address two main problems. The first problem is that we can expect absence to affect individual wages even if there are no direct employer incentives. If you are absent from work, the firm may experience a production loss, there may be costs associated with finding a replacement worker, and the employer may believe that you are unwilling to put in a high effort into the work. The second problem is selection. It is reasonable to believe that individuals with high productivity and high ambitions have high wages as well as low sickness absence. The reform in January 1992 together with a very detailed data set enables me to address both these identification problems. I estimate the individual wage effects using a difference-in-differences strategy, which contrasts the wage increases before the reform and after the reform, between workers who are often absent and workers who are not so often absent from work. If the individual wage increase penalty resulting from being absent from work jump upwards at the same time as the reform, this is a sign of a treatment effect. Since I have very detailed information on each individual and can match each worker to their employer I can perform this analysis on a detailed level.

My results, interestingly show no evidence of any sizeable effect on individual wages from the employer co-insurance reform. This is not a result of lack of individual wage differences. The data reveals sizeable wage increase differences among workers within the same workplace, even after controlling for a rich set of control variables. The insignificant results are neither a result of large standard errors. Extensive robustness analysis also confirms the conclusion that there were no sizeable wage effects as a result of the reform. There are several possible explanations to these precisely estimated insignificant wage effects of the co-insurance reform. One is that even if firms paid no direct tax cost each time their workers were absent from work, employers have substantial indirect costs for absent workers. For example costs due to production losses and costs associated with finding a replacement worker. If these costs are very large the additional cost in the form of the co-insurance tax may be less important. Another possible explanation is that employers regulate their costs by firing or avoiding hiring insurance-prone workers. If employers cannot shift the co-insurance cost over to individual wages, they can avoid the co-insurance costs in this way instead. In other words the non-existent wage effects indicate that cream-skimming may have intensified as a result of the reform.

Essay 3: Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality[4]

The social insurance introduction described a trade-off when setting an optimal unemployment insurance replacement rate. A high replacement rate induces moral hazard which results in longer unemployment periods. A low replacement rate means less economic protection against the income loss associated with becoming unemployed. One way to change this trade-off is to introduce monitoring of unemployed workers and punitive sanctions for those who do not comply with job search requirements. One aim could be to use these measures to reduce the moral hazard problem and at the same time maintain a high replacement rate. In the long run sanctions may also be necessary to maintain confidence in the insurance. Unemployment insurance systems therefore typically include monitoring and sanctions (see e.g. OECD, 2000, for an overview). The effects of such sanctions have been investigated in for instance Abbring et al. (2005) for the Netherlands, Lalive et al. (2005) for Switzerland and Svarer (2007) for Denmark. All these studies document that punitive sanction increase the transition rate from unemployment to employment, thereby decreasing the unemployment rate. Sanctions may though have a wide range of other effects on the behavior and welfare of the unemployed, all of which need to be investigated in order to design optimal sanction procedures. This is the first study that performs a comprehensive evaluation of the effects of sanctions. We do this by studying sanctions in Sweden.

The previous studies examine the effect of a sanction on the transition rate from unemployment to employment; they do not however consider the effect of a sanction on the type of job accepted. From a welfare point of view as well as from the point of view of the unemployed individual job quality effects are important. If the job accepted after a sanction is similar to the job accepted in the counterfactual situation of no sanction, then severe sanctions and intensive monitoring have less adverse long-run effects than otherwise. To be clear: if sanctions only target individuals who misuse the system and thereby restore their search efforts, sanctions may have negligible welfare implications. On the other hand if sanctions force individuals who are already doing everything they can to find employment to take jobs of lower quality sanctions have more negative welfare effects. In our study we first confirm the results from previous studies that sanctions increase the transition rate from unemployment to employment. We also find that a sanctions decrease the wage in the subsequent employment with about 4 percent, and the probability of finding full-time em-

[4]Co-authored with Gerard J. van den Berg, Department of Economics VU Amsterdam, IFAU-Uppsala and IZA.

ployment decreases with about 15 percent. Taken together this implies that unemployed people who experience a sanction accept employment of considerably worse quality. We also investigate whether this is a short-term effect or a long-term effect. Our results indicate that sanctions indeed have long-term effects: those who experience sanctions continue to have jobs of lower quality three years after the exit from unemployment.

The essay also examines whether individuals make job acceptance decisions after a sanction that are more or less irreversible. Specifically, we observe the occupation of the accepted job, and we observe to what extent this differs from the occupation of the pre-unemployment job. On average, acceptance of a job with a lower occupational level involves a larger loss of human capital than acceptance of a job in the same occupation. This loss becomes irreversible as human capital depreciates over time. It may therefore be more difficult for the individual to move out of a bad job match if the job has a lower occupational level. This makes it important to know whether sanctions often lead to a match in a lower occupational level. By measuring the required number of years of education for each occupation, we can quantify the human capital loss due to the occupational downgrading caused by a sanction. Our results show that a sanction increases the likelihood that the unemployed accept employment within a less qualified occupation.

These are the most important contributions of this essay, but we also contribute to the literature in other ways. Before proceeding to these contributions, consider identification of the sanction effect. It poses a difficult causal inference problem. A sanction is imposed because the individual has committed an infringement. Sanctions are therefore not randomly assigned: only special types of unemployed individuals suffer sanctions. Suppose that we observe that the individuals who are sanctioned have relatively short unemployment durations then this could be for two reasons: (1) the individual causal sanction effect is positive, or (2) these individuals have relatively favorable unobserved characteristics and would have found a job relatively fast anyway. The second relation is a spurious selection effect. To control for such spurious effects we take selection on by us observed factors and by us unobserved factors into account. The latter is crucial as the processes leading to a job offer rejection are almost by definition nothing that can be observed in administrative data. To solve the selection problem we use the so-called timing of events framework, introduced by Abbring & van den Berg (2003). It is standard in this literature. The method derives its name from the fact that identification is based on the timing of events, i.e. of sanctions and of exits out of unemployment. Intuitively, what drives the identification of the sanction effect is the extent to which the moments of a sanction and the moment of exit to employment are close in time. If a sanction is quickly followed by exit to employment, no matter how long the elapsed unemployment duration before the training, then this is evidence of a causal effect of a sanction.

The essay also provides a theoretical and a methodological contribution. The theoretical contribution concerns two special features of the Swedish UI monitoring system. First, the monitoring of an unemployed individual is carried out by the same case worker who also provides job search assistance to the individual. Secondly, after inflow into UI, monitoring focuses on job offer decisions, in the sense that unemployed individuals are not supposed to reject suitable job offers. We develop and analyze a theoretical model with monitoring of job offer *decisions* in the presence of wage variation. The theoretical predictions can be contrasted to those from a model with monitoring of job search *effort* or search *intensity*. We use the theoretical model to understand our results, which contribute to the understanding of efficient policy. The methodological contribution is that we show that weighted endogenously stratified maximum likelihood (WESML) can be a useful tool when one is interested in studying rare events.

Essay 4: Cluster Sample Inference Using Sensitivity Analysis: The Case with Few groups

This essay addresses the third object of causal inference: estimation of causal effects using actual data taking sampling variation into account. In the essay I suggest sensitivity analysis as a new tool to take sampling variation into account when faced with so-called cluster samples. The new method is applied to the results of two well known studies. Let one of the applications serve as an introduction to the problem I address in the paper. In the first application I re-analyze the results in Meyer et al. (1995) (MVD), which study the effects of an increase in workers compensation benefits in the state of Kentucky. They study temporary cash benefits for work related injuries. The reform as of July 15, 1980, analyzed by MVD increased the maximum of level benefits that could be collected up to from \$131 to \$217 per week. The replacement rate was left unchanged. Hence, workers with previous high earnings (over the new maximum level) experience a 66% increase in their benefits, while the benefits for workers with previous low earnings (below the old ceiling) were left unchanged. In accordance with the discussion about moral hazard in the section on social insurance, we expect that the increased benefit level should increase the injury duration for the individuals in the treatment group. MVD analyze the effect of the reform using a difference-in-differences estimator, which contrast the difference in injury duration between before and after the reform for the treatment group (over the old ceiling) and the control group (under the old ceiling). If the injury duration increases more among the treatment group this is a sign of a treatment effect. Similar identification strategies are often used in economics, for surveys see e.g. Meyer (1995) and Angrist & Krueger (2000). One reason is that the difference-in-differences strategy has the possibility of offering transparent evidence.

The data set MVD analyze consist of several hundreds of observations for the two groups before and after the reform and they estimate a linear model using regular OLS. Their results suggest that the increased benefits increased the injury duration with about 19 percent. MVD calculate their standard errors based on the assumption that the outcomes for the individuals are independent from each other. In this case this among other things require that the time trend among the two groups is exactly the same, that is that there are no factors beyond the reform that change the relative injury duration between the two groups. Clearly this is restrictive: instead we expect strong intra-group correlation. Moulton (1990) have shown that such correlation may severely bias the regular standard errors. This inference problem often referred to as the clustering problem arises in many situations, for instance when having observations from a number of groups, for example families, regions, municipalities or schools.

The importance of the problem is also reflected in the growing number of studies addressing the inference problem.[5] One key insight from this literature is that the number of groups is important when deciding how to address the clustering problem. If the analysis sample consists of data from a larger number of groups, several solutions to the inference problem are available.[6] However, if the number of groups is few, the problem becomes more complicated. This occurs in many studies. Consider having data for men and women, for a couple of states, or from only a few schools or villages. Here one should think of gender, state and school as groups. In order to address this problem with few groups Donald & Lang (2007) introduced a between estimator based on data aggregated at group level. The problem is that their method only works as long as the number of groups is not very small. In the limit case with a just-identified model, for instance in the MVD example with a two groups and two time periods DID setting, no Donald & Lang (2007) inference is possible to perform.[7] Since no inference is possible to perform, we can neither rule out large positive effects nor rule out large negative effects. We may then be tempted to conclude that nothing could be learned from studies like the study by MVD.

As a response I propose to use sensitivity analysis in this setting. Design sensitivity analysis, or sensitivity analysis in short, has traditionally been used to test whether an estimate is sensitive to different kinds of selectivity bias, see e.g. Cornfield et al. (1959) and Bross (1966), further see e.g. Rosenbaum

[5] See e.g. Moulton (1986, 1990), Arrelano (1987), Bell & McCaffrey (2002), Wooldridge (2003, 2006), Bertrand et al. (2004), Conley & Taber (2005), Donald & Lang (2007), Hansen (2007*a*, 2007*b*), Ibragimov & Muller (2007), Abadie et al. (2007).

[6] The cluster formula developed by Liang & Zeger (1986), different bootstrap procedures (see e.g. Cameron et al. 2008), or parametric methods (see e.g. Moulton 1990).

[7] For more examples see e.g. Ashenfelter & Card (1985), Meyer et al. (1995), Card & Krueger (1994), Gruber & Poterba (1994), Eissa & Liebman (1996), Imbens et al. (2001), Eberts et al. (2002), and Finkelstein (2002).

& Rubin (1983), Lin et al. (1998), Copas & Eguchi (2001), Imbens (2003) and Rosenbaum (2004). As argued in the causal inference introduction, most applied causal inference studies based are on an un-testable assumption. The idea behind sensitivity analysis is to check how sensitive the results are to a violation of one un-testable assumption. If the results are very sensitive one should be more careful in drawing conclusions, whereas if the results are insensitive one can have confidence in the results.

My sensitivity analysis approach is similar, but nevertheless different in spirit. Under the assumption of no within group correlation standard normal i.i.d. inference based on disaggregated data is applicable. If this assumption is violated any standard errors based on the assumption of no within group correlation will be biased downwards. In the case of a just-identified model, for instance the data used in the MVD application, the analysis data does not contain any information on whether this assumption is true or not. So that there is an un-testable assumption and sensitivity analysis is a useful tool. Formally, I show that under certain assumptions the bias in the regular OLS standard errors can be expressed in terms of a few parameters, called sensitivity parameters. In the basic case the bias is expressed in terms of a single sensitivity parameter. The sensitivity analysis then amounts to assessing how much one can deviate from the assumption of no within group correlation before changing the standard error estimate by some pre-specified amount. That is to investigate how sensitive the standard errors are to within group correlation. The approach proposed in this paper is therefore similar to standard sensitivity analysis, since it also assesses how much one can deviate from an important assumption, but it is also different in spirit since it is performed with respect to bias in the standard errors and not with respect to bias in the point estimate.

Besides introducing a new type of sensitivity analysis, my paper contributes in several ways. It is applicable when the analysis sample consists of data from only a small number of groups. It even handles just-identified models. The new method is also able to handle different types of correlation in the cluster effects, most importantly correlation within the group over time and spatial correlation between groups. This is done by introducing several sensitivity parameters.

Essay 5: Bounds on Treatment Effects on Transitions[8]

This essay addresses the first and the second part of causal inference: how to define interesting and meaningful treatment effects, and identification of these effects using idealized data on the full population. In the paper we develop a new way to identify/bound certain average treatment effects when the outcome of interest is a transition from one state to another. As an illustration

[8] Co-authored with Geert Ridder, Department of Economics, University of Southern California.

we apply the new method to data from an American job-bonus experiment. The application serves as a good background to the type of questions that can be addressed using our method. Between mid-1984 and mid-1985, the Illinois Department of Employment Security conducted a controlled social experiment. The goal of the experiment was to explore whether bonuses paid to unemployment insurance beneficiaries reduces the time spent in unemployment. The bonus consisted of a $500 bonus payment (about four times the average weekly unemployment insurance benefit) paid to any unemployed who found a job (of at least 30 hours) within 11 weeks and who retained that job for at least 4 months. It gives the unemployed a direct economic incentive to quickly find a job. To investigate the effect of such bonuses the group of newly unemployed were randomly assigned into different groups; one treatment group was given the offer to collect the bonus, and one control group was given no bonus offer.[9]

The results from this experiment have been investigated in several studies. Woodbury & Spiegelman (1987) concluded that the claimant bonus group had significantly smaller average unemployment duration. As noted by Meyer (1996) this analysis is restrictive since labor supply and job-search theories suggest that the effect on the transition rate from unemployment to employment may vary over the unemployment period. We expect the transition rate from unemployment to employment to be higher for those who are given the bonus offer. As the 11 week deadline comes closer we expect this effect to increase, as the unemployed individuals are in a hurry to claim the bonus. After week 11 when all groups face the same incentives we expect no differences between the bonus group and the control group. In other words we are interested in the treatment effect dynamics, i.e. how the treatment effect varies with the time spent in unemployment.

Consider the causal inference identification problem of such treatment effect dynamics. Treatment is randomly assigned at the start of the unemployment period to one group and not to the other. In line with the coin-flipping argument in the causal inference introduction, this means that the two groups are going to be comparable. However, this only holds at the start of the unemployment period. At later times treated units with unobserved characteristics that have a positive interaction effect with treatment on the transition probability leave the initial state first/last. These unobserved characteristics are then under represented among the treated who are unemployed relative to the controls that are still unemployed. In short, some treated who leave unemployment would not have left unemployment if they had not received the job bonus offer, and vice versa. This means that after say two weeks there is no reason to expect that the treated who are still unemployed are comparable to the controls who are still unemployed. One main point of this essay is therefore that when the outcome of interest is the transition from one state to another only certain

[9]The experiment also included a third group for which any bonus was given to the employers who hired the unemployed.

average effects are non-parametrically *point identified* even under random assignment.

This confounding of the treatment effect by selective dropout due to transitions is usually referred to as dynamic selection. Existing strategies that deal with dynamic selection heavily rely on parametric and semi-parametric models. Two examples are the Mixed Proportional Hazard (MPH) analyzed by e.g. Elbers & Ridder (1982) and the threshold crossing model introduced in Heckman & Navarro (2007). The problem with both these approaches are that they rely on functional form restrictions (and other restrictions) to identify the treatment effects. The second main point of our paper therefore is that we ask what can be identified if the identifying assumptions of the semi-parametric models do not hold. What can we say about the treatment effect dynamics without imposing any restrictions beyond the random assignment. In our job bonus application what can we say about the treatment effect dynamics on the transition rate from unemployment to employment after 2 weeks or more. Formally, we allow for arbitrary functional form, require no additional covariates, and we allow for arbitrary heterogeneous treatment effects as well as arbitrary unobserved differences between the individuals. We derive sharp bounds on a number of interesting average treatment effects. By bounds we mean that even with information on all unemployed it would not be possible to point identify the average treatment effect, that is we can only say that the treatment effect is somewhere between A and B. Besides these general bounds we explore additional weak assumptions like monotone treatment response and monotone exit rate. We show that these weak assumptions may have strong identifying power.

In addition to the bonus experiment there exist many other settings where our method is applicable. Any situation, where treatment is randomly assigned (or unconfounded) either at the beginning of the spell or later during the spell and where the outcome of interest is a transition from one state to another. For instance, two medical treatments can have the same effect on the average survival time. However, for one treatment the effect does not change over time while for the other the survival rate is initially low, e.g. due to side effects of the treatment, while after that initial period the survival rate is much higher. Research on the effects off active labor market policies (ALMP), often documents a large negative lock-in effect and a later positive effect once the program has been completed, see e.g. the survey by Kluve et al. (2007). In other cases a treatment consist of a sequence of sub-treatments assigned at pre-specified points in time to the survivors in the state. If one is interested in disentangling the sub-treatment effects, the treatment effect over the spell has to be investigated.

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Essay 1: Social Interactions in Work Absence: Empirical Evidence from a Natural Experiment[1]

1 Introduction

'Illness is not merely a state of the organism and/or personality, but comes to be an institutionalized role' Parsons (1978).

This quote from Talcott Parsons, the father of medical sociology, illustrates that institutions and social norms have for a long time been considered as an important factor for individuals' perception of sickness or health in medical sociology.[2] It is reasonable to believe that social norms are also important in determining the usage of any health or sickness insurance.

The social norm is the implicit rules in the society or group that are determined by social interactions among the individuals in the same society or group. Hence, evidence that individuals' decisions are affecting each other (i.e. social interactions) suggest that norms are important. The importance of social interactions in the domain of sickness insurance has also been studied empirically. Ichino & Maggi (2000) studied the effects of social interactions on work absence by making use of job movers between branches in a large Italian bank. Lindbeck et al. (2007) used four different identification strategies to identify social interactions in Swedish sickness insurance. Both these studies find evidence of social interactions in work absence due to sickness.

We also estimate the effects of social interactions on work absence due to sickness. Our main contribution is that we make use of a large scale randomised experiment, conducted in the Gothenburg municipality. The treated employees were exposed to less control of their eligibility to use the sickness insurance, which sharply increased their work absence. We study whether this sharp increase among the treated affected the work absence among the non

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[2]Recently there has also been increasing interest in studying the effects of social interactions in economics. See e.g., Azier & Currie (2004) and Bertrand et al. (2000) for two studies on social interactions in public assistance; Glaeser et al. (1996) for a study on social interactions in crime; Mas & Moretti (2009), Falk & Ichino (2006) and Bandiera et al. (2008) for social interactions at workplaces, and Clark (2003), Conley & Topa (2002) and Topa (2001) for studies on social interaction among the unemployed.

treated. We study the effects on *non-monitored* (first seven days in a work absence spell) work absence. This non-monitored work absence is based solely on the individual's perceived health and not on judgments from a doctor or other official. It is therefore easy for the worker to adjust his/her short term work absence in accordance with how their peers take use of the more lenient monitoring. So that we expect strong social interaction effects.

We exploit the experiment in two different ways, which rely on different identification assumptions. In the first analysis is effects of social interactions estimated using difference in differences (DID) estimators. In the second analysis we formulate a theoretical model in which the group mean influences the individual work absence decision (i.e. an endogenous social interaction model). The endogenous effects are estimated using instrumental variables estimators. For both analyses, the experiment enables us to solve the reflection problem, i.e. the problem that individual absence also affects the group behaviour, by studying effects from the experiment on the non-treated individuals. It is worth emphasising that the intervention decreases the control of an individual's eligibility, which does not affect individual's health on a short-term basis. This accordingly allows us to disregard health spill-over effects that, indeed, could be a problem with other types of more or less exogenous chocks in work absence due to sickness (e.g. accidents).

In the DID analysis, we exploit variation in the proximity to the experiment, and in order to control for sorting into neighbourhoods we compare sub groups of non-directly treated within the Gothenburg metropolitan statistical area (MSA) to individuals living in municipalities outside of this MSA in terms of change in work absence between the first and second half of 1988 (i.e. before and during the experiment).[3] We find that non-treated individuals living in Gothenburg municipality (i.e. the experimental control group) increased their non-monitored work absence more than the individuals outside of the MSA. This increase is also larger than the increase for a group living within the MSA but outside the Gothenburg municipality. This pattern of an increasing effect with proximity to the experiment confirms the social interaction hypothesis.

In the instrumental variables analysis, we solve the reflection problem by using the fraction of treated in a network as an instrument for a network's absence. Thus, if the work absence among non-treated changes more in networks with more treated than in networks with less treated this suggests effects from norms. In contrast to the DID analysis, we need to define networks. We let immigrants with the same country of origin living within Gothenburg MSA form a network. This is the same strategy as in Bertrand et al. (2000), where the definition of a network builds on previous work by Borjas (1992, 1995), who has shown that ethnic capital is important.

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[3]Gothenburg MSA and bordering municipalities are described in detail in Section 3.

The identifying variation we use in the instrumental variables analysis (as well as in the DID analysis) is that the experiment was conducted in the Gothenburg municipality and not in the Gothenburg MSA. This means that immigrants living within Gothenburg MSA will have different proportions of directly treated in their network, depending on how many in their network live within or outside of Gothenburg municipality respectively. After controlling for general network heterogeneity, using data from prior to the experiment, our instrument is valid unless there are trends or health shocks correlated with our instrument. Since we have detailed data on daily sickness absence for all individuals in Sweden for the period 1987-1989 this allows for extensive sensitivity analyses. We perform, among others, placebo regressions: with the result that the exclusion restriction seems valid.

The main result from the instrumental variables analysis is that we find a large and statistically significant endogenous social interaction effect on the non-monitored work absence. A 10 per cent increase in the mean absence within the network would lead to a further immediate decrease in the hazard out of work absence by about 5.7 per cent on average. The long-run (equilibrium) effect on the hazard is estimated as 13.3 per cent on average.

The remainder of the paper is organised in the following way: The Swedish sickness insurance system and the randomised controlled experiment conducted in Gothenburg are explained in Section 2. Section 3 describes the data, and Section 4 provides results from the DID estimations. The theoretical framework for the instrumental variables estimation is outlined in Section 5. The empirical results for the instrumental variables estimator are presented in Section 6. Finally, Section 7 offers conclusions.

2 Institutions and the experiment

Sweden has compulsory national sickness insurance. It is financed by a proportional payroll tax, and replaces earnings forgone due to (temporary) health problems that prevent the insured worker from performing their regular work tasks.

Sickness benefits from the public insurance are, and have been, in an international comparison, generous: in 1988 most workers received 90 per cent of their lost income from the first day. A benefit cap excluded workers at the very top of the income distribution from receiving the full 90 per cent.[4] The public insurance further has no limit to how often or how long benefits are paid. Many such sickness spells continue for more than a year, and there are

[4]Most Swedish workers were, however, also covered by negotiated sickness insurance programmes regulated in agreements between the labour unions and the employers' confederations. In general, these insurances replaced about 10 per cent of the forgone earnings, which yielded that the actual replacement rate was 100 per cent for many workers.

examples of even longer durations. These extended spells end mostly in disability insurance, early retirement, or in old age retirement.

The public insurance does not verify claimants' eligibility during the first benefit week. At the start of a spell, the worker has to call the public social insurance office (and their employer) to report sick. Within a week, at the latest on the eighth day of sickness, the claimant should verify eligibility by showing a doctor's certificate that proves reduced work capacity due to sickness. The public insurance office assesses the certificate and decides about further sick leave. Some exceptive rules make it possible for the public insurance office to monitor more (or less) strictly.[5]

2.1 The randomised experiment

The experiment was carried out in the second half of 1988 in Gothenburg municipality, the second largest city in Sweden.[6] It was initiated by the local social insurance office.[7] The purpose of the experiment was to determine whether and how work absence was changed when monitoring of the insurance claimants was reduced. A randomly assigned treatment group was allowed to receive sickness benefits for two weeks without showing a doctor's certificate, instead of one week as usual. The randomisation was performed by using date of birth. Everyone in Gothenburg municipality was exposed to the experiment, except central government employees.[8]

The experiment was a non-blind experiment, in that all were informed about it in advance or at the latest during the experiment. In fact, it was preceded by local information campaigns. Besides the personnel at the local social insurance offices, all employers and medical centres were informed in advance about the set-up of the experiment. Mass media were also an important channel for informing the insured population.

Figure 1 shows the survival rates for the treated and non-treated employed immigrants living in Gothenburg for the half year before the experiment was run, while Figure 2 displays the corresponding survival rates during the expe-

[5]In a case where they suspect abuse, they can visit the claimant at home. Claimants who have been on sickness benefits ten times or more during the past year may be asked to show a doctor's certificate from day one. Moreover, a new sick spell starting within five working days from the first is counted as a continuation of the first, making it impossible to report sick every Monday (and return 'back to work' for the weekends) without ever visiting a doctor.

[6]The same experiment was conducted in Jämtland, a large county in the sparsely populated northern part of Sweden. There are few immigrants in the area; we therefore only use the experiment in Gothenburg in our empirical analysis.

[7]Until recently, the public insurance was administered by 21 independent local social insurance offices that were quite free to design exceptions from the general rules (as long as they were towards more generosity). Today, the administration is centralised.

[8]Government employees were exempted, as they, by law, receive their sick leave compensation from the employer instead of from the social security office. The employer, in turn, receives the benefit from the social security office.

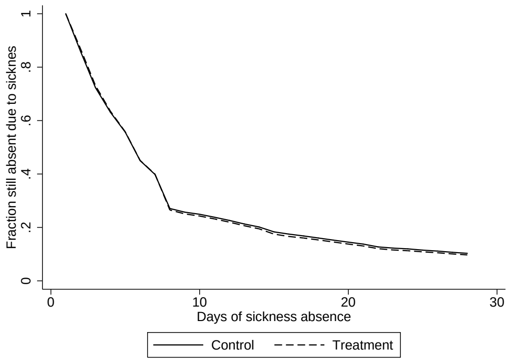

Figure 1: Fraction still absent due to sickness during the period 1 January 1988 - 30 June 1988 for employed immigrants living in Gothenburg. See Section 3 for more details on data.

riment. From these figures it is clear that there are no differences in the pre experiment work absence between the two groups, and that the relaxed monitoring had a large direct impact on the treated work absence behaviour.[9] The result in Hesselius et al. (2005) is that the treated increase their duration of absence by 0.6 days on average. No significant effect on incidence into work absence could be found, however. It should also be noted that this effect cannot be an effect of health shocks, since there is obviously no reason to expect such impacts which only affect individuals born on an even date.

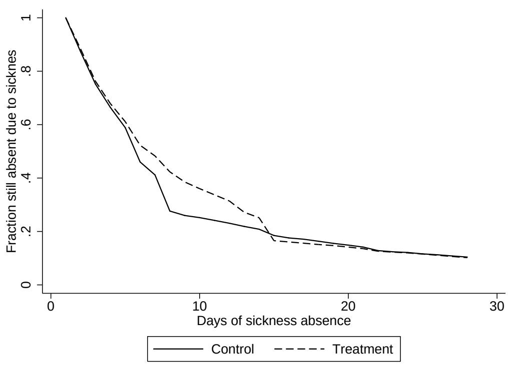

Figure 2: Fraction still absent due to sickness during the experiment period 1 July 1988- 31 December 1988 for employed immigrants living in Gothenburg. See Section 3 for more details on data.

[9]The corresponding figures for all employed individuals in Gothenburg are given in Hesselius et al. (2005). In addition they show that there are no significant differences between the treatment and control groups with respect to other observed characteristics (e.g. income and gender).

3 Data

Our data is taken from Statistics Sweden's RAMS database, to which we have matched data on work absence from the Swedish Social Insurance Agency (SSIA). The RAMS database is a population register which includes a set of socio-economic variables (e.g. age, sex, income, immigration status and employment status). It also includes information on country of birth, which we use to define ethnic groups. The work absence data covers all absence periods for which sickness benefits are paid, and because SSIA at the time of the experiment replaced forgone earnings due to work absence from day one, it is a complete register of all work absence.

3.1 Sample selection

In the analysis, we use two different samples. In the DID analysis we restrict the population to employed individuals living in the Gothenburg MSA and bordering municipalities in 1988. In the instrumental variables analysis, we use only employed immigrants. The immigrant data set includes immigrants from 83 countries, who had more than 10 network members in Gothenburg MSA in 1988. The largest immigrant group is from Finland; other large immigrants groups are from the other Nordic countries, Hungary, former Yugoslavia[10], Poland, Germany, Iran, Estonia, Turkey and Chile. For information on the variation in work absence and a few socio-demographic characteristics of the immigrant groups see A.1 in Appendix A.

The Gothenburg MSA is a homogenous local labour market, defined by Statistics Sweden, including 13 municipalities[11] in the area around Gothenburg municipality. In 1988, the MSA had a total population of 428,730 individuals between 20 and 60 years of age, and 59,152 of those were immigrants. The municipalities are of different sizes, from the smallest, Öckerö, with 5,487 individuals between 20 and 60 years of age to the largest, Gothenburg, with 242,447 individuals in the same age span.

We use the employment register (in the RAMS database) to identify working individuals. An individual is included in the analysis if he or she works for at least 5 months in the first and second half respectively of 1988, and is between 20 and 60 years old, excluding the self-employed, farmers and seamen.

[10]It should be noted that almost all immigrants from the former Yugoslavia were from Serbia see e.g. Magnusson (1989). Therefore, no ethnic conflict should exist within this immigrant network. In addition, as sensitivity analysis we removed the former Yugoslavia from the analysis, and it did not change our results.

[11]The municipalities are Ale, Alingsås, Gothenburg, Härryda, Kungsbacka, Kungälv, Lerum, Lilla Edet, Mölndal, Partille, Stenungsund, Tjörn and Öckerö.

4 Difference-in-Differences analysis

Only those born on an even date living in Gothenburg municipality were exposed to the randomised experiment. However, if social interactions affect work absence, the work absence of the control group in Gothenburg would also be affected by the experiment. In addition, since Gothenburg MSA defines a local labour market, we expect that individuals in the whole Gothenburg MSA to be affected by the experiment. However, since we can expect social interactions to decrease as the proximity to the experiment decreases we expect a smaller effect for this second group. In this analysis we focus on work absence up until day 14 of every absence period (from now on refereed to as short term work absence). The reason for this is that the experiment only affected work absence periods shorter than 15 days.

In order to control for sorting and selection we compare the individuals' changes in work absence and make use of a DID strategy, where the comparison group is individuals living in 27 municipalities bordering Gothenburg MSA (the municipalities used in the estimations are described in Figure A.1, in Appendix A.).[12] Hence, if the change in work absence between the first and second half of 1988, i.e. between before and during the experiment, is higher for non-treated individuals within the Gothenburg MSA as compared with individuals living outside this area, this is evidence of social interactions in work absence.

The results of the estimations are presented in Table 1. Columns 1- 4 show the half-year mean work absence in days for respectively groups during 1987 and 1988, and Columns 5 - 6 show the half-year difference for the two years for respective groups. Column 7 displays the DID estimates, which show the expected pattern: in comparison with the control area, the directly treated increase their short term work absence by 0.83 days (or 10.5 per cent), the non-directly treated in Gothenburg municipality increase their short term work absence by 0.37 days (4.5 per cent) and the individuals in Gothenburg MSA, excluding Gothenburg municipality, increase their absence by 0.19 days (2.7 per cent).

The social interaction effect on the non-treated within Gothenburg municipality is estimated to be 0.37. If we divide this by the direct effect (0.46 = 0.83-0.37) we obtain a social multiplier effect of 0.8. That is, if the mean sickness absence exogenously increased by one per cent, then due to the social multiplier the work absence would increase by a further 0.8 per cent.

The identifying assumption for the DID strategy is violated if the seasonal patterns and/or the long-term trends differ between the groups. However, since we have access to sickness absence also before the experimental year, we

[12]Note that this area may also be affected by the experiment; however, for our purpose of identifying social interactions, this does not matter. All that matters is that individuals living in this area should be affected to a lesser extent than non-treated individuals living closer to the Gothenburg municipality.

Table 1: *Mean short term work absence (days) in 1988 and 1987 (spring and autumn), and the estimates from DID and DIDID estimations*

	Spring 88 [1]	Autumn 88 [2]	Spring 87 [3]	Autumn 87 [4]	Diff.88 [2-1] [5]	Diff.87 [4-3] [6]	DID 88 [7]	DIDID [8]
Gbg municipality	7.15	8.52	7.95	6.87	1.37	-1.08	0.83**	0.87**
-Treated	(0.044)	(0.047)	(0.047)	(0.043)	(0.048)	(0.048)	(0.052)	(0.074)
Gbg municipality	7.15	8.06	8.12	6.94	0.91	-1.19	0.37**	0.53**
-Non-treated	(0.044)	(0.045)	(0.046)	(0.043)	(0.047)	(0.048)	(0.051)	(0.073)
Gbg MSA excluding	6.16	6.90	6.84	5.94	0.73	-0.91	0.19**	0.073
Gbg municipality	(0.033)	(0.033)	(0.033)	(0.031)	(0.036)	(0.036)	(0.041)	(0.058)
Control area	6.47	7.01	7.12	6.09	0.54	-1.03		
	(0.018)	(0.019)	(0.19)	(0.018)	(0.020)	(0.020)		

Notes: The Gothenburg MSA consists of 13 municipalities. Individuals living in 27 municipalities bordering Gothenburg MSA are used as controls. Standard errors robust to within municipality correlation in parentheses. * and ** denote statistically significant results at the 5 and 1 per cent levels, respectively.

can control for the difference in seasonality pattern and trends by subtracting the corresponding (autumn - spring) difference in sickness absence in 1987 from the original DID estimate in a Difference-in-Difference-in-Differences (DIDID) estimation.

The results from these estimations are displayed in Column 8 in Table 1. We obtain the same pattern as before: all three groups have increased their sickness absence in comparison with the control group. As before, the estimates are for the untreated and treated within Gothenburg, but the estimates for the individuals in Gothenburg MSA, excluding Gothenburg municipality, have reduced to an increase with only 0.073 days (0.4 per cent), and this estimate is not statistically significant. Hence, the pattern of gradual decreasing social interactions in the Swedish social insurance is less clear when using the DIDID analysis. However, the effect for the non-treated in the Gothenburg municipality still remains, which suggests that social interactions are important in the Swedish sickness insurance system.

In order to learn more about the social interactions, a theoretical framework that enables instrumental variables estimations is set up in the next section.

5 Instrumental variables estimation

The theoretical framework builds on the work by Brock & Durlauf (2001*b*). The starting point is a regular labour supply model, in which work implies increased monetary income as well as a utility loss in the form of forgone leisure. We also introduce a deterministic social cost of being work absent from work. It introduces a social interaction effect, which causes the individual absence to depend on the group absence. An individual is assumed to work if the utility from work is greater than the utility from being absent from work. Under these assumptions (see Appendix B for details) we obtain that the probability of an individual i in network j to be absent is given as:

$$\Pr(d_{ij} = 1) = h(w, b, \pi_j) = \Pr(\varepsilon_{ij} \geq J + (1-b)w - a_j - J\pi_j), \tag{1}$$

where ε_{ij} is the effort of working, π_j is the mean absence rate in network j, w is the wage, b is the replacement rate, a_j is the baseline value of leisure for individuals in the network, and finally J represents the weights individual i gives to the interaction between individuals in the same network (i.e. the endogenous social interaction effect).

Note that this is a non-linear relationship, which, hence, implies that the marginal effect of an exogenous shift in the mean level, in general, depends on the level of sickness absence.

5.1 Identification and estimation

In this section, we discuss the empirical identification[13] of our model, i.e. of J, and present the model used to estimate the endogenous social interaction effect on work absence. Our interest is in the incidence (into work absence) and duration of work absence.

The empirical identification problem of J consists in that: (i) π_j and a_j are dependent and (ii) d_{ij} affects π_j. That is: (i) the selection problem that there may be unobserved effects that are driving differences in both the individual absence and differences in the group absence. Individuals within the same network tend to have similar characteristics, so that this is an important problem to take into account. Since we have data on work absence prior to the experiment, this enables us to control for such network heterogeneity using fixed effects. Moreover, (ii) the reflection problem that individual absence also affects the group absence. This second problem is solved by estimating the effects on the non-treated work absence, where the experiment is used as an instrument.

5.2 The experiment as an instrument

We assume that the immigrants within the Gothenburg MSA interact. This is a reasonable assumption, considering that Gothenburg MSA is a common labour market, and since there are many ethnic associations and religious mee-

[13] See e.g. Manski (1993, 2000), Brock & Durlauf (2001*a*, 2001*b*, 2007), Graham (2008), Graham & Hahn (2005) and Moffit (2001) regarding the requirements for empirical identification in linear and non-linear models.

ting places within Gothenburg MSA. Since the experiment was conducted only in the Gothenburg municipality this creates a variation in the number of treated within the network as long as immigrants are not perfectly sorted; respectively within and outside of the Gothenburg municipality boarder.[14] We find a large variation in the proportion of treated: ranging from 14 to 59 per cent (for details see Table A.1 in Appendix A).

If there are social interactions with regard to functioning of the insurance, the work absence rate in network j should be affected by the proportion treated in network j, P_j. However as previously mentioned, because of the non-linearity it is evident that effects from P_j should depend on the level of work absence. Taking the work absence before the experiment as a proxy for the level, the first step linear projection is therefore specified as:

$$\pi_{jc} = \beta_j + \beta_c + \beta P_j \pi_{jc-1} D_c + \eta_{jc} \tag{2}$$

where $c = 1$ in the first half-year and $c = 2$ in the second half-year of 1988. Further, D_c is an indicator variable taking the value of one in the second half-year. Note that since we have a fixed effect for the network, β_j , this prohibits us from at the same time controlling for the lagged level of absence, π_{jc-1} , in the regression. We choose π_j to be the mean proportion of days absent for each half-year. The inferences on the social interaction effect are robust to these specifications.

We chose $\pi_{jc}, c = 1,2$, to be the mean proportion of days absent for each half-year. Since the experiment only affected spells shorter than 15 days, we censor spells longer than 15 days in the calculation of π_{jc}.

The first step estimates from the estimation of equation (2) using weighted (by square root of network size) least squares (WLS) are presented in Table 2. The left and right panels provide results for two different definitions of the network size: The main results are given in the first row, from which we can see that the work absence is, as expected, positively affected by the instrument. The effect is statistically significant and of the same magnitude for the two network definitions. Thus the results are insensitive to exclusion of the smallest immigrant groups.

Our model includes network fixed effects, but the exclusion restriction will be violated if there are any seasonal patterns and/or any long-term trends which differ between the groups and which are correlated with the proportion of treated in the networks. We informally test for this by estimating placebo effects. We assume that the same intervention as in 1988 took place in July-December 1987, July-December 1989 and in the Stockholm[15] MSA in July

[14] Since the central government employees were exempted from the experiment, this in addition creates a small variation in the fraction treated between the networks.

[15] Stockholm is the capital of Sweden and the largest city in Sweden. The reason for this choice is that the origin of the immigrants in Stockholm MSA are very similar to the origin of immigrants in Gothenburg MSA and, as for the Gothenburg MSA, the number of immigrants is increasing with distance from the Stockholm municipal centre. We therefore consider a pla-

Table 2: *First step estimates (WLS) (proportion of treated multiplied by lagged group mean level of sickness as an instrument for mean absence)*

	$NS > 10$			$NS > 30$		
	Estimate	Std error	No. Obs.	Estimate	Std error	No. Obs.
1988	0.500**	0.112	1081	0.492**	0.114	855
1987	-0.164	0.086	1029	-0.110	0.090	829
1989	-0.044	0.157	1103	-0.037	0.163	873
Stockholm 1988	-0.268	0.315	1449	-0.233	0.334	1118

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively. Group sizes outside the limit are excluded from the sample. Weighting is by square root of network size.

1988, and then we estimate the same regressions as for 1988. The results from these estimations are given in rows 2 to 4 of Table 2. We find no statistically significant associations. In other words these placebo estimations suggest that the instrument is excludable from the non-treated individuals' outcome equation in the absence of the intervention.

Another concern with our analysis is that the networks are incorrectly specified: for example, one could argue that it is not plausible to assume that the immigrants interact with all persons in the ethnic group. It is then important to note that within each specified network there is a probability, P_j, of having a treated network member. This feature (of our instrument) provides us with a better situation for identification than if certain groups had been targeted for the experiment (e.g. if older age workers or females had been targeted). We have the following result: under *randomisation* within the network (inclusion probability P_j), then the endogenous social interaction effect is identified if: (1) the true network is a (s)ubgroup of the specified network and (2) the level of work absence is the same for these subgroups (i.e. $\pi_{jc} = \pi_j$ for all s).

Assumptions (1) and (2) imply that the marginal endogenous effects on individual work absence are the same for all subgroups (see equation (2)). Under randomisation within specified networks, each non-treated individual will in expectation have the same fraction of treated individuals in his or her subgroup and this enables identification of the (common) endogenous treatment effect, even if we miss-specified the true network of the individuals.

5.3 Modeling duration and incidence

When studying norm effects on duration of work absence, potential duration dependence needs to be addressed. Such correlation is highly likely considering that disutility of work is mainly driven by variations in health, which

cebo experiment, under assumption of placebo treatments of the individuals born on even dates within Stockholm municipality.

is arguably correlated over the duration of work absence (t). To this end, we simply assume that the hazard out-of-work absence spell, that is the discrete decision to work or stay home, can be formulated as a logit hazard regression model.[16] Hence:

$$\Pr(d_{ij,t} = -1 | d_{ij,t-1} = 1) = (1 + \exp(-(\alpha_j - \gamma_1 \pi_j - \delta_t^*)))^{-1}, \tag{3}$$

where $\alpha_j \simeq (\bar{w}_j - a_j)/\sigma$,$\delta_t^* \simeq \delta_t/\sigma$, $\bar{w}_j$ is the average income replacement from work absence in network j, and σ is the standard deviation of the logistic distribution. In this model, the complete set of parameters is not identified. J is only identified up to scale, and hence only $\gamma_1 = J/\sigma$ is identified. By aggregating the hazards over the individuals in each network, we obtain:

$$\ln\left(\frac{h_{jc}(t)}{1 - h_{jc}(t)}\right) = \alpha_j + \alpha_c + \delta_t^* + \gamma_1 \pi_{jc}, \qquad c = 1,2, j = 1,\ldots,N, \tag{4}$$

where $h_{jc}(t)$ is the population average hazard rate out-of-work absence at day t in network j in half-year c, and N is the number of networks. Thus, α_c reflects common time trends.

The aggregated incidence of work absence can be formulated in a similar fashion. Here, we (for good reasons) ignore the duration dependence (i.e. $\delta_t = 0$ for all t), which leaves us with:

$$\ln\left(\frac{p_{jc}(t)}{1 - p_{jc}(t)}\right) = \alpha_j + \alpha_c + \alpha_1 \pi_{jc}, \tag{5}$$

where p_{jc} is the fraction of the individuals in network j in calendar time period c entering work absence due to sickness each day.

5.4 Estimation and descriptive statistics

The population hazard rates and incidence, specified in equation (4) and equation (5), are estimated from the networks' mean hazard rates and the incidence in each network. In order to separate social interaction effects from the direct monitoring effect, we focus on non-monitored work absence. We therefore censor each work absence spell at day eight, i.e. the day when the non-treated have to submit a doctor's certificate.[17] Hence, for the hazard rate, we have fourteen outcome values for each network: seven before and seven during the experiment. With this aggregated data, it is straightforward to estimate (4) and (5) using two stage least squares (2SLS) estimators. We increase efficiency in the estimation by weighting with the square root of the network size. In addi-

[16] We have also estimated linear probability models. The results shown in Section 6 are not qualitatively different when using linear probability models. We take this as evidence that the results are not pertinent on the functional form.

[17] We have also estimated the models with 14, 21 and 28 days before censoring. The parameter estimates change; however, the elasticity estimates are robust to the day of censoring.

Table 3: *Average hazard rates from work absence*

	1988 $NS > 10$	1988 $NS > 30$	1987 $NS > 10$	1989 $NS > 10$	1988 Stockholm $NS > 10$
No. agg. obs.	1,081	855	1,029	1,103	1,449
Mean hazard					
Day 1	0.16	0.15	0.10	0.17	0.17
Day 2	0.18	0.17	0.13	0.19	0.20
Day 3	0.15	0.15	0.13	0.16	0.17
Day 4	0.15	0.14	0.14	0.14	0.16
Day 5	0.21	0.20	0.14	0.25	0.22
Day 6	0.13	0.13	0.16	0.13	0.15
Day 7	0.32	0.33	0.42	0.36	0.37

Notes: *NS* is the network size. Groups with group sizes outside the limit are excluded from the sample.

tion, inference is made robust with respect to heteroscedasticity and correlation between the hazard rates within each network.

Descriptive statistics for the duration and incidence data sets are displayed in Table 3 and Table 4. Table 3 shows that the hazard rate from work absence to work is quite constant at approximately 15 per cent for the first six days in an absence spell. At day seven (when a doctor's certificate is normally required), there is a sharp increase in the mean hazard rate.

The year 1987 differs from the other two years, with a lower initial hazard from a work absence spell. The reason for this difference is most likely that there was a qualifying day in the insurance which was removed on December 1 1987.[18] Hence, sick listed individuals at day one in 1987 probably had worse health than sick listed individuals in 1988 and 1989 on average. There are however no systematic differences with respect to populations (size of networks) and city (Gothenburg/Stockholm), nor any large differences between 1988 and 1989 within the Gothenburg MSA.

From Table 4 we can see that the incidence, measured as number of started absence spells, into work absence is approximately 0.8 per cent in 1988 for both half-years. The incidence is as expected (from the qualifying day) lower in 1987 than for the other years. The incidence is somewhat lower for the second half-year in 1987 and 1989. These half-year differences are, however, not statistically significant.

[18]The income replacement for the first two weeks also differs in 1987 from that of 1988 and 1989.

Table 4: *Mean work absence incidence for the 1988 population and the populations used in the sensitivity analyses.*

	1988		1987	1989	1988
	$NS > 10$	$NS > 30$	$NS > 10$	$NS > 10$	Stockholm $NS > 10$
No. agg. obs.	166	124	166	170	218
Mean incidence					
Jan.–June	0.0077	0.0074	0.0067	0.0079	0.0089
July–Dec.	0.0079	0.0078	0.0059	0.0050	0.0093

Notes: *NS* is the network size. Groups with group sizes outside the limit are excluded from the sample.

6 Results

6.1 Hazard rates

The results from the 2SLS estimation are presented in Columns 1 and 2 in Table 5, together with the reduced form estimate. The 2SLS estimate is negative and statistically significant at the 5 per cent level. The effect from an exogenous shift in work absence level by one percentage point would lead to an immediate reduction in the hazard rate from a work absence spell of 0.035 on average. Evaluated at the mean work absence (2.68 per cent) and mean hazard (0.165), this implies an elasticity of -0.57. Hence, an exogenous shift in the mean absence by 1 per cent would lead to a shift in the hazard rate by 0.57 per cent, on average.

Table 5: *Hazard and incidence regressions, reduced forms and 2SLS estimates for 1988. Excluding individuals in ethnic groups with ten members or fewer.*

	Hazard rate		Incidence	
	Estimate	Std error	Estimate	Std error
Reduced	-0.138**	0.046	0.011	0.031
2SLS	-0.275**	0.103	0.026	0.838
Marginal effect	-0.035		0.0002	

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively. Group sizes outside the limit are excluded from the sample. WLS estimates, weighting with the network sizes.

In other words our hazard rate results indicate strong endogenous social interaction effects. Before proceeding to a more detailed robustness analysis we explore two potential threats to our identification strategy. In both of our strategies we make use of the geographical variation for identification. A potential threat, although unlikely, to both strategies is a random health shock in the second half year that affect individuals living in the Gothenburg municipality more than individuals living outside of the municipality. Another potential threat is that the non directly treated living within the Gothenburg municipality are affected by an information shock. The experiment was not blind: instead it was preceded by quite massive local information campaigns. Some of the non directly treated may have been confused by this information and incorrectly thought that they were subject to the new rules. If that is the case the information campaign may have affected the work absence behaviour of the non directly treated, so that information shocks rather than social norms would explain our results.

Table 6: *Hazard regressions separately for the non-treated living within the municipality and outside the municipality*

	Only inside			Only outside		
	Estimate	Std error	No. obs.	Estimate	Std error	No. obs.
First step	0.492**	0.112	788	0.531**	0.105	1,035
Reduced	-0.167*	0.066	788	-0.203*	0.084	1,035
2SLS	-0.340*	0.135	788	-0.381	0.205	1,035

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively.

In order to study these two potential problems we estimate our regression models separately; for the non-treated that live within Gothenburg municipality and for those who live outside the municipality but within the MSA. If health shocks or information shocks explain our results we expect to find an effect only for the non-treated living within the municipality. On the other hand if social interactions explain our results we expect to find that non-treated within the municipality and non-treated outside the municipality with the same proportion of treated in their network behave in the same way. The results from the two separate hazard regressions are presented in Table 6. We find strikingly similar results for the two groups. The point estimate is actually larger for those living outside the municipality. We therefore conclude that it is very unlikely that either health shocks or information shocks could explain our results.

6.2 Incidence

The results from the reduced form and 2SLS estimation are given in Columns 3 and 4 in Table 5. As expected, we find a positive effect on the incidence. The estimate is, however not statistically significant. One potential explanation for the non-significant results is that individuals on sick leave interact mainly with each other.

Table 7: *2SLS estimates using different instruments and for different samples*

	Hazard rate			Incidence		
	Estimate	Std error	No. obs.	Estimate	Std error	No. obs.
$NS > 10$	-0.275**	0.103	1,081	0.026	0.084	166
$NS > 30$	-0.268*	0.110	855	0.022	0.089	124
Prop. treated as instrument						
	-0.221*	0.091	1,081	0.051	0.072	166

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively. Group sizes outside the limit are excluded from the sample. NS is the size of the network.

6.3 Robustness analyses

To provide some further evidence concerning the validity of our results, we have performed extensive robustness checks. We have checked whether our results are robust to the choice of instrument, the network definition, the inclusion of individual covariates, and checked if our results could be explained by information spill-overs rather than by social interactions.

We start by analysing whether the results are robust to the choice of instrument. In the main analysis we use, as implied by the theoretical model, the proportion of treated interacted with the mean lagged work absence in the network as instrument. As robustness analysis we now instead use only the proportion of treated as instrument. The results from this specification are reported in the last row of Table 7. For sake of presentation we restate our main 2SLS results from Table 5 in the first row of Table 7. As apparent from these results are our results robust with respect to the choice of instrument.

In our main analysis we excluded the smallest networks. Here we tighten this restriction and exclude additional small networks. We exclude all networks with less than 30 members in the whole Gothenburg MSA. The 2SLS estimates with this restrictions are also presented in Table 7. The precision of the estimates is, as expected, lower when we restrict the size of the networks. However, the size of the point estimate of the social interaction effect is very similar to our main results.

We next test whether our results are robust with respect to the network definition. In Table 8 we present estimates of the hazard regressions, but with a different network specification. Here the networks are assumed to consist of immigrants from the same country arriving in Sweden closer in time (five-year periods). This means that we form subgroups within the former networks, all with their unique proportion of treated. The results are very similar to our main results.

Reduced form estimates on the hazard from work absence using individual spell data are presented in Table 9. In the first row, we do not include any control variables, while the second row presents the effects when we control for gender, age, age squared, income, type of employment, parish, and type and level of education. These estimations are performed with Cox regressions using partial maximum likelihood. The results from Table 9 show that the estimates are the same when control variables are included.

Table 8: *Hazard regressions when networks are defined by ethnic group and time of arrival. Excluding groups with ten members or fewer*

	Estimate	Std error	No. obs.
First step	0.323**	0.022	2,42
Reduced	-0.135**	0.052	2,42
2SLS	-0.419*	0.208	2,42

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively.

Table 9: *Reduced form Cox regressions using individual data*

	Estimate	Std error	No. spells
No controls	-0.099**	0.037	64175
With controls	-0.101**	0.037	64175

Notes: Includes network fixed effects. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively. The set of controls include gender, age, age squared, income, type of employment, parish, type of education, and level of education.

The above results quite clearly show that individuals' work absence is affected by the behaviour of the individuals in their network. The theory outlined in Section 5 assumes that the individuals in the network prefer to behave according to the norm in their network. In Subsection 6.1 we concluded that this changed behaviour could not be due to health shocks nor due to information shocks. In addition to this explanation of a norm effect, there is however, potentially, at least one other reason to expect a relation between the mean absence in a network and individual behaviour, and this is information effects.

Immigrants and/or individuals living further away from the Gothenburg municipal centre may be less informed about the rules and the relatively generous replacement rates in the Swedish sickness insurance system. The implementation of the experiment in itself may then have increased the information about the social insurance system, which may in turn have increased the take-up rates among the non-treated. If this hypothesis is correct, one would observe that networks with a high proportion of treated would continue to have higher absence in 1989 after the experiment had finished. However, from Table 10, we cannot see any larger increase in work absence in 1989 for the networks with more treated than for networks with fewer treated. Hence, we cannot find any support for this alternative explanation.

Table 10: *Hazard regressions when extending the study period into the post-experiment period in 1989, first step and reduced forms*

	Autumn 1988		Spring 89	
	Estimate	Std error	Estimate	Std error
First step	0.492**	0.038	-0.024	0.037
Reduced	-0.142**	0.054	-0.076	0.054

Notes: Includes network fixed effects and a calendar time effect. Robust standard errors. * and ** denote significantly different from zero at the 5 and 1 per cent levels, respectively.

6.4 Dynamic multiplier

The calculation of the long-run effects of social interactions is made under the simplifying assumptions of a constant incidence and a constant hazard rate. Then, in steady-state, the prevalence (mean absence) is simply the ratio of the incidence to the hazard rate and, since no significant effect on the incidence was found, we only need to perform the calculations for the hazard of leaving work absence. Under these assumptions, it is quite easy to show (see Appendix C for the derivation) that the long-run elasticity of an exogenous shift in the mean absence (prevalence) is equal to:

$$\bar{\omega} = \frac{-\varepsilon_{\pi}}{1+\varepsilon_{\pi}} \tag{6}$$

where ε_{π} is the short-run elasticity on the hazard from an exogenous shift in the prevalence. Using the short-run estimate of ε_{π} = -0.57, the long-run elasticity is estimated as -1.33 per cent. Note that the DID elasticity estimate (0.8) falls between the short- and the long-run estimates.

7 Conclusion and discussion

Our study adds evidence concerning the importance of social interactions in the usage of social insurance (see e.g. Clark (2003), Conley & Topa (2002), Topa (2001) and Kolm (2005) regarding unemployment insurance, and Ichino & Maggi (2000) and Lindbeck et al. (2007) regarding sickness insurance). In addition to extending this rather short list of studies, we use an exogenous variation in the network insurance usage that previous studies have lacked.

We estimate the effects using a Difference-in-Differences (DID) estimator and a 2SLS estimator. We find evidence of endogenous social interaction effects on short-term (unmonitored) work absence in both analyses. The DID elasticity from an exogenous shift in the prevalence is estimated to 0.8. From the 2SLS estimator we find that a one per cent exogenous increase in mean absence within the network would lead to an immediate decrease in the individual hazard from work absence to work by about 0.57 per cent. This effect is large and the latter estimate is in the same order as the effect from a one per cent decreases in the replacement rate (according to Johansson & Palme (2005), and more than three times as large as the estimates of endogenous effects found in Ichino & Maggi (2000).

In addition to the short-run (direct) endogenous effect, we have also calculated the long-run (or equilibrium) effect to 1.33 per cent, which suggests that norms are very important for non monitored work absence.

It is difficult to speculate on the causes of this rather large difference in comparison with Ichino & Maggi (2000). However, we can think of three aspects of our study that are important for explaining the difference. The first one is that we focus on non monitored work absence, whereas they study sickness absence in general. This non-monitored work absence is based solely on the individual's perceived health and not on judgments from a doctor or other official. It is therefore easy for the worker to adjust his/her short term work absence in accordance with how their peers take use of the more lenient monitoring. It is therefore not surprising that we find strong social interaction effects.

The monitoring is low both from the provider of the insurance (the government), but also from the employer since the direct cost of an absent worker is not taken by the employer, hence we term this short term absence as non-monitored. The second explanation is that the replacement rate was higher in Sweden than in the Italian social insurance system. These two explanations are supported by Lindbeck et al. (2007), who also study the effects of norms in Swedish sickness insurance and find large effects: an increase in mean absence by one day would on average lead to a further increase of about 0.6 days.[19] The third explanation is that the identification strategies are different: we use an intervention.

From a policy perspective, these results are of great interest, primarily because individuals' health is not observed, which suggests problems with moral hazard. Previous research has shown that the problems with moral hazard will be high if the monitoring is low and/or when the replacement rate is high. Our results add another important factor: social work norms. The health level that motivates an unmonitored work absence is simply to a large extent determined by the norms in the society. If we change the monitoring (from the authorities and insurance companies etc.), then there is a spillover. That is, what matters for the usage is not only the monitoring per se, but also what is considered fair usage of public insurance.

[19] Their analysis is based on four different strategies, and the results and interpretation differ depending on the strategy. This result is from their two preferred specifications.

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Appendix A: Data

Table A.1: Descriptive statistics for the countries of origin included in the analysis. Absence rates for group sizes smaller than 50 are not presented for data protection reasons

	Group size	Prop. treated	Prop. control	Mean age	Mean abs. 88:1	Mean abs. 88:2
Finland	10755	0.29	0.45	40.9	2.9	3.5
Former Yugoslavia	3746	0.38	0.58	39.8	2.4	2.9
Norway	2326	0.29	0.45	43.3	2.3	2.6
Denmark	2085	0.25	0.52	45	2.4	2.7
West Germany	1907	0.24	0.48	47.2	1.9	2.1
Poland	1753	0.33	0.34	39.5	2.8	3.4
Hungary	990	0.32	0.6	45.1	2.5	2.7
Iran	922	0.38	0.77	30.5	3.0	3.9
Turkey	820	0.36	0.55	32.8	3.0	3.4
Great Britain	728	0.28	0.58	38.6	1.9	2.3
Chile	548	0.42	0.46	36.7	3.3	4.1
Estonia	543	0.19	0.51	51.8	1.4	1.6
USA	490	0.28	0.51	40.6	1.8	2.4
Portugal	484	0.39	0.54	36.7	2.7	3.1
Former Czechoslovakia	426	0.26	0.51	42.2	1.9	2.4
Greece	416	0.33	0.65	38.6	1.9	2.5
Italy	394	0.31	0.73	45.3	2.1	2.6
Spain	307	0.30	0.64	42.6	2.0	2.4
Austria	302	0.25	0.61	44.3	2.0	2.0
Netherlands	247	0.27	0.62	43.5	1.7	2.0
Former USSR	231	0.22	0.45	48.3	2.3	2.2
Uruguay	209	0.36	0.53	38.6	3.5	4.1
Iraq	214	0.2	0.82	32.8	4.9	5.1
Romania	214	0.36	0.55	39.6	2.8	3.6
Iceland	214	0.35	0.43	36.1	2.3	3.0
India	211	0.26	0.58	37.2	2.4	3.0
France	190	0.31	0.57	41	1.7	1.9
Lebanon	181	0.34	0.83	30.8	4.8	5.5
China	171	0.37	0.59	41.8	1.5	2.1
Ethiopia	161	0.34	0.67	31.7	2.8	3.7
Bolivia	158	0.44	0.54	34.2	3.3	4.6
Morocco	146	0.29	0.73	37.3	3.6	3.8
Philippines	142	0.4	0.27	34.8	3.6	4.0
Vietnam, Rep.	141	0.45	0.65	31.2	4.2	4.5
Thailand	121	0.26	0.1	33.4	3.1	4.0
Argentina	113	0.37	0.44	39.8	2.2	2.4
Korea	98	0.26	0.15	24.1	1.9	2.1
Pakistan	97	0.36	0.69	36.5	2.6	3.1

Table A.1: Continued

	Group size	Prop. treated	Prop. control	Mean age	Mean abs. 88:1	Mean abs. 88:2
Brazil	82	0.29	0.33	36.4	2.6	3.5
Uganda	80	0.35	0.61	33.8	3.3	5.0
Japan	71	0.28	0.28	42.1	1.4	1.7
Switzerland	71	0.27	0.58	42.9	1.3	2.0
Latvia	66	0.27	0.5	51.6	1.9	2.3
Bulgaria	66	0.41	0.58	42.5	2.6	1.7
Tunisia	62	0.31	0.87	36.7	3.6	4.0
Colombia	59	0.32	0.54	36.2	2.0	3.3
Australia	59	0.37	0.47	33.5	2.6	2.9
Canada	55	0.22	0.49	36.4	2.2	2.5
Syria	55	0.4	0.6	34.7	2.9	4.4
Gambia	53	0.32	0.81	36.8	4.3	4.1
Belgium	51	0.30	0.51	41.1	2.6	3.0
Peru	49	0.31	0.45	35.2	-	-
South Africa	49	0.31	0.65	39.7	-	-
Malaysia	48	0.48	0.69	37	-	-
Vietnam	45	0.15	0.92	32.8	-	-
Algeria	43	0.28	0.65	39.9	-	-
Israel	41	0.2	0.83	37.4	-	-
DDR	38	0.26	0.23	35.2	-	-
Ireland	36	0.33	0.34	39.5	-	-
Indonesia	36	0.42	0.64	42.8	-	-
Ghana	35	0.51	0.66	37.9	-	-
Sri Lanka	34	0.44	0.53	37.6	-	-
Egypt	29	0.14	0.62	41.7	-	-
Somalia	28	0.29	0.79	32.1	-	-
Kampuchea	25	0.36	0.56	30.8	-	-
Mexico	25	0.36	0.52	34.1	-	-
Kenya	24	0.42	0.54	35.9	-	-
Cap Verde	22	0.59	0.23	34	-	-
Nigeria	22	0.27	0.68	33.3	-	-
Taiwan	22	0.59	0.5	32.7	-	-
Jordan	21	0.29	0.67	37.9	-	-
Palestine	21	0.24	0.91	44.3	-	-
El Salvador	20	0.2	0.6	30.2	-	-
Tanzania	20	0.45	0.6	35.9	-	-
Trinidad	18	0.33	0.5	37.5	-	-
Cyprus	18	0.39	0.5	36.3	-	-
Venezuela	17	0.24	0.65	37.4	-	-
Liberia	15	0.4	0.27	24.3	-	-

Notes: The Table includes all countries of origin, i.e. those countries with more than 10 working individuals in the Gothenburg MSA. Mean absence refers to average short-term absence as a percentage (spells of 15 days or shorter) during the first and the second half-years of 1988, respectively.

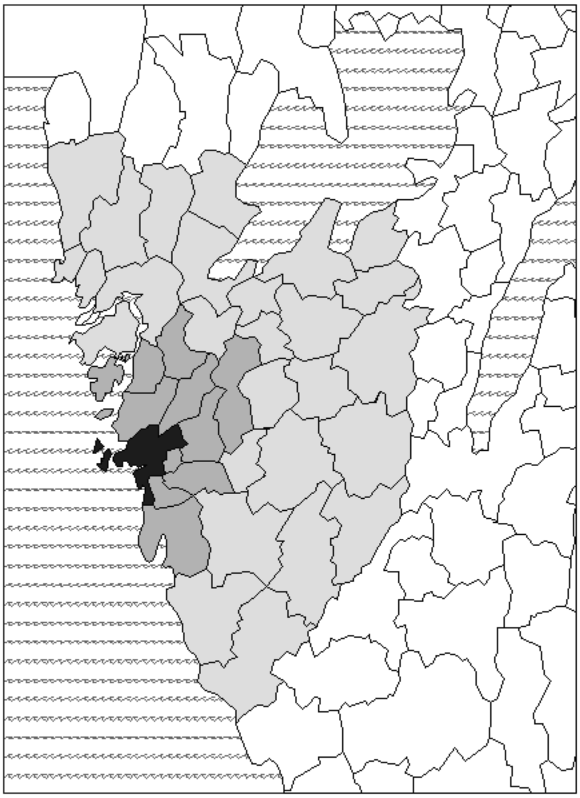

Figure A.1: Map of Gothenburg MSA and the bordering municipalities. The black area is Gothenburg municipality (for directly treated and for non-directly treated). The dark grey area shows the other municipalities in Gothenburg MSA. The light grey area shows the reference group municipalities.

Appendix B: Theoretical model

We assume that the individuals belong to a well-defined network j, consisting of n_j individuals. Let $d_{ij} = 1$ if individual i in network j is absent from work and $d_{ij} = -1$ if working. We denote the vector of work absence for this individual network, excluding the individual self, $\tilde{d}_{-ij}$, thus $\tilde{d}_{-ij} = (d_{1j}, \ldots, d_{i-1,j}, d_{i+1,j}, \ldots, d_{n_j,j})$.

The asset values for individual i in network j associated with work and work absence are:

$$V_{ij}^W = w - e_{ij} \tag{B.1}$$

and

$$V_{ij}^S = bw + a_j - g(\mathbb{E}_i(\tilde{d}_{-ij})) \tag{B.2}$$

Here, $\mathbb{E}_i(\tilde{d}_{-ij})$ is individual i's beliefs about the work absence of the network members and $g(.)$ is the deterministic social cost function. If the individual is present at work, they receive the wage,w, but face a cost e_{ij}, that is, the effort of working. e_{ij} is assumed to be a function of individual health shocks. The utility if on sick leave is $bw + a_j$, where b is the replacement rate when absent from work and a_j is the baseline value of leisure for individuals in network j. For simplicity, we assume that e_{ij} is independent of the individual's beliefs about the network members' choices, as well as independent between the individuals in the network conditional on a_j. Our basic assumption regarding the deterministic social cost, $g(\mathbb{E}_i(\tilde{d}_{-ij}))$, is that individuals prefer to behave in the same way as those in their network, that is, the social cost from being absent is low for individuals in high work absence networks.

Individual i will be absent from work if $V_{ij}^S - V_{ij}^W \geq 0$, that is using equations (B.1) and (B.2) if:

$$e_{ij} \geq (1-b)w - a_j + g(\mathbb{E}_i(\tilde{d}_{-ij})) \tag{B.3}$$

In this simplified model, each individual in network j is assumed to have the same wage and, since the replacement rate is the same for all individuals, this implies that the cut-off value will be the same for all individuals in the network. The cut-off value is increasing in wages, but decreasing in the replacement rate, network j's value of leisure, and in the social cost of work absence. The probability that a randomly drawn individual in network j is absent is then equal to

$$\Pr(d_{ij} = 1) = \pi(w, b, \mathbb{E}_i(\tilde{d}_{-ij})) = \Pr(e_{ij} \geq (1-b)w - a_j + g(\mathbb{E}_i(\tilde{d}_{-ij}))). \tag{B.4}$$

In order to test empirically for endogenous social interactions, we need assumptions concerning: (i) how the interactions are formed, (ii) the networks, (iii) how the individuals make their predictions, and (iv) the distribution e_{ij}.

Assumptions (i)-(iii) are discussed in the next subsection. The distribution assumption of e_{ij} is deferred to empirical specification.

Social work norms

To obtain a closed form expression for the social cost, we follow Brock & Durlauf (2001*b*) and assume (i) quadratic conformity effects. That is, the social cost is specified as:

$$g(\mathbb{E}_i(\tilde{d}_{-ij})) = \mathbb{E}_i\Big(\sum_{k\neq i}^{n_j} \frac{J_{ik}}{2}(d_{ij}-d_{kj})^2\Big), \tag{B.5}$$

where J_{ik} represents the weights individual i gives to the interaction between individual i and individual k in the same network. If $J_{ik}=0$, then individual i disregards the actions of individual k. We furthermore assume (ii) that all individuals have the same weights when forming the expectation, and that the social interaction parameter is constant across the population, thus $J_{ik} = J_i/n_j = J/n_j$ for all j,k. We also assume (iii) that the individuals have rational expectations (this means that $\mathbb{E}_i = \mathbb{E}$, for all i, where $\mathbb{E}$ is the mathematical expectation). A specific set of actions by the individuals then constitutes an equilibrium if the individuals correctly anticipate the actions by their network members, thus $\mathbb{E}(d_{ij}) = \pi_j$, where π_j is the mean absence rate in network j.

Under these three assumptions, we obtain the following expression for the social cost:[20]

$$g(\mathbb{E}_i(\tilde{d}_{-ij})) = J(1-d_{ij}\pi_j) \tag{B.6}$$

The social cost of being absent is, hence, proportional to the mean absence level in the network. This gives us a closed expression for the probability to be absent:

$$\Pr(d_{ij}=1) = h(w,b,\pi_j) = \Pr(e_{ij} \geq J + (1-b)w - a_j - J\pi_j), \tag{B.7}$$

[20] The social utility term is, since $d_{ij}^2 = d_{ik}^2 = 1$, equal to:

$$g(\mathbb{E}_i(\tilde{d}_{-ij})) = \mathbb{E}_i\Big(\sum_{k\neq i}^{n_j}\frac{J_{ik}}{2}(d_{ij}-d_{kj})^2\Big) = \sum_{k\neq i}^{n_j}\frac{J_{ik}}{2}\Big(d_{ij}^2 - d_{ij}\mathbb{E}_i(d_{ij}) + d_{kj}^2\Big) = \sum_{k\neq i}^{n_j} J_{ik}(1-d_{ij}\pi_j).$$

Further, using $J_{ik} = J/n_j$, we obtain: $g(\mathbb{E}_i(\tilde{d}_{-ij})) = J(1-d_{ij}\pi_j)$. Then, imposing the self-consistency condition leaves us with equation (B.6).

Appendix C: Dynamic multiplier

The short-run hazard rate elasticity from an exogenous shift in the mean absence (i.e., prevalence) from π_t to π_{t+1} is defined as:

$$\varepsilon_\pi = \Big(\frac{h_{t+2}-h_{t+1}}{h_{t+1}}\Big)/\Big(\frac{\pi_{t+1}-\pi_t}{\pi_t}\Big) \tag{C.1}$$

The initial social interaction effect on the hazard is hence assumed to occur between $t+1$ and $t+2$. The hazard at time period $t+2$ is now:

$$h_{t+2} = \left(1+\varepsilon_\pi\left(\frac{\pi_{t+1}-\pi_t}{\pi_t}\right)\right)h_t \tag{C.2}$$

where we use $h_{t+1} = h_t$. Under the assumption of of a constant incidence at I_t (i.e. the incidence is not affected by the social interactions), hence $I_t \equiv I_{t+1} \equiv I_{t+2},\ldots$, we get that the mean prevalence in period $t+2$ is $\pi_{t+2} = I_t/h_{t+2}$. Now, in $t+3$, there is a further decrease in the hazard:

$$h_{t+3} = \Big(1+\varepsilon_\pi\frac{\pi_{t+2}-\pi_{t+1}}{\pi_{t+1}}\Big)h_{t+2} = h_{t+2}-\varepsilon_\pi(h_{t+2}-h_{t+1}) \tag{C.3}$$

The last equation is obtained by using:

$$\Big(\frac{\pi_{t+1}-\pi_t}{\pi_t}\Big) = \frac{\frac{I_t}{h_{t+1}}-\frac{I_t}{h_t}}{\frac{I_t}{h_t}} = -\Big(\frac{h_{t+1}-h_t}{h_{t+1}}\Big) \tag{C.4}$$

By using recursive substitution, we obtain:

$$h_\infty = h_{t+2}+(h_{t+2}-h_{t+1})\sum_{k=0}^{\infty}(-\varepsilon_\pi)^k \tag{C.5}$$

and by subtracting each side with h_{t+1} and using that $h_{t+1} = h_t$ we obtain:

$$\frac{h_\infty - h_t}{h_t} = \frac{h_{t+2}-h_{t+1}}{h_{t+1}}\sum_{k=0}^{\infty}(-\varepsilon_\pi)^k \tag{C.6}$$

The long-run elasticity on the hazard rate of a 1 per cent exogenous increase in the prevalence is then equal to:

$$\bar{\omega} = \frac{h_\infty - h_t/h_t}{\pi_{t+1}-\pi_t/\pi_t} = \sum_{k=1}^{\infty}(-\varepsilon_\pi)^k \tag{C.7}$$

Under the assumption that $-1 < -\varepsilon_\pi < 1$, we obtain:

$$\bar{\omega} = \frac{-\varepsilon_\pi}{1+\varepsilon_\pi} \tag{C.8}$$

Essay 2: The Effect of Employer Incentives in Social Insurance on Individual Wages

1 Introduction

Experience rating, co-insurance and other types of employer incentives are key components of many social insurance systems. Employer incentives are, for instance, present in workers compensation/disability insurance in the USA, Canada, Australia, the Netherlands, and New Zeeland, in unemployment insurance in the USA, and in sickness insurance in Germany and Sweden. In these schemes the insurance tax rate each firm pays is adjusted upwards or downwards to reflect the costs of the insurance claims made by their workers, and/or the firm is responsible for paying parts of the benefits directly to their workers. The main idea behind these policy instruments is to correct the incentives faced by employers in order to avoid inefficiently high social insurance take-up rates. For instance, if workers are covered by social insurance employers have less incentive to reduce temporary lay-offs, and less incentive to improve the work environment, as the government is responsible for paying social insurance benefits. The existent literature mainly confirms that employers' incentives can indeed decrease the social insurance usage.[1] These employer incentives may though have unintended side-effects. In this paper I investigate whether employer incentives in social insurance affect individual wages.

Employer incentives in the form of experience rating and co-insurance introduce a direct cost for employers when the insurance is used by their workers. Besides taking action to decrease the take-up rates, employers have other ways to avoid this direct cost. Specifically they may shift over the costs to the workers by adjusting individual wages, giving insurance-prone workers lower wage increases. If such wage effects are present in health-related insurances like disability insurance and sickness insurance, workers with worse health will pay the employers' direct costs through lower wages. It will not only have large distributional effects, it will also transform the employer incentives into

[1]For studies on experience rating in unemployment insurance in USA, see Topel (1983, 1985), Deere (1991), Card & Levine (1994), Anderson & Meyer (1994), Anderson & Meyer (1994) and Jurajda (2004). For studies on disability insurance and sickness insurance in Canada see Bruce & Atkins (1993), Hyatt & Thomason (1998), and from the Netherlands in see de Jong & Lindeboom (2004) and Koning (2004), and finally for the USA disability insurance see Ruser (1985, 1991, 1993), Moore & Viscusi (1989), and Thomason (1993).

worker incentives. The direct costs also provide employers with incentives to engage more in cream-skimming, avoiding hiring workers with worse health and firing workers with declining health status. A final side-effect is that employers may try to decrease workers' access to the insurances, by contesting individual insurance claims.[2]

These side-effects have largely been ignored in the empirical literature. For social insurance, four exceptions are Anderson & Meyer (2000), Hyatt & Kralj (1995), Thomason & Pozzebon (2002), and Harcourt et al. (2007). Anderson & Meyer (2000) find wage effects and that employer's claim-contesting rate increases as a result of experience rating in unemployment insurance. The results in Hyatt & Kralj (1995) and Thomason & Pozzebon (2002) suggests that employers' claim-contesting rate increases as a results of experience rating in Canadian disability insurance. Finally, Harcourt et al. (2007) find that experience rating induces firms to more often discriminate against insurance-prone workers in their hiring procedure. Related studies are also Gruber (1994) and Baicker & Chandra (2005) who study the individual wage effects of introducing mandated maternity benefits and from growth in health insurance premiums, respectively. This quite limited evidence is unfortunate, since all the effects of different incentives have to be taken into account in order to design an optimal insurance system.

This paper aims to fill one gap in this literature. I estimate the individual wage effects from an employer co-insurance reform in the Swedish sickness insurance in January 1992. The sickness insurance replaces forgone income due to temporary health problems. Prior to 1992 the benefits were financed by uniform pay-roll taxes and all benefits were paid directly from the government. The reform in January 1992 gave employers the responsibility to pay the full cost for all absence during the first fourteen days of each absence period among their workers. As the incidence of short-term absence varies substantially, the reform increased employers cost of some workers, and for others the costs were even reduced.

The individual wage effects are estimated using a long population panel database. The data set has several features which that makes it especially suitable for investigating individual wage effects. It includes a large set of individual variables. The individuals can be followed over a long time period. Each worker can be matched to their current and past employers. The data set is also beneficial since it includes survey information on actual wages, and not wages created from annual earnings and some measure of hours worked. There is also very detailed information on the absence of each individual, including the start and end date of every single absence spell. Since the employer co-insurance cost depends on the number of absence days, I can infer the co-insurance costs the employers have for each worker. In this way I can follow the absence decisions, employment status and the nominal wages for

[2]The employers may also try to discourage workers from submitting claims, or delaying submitting information to the insurance authority.

each individual several years before as well as after the reform. All these features of the data allow me to deliver arguably more credible evidence on the individual wage effects.

Besides adding new evidence to the previous limited evidence this study contributes in other ways. First of all, this study provides evidence for Swedish sickness insurance, which resembles many workers compensation and disability insurances around the world. Previous evidence on the other hand is for other types of insurances, and as there is no reason to expect that the effect is the same across different types of insurances this study contributes with valuable insights. Second, previous evidence is concentrated to USA, where wages are more often bargained on an individual level compared with many European countries. My study therefore contribute with estimation results that are very relevant for labor markets with a somewhat lower degree of individual wage bargaining, such as in many European countries. As a comparison, Nilsson (1993) estimate that locally bargained wage increases accounted for 45 percent of the total wage increases in Sweden, in other words individual wage bargaining is an important feature also in the Swedish labor market.

The paper is outlined as follows. Section 2 outlines a simple bargaining model, which can be used to analyze the expected effects of employer incentives in social insurance. The model is set up in two stages with exogenous respectively endogenous sickness absence. It gives several important insights that guide the empirical model. Section 3 describes Swedish sickness insurance and the employer co-insurance reform in 1992. Section 4 presents the data and section 5 presents the empirical strategy. The main results are presented in section 6, extensive robustness analyses are presented in section 7. Finally section 8 concludes.

2 Theoretical model

The purpose of the theoretical model is to analyze how employer incentives in the form of a direct tax cost for all absence within the firm is expected to affect individual wages and individual sickness absence. The focus is on a mandatory public insurance system where all workers are entitled to benefits, regardless of the size of the tax cost. First a model with exogenous sickness absence is considered, and then is the model extended to allow for endogenous sickness absence. The simple bargaining model gives a couple of important insights, which are used to guide the empirical model and to interpret our results.

2.1 Model with exogenous sickness absence

The basic set up is as follows. Each firm employs one worker who is permanently attached to the firm. The firms produce one good using labor as the only input, and for simplicity it is assumed that they operate using a constant return to scale technology. The price of the good is further normalized to one. The permanently attached worker has a pre-specified contract of normal working time, h. The contracted working time is set by the labor market institutions, and is therefore taken as exogenous by both the firms and the workers. The productive working time is then the contracted time minus the time the worker is absent from work, s.

The firm has several costs, in addition to the labor cost which is simply the number of hours worked times the hourly wage, w. They have a fixed cost, c. Further a direct cost, τ, for each hour their single worker is absent from work. If $\tau = 0$ it corresponds to a social insurance system with no direct employer incentives. The firm's profit function is then

$$\pi = A(h-s) - w(h-s) - \tau s - c, \tag{1}$$

where A represents the productivity of the firm.

The worker receives utility from consumption and leisure. Consumption equals the sum of income from work and the income in the form of social insurance benefits collected while being absent from work. Leisure equals the number of absence hours. I assume the following utility function

$$u = (h-s)w + bs + \delta \ln(s), \tag{2}$$

where b is the hourly social insurance benefit level, and δ is the value the worker places on leisure. Assuming $\delta > 0$, means that the worker values leisure but at a declining rate.

The worker and the employer bargain over the wage, and I assume that the outcome of the bargaining game is given by the Nash bargaining solution. The firm's agreement point π_0 is assumed to be zero, and the worker's agreement point u_0 is assumed to be

$$u_0 = bh + \delta \ln(h), \tag{3}$$

That is the utility the worker gets if the work is terminated under the assumption that the worker can collect social insurance benefits corresponding to full working time.

Introducing β as the bargaining power of the worker, the solution to the Nash problem is then given by

$$w = \arg\max[\pi]^{1-\beta}[u-u_0]^{\beta}. \tag{4}$$

Solving for the first order condition for the maximum gives a closed form solution for the hourly wage, w, as

$$w = A\beta + b(1-\beta) + \frac{\delta(1-\beta)(\ln h - \ln s)) - c\beta - s\tau\beta}{h-s}. \tag{5}$$

Note that the wage is increasing in the worker's bargaining power, the productivity of the firm, the social insurance benefit level, the worker's valuing of leisure, and decreasing in the costs associated with sickness absence. As individual wage effects are the topic of this paper, I more closely investigate how the wage depends on the sickness absence rate, s. I have

$$\frac{dw}{ds} = -\frac{cs\beta + hs\tau\beta + \delta(1-\beta)(h - s - s\ln h + s\ln s)}{s(h-s)^2}. \tag{6}$$

This expression provides several interesting insights. The worker suffers a wage penalty for each day of absence.[3] The effect goes through two channels. It is increasing in the fixed cost for the firm, c, and in the direct tax cost, τ, associated with sickness absence. Naturally if the tax cost the employer has to pay is larger one expect a larger individual wage effect. The wage penalty effect which goes through c is less expected and something that might have been missed without a formal model. The intuition is however straightforward. If the worker is often absent the fixed cost per actual hour worked is larger, making the firm less profitable which in turn affect wages. It means that even if the tax cost τ is zero I expect to find a negative relation between the wage and the sickness absence. Note that this conclusion is made even under the assumption that there are no additional costs associated with sickness absence. In a real world economy one could think about costs associated with, for instance, finding replacement workers. If such costs are present this would be another reason to find a negative relationship between wages and absence. It is also clear that the wage penalty depends on the bargaining power, β. The wage penalty is low for individuals with low bargaining power.

2.2 Model with endogenous sickness absence

In the above model the individual sickness absence was assumed to be exogenous. However, when the worker decides whether to go to work or not, for example when having a cold, it is reasonable to believe that they take any wage effect from being absent into consideration. The model is therefore extended into a simple game allowing for endogenous sickness absence. The set up of the game is as follows: in the first step the worker decides their sickness absence: and in the second step of the game the worker and the employer bargain over the wage.[4] I further assume that the worker has full information on the outcome of the wage bargaining. The solution to the second step is thus the same as for the model with exogenous sickness absence.

[3] Note that the second part of the expression $\delta(1-\beta)(h-s-s\ln h+s\ln s)$ is always positive, as δ, the value of leisure, is assumed to be positive, β, the individual bargaining power, is between zero and one, and because h, the contracted number of hours, is larger or equal to s, the number of hours of sickness absence.

[4] One could also consider a repeated game. The solution to our simple game would then be the equilibrium solution to the repeated game.

Now consider the first step of the game. In the absence decision the worker faces a trade-off: higher sickness absence means increased utility from leisure, but also decreased consumption. The solution to this optimization problem is found by substituting the wage as a function of sickness absence in equation (5) into the worker utility function in equation (2). The utility is then only a function of sickness absence and for the worker exogenous variables

$$u = (h-s)(A\beta + b(1-\beta)) + \delta(1-\beta)(\ln h - \ln s)) - c\beta - s\tau\beta + bs + \delta(\ln(s) \tag{7}$$

Solving the first order condition gives us a closed expression for the sickness absence rate

$$s = \frac{\delta}{A-b+\tau}. \tag{8}$$

This expression shows the expected relations. The absence rate is decreasing in the productivity of the firm A, since higher productivity implies a larger production loss if absent from work and thereby a higher wage. The absence rate is further increasing in the value the worker places on leisure, δ, and the social insurance benefit level, b. This is natural since they both increase the value of being absent from work. The absence rate is decreasing in the size of the employer incentives tax, τ. If the tax is high it implies a larger wage penalty and thereby increases the cost of being absent from work. It is also clear that it is predicted that individuals who value leisure to a high degree will be the once who are relatively more often absent from work both in a world with small respectively large employer incentives. The endogeneity of the absence level, of course, also has important implications for the specification of the empirical model.

To summarize, the models predict that employer incentives in the form of a direct tax cost for all absence within the firm affects individual wages, and that the effect is relative to the absence level of each individual. The model also shows that due to fixed costs I expect a negative relationship between wages and absence even without such direct costs. Furthermore, it shows that the absence level should be treated as endogenous. All these points have important implications for the empirical model.

3 Institutional background and the reform in 1992

3.1 Wage bargaining in Sweden

Any paper investigating individual wage effects in Sweden has to discuss the so-called 'Swedish model'. The Swedish model is an often used term for describing the institutions in the Swedish labor market. Some key features are/were centralized collective wage bargaining and extensive use of active labor market policy. Collective bargaining aimed at promoting wage equality. But this stylized description is however not fully accurate. Historically there has always been wage bargaining at different levels, including local and individual wage bargaining. For example Nilsson (1993) estimate that locally bargained wage increases accounted for 45 percent of the total wage increases. Wage-setting institutions have also changed during the last three decades. The degree of centralized bargaining started to decrease at the beginning of the 1980's, see e.g. Edin & Holmlund (1995). The wage data used in this paper further demonstrate that individually bargained wage changes are important.

There are also large differences between sectors in the bargaining power as well as in the degree of individual wage bargaining. In general, individual wage bargaining is more important for highly educated workers and workers employed in the private sector. The wages for public servants are more often dictated by collective agreements. This suggests that any individual wage effects should be more prominent for the highly educated in the private sector. As I theoretically expect larger wage effects for individuals with high bargaining power, this also suggests larger individual wage effects for highly educated workers. Detailed analysis of heterogeneous effects is therefore performed.

3.2 Swedish sickness insurance

This section presents the main features of the Swedish sickness insurance during the research period (1989-1994). Sweden has compulsory national sickness insurance. It is mainly financed by a proportional payroll tax and replaces earnings forgone due to (temporary) health problems that prevent the insured worker from doing his/her regular work tasks. The benefits could be collected for any health problem, ranging from a cold to a serious work related injury. Sickness benefits from the public insurance are and have been generous in an international comparison. This can, for instance, be shown by the fact that most workers received 90 percent of their lost income from the first day in the late 1980s. A benefit cap excluded workers at the very top of the income distribution from receiving the full 90 percent. Most Swedish workers were, however, also covered by negotiated sickness insurance programmes regulated in agreements between the labor unions and the employer confederations.

The public insurance does not verify claimants' eligibility during the first benefit week. At the start of a spell, the worker has to call the public social insurance office (and the employer) to report sickness. The individual is then entitled to collect benefits from the first day of their absence spell.[5] Within a week, at the latest on the eighth day of sickness, the claimant should verify eligibility by showing a doctor's certificate that proves reduced work capacity due to sickness. The public insurance office judges the certificate and decides about further sick leave. The public insurance had until recently no limit as to

[5]In 1993 a qualifying day was introduced.

how often or how long benefits would be paid. Many sickness absence spells continue for more than a year. These spells end mostly in disability insurance, early retirement or old age retirement.

3.3 The 1992 employer co-insurance reform

Before 1992 the government was responsible for paying all sickness absence benefits. Every employee could file a claim and receive benefits directly from the government if they had temporary health problems. These benefits were financed by a uniform proportional pay-roll tax. In January 1992 the system was changed, and employers were obliged to pay sickness benefits for their own workers during the first two weeks of every sickness absence period. Hence, these benefits are denoted sickness pay, and I will refer to absence during the first two weeks of every absence period as short-term absence, and all other absence as long-term absence. Since there were no qualifying day in 1992, this meant that employers were given responsibility for paying sickness pay even if the worker was absent from work for a single day. For absence spells longer than two weeks the government continued to pay the benefits in the same way as before the reform. In return the social insurance part of the pay-roll tax was reduced from 10.1% to 8.2%. The new system can therefore most accurately be described as an employer co-insurance system, where the financial costs for a single worker claim are divided between the government and the employer.

The government declared several reasons for changing the system. First, administrating every short-term sickness absence was an administrative burden for the insurance system. Second, it was believed that the reform would induce the employers to improve the work environment and increase the firm's monitoring efforts. Third, it was intended to make the insurance fairer. Employers in general have more information about their workers compared to the government. It was therefore believed that employers would be able to make more accurate benefit payments, which would make the insurance system fairer.

The reform has several features that makes it suitable for investigating individual wage effects. The reform was rapidly implemented, and thus individuals had little chances to change their behavior before the reform was implemented. It is also reasonable to expect that costs associated with short-term absence are important for firms. The relative individual short-term absence is quite stable over time, and in contrast with long-term absence, workers with regular spells of short-term absence usually stay in the workplace. Combined with the fact that short-term absence varies a lot between individuals, this means that the employer co-insurance reform introduced large stable insurance cost for some workers and small stable insurance costs for other workers. The employers therefore have large incentives to shift over the cost, introduced by the reform, to their workers.

4 Data

The data set used in the analysis come from several different databases. From Statistics Sweden I have a set of socio-economic variables (e.g. age, sex, income, immigration status and employment status), and also information that allows us to match each worker to their current as well as past employers. I have sickness absence data from the Swedish Social Insurance Agency (SSIA). The work absence database covers all absence periods for which sickness benefits are paid from the government. Before the reform in 1992 forgone earnings due to work absence were replaced from day one of each spell, and thus include the register information on all absence due to sickness before the reform. Unfortunately the government did not collect information on the sickness pay paid by the employers after the reform. This means that I have no information on short-term absence, i.e. absence up until day 14 of every absence spell, after the reform. Long-term absence data are on the other hand available for both before and after the reform.

The data set also include survey data on wages from Statistics Sweden's wage statistics, consisting of high-quality information on actual wages, and not wages created from annual earnings and some measurement of hours worked. These wage data are collected by Statistics Sweden in cooperation with employer organizations, and include the whole public sector, all large private firms and a random sample of small firms (firms with fewer than 200 workers). In total they cover about 50 percent of all private sector workers.

In the analysis wage data for 1989-1994 and sickness absence data from 1986 and onwards are used. The sample consists of all workers of working age (25-55), who worked at the same firm during two consecutive years. Working is defined as having income above one base price amount, and collecting no unemployment insurance benefits. The reason for this is that I want to focus our analysis on wage effects, and rule out any variation in wages due to individuals changing firm. The analysis is restricted to individuals working at least three consecutive years and for whom I have wage data.[6] I also exclude some extreme observations, those with 20 percent wage increase/decrease and/or more than 100 days of short-term absence on average. Extensive robustness analysis shows that the results are quite insensitive to these restrictions.

4.1 Descriptive statistics

Table 1 presents some descriptive statistics over wages and absence for the individuals in our sample. The fact that a population database is used is reflected in the large number of observations. Also, note that there are more females

[6]This condition is imposed since I study the wage increase using sickness absence lagged two periods as an instrument for current absence, and the difference in absence lagged two periods as an instrument for the difference in absence. In order to take the wage difference and observe lagged sickness absence I need that the individual worked three consecutive years.

Table 1: *Summary statistics for the main sample used in our analysis*

		Wage		Short-term absence	
	Nr. Obs.	Mean	Std.	Mean	Std.
1990	640,577	13,960	4220	9.0	11.1
1991	760,507	13,880	4140	8.5	10.8
1992	777,297	14,550	4140		
1993	847,711	14,630	4200		
1994	878,611	15,580	4840		
For 1991					
Central government	184,392	14,960	3870	8.3	10.8
Regional municipality	195,145	13,350	4380	8.6	10.7
Local municipality	266,631	12,470	2670	9.6	11.4
Private Blue-Collar	19,603	11,630	1530	10.6	12.4
Private White-Collar	94,736	17,320	5180	5.4	7.9
Female	485,702	12,580	2570	9.7	11.4
Male	274,805	16,180	5240	6.4	9.3
Non-immigrant	704,809	13,910	4130	8.4	10.6
Immigrant	55,700	13,460	4250	10.5	12.5
Age -30	71,462	13,330	3660	9.0	11.1
Age 30-45	382,036	13,560	3590	8.7	10.8
Age 45-	307,009	14,410	4780	8.2	10.8
Absence 0-10 days	541,136	14,330	4500		
Absence 10-20 days	126,569	13,060	3060		
Absence 20 days	92,802	12,390	2240		

Notes: Wages is monthly full-time wages in SEK (not deflated). Absence is yearly absence in days. Short-term all days from day 1-14 of every spell. Sector of employment is defined using Statistic Sweden's wage statistics.

than males in our main sample. This is because females more often work in the public sector, and I observe wages for everyone working in the public sector, but only for sub-set of everyone working in the private sector. The summary statistics show the expected patterns. Males, non-immigrants, and more experienced workers have higher wages. There is a gradual increase in the mean wage during the period. Private white-collar workers have the highest wages. There is a clear correlation between wages and sickness absence. Those who are more often absent from work earn substantially less than those who are never absent from work. The descriptive statistics for the absence data also show the expected patterns. Females, immigrants and older workers are more often absent from work. Finally, absence is much higher among blue-collar workers compared to white-collar workers.

4.2 Are there any wage and absence differences?

The focus of this paper is to investigate whether the employer incentives introduced by the co-finance reform affected individual wages. An important question is therefore how large the variation in absence and wage increases are within firms: that is does everyone in the same workplace receive the same wage increase and how much do the absence vary? In order to answer these questions I have produced three figures. Figure 1 displays the histogram for short-term absence in 1991, and Figure 2 and Figure 3 presents the residuals from regressions for short-term absence in 1991 respectively for the wage change between 1992 and 1991. These regressions include controls for gender, immigrant status, number of children, education level, type of education, sector of employment and workplace fixed effects, so that all focus is on the size of the within-firm variation.

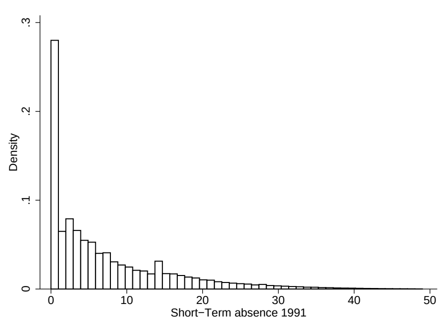

Figure 1: Histogram for short-term absence in 1991.

These figures clearly show that there is large variation in both sickness absence and in wage changes. Figure 1 shows that there are a considerable number of workers who are never absent from work in a given year, whereas there are some workers who are absent for more than 20 days a year. The large variations imply substantial differences in employer costs. Consider a worker who works about 220 days a year and is absent 20 days. Take a mean replacement rate of 80 percent. The employer provided sickness pay then amounts to about 7 percent of the wage cost. One could also note the spike for 14 days of short-term absence. The reason for this is that short-term absence is defined as the total number of absence days up until day 14 of every absence period (the only absence that is covered by the employer co-insurance). It means that individual who has one single absence spell of 14 days or longer will have 14 days of short-term absence. As apparent from Figure 2 these large absence differences also persist after controlling for a large set of control variables, including workplace fixed effects.

Similarly Figure 3 displays large variation in wage increases, even after controlling for a large set of variables, including workplace fixed effects and

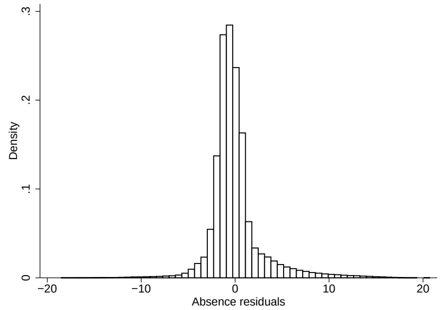

Figure 2: Histogram for short-term absence residuals (1991).

education level. This figure together with the institutional details in Section 3 give a clear indication that there is room for individual wage bargaining on the Swedish labor market.

Figure 3: Histogram for wage increase residuals (1992-1991).

5 Empirical strategy

From the theory it follows that the individual wage, w, is likely to depend on productivity A, the individual social insurance benefit level b, the individual value of leisure δ, the individual bargaining power β, the fixed cost c and the tax cost associated with sickness absence τ. Furthermore, the wage effect arising from the fixed costs c and the tax costs τ is directly related to the sickness absence, S, for individual i. Without loss of generality the wage effect from individual bargaining power, the benefit level and the leisure value can be separated into a fixed individual part α_i and a time changing individual part v_{it}. Taking a linear model for the logarithm of the wage I have the wage for individual i in time period t as

$$\ln w_{it} = \alpha_t + \gamma_c S_{it} + \gamma_\tau \tau_t S_{it} + \alpha_i + v_{it}. \tag{9}$$

Here γ_c and γ_τ measure the impact of sickness absence on the individual wage going through c and τ, respectively. My main interest is to estimate γ_τ, which measures the causal effect of an additional day of employer paid absence on individual wages. If $\gamma_\tau < 0$, it means that after the co-insurance has been implemented those often absent suffer a wage penalty for each day they are absent from work.

In order to consistently estimate γ_τ some identification problems have to be addressed: i) how to separate γ_τ from γ_c, ii) the selection problem that α_i most likely is correlated with S_i, iii) the endogeneity problem that w_{it} also affects S_i, and iv) that there may be trends in v_{it} correlated with S_i. In the following I give the intuition behind and explain in detail how these four identification problems are addressed.

The first problem arises because the individual absence level is expected to have a causal effect on the individual wage even without employer co-insurance. In order to solve this problem I exploit the exogenous variation in τ the size of the employer's tax cost associated with each additional day of worker absence, offered by the co-insurance reform in January 1992. In equation (1) it can be expressed as $\tau = 0$ before the reform, and if one normalize τ according to the size of the Swedish co-insurance employers' incentives as $\tau = 1$ after the reform. Utilizing the panel structure of our data it is then possible to separate the general individual wage effects from being absent from any additional wage penalty as a result of the co-insurance reform in 1992.

Second, α_i is most likely correlated with S_i. For instance, it is reasonable to believe that individuals with high productivity and high ambitions have both high wage as well as a low sickness absence. In addition individuals with high bargaining power likely have both a high wage as well as a job good work environment, implying low sickness absence. Controlling for individual heterogeneity is therefore central. I control for all fixed individual heterogeneity by focusing on wage increases. Taking the first difference of equation (1) it follows that

$$\Delta \ln w_{it} = \alpha_t - \alpha_{t-1} + \gamma_c (S_{it} - S_{it-1}) + \gamma_\tau (\tau_t S_{it} - \tau_{t-1} S_{it-1}) + v_{it} - v_{it-1}.$$

Consider the wage evaluation for different individuals after the reform. As $\tau_t = 0$ before the reform in 1992 and $\tau_t = 1$ after the reform, I have for 1992

$$\gamma_\tau (\tau_{92} S_{i92} - \tau_{91} S_{i91}) = \gamma_\tau S_{i92}.$$

However, note that this only holds if the employers are able to immediately transfer the full cost to their workers. This is not likely, instead the wage increases are most likely also negatively affected for often absent individuals also in 1993 and 1994. This can be taken into account by adding a time subscript on γ_τ, so that $\gamma_{\tau,92}$ measures the wage effect in 1992 and so on. Making the approximation that the absence level is constant, and noting that $\tau_{93} = \tau_{92} = 1$ I have for 1993

$$\gamma_{\tau,93}\tau_{93}S_{i93} - \gamma_{\tau,92}\tau_{92}S_{i92} \approx (\gamma_{\tau,93} - \gamma_{\tau,92})S_{i93}.$$

Making the same approximation for 1994 gives

$$\Delta \ln w_{it} = \lambda_t + \gamma_c(S_{it} - S_{it-1}) + \gamma_{\tau,92}D_{92}S_{it} + (\gamma_{\tau,93} - \gamma_{\tau,92})D_{93}S_{it} \qquad (10)$$

$$+(\gamma_{\tau,94} - \gamma_{\tau,93})D_{94}S_{it} + \varepsilon_{it}$$

where D_t is a indicator function taking the value one in year t, and zero otherwise. The hypotheses to test are then that $\gamma_{\tau,92} < 0$, $\gamma_{\tau,93} - \gamma_{\tau,92} < 0$, and $\gamma_{\tau,94} - \gamma_{\tau,93} < 0$, i.e. an initial wage increase effect in 1992, and additional wage increase effects in 1993 and 1994. Note that the equation has been simplified by defining $\lambda_t \equiv \alpha_t - \alpha_{t-1}$ and $\varepsilon_{it} \equiv v_{it} - v_{it-1}$.

The third problem, the endogeneity of S_t, follows directly from the theory, which show that the wage is an important determinant of individual absence. The outcome of interest is the wage increase between time period t and $t-1$. One way to address the endogeneity problem is to instrument current absence level, S_t, using absence lagged two period, S_{t-2}. Unless individuals are extremely forward looking the absence level today should by quite unaffected by future wage increase. If such forward looking behavior is present it is likely of second order. The exclusion restriction is thus likely to be fulfilled. Later it is also shown that lagged absence is highly correlated with present absence, yielding a strong instrument. In a similar way the difference in absence is instrumented using the absence difference lagged two periods.

The final problem arises since one may suspect trends in v_{it} to be correlated with S_i. For instance, the wage increases may vary across sectors and/or across individual characteristics in a way that is correlated with individual sickness absence. If such trends are not taken into account the estimates will be biased. I control for this problem in two ways. First of all, I stepwise introduce different observed variables, like gender, immigrant status, sector of employment, firm controls and residence area into the wage difference equation. I also flexibly interact these variables with calendar time. This will control for all trends in by me observed variables. Second, S_t is included into the model also before the reform, and the effect of the reform is measured by S_t time interactions. This will control for trends in by me unobserved variables, as it controls for trends that are correlated with S_t. The final model to estimate using IV is then

$$\Delta \ln w_{it} = \lambda_t + \beta X_{it} + \gamma_c(S_{it} - S_{it-1}) + \gamma_s S_{it} + \gamma_{\tau,92}D_{92}S_{it} + (\gamma_{\tau,93} - \gamma_{\tau,92})D_{93}S_{it} \qquad (11)$$

$$+(\gamma_{\tau,94} - \gamma_{\tau,93})D_{94}S_{it} + \varepsilon_{it}$$

The final model could be interpreted as a Difference-in-Differences model, as it contrasts the change between before and after the reform in the wage increases for those with a high amount of short-term absence compared with those with a low amount of short-term absence. If the wage increases jump downward for those often absent from work at the same time as the reform, this is evidence of an effect of employer co-finance on individual wages.

To summarize, the estimation strategy has several advantages: I have exogenous variation in the absence tax cost, I can control for unobserved heterogeneity in a flexible way, and I handle the endogenous relation between wages and sickness absence. The detailed information on every single absence spell enables a detailed estimate of the co-insurance cost that the employers have for each worker, as the number of short-term absence days. However, the reform also introduces a data problem. As described in Section 4 I only have data on short-term absence before the reform, i.e. there is no information on short-term absence in 1992, 1993 and 1994. Obviously, as I do not have information on the endogenous explanatory variable after the reform I cannot obtain proper IV estimates. Instead I run informative reduced form regressions using sickness absence lagged two periods.[7] In addition I estimate informative first regressions for the years before the reform. In that way I can, given that the first stage relationship stays the same before and after the reform, reconstruct an IV estimate.

6 Results

6.1 First step estimates

This section presents the first step estimates. Remember that the analysis data set does not include information on short-term absence after the reform, and thus no proper IV estimates could be obtained. As mentioned, instead I run informative placebo first-step regressions for the pre-reform period, and reduced form regressions of the individual wage effect for the full period. The reduced form estimates will be highly informable on the effects of the co-insurance reform. Let us start with the first-step regressions for the pre-reform period. For the first step estimates absence data for 1991 is used. The results from different first step regressions are displayed in Table 2. Column 1 and 2 report first-step estimates for the level of sickness absence, with and without the control variables. The control variables include individual variables, and controls for municipality and firm fixed effects, the same variables as in the most extended specification of equation (11), the final model. The results show a very strong positive correlation between sickness absence and lagged sickness absence, and the relation is basically the same with or without control

[7]For 1994 I have to use sickness absence lagged three periods as short-term absence is only available up until 1991.

Table 2: *First step estimates*

Outcome	(1) S_t	(2) S_t	(3) S_t	(4) $S_t - S_{t-1}$	(5) $S_t - S_{t-1}$
S_{t-2}	0.463**	0.438**			
	(0.00431)	(0.00427)			
S_{t-3}			0.405**		
			(0.00434)		
$S_{t-2} - S_{t-3}$				-0.0098**	-0.0101**
				(0.00145)	(0.00146)
Controls	No	Yes	No	No	Yes
Observations	760,507	760,507	760,507	760,507	760,507
R^2	0.248	0.262	0.212	0.000	0.002
F	11553.6	10501.0	8745.3	45.40	47.42

Notes: The table reports first stage estimates for number of short-term absence days in 1991 and change in absence between 1991 and 1990. Controls include a set of individual variables and section of occupation (2 digits). Standard errors robust to heteroscedasticity and within firm correlation in parentheses. *(**) indicates significance at 5(1) percent level.

variables. Column 3 presents the results when I use absence lagged three periods instead of absence lagged two periods. This result is presented since for 1994 absence lagged three periods is used as instrument, as information on short-term absence is only available up until 1991. Note that the coefficient is almost identical as to that for absence lagged two periods.

The last two columns of Table 2 present the first step estimates for the one period difference in absence, with and without controls. There is a strong negative correlation, which means that those who previously increased their absence two years later in general experience a decrease in their absence. This is likely an effect of mean reversion, where the past increase (decrease) reflects a negative (positive) health shock and the later decrease (increase) reflects the temporary nature of the previous shock.

The presented first step estimates clearly show that lagged absence is highly correlated with present absence during the pre-reform period. But the theoretical prediction is that individuals change their absence as a response to the reform. If all individuals are less absent after the reform I still have a valid instrument, but with a lower coefficient for the first stage relationship between absence and lagged absence. Using the above first stage relationship to reconstruct an IV estimate would then underestimate the true effect. More importantly, in a worst case scenario, those often absent from work before the reform are not so often absent after the reform, and those not absent before the reform start to be absent after the reform. If such flipping behavior is present, it means that I only have a valid instrument before the reform. From the perspective of my theoretical model this is an unlikely outcome of the reform. It is

Table 3: *Placebo first step estimates. Long-term absence explained by lagged long-term sickness absence*

	Pre-reform 1991		Post-reform 1992	
	Day 14-	Day 14-56	Day 14-	Day 14-56
S_{t-2}	0.114**	0.149 **	0.083 **	0.097 **
	(0.0033)	(0.0024)	(0.0032)	(0.0025)
Controls	Yes	Yes	Yes	Yes
Observations	735,803	735,803	750,618	750,618
R^2	0.021	0.035	0.016	0.025
F	1235.7	3740.0	668.1	1521.4

Notes: The table reports first stage estimates for number of long-term absence days in 1991 and 1992. Day 14- refers to total number of absence days from day 14 and onwards of each spell, and 14-56 all such days between day 14 to day 56. Controls include a set of individual variables and section of occupation (2 digits). Standard errors robust to heteroscedasticity and within firm correlation in parentheses. *(**) indicates significance at 5(1) percent level.

also possible to perform a informal test of the flipping hypothesis using long-term absence. If the first stage relationship for long-term absence stays the same before and after the reform, it strengthens the argument against flipping behavior.

Table 3 presents the first step estimates for the pre-reform period in 1991 and the post-reform period in 1992 for long-term absence. Long-term absence is defined in two ways; as total number of days from day 14 and onwards of each spell, and as all absence between days 14 and 56 of each absence spell. The estimates show that in general lagged long-term absence is a less strong predictor of future long-term absence compared with short-term absence and lagged short-term absence. But most importantly, lagged long-term absence are a strong predictor of present absence both before and after the reform. The relationship is somewhat weaker after the reform, but the two estimates are only significantly different from each other when using full long-term absence (day 14-). I am therefore confident in the validity of the instrument both before and after the reform.

6.2 Main results

I now turn to the main reduced form estimates. Before presenting the estimates of equation (11), consider the results from a simple cross-sectional model as presented in Column 1 of Table 4. The outcome is the wage in 1992 (the year of the reform) and as explanatory variable I have the absence level lagged two periods. The estimates show that there is a strong significant cross-sectional relation between the wage and lagged absence. I have multiplied the wage with 100 and the coefficient should therefore be interpreted as a 0.5 percent

Table 4: *Reduced form estimates of the relationship between short-term absence and wages*

Outcome	(1) $\ln w_{i92}$	(2) $\Delta \ln w_{it}$	(3) $\Delta \ln w_{it}$	(4) $\Delta \ln w_{it}$	(5) $\Delta \ln w_{it}$
$D_{92}S_{t-2}$	-0.461**	-0.0094	-0.00091	-0.0013	-0.0014
	(0.0205)	(0.0054)	(0.0047)	(0.0018)	(0.0018)
$D_{93}S_{t-2}$		-0.0212**	-0.0102*	0.00070	0.00061
		(0.0063)	(0.0052)	(0.0024)	(0.0024)
$D_{94}S_{t-2}$		-0.0208**	-0.0064	-0.0073**	-0.0074**
		(0.0052)	(0.0038)	(0.0019)	(0.0019)
S_{t-2}		0.0140*	-0.00079	-0.0026	-0.0017
		(0.0056)	(0.0029)	(0.0019)	(0.0018)
$S_{t-2} - S_{t-3}$		-0.0010	0.0024**	0.0019**	
		(0.0011)	(0.00040)	(0.00040)	
Observations	777,297	3,903,359	3,903,359	3,903,359	3,903,359
R^2	0.052	0.480	0.542	0.605	0.605
Time	Yes	Yes	Yes	Yes	Yes
Individual	No	No	Yes	Yes	Yes
Sector(2 dig)	No	No	Yes	Yes	Yes
Municipality	No	No	Yes	Yes	Yes
Firm	No	No	Yes	Yes	Yes
Time X Ind.	No	No	No	Yes	Yes
Time X Sector	No	No	No	Yes	Yes

Notes: The outcome variable is the wage in 1992 (column1) and the difference between time period t and time period t-1 in the logarithm of the wage times 100. Individual variables include sex, immigrant, age, age squared, type of education and level of education. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses. *(**) indicates significance at 5(1) percent level.

wage decrease for each additional day of short-term absence. However, as previously argued this estimate can reflect both selection as well as a general wage effect from absence, and is not necessarily an effect of the co-insurance reform.

Next, consider our causal estimates of equation (11) presented in column 2-5 of Table 4. The coefficients of interest are the three interaction variables, $D_{92}S_{t-2}$, $D_{93}S_{t-2}$, and $D_{94}S_{t-2}$, which measure the reduced form estimate of the additional wage increase penalty from an additional day of absence introduced by the employer co-insurance reform. Remember that the outcome is the difference in the logarithm of the wage times 100, so that for instance the estimate for $D_{92}S_{t-2}$ in Column 2 of -0.0094 means that one additional day of absence decreases the wage change with about 0.01 percent. It also means that the coefficients for 1993 and 1994 measure the additional wage penalty in 1993 and 1994, respectively. The model in Column 2 includes only the five main variables and a set of time controls. The coefficient for 1992 the year of the reform is negative but insignificant, and the coefficients for 1993 and 1994 are both negative and significant at one percent confidence level. This suggests that the effect of the reform is delayed one year. Column 3-4 present results from models when additional control variables are added stepwise into the model. Adding more and more control variables changes the initial conclusion. The size of the 1993 and 1994 coefficients gradually decreases as more controls are included into the model. The full model, presented in Column 4, includes a full set of individual variables and sector dummies, as well as these interacted with calendar time. In this model only the 1994 coefficient is significant. The estimate of the wage effect in 1993 even has incorrect sign. The same result is obtained from a simpler model excluding the lagged difference in absence, presented in Column 5.

The results for 1992 and 1993 suggest that there is no individual wage effect. The question then becomes whether the significant result for 1994 means that there are important wage effect that are delayed two years. If I take the pre-reform period first step estimate and reconstruct an IV estimate this suggests that one day of additional sickness absence decreases the wage increase with about 0.018 percent.[8] The difference between P75 and P25 of short-term absence is 13 days, which implies that an individual at P75 can expect about a 0.24 percent (0.018*13) lower wage change increase compared to an individual at P25. This can be compared with the mean wage increase in 1992 of 3.4 percent, in other words a quite small effect. Moreover, the average worker in Sweden works about 220 days a year, it means that 13 days of absence amount to about 4.7 percent of the labor costs.[9]. Compared to this the wage change effect of 0.24 percent is very small. All coefficients are also very precisely estimated. Based on these main results, with small and precisely estimated coefficients, I rule out any sizeable individual wage effects from the co-insurance reform.

Before proceeding to a more detailed robustness analysis I will explore a potential threat with using the co-insurance reform as a quasi-experiment. The beginning of the 1990s was a turbulent period for the Swedish economy. In the late 1980s the unemployment rate in Sweden was extremely low (about 2% in 1988), and by 1994 it had increased to about 8%. It is natural to expect that this would affect wages. Worsened economic conditions will decrease worker's bargaining power. Crucially, they may affect bargaining power asymmetrically across workers with different absence levels. As discussed above those often absent include workers with bad health. In a recession it is reasonable

[8]The reduced form estimate is -0.0073 and the first step estimate is 0.40, which gives an IV estimate of $-0.0073/0.40 \approx -0.018$. Also note that the outcome is measured as the wage increase in percent (the logarithm times 100).

[9]Take a replacement rate of 80 percent. The wage and the benefit are both taxable. I then have labor cost percentage as $13 * 0.8/220 \approx 0.047$

Table 5: *Sample selection. Reduced form estimates of the relationship between absence and wages*

	(1) Main sample	(2) Only if working all years
$D_{92}S_{t-2}$	-0.00133	-0.00603**
	(0.00179)	(0.00228)
$D_{93}S_{t-2}$	0.000698	0.00387
	(0.00238)	(0.00341)
$D_{94}S_{t-2}$	-0.00729**	-0.00515
	(0.00193)	(0.00279)
S_{t-2}	-0.00260	-0.00378
	(0.00191)	(0.00263)
$S_{t-2} - S_{t-3}$	0.00187**	0.00236**
	(0.000400)	(0.000579)
Observations	3,903,359	1,142,229
R^2	0.605	0.740

Notes: The main sample is the sample presented in the data section. The smaller sample imposes the additional restriction that the individual should be employed all years between 1989 and 1994. The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Individual variables include sex, immigrant, age, age squared, type of education and level of education. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses. *(**) indicates significance at 5(1) percent level.

to expect that these worker's face a higher risk of being fired, and their bargaining power is most likely more negatively affected compared with other workers. This creates two potential problems.

First, the composition of employed workers in the late 1980s will be different compared with the composition at the beginning of the 1990s. This is taken into account by re-estimating the final model for a smaller sample of individuals including only those who were employed during the whole research period (1989-1994). The results from this exercise are presented in Table 5. Column 1 restates the main results (full sample), and Column 2 presents the results from our smaller sample of individuals employed during the whole period. The results from the smaller sample differ somewhat from the results from the main analysis. The estimate of the wage effect in 1992 is negative and significant, the estimate for 1993 is positive and insignificant, and the estimate for 1994 is insignificant. The size of all the estimates is very small. Hence, these estimates give no reason to alter the conclusion about no sizeable wage effects.

Second, if the bargaining power of insurance-prone workers decreases more as a result of the recession it will bias the results towards showing stronger wage effects. Workers who are often absent may simply experience smaller wage increases at the beginning of the 1990s as a result of decreasing bargaining power. This means that our small and precisely estimated insignificant wage effects could be considered as an upper bound on the wage effects. To investigate this conclusion more carefully I now turn to different kinds of robustness analysis.

7 Robustness analysis

7.1 Placebo regressions

To further analyze the conclusion of no sizeable wage effects from the co-insurance reform I run placebo regressions, which test for any pre-existent trends by interacting S_{t-2} with a dummy for each of the five years used in our analysis. The coefficients for 1991 and 1990 then represent treatment effects for non-existent reforms. Any significant estimates for these two years before the reform indicate a problem with pre-existent trends unaccounted for by our large set of control variables. Column 1 of Table A.1 presents results for the full sample and Column 2 for the smaller sample of individuals employed during the whole research period. In both models I find insignificant coefficients for 1990 and 1991 the two years before the reform. It seems that the full model is able to account for all pre-reform trends, thereby strengthening the main conclusion.

7.2 Effect on firm level?

In the baseline specification it was assumed that employers could shift their insurance cost over onto individual wages. Even if there are individual wage differences, it may be the case that instead of individual wage effects, all workers in high-absence firms receive lower wage increases as an result of the co-insurance reform. To test this hypothesis I estimate the same models again, but replace individual absence with firm absence. The results from first step estimates as well as different reduced form estimates are displayed in Table A.2. The first step estimates, reported in Column 1, show a very high correlation between present firm absence and firm absence lagged two periods. The correlation is even stronger than for individual absence.

Next consider the results from the reduced form estimates. Here I simplify the exposition by assuming that the wage effect is the same in 1992, 1993 and 1994. The same simplification is used throughout the remaining robustness analyses. Column 2 reports results from our main model with a full set of control variables. Column 3 presents a nested model including both firm absence and individual worker absence into the same model. The results from these two specifications suggest significant firm level wage effects from the co-insurance reform. However, the results in Column 4 reverse this conclusion. Column 4 reports the results from a placebo regression, where I have interacted firm level absence with a dummy for each year. These results reveal strong pre-existent trends in 1991 and 1990. In addition the sign of the effect for 1992 is now reversed, indicating a positive wage effect of the co-insurance reform. Based on these results I conclude that there is no robust evidence for any important wage effects at the firm level.

7.3 Heterogeneous treatment effects

One key assumption for the analysis presented so far is that wages are set at least partly individually. The degree of individualized wages differs a lot between different types of workers. Workers with high education and workers employed in the private sector face more individualized wages, which theoretically suggests larger individual wage effects. In principle it could the case that any important individual wage effects in some sectors or in some education levels are hidden in the insignificant estimates above. I test for this by re-estimating the model allowing for heterogeneous γ_τ by sector and by level of education.

The results from this analysis are presented in Table A.3 and Table A.4. If the hypothesis about only wage effects in sectors and education groups with high degree of individualized wages is true, there should be negative and significant signs for white-collar workers, central government workers, and for highly educated workers. The results for different sectors are inconsistent with this pattern. I find negative significant coefficients for central government, regional government workers and blue-collar workers and significant positive coefficients for white-collar workers. The pattern for different education groups is more in line with the individual wage hypothesis, as expected the coefficients for the most highly educated groups are significant and negative coefficients, but again the size of the estimates is very small. Taken together there is no reason to alter the conclusion that there are no important individual wage effects from the co-insurance reform.

7.4 Functional form

The basic model estimated above specifies a linear effect, which may be too restrictive. As an additional robustness analysis I therefore present results from two additional more flexible specifications, including a model with four polynomials of the lagged mean absence, and a second model were the individuals have been grouped into six groups according to their lagged sickness absence. Table A.5 displays these results. The results from the polynomial model give very similar results to the baseline specification. The linear effect is similar to above and counteracted by a positive and significant second order polynomial coefficient. In addition the second specification with individuals divided into groups according to their lagged absence produces no significant estimates.

7.5 Sample selection

The final robustness analysis regards the sample selection. As discussed in Table 4, several criteria have to be fulfilled in order to include the individual in the analysis sample. The main restrictions are that only working individuals who stay at the same firm during at least two consecutive years are used in the analysis. In addition some individuals with extreme wage increases respectively and some individuals with extreme sickness absence are excluded from the sample. In this section I investigate whether these restrictions influence our estimates. Table A.6 presents this robustness analysis. Column 1 restates our main results in order to simplify the comparison. The sample used in the second model excludes all individuals with exactly zero wage increase, since this is a indication of misreporting in the wage survey data. The sample used in model 3 excludes additional individuals with extreme sickness absence, and models 4-5 exclude individuals with additional extreme wage increase. The results from these specifications show that the results are quite insensitive to these different sample restrictions.

8 Conclusions

This paper has investigated whether introducing direct employer incentives in the form of employer co-insurance into the Swedish sickness insurance system affected individual wages. The reform introduced a direct cost for employers for each day of short-term absence among their workers. Since sickness absence varies substantially between individuals, the reform meant that employer's costs increased sharply for some workers and decreased the costs for other workers. Using detailed information on the absence of each individual, past and current employment, and survey information on wages, I provide a direct test of a wage effect from increased labor costs in the form of co-insurance.

The result interestingly shows small and insignificant individual wage effects from the co-insurance reform. Since I am using a population database the estimates are also very precisely estimated. Extensive robustness analyses have also been performed, with respect to placebo regressions, functional form, sample selection, and I have checked for wage effects in certain sectors and for certain education level groups. They all support the main conclusion of no sizable wage effects. In addition any bias due to business cycle effects would have been towards showing wage effects. I can therefore rule out any sizeable wage effect from the Swedish co-insurance reform.

These results could be related to the previous scarce evidence on individual wage effects and employer incentives. Anderson & Meyer (2000) find wage effects from experience rating in unemployment insurance, Gruber (1994) find wage effects from mandated maternity benefits, and Goldman et al. (2005) find wage effects as a result of increased health insurance premiums. Interestingly Baicker & Chandra (2005) find no significant wage effects from a growth in health insurance premiums; instead they find significant effects on hours worked and on individual employment. This study contributes to this literature for at least two reasons. First of all, I provide evidence concerning the Swedish sickness insurance system, which resembles many workers compensation and disability insurance systems around the world. Previous evidence on the other hand is for other types of insurances, and as there is no reason to expect that the effect is the same across insurances types this study contributes with valuable insights. Second, previous evidence is concentrated to USA where wages are more often bargained on an individual level compared with many European countries. This study gives results that are very relevant for labor markets with somewhat more centralized wages, such as many European countries. It is however important to note that our results are not entirely driven by lower levels of individual wage bargaining, as our data reveal sizeable wage increase differences among workers within the same workplace, even after controlling for a rich set of control variables.

There are several possible explanations to these precisely estimated insignificant wage effects of the co-insurance reform. Even if firms had no direct tax cost each time their workers are absent from, employers have substantial indirect costs for absent workers such as costs due to production losses and costs associated with finding a replacement worker. If these costs are very large the additional cost in the form of the co-insurance tax may be less important. This conclusion is indirectly supported by the results in Andren & Palmer (2001), Hansen (2000) and Hesselius (2004), which indicate that work absence in general has a strong impact on individual wages. Another possible explanation is that employer regulate their costs by firing or avoiding hiring insurance-prone workers. If employers cannot shift the co-insurance cost over to individual wages, they can avoid the co-insurance costs by firing and/or avoiding hiring insurance prone workers. In other words the non-existent wage effects indicate that cream-skimming may have intensified as a result of the reform. Cream-skimming has severe negative impact on the employment possibilities of insurance-prone workers, i.e. workers with bad health. This conclusion is supported by the results in Harcourt et al. (2007) and Baicker & Chandra (2005), who both find employment effects.

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Appendix

Table A.1: *Placebo regressions. Reduced form estimates of the relationship between absence and wages*

	(1) Basic Model	(2) Only if working all years
$D_{90}S_{t-2}$	-0.00555	-0.00756
	(0.00344)	(0.00438)
$D_{91}S_{t-2}$	0.000127	0.000781
	(0.00112)	(0.00121)
$D_{92}S_{t-2}$	-0.00382**	-0.00970**
	(0.00103)	(0.00157)
$D_{93}S_{t-2}$	-0.00214	-0.000825
	(0.00115)	(0.00154)
$D_{94}S_{t-2}$	-0.00804**	-0.00800**
	(0.000901)	(0.00116)
Observations	3,904,703	1,142,229
R^2	0.605	0.740

Notes: The main sample is the sample presented in the data section. The smaller sample imposes the additional restriction that the individual should be employed all years between 1989 and 1994. The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses. *(**) indicates significance at 5(1) percent level.

Table A.2: *Firm absence. First step and reduced form estimates of the relationship between absence and wages*

Outcome	(1) $Sfirm_t$	(2) $\Delta \ln w_{it}$	(3) $\Delta \ln w_{it}$	(4) $\Delta \ln w_{it}$
$DSfirm_{t-2}$	0.798**	-0.195**	-0.198**	
	(0.000323)	(0.0343)	(0.0345)	
$Sfirm_{t-2}$		0.196**	0.204**	
		(0.0281)	(0.0285)	
$Sfirm_{t-2} - Sfirm_{t-3}$		-0.0942**	-0.0969**	-0.160**
		(0.0187)	(0.0188)	(0.0262)
S_{t-2}			-0.00808**	
			(0.00158)	
DS_{t-2}			0.00197	
			(0.00147)	
$S_{t-2} - S_{t-3}$			0.00330**	
			(0.000396)	
$D_{90}Sfirm_{t-2}$				0.406**
				(0.0577)
$D_{91}Sfirm_{t-2}$				-0.103**
				(0.0376)
$D_{92}Sfirm_{t-2}$				0.160**
				(0.0330)
$D_{93}Sfirm_{t-2}$				-0.129**
				(0.0377)
$D_{94}Sfirm_{t-2}$				-0.0342
				(0.0225)
Observations	760,507	3,903,359	3,903,359	3,903,359
R^2	0.889	0.606	0.606	0.609

Notes: The table reports first stage estimates for number of short-term absence days in 1991 and reduced form estimates for the wage increase between time period t and time period $t-1$ in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses.*(**) indicates significance at 5(1) percent level.

Table A.3: *Reduced form estimates. Heterogeneous effects by sector of employment*

	Estimate	S.e.
Central Gov.	-0.00767*	0.00335
Regional Gov.	-0.00748*	0.00300
Municipal. Gov.	0.00301	0.00249
Blue-Collar	-0.00700*	0.00310
White-Collar	0.00243	0.00302
Observations	3,904,703	
R^2	0.605	

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation. *(**) indicates significance at 5(1) percent level.

Table A.4: *Reduced form estimates. Heterogeneous effects by education level*

	Estimate	S.e.
Education Level 1	-0.000349	0.00198
Education Level 2	-0.00179	0.00199
Education Level 3	0.0000685	0.00187
Education Level 4	-0.00187	0.00196
Education Level 5	-0.00593**	0.00174
Education Level 6	-0.00854**	0.00189
Observations	3,904,703	
R^2	0.605	

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation. *(**) indicates significance at 5(1) percent level.

Table A.5: *Reduced form estimates. Polynomial models and individuals grouped by lagged absence*

	(1)	(2)
DS_{t-2}	-0.00840**	
	(0.00199)	
DS^2_{t-2}	0.000265**	
	(0.0000684)	
DS^3_{t-2}	-0.00000296*	
	(0.00000132)	
DS^4_{t-2}	9.56e-09	
	(7.80e-09)	
$DS_{group2,t-2}$		-0.0405
		(0.0282)
$DS_{group3,t-2}$		-0.0627
		(0.0430)
$DS_{group4,t-2}$		-0.0625
		(0.0578)
$DS_{group5,t-2}$		-0.0976
		(0.0745)
$DS_{group6,t-2}$		-0.0918
		(0.115)
Observations	3,904,703	3,904,703
R^2	0.605	0.605

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses. *(**) indicates significance at 5(1) percent level.

Table A.6: *Robustness analysis sample selection. Reduced form estimates of the relationship between absence and wages*

	(1) Basic Model	(2) Exclude if $w_t = w_{t-1}$	(3) Exclude if $S_{t-2} > 40$	(4) Exclude if $w_t - w_{t-1} < -(\bar{w}_t - \bar{w}_{t-1})$	(5) Exclude if $w_t - w_{t-1} < 0$
DS_{t-2}	-0.00223	-0.00272	-0.00445*	-0.00323	-0.00213
	(0.00185)	(0.00183)	(0.00188)	(0.00175)	(0.00183)
S_{t-2}	-0.00247	-0.00262	-0.00106	-0.000426	-0.000714
	(0.00193)	(0.00195)	(0.00200)	(0.00189)	(0.00194)
$S_{t-2} - S_{t-3}$	0.00159**	0.00165**	0.00135**	0.00115**	0.000824
	(0.000407)	(0.000486)	(0.000503)	(0.000422)	(0.000446)
Observations	3,904,703	3,426,057	3,183,205	3,017,082	2,910,583
R^2	0.605	0.584	0.579	0.687	0.708

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust to heteroscedasticity and within firm correlation reported in parentheses. *(**) indicates significance at 5(1) percent level.

Essay 3: Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality[1]

1 Introduction

Unemployment Insurance (UI) systems typically include monitoring of unemployed workers and punitive sanctions for those who do not comply with job search requirements (see e.g. OECD, 2000, for an overview). Van den Berg et al. (2004) is the first published study of the causal effect of a punitive sanction on the transition rate from non-employment to employment. Since then, a range of similar studies has been carried out for different countries and time periods. See Van den Berg & Van der Klaauw (2005, 2006), for overviews. These studies do not consider the effect of a sanction on the type of job accepted. From a welfare point of view as well as from the point of view of the unemployed individual, such effects are important. If the job accepted after a sanction is similar to the job accepted in the counterfactual situation of no sanction, then severe sanctions and intensive monitoring have less adverse long-run effects than if the former job is often worse than the latter. This relates to the more general issue of how steeply benefits should decline as a function of the elapsed unemployment duration, to balance moral hazard with the likelihood that unemployed individuals are driven into sub-optimal job matches (see e.g. Acemoglu & Shimer, 2000).

In this paper, we address the effects of sanctions on the quality of the job that is accepted. We distinguish between effects on the wage and on working hours (specifically, full-time versus part-time). Wages and hours are potentially relevant margins along which unemployed individuals make job acceptance decisions. We use register data covering the full Swedish population over 1999–2004. This includes several hundreds of thousands of unemployment spells. The register data also include information on a large range of background characteristics of the individual, his/her household, and his/her local labor market conditions. If a spell is observed to end in a transition to work then in many cases we observe the above-mentioned job characteristics. Notice that observation of a wage rate is very unusual in register data on employment or, indeed, in annual longitudinal panel survey data. Such data typically only record annual income or annual earnings, which are compo-

[1]Co-authored with Gerard J. van den Berg, Department of Economics VU Amsterdam, IFAU-Uppsala and IZA.

site measures based on both wages and hours worked. Our data enable us to distinguish between effects on wages and effects on hours.

One may argue that any effects on characteristics of the first accepted job after unemployment may fade away swiftly as individuals have the opportunity to search on the job and make transitions to jobs with better characteristics. We investigate this by examining the job conditions that prevail several years after the sanction. Moreover, we examine whether individuals make job acceptance decisions after a sanction that are more or less irreversible. Specifically, we observe the occupation of the accepted job, and we observe to what extent this differs from the occupation of the pre-unemployment job. On average, acceptance of a job with a lower occupational level involves a larger loss of human capital than acceptance of a job in the same occupation. This loss becomes irreversible as human capital depreciates over time. It may therefore be more difficult for the individual to move out of a bad job match if the job has a lower occupational level. This makes it important to know whether sanctions often lead to a match in a lower occupational level. By measuring the required number of years of education for each occupation, we can quantify the human capital loss due to the occupational downgrading caused by a sanction. Because of the existence of separate educational tracks, this is likely to be a lower bound of the true loss.

The empirical analyses are based on the "timing of events" approach (see e.g. Abbring & Van den Berg, 2003). This involves the estimation of duration models for the duration to job exit and the duration until treatment (i.e., a sanction), exploiting random variation in the timing of the treatment and taking into account that treatment assignment may be selective in that the durations may be affected by related unobserved determinants. This is the standard approach in the literature on sanction effects. Indeed, one may claim that punitive treatments provide a best case application for this approach. This is, first of all, because the moment at which an individual is caught is by definition unanticipated by the individual, so that the "no anticipation" assumption on the joint distribution of counterfactuals is satisfied. Accordingly, the time until treatment. is to some extent driven by an element that is random from the individual's point of view. Secondly, unconfoundedness assumptions are almost by definition likely to be invalid, because individuals can only logically display inadmissible behavior if this behavior or its determinants are not fully observable in standard registers. To address effects of dynamically assigned treatments on post-duration outcomes, like post-unemployment wages, it becomes a necessity to deal with dynamic selection due to unobserved heterogeneity even if the assignment process is randomized (see Ham & LaLonde, 1996, and Abbring & Van den Berg, 2005).

In addition to the analysis of sanction effects on job characteristics, our paper makes three other major contributions to the literature (for convenience, we refer to these as contributions 2, 3 and 4). To understand the importance of two of these, we should start by pointing out two special features of the Swedish UI monitoring system. First, the monitoring of an unemployed individual is carried out by the same case worker who also provides job search assistance to the individual. This case worker is the only person who can take the initiative to give a sanction. This is a marked difference with monitoring in other countries, which is typically carried out by agencies that are distinct from the agencies providing job search assistance to the unemployed. Secondly, after inflow into UI, monitoring focuses on job offer decisions, in the sense that unemployed individuals are not supposed to reject suitable job offers. This is also in contrast to monitoring in other countries, which typically focuses on search effort, as measured by the number of applications sent out or indicators of the willingness to adhere to job search guidelines.

The second major contribution of the paper is that we study a policy change in the monitoring system during the period under observation. Before February 5, 2001, the only possible punishment rate was a rate of 100% (i.e., complete UI benefits withdrawal) for a certain amount of time. After that, the default rate was 25%. The underlying motivation for this change was that the personal connection between the case worker and the person he/she was supposed to help made it difficult for the former to propose a punishment that amounted to the full withdrawal of the latter's income. It was felt that more modest sanctions would increase the threat effect of sanctions and thereby would increase the exit rate to work for those not (yet) punished. The decision to change the punishment rate was made and announced only shortly before the implementation date. In theory, this provides a "regression discontinuity" that the analyst may use to identify the threat effect of a monitoring system. With our population-level data, we aim to pursue this. We examine changes in sanction rates and the exit rate out of unemployment before and after the policy change.

The estimation results and differences with estimates in the literature can be understood by resorting to a job search theoretical model framework. The third major contribution of the paper is that we develop and analyze a theoretical model with monitoring of job offer *decisions* in the presence of wage variation. The theoretical predictions can be contrasted to those from a model with monitoring of job search *effort* or search *intensity*. We find some qualitative differences, and these by itself contribute to the understanding of efficient policy. Notice that monitoring of offer decisions increases the relevance of studying effects on job quality, because rejected offers typically concern jobs with a low job quality.

The fourth major contribution is methodological. "Timing of events" models are usually estimated with random samples from the inflow into the state of interest, by maximum likelihood. However, in the case of a rare treatment, the random sample needs to include many individuals in order to obtain a sufficient number of individuals who are observed to be treated. Estimation with very large samples is computationally demanding. We therefore propose to estimate the models with endogenously stratified samples, using weighted exogenous sampling maximum likelihood (WESML). Accordingly, the sample we use contains all individuals observed to get a sanction, plus a subsample of the other individuals. This estimation method has not yet been used in the context of bivariate dependent-duration models (see Ridder, 1986, and Amemiya & Yu, 2006, for applications to univariate duration analyses with endogenously stratified samples). The method requires certain aggregate population statistics, but recall that we observe the complete population of Sweden.

The main empirical result of the paper is that, on average, sanctions increase the transition rate into work with 23%, but cause individuals to accept jobs with a lower hourly wage and less working hours per week. The estimated average reduction in the accepted wage is almost 4%. In addition, sanctions causally increase the likelihood of the acceptance of a job at a lower occupational level, incurring a permanent human capital loss that is on average equivalent to at least some weeks of formal education. The theoretical analysis suggests that these adverse effects can be partly (but not fully) prevented if the system of job-offer-decision monitoring is replaced by a system of search-effort monitoring. The combination of the theoretical analysis and the data analysis suggest that the current Swedish system does not exert substantial "ex ante" or threat effects of monitoring on the job exit rate of not-yet punished unemployed individuals. It is plausible that a system of search-effort monitoring that is not carried out by the case worker who provides job search assistance would actually create a larger threat effect. Methodologically, our paper suggests that WESML with an endogenously stratified sample containing all treated is a very useful method for the estimation of causal effects of rare endogenous events on duration outcomes, if one has access to a large data set and population statistics. In particular, it is very useful for the estimation of dynamically assigned treatments on duration outcomes if treatments are rare and one has population-level register data.

The outline of the paper is as follows. Section 2 presents the institutional setting. It discusses the Swedish UI system and the role of monitoring and sanctions in that system. It also describes the monitoring policy reforms in our observation window. Section 3 provides the theoretical job search framework and derives theoretical predictions. Section 4 gives a detailed description of the data. In Section 5 we discuss the empirical approach and the WESML estimation method. Section 6 presents the empirical results. Section 7 concludes.

2 Unemployment insurance

2.1 Unemployment insurance entitlement

This subsection describes the relevant features of the UI system on January 1, 2001. In Subsections 2.2 and 2.3 we discuss the monitoring system and the corresponding policy change in 2001. For a detailed description of other UI reforms during our observation window see e.g. Olli Segendorf (2003). These are mostly reforms in local features of the function from the labor market history to the UI level.

An unemployed (part-time or full-time) individual in Sweden is entitled to UI benefits if a range of conditions are fulfilled. First, the individual must have been member of an unemployment insurance fund for at least 12 months and should have had a job for at least six months in the past 12 months. Secondly, (s)he needs to be registered at the public employment service (PES) and has to be able and willing to work at least three hours a day and at least 17 hours per week. Further, (s)he must state that (s)he is actively searching for employment.

Those who fulfill these conditions are entitled to wage-related UI benefits. These amount to 80% of the average earnings during the latest six months of employment, with a floor and a ceiling. In the beginning of 2001 these were SEK 270 (≈ €25) and SEK 580 a day (≈ €55) per day. Individuals who have not been a member of an UI fund for at least 12 months may qualify for the Unemployment Assistance (UA) system. Compensation in UA is unrelated to previous earnings and the generosity of UA is much lower than UI. In our analysis we restrict attention to UI recipients. To retain UI during a spell of unemployment, the individual needs to remain eligible.

In 2001, the entitlement duration of UI benefits was 300 days for everyone. The benefits could either be collected continuously or with breaks in between the collection periods. If the individual finds a job and retains it for six months then he qualifies for new entitlement period. The individual also continues to collect UI benefits while being enrolled in a specific labor market program (the activity guarantee).[2] UI benefits are mainly financed by proportional pay-roll taxes.

2.2 Monitoring and sanctions

The monitoring of an unemployed individual is carried out by the case worker at the PES office. This is the same person as the case worker who provides job search assistance to the individual. The case worker's identity usually does not change during the unemployment spell.

The case worker is supposed to examine whether the individual's job search behavior is in accordance to the UI guidelines. This concerns the verification that the individual has not rejected suitable job offers. The case worker is the only person who can take the initiative to give a sanction. A sanction is a benefits reduction for a limited time as a punishment for violation of the

[2]Case workers assess the need for program participation if individuals are close to the end of their entitlement period. If such need is found then the individual is assigned to the "activity guarantee" which includes different monitoring activities.

guidelines.[3] The case worker is also supposed to verify during the course of an unemployment spell that the unemployed individual does not violate the UI entitlement conditions in the first place. This concerns, for example, unreported employment. If the individual is deemed non-eligible then he is not registered anymore as being unemployed. Moreover, his UI benefits payment is terminated immediately and for an indefinite period of time.[4]

The assignment of a sanction involves a number of stages. First, the case worker at the PES office observes an infringement. The employment office then prepares a report to the unemployment insurance fund, stating the infringement but not the sort of sanction it thinks is suitable. The unemployed individual is informed about the report and is given the opportunity to comment on his behavior. In practice, case workers may contact the unemployed individual before preparing the report, to prevent that the apparent infringement was due to a misunderstanding. A copy of the report is sent to the central public unemployment office (AMS).[5] In the third stage, a decision about the sanction is made by the unemployment insurance fund, and a motivation is provided. In 86%, the PES report results in approval of a sanction by the board; see IAF (2007). In a fourth stage, there may be an appeal to revert the decision. About 10% of all decisions are asked to be reverted, but in only about 20% of these is the decision partly or fully reversed. Subsequently, one may appeal against a sanction at the county administrative court (Länsrätten).

There are several unpredictable events in this process. The case workers do not always observe that an unemployed has turned down a job offer. Whether a report is written or not depends on the attitude of the case worker (Swedish overviews, like IAF, 2006, state that case workers report themselves that there are differences in interpretation of the regulations between counties and employment offices and between individual case workers working at the same employment office). The benefit sanction decision may also depend on the board members attending the UI fund meeting. All this makes it unlikely that UI claimants anticipate the imposition of the sanction with great accuracy.

Before the reform of February 5, 2001, the only available sanction was a 100% reduction of the benefits level for a period of 10 to 60 days. The choice of the length of the sanction period was supposed to take the (subjectively

[3]In addition to this, UI benefits can be reduced if the individual has left employment without a valid reason or due to improper behavior at the work floor. UI is then suspended for a maximum of 45 days. We do not analyze this type of temporary benefits reduction because our data do not allow for a distinction between causal effects and selection effects of treatments that start at the beginning of a spell.

[4]In addition, eligibility is terminated if the individual sabotages cooperation with the employment office, for example by refusing participate in an individualized "action plan" which is a pathway back to work with possibly active labor market program participation. In accordance with the definition of unemployment, we regard such eligibility losses as exits from the state of unemployment.

[5]Nowadays, the inspection of the unemployment insurance (IAF) rather than AMS receives a copy.

assessed) expected duration of the rejected employment into account. In practice, however, only a period of 60 days was used.

As noted in Section 1, the Swedish monitoring system was (and is) notably different from the systems in many other countries (see Grubb, 2000, for details about the systems in other countries). First, monitoring and job search assistance are carried out by the same case worker. In other countries, monitoring is typically carried out by agencies that are distinct from the agencies providing job search assistance. Secondly, after inflow into UI, monitoring mainly restricts attention to job offer rejections. Other countries focus primarily on search effort, as captured by the number of applications sent out or indicators of the willingness to adhere to job search guidelines.

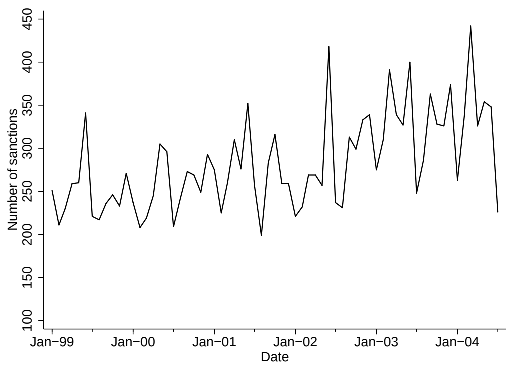

Figure 1: Monthly number of sanctions 1999-2004.

Accordingly, Sweden is an outlier in aggregate statistics of sanctions. First, the number of sanctions issued is very low. Figure 1 displays this number per month, between January 1999 and November 2004. In 2000, about 3000 sanctions were issued, on an average stock of 210,000 full-time unemployed UI recipients. In Gray (2003)'s ranking of countries by sanction occurrence (which, roughly speaking, is defined as number of sanctions divided by the number of unemployed), Sweden is the lowest among the nine European countries considered (Sweden 0.79, Germany 1.14, Belgium 4.2, Denmark 4.3, Finland 10.2, United Kingdom 10.3, Norway 10.8, Czech Republic 14.7, Switzerland 40.3). Figures in other OECD countries are typically much higher than the Swedish figure as well. Abbring et al. (2005) report that around 3% of the inflow of UI recipients receive a sanction during the UI spell, in The Netherlands in 1993. Contrary to Sweden, a number of these countries, including Germany, The Netherlands, and Denmark, has witnessed increases in the occurrence of sanctions since the early 2000s (see e.g. Svarer, 2007, and Schneider, 2008). We shall argue below that the low Swedish sanction occurrence can be explained by institutional differences in the monitoring system.

2.3 Policy change of the monitoring regime

The uniquely low occurrence of sanctions in Sweden can be explained by a low effective level of monitoring. In the late 1990s it was felt that the magnitude of the punishment (100% UI benefits reductions for 60 days) was too large in the eyes of the case workers. After all, the case worker is primarily trying to help the unemployed individual, and the former would find it morally difficult to punish the latter harshly. This could prevent case workers from reporting violations. At the time, many other countries have policies where sanctions are smaller than 100% of the UI level. Accordingly, the Swedish government changed the policy design on February 5, 2001 (see e.g. government prop. 1999/2000:139 for the motivations behind the reform.) From then on, UI is reduced by 25% for 40 days for first-time offenders, and by 50% for 40 days second-time offenders. A third violation during the same UI entitlement period entails a full loss of benefits until new employment has been found. The decision to change the monitoring policy was made on December 21, 2000, which is 1.5 month before enforcement. The public employment office AMS arranged regional meetings to inform the case workers about the policy change. These were held between the middle of February, 2001, and April, 2001. Case workers complained that after these meetings certain details of the new policy regime were still not clear to them (personal communications).

Despite the policy change, the occurrence of sanctions has remained low by international standards. In Subsection 4.3 we examine whether the occurrence of sanctions in our individual data register displays differences before and after the implementation date.

3 Theoretical insights

3.1 A job search model with monitoring of job offer decisions

In this subsection we present a job search model with monitoring of job offer decisions. This model takes distinguishing features of the Swedish UI monitoring system into account and has not been analyzed in the literature. It is a model of optimal behavior of unemployed individuals given the presence of a particular system in which sanctions can be imposed. The model helps to understand the effects of such a system on individual behavior. It also provides insights into the determinants of the rates at which jobs are found and sanctions are imposed and the relationships between these rates.

Our point of departure is a basic job search model with a fixed individual search intensity. Consider an unemployed individual who searches sequentially for a job. Job offers arrive according to the rate λ. Jobs are heterogeneous in their characteristics. For expositional convenience we take the wage as the only possible job characteristic in this subsection. Offers are random drawings from a wage offer distribution $F(w)$. Every time an offer arrives the decision has to be made whether to accept it or to reject it and search further. Once a job is accepted it will be held forever at the same wage. During unemployment, a flow of benefits b is received, possibly including a non-pecuniary utility of being unemployed. The individual aims at maximization of the expected present value of income over an infinite horizon.

It is well known that in this model, under some regularity conditions, the optimal strategy of unemployed individuals can be characterized by a reservation wage ϕ, giving the minimal acceptable wage offer. The transition rate to work equals $\lambda(1-F(\phi))$.

Now let us introduce monitoring in this model framework. We assume that the case worker samples a fraction p of rejected job offers, and that on average a fraction q of these rejected offers are deemed to be sufficiently suitable for the unemployed worker. Then a fraction pq of the rejected offers should not have been rejected. Accordingly, the sanction rate equals $\lambda F(\phi)pq$. If $p = 1$ then all offers are monitored, and if $p = q = 1$ then each rejected offer entails a sanction. For a given p and q, we assume that the individual does not know which rejected offers are sampled or which are deemed acceptable by the case worker, but that he does know the values of p and q.

Some individuals will be more willing to take the risk of being given a sanction than others, *e.g.* because they have a higher non-pecuniary utility of being unemployed. Obviously, if $p = q = 1$ and the punishment is sufficiently severe in comparison to a job with the lowest possible wage, then all job offers are always accepted, and sanctions would never be given. To proceed, we need to be specific about what occurs after the imposition of a sanction. First of all, benefits (b) are reduced substantially. Secondly, p is likely to increase. If the individual again violates the rules concerning job offer decisions, and this is observed by the case worker, then additional benefits reductions are imposed. We assume that the punishment for additional violations is so severe that the individual will avoid this at all cost, so we assume that all offers are accepted after imposition of a sanction. This implies that sanctions are imposed at most once in a given spell of unemployment. (A strategy in which individuals take a job upon imposition of a sanction, and quit immediately in order to make a "fresh start" in UI, would not be optimal: UI would be reduced again immediately after quitting because of "insufficient effort to prevent job loss"; see Section 2.)

For simplicity, we assume that the parameters b_1 (being the benefits level before a sanction is imposed), F, λ, p, q and the discount rate ρ are constant as a function of unemployment duration. Upon imposition of a sanction, b is permanently reduced from b_1 to b_2, with b_2 constant as a function of unemployment duration. As a consequence, both within the time interval before a sanction and within the time interval after a sanction, the optimal strategy is constant over time.

Let R_1 and R_2 denote the expected present value of income before and after imposition of a sanction, respectively, and let ϕ_1 denote the reservation wage before the sanction. We obtain

$$\rho R_1 = b_1 + \lambda \mathbb{E}_F \left[\max\{\frac{w}{\rho} \ , \ (1-pq)R_1 + pqR_2\} - R_1 \right] \tag{1}$$

$$\rho R_2 = b_2 + \lambda \mathbb{E}_F \left[\frac{w}{\rho} - R_2 \right] \tag{2}$$

$$\text{with } \phi_1 = (1-pq)\rho R_1 + pq\rho R_2$$

Equation (1) can be understood by interpreting R_1 as an asset for which the return flow equals the flow of what one expects to gain from holding the asset. The latter consists of two parts: (i) the flow of benefits, (ii) the job offer arrival rate times the expected gain of finding an acceptable job over staying unemployed. The second part is the mean over F of the gain corresponding to a wage offer w. If one accepts w then the associated present value is w/ρ, so the gain is $w/\rho - R_1$. If one rejects it then there is a probability pq that one is caught, in which case the associated present value is R_2, and a probability $1-pq$ that one is not caught, with present value R_1. The gain is again equal to the new present value minus R_1. The derivation of (2) is analogous. Equations (1) and (2) can also be derived as Bellman equations from first principles.

Notice that with the strictest possible monitoring, i.e., $p = q = 1$, the outside option when considering an offer is equal to a certain punishment, so then $\phi_1 = \rho R_2$. This implies that extreme monitoring does not necessarily entail the absence of punishments. With certain model parameter values, it may still be optimal for an individual to prefer a sanction and a forced future job offer acceptance over a current low offer. This is particularly likely if the offer under consideration is much lower than the average offer, and if the punishment $b_2 - b_1$ is small.

It is also interesting to consider the expected present value $\widehat{R}_1$ and optimal reservation wage $\widehat{\phi}_1$ in the absence of a monitoring system,

$$\rho \widehat{R}_1 = b_1 + \lambda \int_{\widehat{\phi}_1}^{\infty} (\frac{w}{\rho} - \widehat{R}_1) dF(w), \quad \text{with } \rho \widehat{R}_1 = \widehat{\phi}_1 \tag{3}$$

By elaborating on equations (1) and (2) we obtain the following expression for ϕ_1,

$$\phi_1 = pqb_2 + (1-pq)b_1 +$$

$$\frac{\lambda}{\rho} \left[(1-pq) \int_{\phi}^{\infty} (w - \phi_1) dF(w) + pq \int_0^{\infty} (w - \phi_1) dF(w) \right]$$

which has a similar structure as the reservation wage equation in a standard job search model. Clearly, the latter is obtained by imposing $p = q = 0$. For general p, q, we obtain a weighted average of the reservation wage in a market without monitoring, and the present value flow after having been punished.

Using obvious notation, the transition rates from unemployment to employment before and after imposition of a sanction equal

$$\theta_{u,1} = \lambda(1 - F(\phi_1)) \text{ and } \theta_{u,2} = \lambda \tag{4}$$

For a system with given p and q, the probability that a sanction occurs before a job exit is equal to $\lambda pqF(\phi_1)/(\lambda pqF(\phi_1) + \lambda(1 - F(\phi_1))) = pqF(\phi_1)/(1 - (1 - pq)F(\phi_1))$. This can be seen by noting that a newly unemployed individual faces competing risks (a sanction and job exit) with constant rates $\lambda pqF(\phi_1)$ and $\theta_{u,1}$, respectively. The proportionate effect of the sanction on the job exit rate equals $\theta_{u,2}/\theta_{u,1} = 1/(1 - F(\phi_1))$. This correspond to a parameter of the empirical model. The additive effect of a sanction on the mean accepted wage equals $\mathbb{E}_F(w) - \mathbb{E}_F(w|w > \phi_1)$. The empirical model contains a parameter that captures the additive effect on the mean log accepted wage $\mathbb{E}_F(\log w) - \mathbb{E}_F(\log w|w > \phi_1)$. Of course the empirical parameters are not constrained to have a particular sign, and they may themselves depend on deeper determinants and characteristics of the individual and the labor market.

The additive effect on the job exit rate equals $\theta_{u,2} - \theta_{u,1} = \lambda F(\phi_1)$. Notice that this is bounded from above by λ.

3.2 Theoretical predictions

A number of insights follow from the model. Consider the general case where the model parameters are such that $\phi_1 > \underline{w}$: the reservation wage before a sanction is imposed exceeds the lowest possible wage offer in the market. This is a necessary condition to observe sanctions at all. It is clear that $\widehat{R}_1 > R_1 > R_2$, and consequently $\widehat{\phi}_1 > \phi_1$. From the point of view of the individual, monitoring reduces the expected present value, and so does an actual punishment in a world with monitoring. By implication, $\theta_{u,1} < \theta_{u,2}$, and both are larger than the transition rate in a world without monitoring.

Consequently, monitoring affects the transition rate of all individuals (except for those who have a very low reservation wage $\widehat{\phi}_1$ anyway). This is the *ex ante* effect of the monitoring system, as opposed to the *ex post* effect due to imposition of a sanction.

Notice that if $\phi_1 \leq \underline{w}$ then the individual probability of job acceptance is equal to one, so there will not be any sanctions. If the case worker is very lenient ($q = 0$) then the sanction rate is zero as well. Conversely, we have seen that in the strictest possible monitoring system ($p = q = 1$), an individual may still prefer to reject a low-wage offer in favor of a sanction. This reflects a first fundamental difference with monitoring schemes that target an endogenously chosen level of search effort by the individual (see Abbring et al., 2005, for a theoretical analysis). In the latter scheme, perfect monitoring leads to absence of sanctions, even if the punitive benefits reduction is small. This is because perfect search effort monitoring is instantaneous and continuous in time and the effort constraint will be strictly enforced after a violation. Perfect monitoring of *offers* only takes place after offer rejections, and a rejection followed by a sanction may be worthwhile if it is followed by a high wage offer at a later point in time.

It is interesting to consider the ex post effect and the occurrence of sanctions for different subgroups of individuals. First, consider individuals for whom $F(\phi)$ is very small. Since $\phi_1 := (1-pq)\rho R_1 + pq\rho R_2$, it follows that their expected present value of unemployment after rejection of an offer is low. At the same time, they are unlikely to reject an offer and therefore unlikely to get a sanction. These may be individuals with a low R_1 due to a low job offer arrival rate λ and low benefits b_1. Their sanction effect is small as well. Notice that for moderate values of $F(\phi_1)$, the probability $pqF(\phi_1)/(1-(1-pq)F(\phi_1))$ that a sanction occurs before a job exit can still be extremely small if q is very small. In that case the sanction effect is not necessarily extremely small.

Secondly, consider the opposite case where $F(\phi)$ is large (i.e., close to one). This may capture long-term unemployed individuals who enjoy generous benefits b_1 whereas their skills have become obsolete and most offers that are made to them concern low-skill jobs with wages below b_1 (see Ljungqvist & Sargent, 1997, for an equilibrium analysis). Such individuals have a very high sanction rate and sanction effect. But now let us consider what happens if individuals can optimally choose their search effort s as well. Let the job offer arrival rate now be specified as λs, and let the search cost flow $c(s)$ be a convex increasing function of s with $c(0) = c'(0) = 0$, so that the instantaneous income flow before a sanction equals $b_1 - c(s)$. The optimal value of s before a sanction follows from maximization of the right-hand side of the suitably adjusted equation (1), leading to

$$c'(s) = \max\{0, \frac{\lambda}{\rho}\int_{\phi}^{\infty}(w-\phi_1)dF(w) - \lambda pq(R_1 - R_2)\}$$

If ϕ_1 is at the upper bound of the support of F, then the integral in the above expression vanishes, implying that $s = 0$. The same result holds for values of ϕ_1 close to the upper bound. If the monitoring regime is stringent then the last term on the right-hand side increases, so the reduction of optimal search effort is exacerbated. In sum, when these individuals can choose their level of search effort, then offer decision monitoring will be counteracted by a reduction of search effort. To put it bluntly, monitoring of offer decisions causes individuals with high benefits (or a high utility flow of being unemployed) to prevent that they will ever get an offer. The ex ante effect of monitoring may then be perverse: more monitoring implies a lower job exit rate. We view this as a potentially important insight. Whereas job search effort monitoring always generates a positive ex ante effect, job offer decision monitoring does not.

We briefly mention two other differences between job search effort monitoring and job offer decision monitoring. These concern outcomes after the sanction. Recall that we assume perfect monitoring after the sanction. The first of the two differences concerns the magnitude of the ex post effect on the job exit rate. Suppose that search effort s is endogenously determined. In the case of job offer decision monitoring, the job exit rate after a sanction equals $\theta_{u,2} = \lambda s_2$, where s_2 is the optimal search effort after a sanction. Conversely, in the case of search effort monitoring, this rate equals $\lambda s^*(1-F(\phi_2))$, where ϕ_2 is the optimal reservation wage after a sanction and s^* is the minimum required search effort as postulated by the UI agency. In the latter case, by choosing an appropriately high s^*, the job exit rate, and by implication the ex post sanction effect, can be pushed upwards to arbitrarily high values. In the former case this is not possible. Intuitively, the effect of job offer monitoring is bounded from above by the rate at which job offers arrive. (Of course, by pushing up $s*^*$ in search effort monitoring, the privately incurred search costs $c(s)$ increase at an even higher speed. Also, if s^* becomes very large then the distribution of the associated wage offers may change at the margin.)

The fourth and final difference between the monitoring regimes was already mentioned in the introduction of the paper, namely that the adverse effects of sanctions on post-unemployment outcomes may be smaller with search effort monitoring than with job offer decision monitoring. Perfect monitoring after a sanction implies full compliance after the sanction. With job offer decision monitoring, this means that compared to the situation before a sanction, punished individuals now also have to accept all offers of jobs with the lowest wages. With search effort monitoring, however, full compliance means that punished individuals have to search harder for any possible job. The latter includes both high-wage jobs and low-wage jobs.

All results in this section generalize to non-wage job characteristics. Basically, if the individual's utility flow function depends on the wage and on other characteristics then the role of the income flow variables in the present section is replaced by the corresponding utility flows.

We finish this section by briefly mentioning some implications of the above that are of importance for the specification of the empirical model. The empirical model is a reduced-form model in which hazard rates are allowed to vary over time and across observed and unobserved individual characteristics. The implications below also follow from models with monitoring of an endogenously determined search effort (see Abbring et al., 2005). First, at the individual level, the transition rate from unemployment to employment makes a discrete upward jump upon imposition of a sanction. If individuals are homogeneous then the size of this jump, which is the causal effect of the sanction treatment, can be estimated from an unemployment duration model in which the moment at which a sanction occurs is a time-varying exogenous covariate.

Empirical analyses of duration data from a market with a given monitoring system do not allow for non-parametric identification of *ex ante* effects. So such analyses cannot be used to evaluate the effect of the monitoring system on unemployment durations. The latter objective requires at least some observed variation in the monitoring system itself.

Both the transition rate from unemployment to employment and the rate at which a sanction arrives depend on all the variables that the individual uses to determine his strategy. This is because both depend on ϕ_1 (provided that $\phi_1 > \underline{w}$). In reality, individuals are heterogeneous with respect to determinants of search behavior. Suppose that the individuals know their own value of some characteristic but that these values are not observed in the data. As we argued in Section 1, with punitive treatments, such a setting is plausible. Then both the transition rate from unemployment to employment and the rate at which a sanction is imposed depend on this unobserved characteristic. This creates a spurious relation between the duration until a sanction is imposed and the duration of unemployment. Note that a similar spurious relation is created if the policy parameters p and q of the sanction rate itself differ across individuals in a way that is not observed by the researcher.

4 Data

4.1 Data registers

Our main data are taken from a combination of two Swedish register data sets called Händel (from the official employment offices) and ASTAT (from the unemployment insurance fund). Händel covers all registered unemployed persons.[6] It contains day-by-day information on the unemployment status, whether the unemployed is covered by UI, entries into and exits from active labor market programs and part-time unemployment, and the reason for the unemployment spell to end. As a rule, UI spells end in transitions into re-employment, education, social assistance, or other insurance schemes. Händel also includes a number of background characteristics, recorded at the beginning of the unemployment spell. ASTAT provides information on all benefits sanctions, including information on the timing of the sanction, the main reason for the sanction, and the size of the benefit reduction.

Our observation window runs from January 1, 1999 until December 31, 2003. We only use information on individuals who become unemployed at least once within the observation window. An individual becomes unemployed at the first date at which he registers at the employment office

[6] According to Carling et al. (2001), more than 90% of the individuals who are ILO-unemployed according to labor force surveys also register at the employment offices.

as being "openly" unemployed. We ignore unemployment spells that are already in progress at the beginning of the observation window, because using them would force us to make assumptions about the period before the beginning of the window. We focus on re-employment durations, and consider any employment, full-time or part-time, which is retained for at least 10 days as employment. At later stages we separately model the decision to accept part-time employment. UI spells that terminate for other reasons than re-employment are considered being right-censored re-employment durations. We stop time while unemployed are enrolled into active labor market programs. Robustness analysis shows that our results are insensitive to this restriction. Apart from that, individuals are only followed up to December 2004. Ongoing spells at that date are right-censored. We restrict our analysis to everyone who was between 25-55 at the time of entry into unemployment and covered by UI.[7] We only model the first sanction during an unemployment spell. Any effects of a second or third sanction are considered to be a part of the first sanction treatment effect. Finally, we exclude all unemployment spells for a specific individual that occur after a spell during which a sanction was given to that individual. This is because we exploit multiple spells to enhance the quality of the results, and we cannot rule out that a sanction also affects future subsequent spells.

The sanction and unemployment data are combined with survey data on wages and hours worked from Statistics Sweden's wage statistics. It provides us with information on actual wages per time unit, so these are not wages created from annual earnings and some measurement of hours worked. The wage is recorded as the monthly full-time equivalent wage. The survey is collected annually (during the fall) by Statistics Sweden in cooperation with employer organizations. It covers the whole public sector, all large private firms and a random sample of small firms (about 50 percent of all private sector employees). If we observe a wage within one year after the exit to employment we use this wage, otherwise the wage is considered to be missing. The information on hours worked is used to construct an indicator variable for full-time employment, defined as working 34 hours or more a week.

The wage data also include individual occupations. These are classified using SSYK 96 (*Standard för svensk yrkesklassificering 1996*), which follows the international standard ISCO-88. Each occupation is classified into 355 separate groups of occupations (four digits). The first digit classifies occupations by the general qualifications required to perform the tasks associated with each occupation. It divides the occupations into four levels: the occupations in group 1 normally require no or limited education, level 2 occupations require high school competence, level 3 occupations short university education, and the occupations at level 4 require longer university education (3-4

[7] We also exclude disabled individuals and everyone who some time during the research period participated in sheltered employment, because these are intended for unemployed with some kind of disability or handicap.

years or more). Additional digits capture the specialization skills associated with each occupation. We matched occupations to individual education levels taken from Statistics Sweden's database "Louise".

4.2 Descriptive statistics

Table 1 provides statistics on the unemployment spells and the duration until a sanction. In Subsection 5.4 below we explain that we choose to estimate models with an endogenously stratified sample. The current subsection provides information on the full data set and on the sample used for the model estimation. A large part, 65.7%, of the re-employment spells in our analysis data set is not right-censored. Remember that the remaining 34.3% of the spells are ongoing at the end of the data period, or UI spells that are completed for other reasons than re-employment. During only 0.18% of the unemployment spells in our full sample a sanction is imposed, compared with 8.4% in the data set used for the model estimation. Relatively many sanctions, 46.7%, are imposed during the first 100 days of unemployment. There is also a substantial number of sanctions, 16%, imposed after 300 days or more in unemployment. Because of censoring, these raw figures underestimate the incidence of sanctions and the duration at which these are imposed. About 8% of the sanctions are given to second-time offenders and only about 0.5% to third-time offenders.

Table 2 provides statistics on the job-quality measures. For about 35% of the spells for which observe an exit to employment we observe the wage within one year after the exit. Not observing the wage can be due to fact that the individual is employed in small private firms or due to fact that the individual already left employment before the time of the survey. As the wage survey is conducted annually, the mean time from the exit to employment to the time of the wage survey is about half a year (179 days). Note that, because the survey is mainly conducted during the fall and because there is seasonal variation in exits from unemployment, the time from the exit to the survey is not uniformly distributed over 1-12 months. The mean monthly wage is about SEK 17,840 among the individuals for whom we observe the wage, and about 57% of these individuals have full-time employment. Furthermore, 57% find a job in the public sector, 31% in a large private firm, and 21% find a job in a small private firm. Here, a large firm is defined as having 200 employees or more.

The missing wage data may not be missing at random. First of all, remember that we observe the wage for all public sector employees, all employees at large private firms, and a random sample of those working in small firms. Suppose that individuals who are sanctioned accept lower wages on average. Small firms tend to pay lower wages than large firms, so there may be a selectivity in the wage observations, and this may lead to an under-estimation in absolute values of the negative effect of sanctions on wages. To explore this, we specify a logit model for the choice between accepting public sector or private sector employment, and, given the choice to enter the private sector, a

Table 1: *Sample statistics for duration in unemployment and duration until a sanction*

	Full sample	Our sample
Regardless of treatment		
No. individuals	827,074	16,941
No. spells	1,665,420	35,055
% with exactly one spell	48.7	49.4
% with exactly two spells	24.2	24.0
% with more than two spells	7.1	7.0
% t_s observed	0.18	8.4
% t_e observed	65.7	65.2
average observed t_e	104.4 (112.4)	114.5 (122.9)
median observed t_e	68	74
Concerning spells with sanction observed		
No. Spells	2941	2941
% t_e observed	56.1	56.1
average observed t_s	240.2 (174.0)	240.2 (174.0)
median observed t_s	193	193
average observed t_e	140.6 (134.0)	140.6 (134.0)
median observed t_e	96	96
% t_s in		
0-50 days	27.1	27.1
50-100 days	19.7	19.7
100-150 days	12.3	12.3
150-200 days	10.6	10.6
200-250 days	7.5	7.5
250-300 days	6.1	6.1
300- days	16.6	16.6
Type of sanctions		
% 100% reduction for 60 days	68.0	68.0
% 25% reduction in 40 days	32.0	32.0

Notes: The time unit is day. t_s is time until sanction, and t_e time in unemployment. Standard deviations in parentheses. Full sample is the full sample of all unemployment spells, and our sample the selected sample described in the data section.

Table 2: *Sample statistics for wages and hours worked*

	Full sample	Our sample
Wage data		
% exit to employment observed	65.7	65.2
Of which		
Observe wage %	36.5	35.1
Observe hours worked %	30.4	29.2
Public sector employment %	55.9	57.2
Private sector firm $\geq$ 200 workers	31.2	30.4
Private sector firm $<$ 200 workers	21.7	21.0
Monthly wage in SEK	17941 (4371)	17843 (4446)
Full time ($\geq$ 34 hours a week) %	58.7	57.0
Average time between exit and wage survey	179.5 (107.6)	178.9 (108.3)
Median time between exit and wage survey	161	161
Time between exit and wage survey		
-60 days	13.7	14.5
61-120 days	22.3	21.8
121-180 days	18.7	19.0
181-240 days	13.7	13.4
241-300 days	14.6	14.0
301- days	17.0	17.4
Individual		
Male (%)	50.2	52.2
Education in occupation (%)	64.6	65.5
Experience in occupation (%)	39.6	39.7
Needs Guidance (%)	22.8	23.2
Age	36.4 (8.14)	36.4(8.11)
North (%)	22.1	22.3
Central (%)	37.5	36.9
South (%)	40.4	40.8
Less than high school (%)	20.3	21.3
High school education (%)	54.3	55.3
University education (%)	25.4	23.4
Local unemployment (%)	5.15(1.53)	5.14 (1.54)
Time of inflow		
1999	21.9	21.9
2000	20.0	20.4
2001	19.0	19.6
2002	19.4	19.6
2003	19.7	18.5

Notes: Wage is the first observed (within one year) after the exit from unemployment. Time of inflow is defined as the calendar year the unemployment spell starts. Full sample is the full sample of all unemployment spells, and our sample the selected sample described in the data section. Standard deviations in parentheses.

logit model for the choice between accepting employment in a large firm or a small. In both models we control for a number of covariates, such as sex, age, level of education, time of inflow into unemployment, regional variables, level of education, the kind of profession the unemployed is searching for and whether the unemployed has education respectively previous experience in that occupation. We estimate these two logit models jointly using maximum likelihood, and the results are presented in Table 3. The results show no evidence of selection due to a sanction into small private firms. We therefore feel confident in assuming that it is random whether we observe the wage or not. The same holds for hours worked.

The second concern regards the fact that, in most cases, some time elapses between the exit from unemployment and the wage survey. It means that we do not observe the first wage after unemployment for individuals who have quickly moved into a second or even third employment. We neither observe the wage for those who have become unemployed or left the labor market entirely before the wage survey is conducted. Both these factors may bias our job quality estimates. If there is an effect of a sanction on the job security, relatively more individuals with sanctions will go back into unemployment before the time of the wage survey. As these individuals can be expected to be on the lower end of the wage distribution it will also bias our job quality estimates upwards. In addition, if unemployed with sanctions move relatively faster into a second employment, with a higher wage, it will also bias our job quality estimates upwards. To proceed ahead, even with these potential biases we find significant negative job quality effects.

4.3 Around the date of the monitoring policy regime change

In this subsection we provide descriptive statistics on the occurrence of sanctions shortly before and after the policy change of February 5, 2001. Ideally, a change in the monitoring regime offers an opportunity to investigate the ex ante threat effect.

As apparent from Figure 1, the reform did not lead to a substantial increase in the number of sanctions issued. Instead, apart from seasonal fluctuations, this number has been increasing slowly and steadily after the reform.

In addition, there are large regional differences in the development of the number of sanctions over time. Regional variation is to some extent due to the fact that only the individual case worker and the chief of the local PES office decide about whether a report should be sent to the unemployment insurance fund (recall the statements in IAF, 2006 mentioned in Subsection 2.2). Table 4 lists the mean number of sanctions per quarter by region. In Figure 2 and Figure 3 we display an index of the quarterly number of sanctions for each of the years 2000-2004 using the quarters of 1999 as base period. An index value of 2 in 2003 means that sanctions are twice as frequent in 2003 as in the same quarter in 1999. We display this for three regions in the southern and the

Table 3: *Logit estimates for the choice between private and public sector employment and the choice between large and small private firm*

	Public sector		Large private firm	
	Est.	S.e..	Est.	S.e.
Sanction effect	0.038	0.107	0.067	0.151
Individual				
Male	-0.996	0.058	0.018	0.082
Education in occupation	0.033	0.063	0.186	0.081
Experience in occupation	-0.030	0.063	0.193	0.086
Needs Guidance	0.045	0.068	-0.002	0.091
Log(age)	0.739	0.126	-0.417	0.167
North	0.423	0.082	-0.489	0.113
South	0.040	0.062	-0.178	0.083
High school Education	0.270	0.078	-0.314	0.096
University Education	0.523	0.095	-0.173	0.127
Local unemployment	0.008	0.022	0.064	0.031
Inflow time				
2000	-0.098	0.085	-0.001	0.117
2001	-0.057	0.098	0.216	0.133
2002	0.061	0.103	0.334	0.140
2003	-0.250	0.098	0.155	0.132
Searched profession				
Administrative and managerial	-0.815	0.171	-0.474	0.096
Sales	-0.476	0.158	-1.600	0.093
Agricultural, forestry and fishing	-0.438	0.164	-2.148	0.108
Technical and related	-1.426	0.212	-2.110	0.165
Transport and communication	-0.741	0.186	-2.028	0.135
Production	-0.295	0.149	-2.517	0.096
Service	-0.530	0.170	-1.165	0.099
Constant	-1.099	0.484	2.051	0.652
No. Observations	8017			
Mean of outcome	0.572		0.583	
Log Likelihood	-11872			

Notes: Public sector defined as an indicator variable taking the value one if the unemployed finds employment in the public sector. Large private firm defined as an indicator variable taking the value of if unemployed finds employment in a firm with more than 200 employees, given that the unemployed have found private sector employment. Sample consist of everyone in our analysis sample for which we observe exit to employment and have information on the type employment within one year after the exit. Estimated using WESML.

central parts of Sweden, respectively. They reveal a wide regional variation in patterns after 2000. We observe permanently increased sanction numbers in some regions, no change in some other regions, and temporary increases in sanctions in yet other regions. To focus more closely on the moment of the policy change, we list in Table 4 the ratio between sanction occurrences in the first quarter of 2001 and the first quarter of 2000, and the same for the other quarters in 2001 and 2000. These ratios are purged from seasonal variation. The statistics confirm the patterns in Figure 2 and Figure 3.

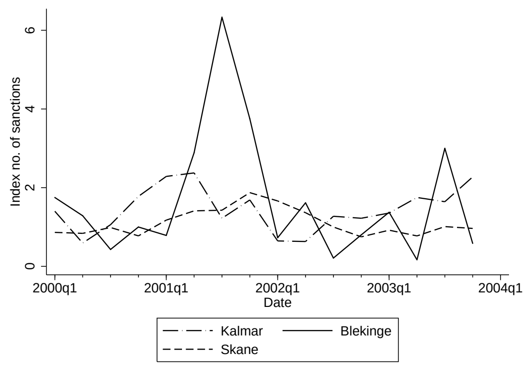

Figure 2: Index over quarterly number of sanctions 2000-2004 for three regions in southern Sweden. 1999 is base year.

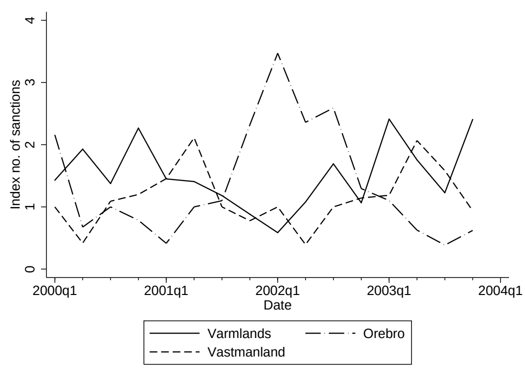

Figure 3: Index over quarterly number of sanctions 2000-2004 for three regions in the central parts of Sweden. 1999 is base year.

Clearly, from a methodological point of view, it is hard to reconcile the erratic and region-specific fluctuations in the occurrence of sanctions after the policy change to the idea of exploiting the discontinuity in the monitoring system for the estimation of ex ante effects. But at the very least we may conclude that the occurrence of sanctions has not increased substantially after

Table 4: *Summary statistics for regional occurrence of sanctions*

		NS 2001 / *NS* 2000			
	$1000NS/NU$	Q1	Q2	Q3	Q4
Stockholm	1.46	1.12	1.32	1.18	1.19
Uppsala	0.68	1.00	1.00	0.88	0.69
Södermanland	0.99	5.60	0.95	0.72	0.21
Östergötland	0.75	0.44	0.64	1.69	0.81
Jönköping	1.10	1.21	1.35	1.74	0.70
Kronoberg	1.12	1.83	1.90	0.63	0.67
Kalmar	1.64	2.40	2.38	1.17	1.69
Blekinge	0.79	0.79	2.89	6.33	3.75
Skåne	1.12	1.14	1.38	1.39	1.81
Halland	0.67	1.33	0.81	0.90	0.74
Västra Götaland	0.82	0.95	0.86	0.75	0.96
Värmland	1.21	1.45	1.41	1.14	0.76
Örebro	1.35	0.41	1.04	1.05	2.23
Västmanland	0.78	1.45	2.11	1.00	0.56
Dalarna	0.72	0.73	1.33	0.52	1.13
Gävleborg	0.69	0.57	0.69	1.05	0.72
Västernorrland	0.89	1.33	1.75	1.13	1.56
Jämtland	0.97	1.64	1.00	0.40	0.23
Västerbotten	1.16	1.42	0.57	0.83	0.86
Norrbotten	1.07	1.67	1.11	1.11	2.10

Notes: *NS* is the mean number of sanctions in the region, and *NU* is the mean stock of full-time unemployed collecting unemployment insurance benefits in the region. Q_i stands for the i'th quarter.

the policy change. According to our theoretical model, there are two possible explanations for this. First, the case workers have decided to not to act on policy change but instead to continue not to recommend sanctions in case of violations, because they find a 25% benefits reduction still too severe. Obviously, in the new system, the punishments are less harsh than before, but from an international perspective they are still substantial. In the Netherlands, where sanctions are less severe, and monitoring is carried out by different individuals than the case workers, the individuals who carry out the monitoring state that they are less likely to issue a sanction if they feel that the unemployed individual faces adverse labor market conditions (see Van den Berg & Van der Klaauw, 2006). In agreement to this, studies with Dutch data find that individual characteristics that are associated with a low exit rate to work are also associated with a low sanction rate, confirming that the monitoring intensity depends positively on the individual's labor market conditions. In terms of our theoretical model, this first explanation would mean that the policy change does not lead to any substantial changes in the parameters in the decision problem for the unemployed individual.

The second explanation for the low occurrence of sanctions after the policy change is that a more stringent monitoring scheme may motivate many individuals to avoid violations at all costs, i.e. that the policy change induced a strong ex ante effect. The net effect of an increase in the monitoring and a decrease in violations may then be that the occurrence of sanctions remains low. In terms of our theoretical model, the policy change is captured by an increase of q which leads to a decrease of ϕ_1 such that virtually all offers are accepted. In Subsection 3.2 we also showed that an increase of q may lead to a reduction of search effort to zero, such that no offers are generated in the first place, and consequently sanctions do not occur. However, this is potentially only relevant for a subset of individuals. Obviously, a zero effort gives rise to extremely long unemployment spells.

A third explanation is that monitoring was virtually perfect in both regimes, but this seems borne out by the motivation for the policy change as well as by the variation in enforcement across case workers.

To distinguish between these explanations we have to examine the unemployment duration outcomes and the post-unemployment outcomes. The first explanation implies that the job exit rate $\theta_{u,1}$ is the same in both regimes. The second explanation implies that this rate changes after the policy change. This is because in the first case ϕ_1 does not change whereas in the second case it decreases. We return to the issues of this subsection after having presented the duration model estimates in Section 6.

5 Empirical model

5.1 Timing of Events model

This section presents our empirical model. In Subsection 5.1, we present a basic bivariate duration model, for the duration until employment and the duration until the imposition of a sanction. This "timing of events" approach (Abbring & Van den Berg, 2003) is the standard approach in the literature on sanction effects. In Subsection 5.2 we extend this well known model into our full model, incorporating the job quality into the same model.

We normalize the point of time at which the individual enters unemployment to zero. We are interested in investigating how the duration t_s until the imposition of a sanction affect the duration until employment, t_e. In order to illustrate the basic identification problem, suppose that we observe that the individuals who are sanctioned at t_s have relatively short unemployment durations then this can be for two reasons: (1) the individual causal sanction effect is positive, or (2) these individuals have relatively favorable unobserved characteristics and would have found a job relatively fast anyway. The second relation is a spurious selection effect. To control for such spurious effects, we analyze both the distribution of t_e for a given t_s and the distribution of t_s jointly. It is well known that these distributions can be conveniently represented by the corresponding hazard rates.

First, consider individuals who are unemployed for t units of time. We assume that all individual differences in the re-employment rate at t can be characterized by observed characteristics x, unobserved characteristics V_e, and a sanction effect if a sanction has been imposed before t. Next, consider the rate at which a sanction is imposed on an unemployed individual. Similarly as for the re-employment hazard, we assume that all individual differences in this rate can be characterized by observed characteristics x and unobserved characteristics V_s. We further assume that the re-employment rate denoted by $\theta_e(t|x,V_e,t_s)$, and the sanction rate denoted by $\theta_s(t|x,V_s)$ both have the familiar Mixed Proportional Hazard (MPH) specification, this gives

$$\theta_e(t|x,V_e,t_s) = \lambda_e(t)\ \exp(x'\beta_e)\ \exp(I(t > t_s)\delta(t|t_s,x))\ V_e, \tag{5}$$

$$\theta_s(t|x,V_s) = \lambda_s(t)\ \exp(x'\beta_s)\ V_s. \tag{6}$$

Here $I(.)$ is an indicator function taking the value one if the argument is true and zero otherwise. $\delta(t|t_s,x)$ then represent the sanction effect, which we allow to vary both with observed characteristics and with time, $t - t_s$, since the imposition of a sanction. Further, $\lambda_e(t)$ and $\lambda_s(t)$ represents the duration dependence in the re-employment hazard and the sanction hazard, respectively.

Equation (5) and (6) give the joint distribution of $t_e,t_s|x,V_e,V_s$. Our data provide information on the distribution of $t_e,t_s|x$. Let G denote the joint distribution of $V_e,V_s|x$ in the inflow into unemployment. It is clear that a specification of G, together with the specification of the joint distribution of $t_e,t_s|x,V_e,V_s$, fully determines the distribution of $t_e, t_s|x$, and thus the data. Abbring & Van den Berg (2003) show that all components of this model, including δ, are identified, provided we make assumptions similar to those usually made in standard univariate MPH models with exogenous regressors. Identification is semi-parametric in the sense that given the MPH structure it does not require any parametric assumptions on the components of the model. In fact, with a non-parametric specification of G, we allow for general dependence between t_e and t_s through both the causal effect of sanctions and related unobservables.

Note that this identification does not rely on conditional independence assumptions. The identification does not either rely on any exclusion restrictions on the effects of x on the specifications of θ_e and θ_s. This is important, since from theory we have that all variables that affect the re-employment rate also affect the sanction rate, and vice versa. Instead, identification is based on the timing of events, i.e. the timing of sanctions and of exits out of unemployment. Intuitively, what drives the identification of the sanction effect, δ, is the extent to which the moments of a sanction and the moment of exit to employment are close in time. If a sanction is quickly followed by exit to employment, no matter how long the elapsed unemployment duration before the sanction, then this is evidence of a causal effect of a sanction. Any spurious selection effects through dependence between V_s and V_e, gives a second relation between the two duration variables, but it can be shown that that relation does not give rise to the same type of quick succession of events. So the interaction between the moment of exit and the moment of a sanction in the conditional rate of events allows one to distinguish between the causal effect and selectivity. The Monte Carlo simulations in Gaure, Røed & Zhang (2007) support the use of this approach by showing that the estimates of the parameters of interest are robust with respect to functional form assumptions.

Formally, identification of the model relies on a number of implicit and explicit assumptions. We assume that a sanction does not affect the re-employment rate before the moment of the sanction, whereas the effects of the unobserved covariates are fixed during the spell. The former is often referred to as the no-anticipation assumption. With sanctions, the moment at which an individual is caught is almost by definition unanticipated by the individual. As explained in Section 2 there are also several sources of unpredictability in the sanction process, which makes it even less likely that UI claimants anticipate the actual timing of the sanction. Next, since we specified the hazard rate it means that we implicitly assumed that there is a random component in the assignments that is independent of all other variables. Based on the randomness in the sanction process and the obvious randomness in the job-search process, we are confident that this assumption is satisfied.

Identification with single-spell data also requires that (i) x on the one hand and V_u, V_s on the other hand are independent in the inflow, and (ii) there is sufficient variation in x. However, since we often observe multiple UI spells for a given individual we can relax these two assumptions. We assume that multiple spells for one individual given the characteristics are statistically independent of each other, that the unobservables V_u and V_s are fixed across spells, and that the length of intervening spells between any two unemployment spells of a single individual are independent of V_u and V_s. As shown by Abbring & Van den Berg (2003), under these assumptions, the assumptions (i) and (ii) can be discarded.

5.2 Extension to post-unemployment outcomes

We measure job quality by the monthly wage, and by whether the accepted job is full-time or part-time. These outcomes can be expected to depend on unobserved factors that are related to the unobserved determinants of the job exit rate and the sanction hazard. For instance, ability plays an important role for all these outcomes. In order to identify the effects of a sanction on the job quality we need to impose some structure. We assume that the causal effect and the selection effect only affect the mean log wage, and we assume that these effects are additive. Specifically, the wage at the start of the new employment can be expressed as

$$\ln w = x'\beta_w + \gamma_w I(t_s < t_e) + V_w + e_w, \tag{7}$$

where γ_w is the sanction effect, V_w unobserved individual characteristics, and e_w is an error term which reflects random variation in the hourly wage. e_w is assumed to be normally distributed with mean zero and variance σ_w^2. Similarly, we specify the decision to accept full-time employment as

$$h = \mathbf{1}[x'\beta_h + \gamma_h I(t_s < t_e) + V_h + e_h > 0] \tag{8}$$

where $h = 1$ if the individual finds full-time employment. As before γ_h is the sanction effect, V_h unobserved individual characteristics, and e_h an error term which reflects truly random variation. e_h is assumed to have a standard logistic distribution.

We also acknowledge the tight link between the unobserved effects in the two job quality measures and the unobserved effects in the sanction hazard and the exit hazard. We take a simple linear form for this relation, as

$$V_w = \beta_{we}V_e + \beta_{ws}V_s, \tag{9}$$

and

$$V_u = \beta_{he}V_e + \beta_{hs}V_s. \tag{10}$$

Here β_{we},β_{ws}, β_{he}, and β_{hs} captures the correlation between the unobservables in the model.

Abstracting from censoring, the joint density of $T_e, T_s, W, H|x$ at $T_e = t_e, T_s = t_s, W = w, H = 1$ is then

$$\int_0^\infty \int_0^\infty (\lambda_e(t_e)\exp(x'\beta_e)v_e \exp(I(t_e > t_s)\delta(t_e|t_s,x))$$
$$\exp\left(-\exp(x'\beta_e)v_e\left[\int_0^{min(t_e,t_s)} \lambda_e(k)dk + I(t_e > t_s)\int_{t_s}^{t_e} \lambda_e(k)\delta(k|t_s,x)dk\right]\right)$$
$$\lambda_s(t_s)\exp(x'\beta_s)v_s \exp\left(-\exp(x'\beta_s)v_s \int_0^{t_s} \lambda_s(k)dk\right)\times$$
$$\frac{1}{\sigma\sqrt{2\pi}}\exp\left(-\frac{(\ln w - x'\beta_w - \gamma_w I(t_e > t_s) - \beta_{we}v_e - \beta_{ws}v_s)^2}{2\sigma^2}\right)\times$$
$$\frac{exp(x'\beta_h + \gamma_h I(t_e > t_s) + \beta_{he}v_e + \beta_{hs}v_s)}{1 + exp(x'\beta_h + \gamma_h I(t_e > t_s) + \beta_{he}v_e + \beta_{hs}v_s)} G(v_e, v_s) \quad (11)$$

We jointly estimate this full model.

Consider identification of this full model. In short the duration part of the model identifies G, and given this we can estimate β_{we},β_{ws}, β_{he}, and β_{hs}. We have then uncovered the selection process in the job quality decisions. It allows us to integrate out the unobserved effects in the wage equation and the hours worked equation.

5.3 Parameterizations

Given the assumptions discussed above, including the MPH structure, the model is identified without any further parametric restrictions. However from a computational point of view we need to specify some parametric structure. We take flexible specifications of both the duration dependence functions and the bivariate unobserved heterogeneity distribution. We take both $\lambda_e(t)$ and $\lambda_s(t)$ to have a series representation

$$\lambda_i(t) = \sum_{j=0,1,...} \alpha_{ij}t^j. \quad (12)$$

Note that with a large number of polynomials any duration dependence pattern can be approximated closely. In the basic analysis we take polynomials of seventh order and lower for the exit hazard, and polynomials of third order and lower for the exit hazard. We have experimented with both more and less polynomials. The results are insensitive such changes, unless the number of polynomials are very few.

We use a bivariate discrete distribution with unrestricted mass point locations for G. This provides a very flexible specification as well as being computationally feasible. In our basic specification we take V_e and V_s to have two points of support each: V^1, V^2 and V^3 and V^4, respectively. The associated probabilities are denoted as follows:

$$\begin{aligned} &\Pr(V_e = V^1, V_s = V^3) = p_1, && \Pr(V_e = V^2, V_s = V^3) = p_2 \\ &\Pr(V_e = V^2, V_s = V^3) = p_3 \;\; \textit{and} && \Pr(V_e = V^2, V_s = V^3) = p_4, \end{aligned} \tag{13}$$

with $0 \leq p_i \leq 1$ for $i = 1,..,4$, and $p_4 = 1 - p_1 - p_2 - p_3$.

5.4 Weighted exogenous sampling maximum likelihood estimation

Our full data set contains over 1.6 million unemployment spells of about 827,000 individuals. In only about 3000 of these spells a sanction is imposed. To keep the empirical analysis manageable from a computational point of view and at the same time have enough spell with sanctions, we use weighted exogenous sampling maximum likelihood (WESML) estimation with an endogenously stratified sample. This method has not been used yet in the context of bivariate dependent-duration models, and is not widely used in labor economics in general (see Ridder & Moffitt, 2007, for a detailed econometric overview).

With exogenous sampling, a sequence of individuals is sampled and their outcomes and characteristics are recorded. In contrast, with endogenous sampling, a sequence of outcomes are sampled and the characteristics of the individuals with these outcomes are recorded. Endogenous stratified sampling has, for instance, previously been used in transportation economics (see e.g. Manski & Lerman, 1979, and Garrow & Koppelman, 2004) and biostatistics. A key example is the study of rare diseases, for which it is reasonable to oversample individuals with rare disease.

In our case we wish to use all information on the individuals who receive a sanction. We therefore sample all individuals who experience at least one sanction in the observation window, and take a smaller random sample (14,000) of individuals who do not experience a sanction during this window. For these individuals, both sanctioned and non-sanctioned, we take all unemployment spells during the research period, leaving us with about 35,000 spells.

As shown by Manski & Lerman (1977), WESML provides a consistent estimator. Each observation is weighted with the ratio between the population fraction and the sample fraction of the strata it belongs to. Define L_i as individual i's contribution to the likelihood function. Then, formally, WESML amounts to maximization of the weighted likelihood function

$$\ln L_w = \sum_{i=1}^{N} \sum_{s=1}^{S} d(s) \frac{Q(s)}{H(s)} L_i \tag{14}$$

where $d(s)$ is an indicator variable taking the value one if individual i experience outcome s, $Q(s)$ the actual fraction of the population selecting alternative s, and $H(s)$ the probability that an individual selecting alternative s is included in the sample. In our case, we have two alternatives: $s = 1$ if the individual experiences a sanction during the research period, and $s = 0$ otherwise.

Inference on precision also has to be adjusted. Manski & Lerman (1977) show that the appropriate covariance matrix is the familiar sandwich estimator $V = A^{-1}BA^{-1}$, with

$$A = -\mathbb{E}\left[(\frac{\partial^2 \ln L_i}{\partial\theta\partial\theta'})_{\theta=\theta^*} \right] \qquad \text{and} \qquad B = \mathbb{E}\left[(\frac{\partial \ln L_i}{\partial\theta})_{\theta=\theta^*} (\frac{\partial \ln L_i}{\partial\theta'})_{\theta=\theta^*} \right].$$

The WESML estimates are not efficient. Efficient estimators based on endogenously stratified samples are developed in Imbens and Lancaster (1996). The basic idea is to use the populations moments as moment restrictions in order to improve efficiency. We decide not to pursue this approach. The reason for this is that our analysis sample will be large, and efficiency is not a crucial issue. Furthermore, in our case the most efficient estimator is to use the full sample of 1.6 million unemployment spells and estimate using standard ML.

6 Results

6.1 Baseline results

This subsection presents the baseline estimation results for the Timing of Events model, with a sanction effect that is constant over the population and over time. In the next subsection, we investigate the importance of temporal and cross-sectional variation in δ. From Subsection 6.3 and onwards we present the results from our full model, testing whether a sanction affects the quality of the accepted employment.

Table 5 presents the parameter estimates of the basic model. In this estimation we use the analysis sample presented in Section 4, and estimate the model using WESML. We use the individual characteristics listed in Table 2, and a set of inflow time dummies as observed covariates. As we will not normalize the scale of the unobservables, we have to exclude a constant from the regressors and one category from each set of dummies.[8] We further normalize the two constants in the duration dependence, $\alpha_{s0} = \alpha_{e0} = 1$.

The parameter of interest is the sanction effect δ. The estimate of δ is positive and significant at the 1% level. The estimate indicates that a sanction increases the transition rate to employment with about 23%. Compared to other studies on UI sanctions effects on the job exit rate this is a rather small

[8] Our base category consists of women, with neither education nor experience in their occupation, who do not need guidance, living in the central parts of Sweden, with less than high school education and who started their unemployment spell in 1999.

Table 5: *Estimates of basic model. Exit hazard and sanction hazard*

	Exit Hazard		Sanction Hazard	
	Est.	S.e.	Est.	S.e.
Sanction effect, δ	0.205	0.035		
Unobserved heterogeneity				
V^1/V^3	-4.646	0.151	-5.630	5.003
V^2/V^4	-3.362	0.153	-5.860	1.268
$\Pr(v_u = V^1, v_s = V^3)$	0.005			
$\Pr(v_u = V^1, v_s = V^4)$	0.610			
$\Pr(v_u = V^2, v_s = V^3)$	0.248			
$\Pr(v_u = V^2, v_s = V^4)$	0.136			
Individual				
Male	-0.084	0.017	0.075	0.039
Education in occupation	0.231	0.018	0.069	0.041
Experience in occupation	0.014	0.018	-0.060	0.044
Needs Guidance	-0.006	0.019	0.011	0.049
Log Age	-0.373	0.039	-0.405	0.088
North	0.232	0.026	-0.099	0.059
South	-0.007	0.019	-0.191	0.043
High school Education	0.123	0.021	-0.131	0.047
University Education	0.068	0.026	-0.632	0.062
Local unemployment	-0.025	0.007	-0.100	0.017
Inflow time				
2000	0.015	0.025	0.165	0.067
2001	-0.045	0.028	0.147	0.074
2002	-0.104	0.030	0.377	0.075
2003	-0.250	0.028	0.500	0.074
Duration dependence				
α_1	$3.674 \cdot 10^{-3}$	$0.915 \cdot 10^{-3}$	$7.416 \cdot 10^{-3}$	$1.505 \cdot 10^{-3}$
α_2	$-19.343 \cdot 10^{-6}$	$10.010 \cdot 10^{-6}$	$-12.124 \cdot 10^{-6}$	$4.077 \cdot 10^{-6}$
α_3	$34.355 \cdot 10^{-9}$	$46.081 \cdot 10^{-9}$	$6.580 \cdot 10^{-9}$	$3.106 \cdot 10^{-9}$
α_4	$-26.234 \cdot 10^{-12}$	$103.956 \cdot 10^{-12}$		
α_5	$4.345 \cdot 10^{-15}$	$120.298 \cdot 10^{-15}$		
α_6	$4.246 \cdot 10^{-18}$	$68.365 \cdot 10^{-18}$		
α_7	$-1.594 \cdot 10^{-21}$	$15.098 \cdot 10^{-21}$		
No. Individuals	16,491			
No. Spells	35,055			
Log Likelihood	-175,709			

Notes: Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors. The omitted category is: living in the central parts of Sweden with less than high school education. Local unemployment is the regional unemployment in percent at the time of inflow.

effect. For the Netherlands, Abbring et al. (2005) find that a sanction doubles the job exit rate. For Switzerland, Lalive et al. (2005) estimate that the job exit rate increases with about 25% if a sanction warning is issued and with another 25% if a sanction is actually imposed. For Denmark, Svarer (2007) estimates increases of about 50% for men and a doubling for women. We can only speculate about the reasons behind these differences. Presumably, the institutional settings play a role. As described in Section 3.2, a system of job-offer decision monitoring, like the system in Sweden, places a natural upper bound on the sanction effect, because even if all offers are accepted, the job exit rate is bounded from above by the job offer arrival rate. A system where a minimum search effort is imposed after a sanction does not give rise to such an upper bound.

The signs of the regressor effects on both hazards are mostly as expected. Not surprisingly, we find selection on observables. For example, the dummy for individuals with university education generates a negative selection effect: highly educated unemployed have high re-employment rates and low sanction rates. Omitting this dummy as an explanatory variable would have resulted in underestimation of δ (if it is not captured by the unobservables). Further as expected, is the re-employment rate higher for highly educated, and for unemployed which have education in their profession. The effect of the observables on the sanction rate also reveals some interesting patterns. The gender dummy is insignificant, indicating that discrimination is not important for the sanction decision. We also note that the sanction effect is significantly lower among older workers.

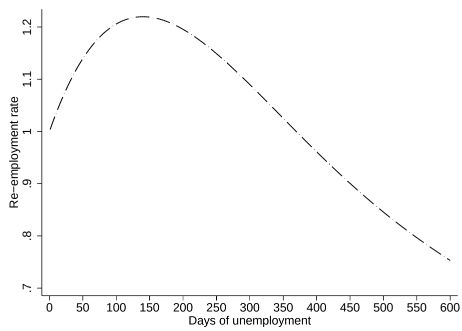

Figure 4: Estimated duration dependence. Normalized re-employment rate.

Table 5 also reports the estimates for the baseline hazard as a set of coefficients for the polynomials of order one to seven. In order to provide more intuition behind these estimates we have produced two figures: Figure 6.1 and Figure 6.1, which display the estimated duration dependence at daily basis for the exit rate and sanction rate, respectively. Remember that the baseline hazard at time point zero is normalized to one. The exit rates to employment initially decrease, but after about 150 days of unemployment the exit rate starts to go down. For instance, after 600 days in unemployment the re-employment rate is about 30% lower compared with at the start of the unemployment period. Apparently, stigmatization and discouragement play a significant role for individual unemployment durations. The sanction rate gradually rises with time spent in unemployment. After about 300 days the maximum sanction rate is attained, and at longer durations there is a tendency towards decreased sanction rate. This is consistent with the fact that sanctions that are imposed because of some violation during the unemployment spell cannot be given at the start of that spell.

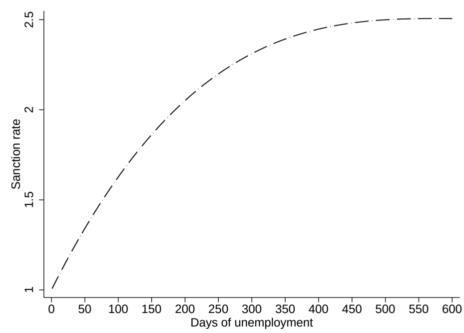

Figure 5: Estimated duration dependence. Normalized sanction rate.

6.2 Effect heterogeneity

We now allow the sanction effect to vary over the population. We first specify δ as a function of explanatory variables $\mathbf{x}$, as follows: $\delta = x'\gamma$, for some parameter vector γ that replaces the single effect parameter δ. Since our sample only contains a limited number of sanctions, we only include a small number of variables. We test for heterogeneous effects by sex, age, level of education, local unemployment rate, type of sanction regime and local sanction volume. Table 6 presents the estimated sanction effects. The other estimates are very similar as for the basic model, and are therefore not reported. We find interesting heterogeneous treatment effects: the sanction effect is significantly lower for males, and significantly lower for older unemployed. There is further no difference in the sanction effect by level of education, nor by local unemployment rate. We also included the regional occurrence of sanctions (number of sanctions divided by the number of unemployed), interacted with the treatment. If stigma is an important part of the sanction effect, the sanction effect should be lower in regions where sanctions are more common. However, we find no such differences.

Table 6: *Estimates of heterogenous sanction effect*

	Exit Hazard	
	Est.	S.e.
General	0.292	0.142
Male	-0.202	0.057
Log(age)	-0.306	0.129
High school Education	-0.068	0.069
University Education	0.066	0.085
Local unemployment	-0.017	0.021
New system	0.222	0.070
Regional sanction occurrence	-0.033	0.107
No. Individuals	16,491	
No. Spells	35,055	
Log-Likelihood	-175,695	

Notes: The model also includes controls for observed and unobserved variables. These estimates can be obtained from the authors upon request. Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors. Local unemployment is the regional unemployment in percent at the time of inflow. Regional sanction occurrence is the ratio of the annual number of sanctions in the region and the annual mean stock of unemployed in the region, times 1000. Measured at the time of the sanction.

Next, consider how the monitoring regime affects the sanction effect. As explained in Section 2 the Swedish sanction regime was changed in February, 2001. The reform introduced new, softer, sanctions, which reduced the size of the benefit reduction from 100% to 25%. The new sanctions may influence the average sanction effect in two ways. First, the new sanctions are softer so that we expect the sanction effect to decrease for each individual. Second, the reform increased the sanction volume, implying that unemployed who commits less serious violations are also sanctioned after the reform. These individuals are most likely more sensitive to sanctions, which gives an upward tendency in the average sanction effect. The effect of the reform on the average sanction effect is therefore theoretically ambiguous. We find that the average sanction effect is significantly higher under the new sanction regime. We draw two conclusions from this result: (i) unemployed who commits less serious violations are more responsive to sanctions, and (ii) the new softer sanctions are also considered as a severe punishment.

It is also possible that sanctions have an effect only shortly after they have been imposed. To investigate this we introduce duration dependence in the effect parameter, as follows: $\exp(\delta) = \exp(\delta_1 + \delta_2(t - t_s))$. If δ_2 is negative this means that the sanction effect decreases over time. Table 7 reports the estimates of δ_1 and δ_2. These results indicate very persistent effects of a sanction. We have multiplied the coefficient for δ_2 with 100. It means that the average sanction effect on the re-employment hazard after 100 days is about

Table 7: *Estimates of time-varying sanction effect*

	Exit Hazard	
	Est.	S.e.
General, δ_1	0.204	0.043
$t - t_s$, δ_2	-0.031	0.026
No. Individuals	16,491	
No. Spells	35,055	
Log-Likelihood	-175,695	

Notes: δ_2 have been multiplied with 100. The model also includes controls for observed and unobserved variables. These estimates can be obtained from the authors upon request. Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors.

19%, compared to 23% directly after the sanction has been imposed. There are several potential explanations to this persistent effect. It is reasonable to believe that individuals who have experienced a sanction are subject to intensified monitoring and attention from the case workers. In addition, second time offenders are punished harder, so that the persistent effect may be an effect of that the unemployed is eager to avoid future sanctions.

6.3 Job quality

We now consider the effect of a sanction on characteristics of the subsequent employment. From a welfare point of view, as well from the point of view of the unemployed individual, any such effects are important. If the job accepted after a sanction is similar to the job accepted in the counterfactual situation of no sanction, then severe sanctions and intensive monitoring have less adverse effects than otherwise.

Table 8 presents the estimates of the full model. The parameters of interest are δ_w and δ_h, the sanction effect on the wage and hours worked, respectively. Our estimates show a negative and significant (at 1% level) sanction effect on both the wage and on hours worked. A sanction decreases the accepted wage with about 3.8%. We measure hours worked using an indicator variable taking the value one for full-time employment and zero otherwise. Recalculated into marginal effects the results in Table 8 imply that a sanction increases the probability to accept part-time work with about 10.3 percentage points, or 15 percent. Part-time work is more often associated with a less secure employment, and of course, a lower income. We therefore interpret the effect on hours worked as a re-enforced negative effect on the job quality. It means that a sanction has a quite large negative impact on quality of the subsequent job.

Table 8: Estimates of the full model

	Exit Hazard		Sanction Hazard	
	Est.	S.e.	Est.	S.e.
Sanction effect	0.222	0.030		
Unobserved heterogeneity				
V^1/V^3	-4.653	0.146	-5.726	0.345
V^2/V^4	-3.443	0.147	-5.912	0.338
$\Pr(v_u = V^1, v_s = V^3)$	0.041			
$\Pr(v_u = V^1, v_s = V^4)$	0.594			
$\Pr(v_u = V^2, v_s = V^3)$	0.062			
$\Pr(v_u = V^2, v_s = V^4)$	0.303			
β_1/β_3				
β_2/β_4				
Individual				
Male	-0.090	0.017	0.075	0.038
Education in occupation	0.227	0.018	0.070	0.041
Experience in occupation	0.009	0.018	-0.063	0.044
Needs Guidance	-0.017	0.019	0.014	0.048
Log(age)	-0.358	0.037	-0.414	0.087
North	0.230	0.025	-0.099	0.059
South	-0.002	0.019	-0.189	0.042
High school Education	0.122	0.021	-0.133	0.046
University Education	0.061	0.025	-0.632	0.059
Local unemployment	-0.024	0.007	-0.099	0.017
Inflow time				
2000	0.011	0.024	0.175	0.066
2001	-0.050	0.028	0.156	0.072
2002	-0.108	0.029	0.387	0.071
2003	-0.252	0.028	0.508	0.067
σ				
Duration dependence				
α_1	$5.007 \cdot 10^{-3}$	$0.973 \cdot 10^{-3}$	$9.745 \cdot 10^{-3}$	$1.627 \cdot 10^{-3}$
α_2	$-34.711 \cdot 10^{-6}$	$10.803 \cdot 10^{-6}$	$-20.109 \cdot 10^{-6}$	$4.757 \cdot 10^{-6}$
α_3	$91.232 \cdot 10^{-9}$	$50.430 \cdot 10^{-9}$	$13.193 \cdot 10^{-9}$	$3.799 \cdot 10^{-9}$
α_4	$-125.542 \cdot 10^{-12}$	$115.532 \cdot 10^{-12}$		
α_5	$94.318 \cdot 10^{-15}$	$135.617 \cdot 10^{-15}$		
α_6	$-36.398 \cdot 10^{-18}$	$77.905 \cdot 10^{-18}$		
α_7	$5.652 \cdot 10^{-21}$	$17.302 \cdot 10^{-21}$		

Table 8: Continued

	Wage		Hours worked	
	Est.	S.e.	Est.	S.e.
Sanction effect	-0.038	0.007	-0.425	0.105
Unobserved heterogeneity				
β_1/β_3	-0.073	0.003	-0.306	0.080
β_2/β_4	3.349	1.597	3.271	1.656
Individual				
Male	0.071	0.003	1.507	0.050
Education in occupation	0.026	0.003	0.145	0.052
Experience in occupation	-0.004	0.003	-0.052	0.054
Needs Guidance	-0.006	0.003	-0.108	0.059
Log(age)	0.013	0.006	-0.354	0.107
North	-0.013	0.004	-0.103	0.066
South	-0.016	0.003	-0.051	0.052
High school Education	0.017	0.003	-0.098	0.068
University Education	0.113	0.004	0.392	0.074
Local unemployment	-0.044	0.001	-0.015	0.020
Inflow time				
2000 -0.009	0.006	-0.323	0.108	
2001 -0.022	0.008	-0.301	0.154	
2002 -0.009	0.010	-0.473	0.178	
2003 0.005	0.011	-0.604	0.203	
Observation time				
2000	0.033	0.006	0.713	0.111
2001	0.072	0.007	0.365	0.147
2002	0.110	0.009	0.520	0.172
2003	0.147	0.010	0.726	0.195
2004	0.173	0.011	0.930	0.219
2005	0.188	0.019	1.521	0.416
Constant	9.624	0.022	0.981	0.416
σ	0.133	0.001		
No. Individuals	16,491			
No. Spells	35,055			
Log Likelihood	-176,592			

Notes: Wage is the full-time monthly wage in SEK, and hours worked an indicator variable taking the value on if it is full-time employment and zero otherwise. Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors. Local unemployment is the regional unemployment in percent at the time of inflow.

The signs of the regressor effects on both the wage and hours worked are as expected. Males receive higher wages compared to women. Unemployed with high school education earn about 2% more than unemployed with less than high school education. The corresponding number for unemployed with university education is 11%. We find similar patterns for hours worked. Males, highly educated, and unemployed in low employment areas, tend to find full-time employment to a higher degree. This confirms that wage and full-time employment both are perceived as attractive job characteristics.

6.4 Long run effects

It may be that the sanction effect on the accepted job is a short-term effect, and that those who suffer a sanction catch up quickly, say after two or three years. This would be in line with the results in Zijl et al. (2009), who find that temporary jobs often serve as a stepping-stone into regular work. On the other hand it may also be the case that those who suffer a sanction end up on a lower job quality trajectory, with long-term or even permanent job quality effects. Obviously, if there are long run effect the negative welfare effects of sanctions are smaller. Investigating the long run effects is thus crucial from a policy perspective.

In order to investigate the long run effects of a sanction we re-estimate our full model using the wage and full-time status after two, three and four years instead of the wage and full-time status directly after the exit from unemployment. We use the same full model as specified in Subsection 5.2, including a normally distributed wage, a logit specification for the full-time part-time decision, and a flexible specification of the observed and unobserved effects in the model. Table 9 presents some descriptive statistics for these long run outcomes. Obviously, we cannot observe the wage for those who have left the labor market and not for those who once again are unemployed. As expected we therefore observe less and less wages as time passes on after the exit to employment. It means that we estimate these models with some reservations. However, as the wage several years after the exit from unemployment is rarely observed, we find this exercise meaningful.

Table 10 presents our long-term job quality estimation results. Here we only present the sanction effects, but remember that the models also include the duration until a sanction, as well as extensive controls for observed and unobserved heterogeneity. We find that sanctions have very persistent job quality effects. Our previous results indicated that a sanction decreases the wage directly after the exit from unemployment with 3.8 percent. Here, we find that this wage effect is 3.4, 4.3 and 4.7 after two, three and four years after the exit from unemployment, respectively. We find similar long run effects for hours worked: a sanction has negative and significant effect on the probability to get full-time employment. We conclude that those who get a sanction do not catch up quickly. From a welfare perspective this is an important result.

Table 9: *Sample statistics for long–term wages and hours worked*

	Full sample	Our sample
Exit to employment observed		
Of which	65.7	65.2
Observe wage after 1 year	25.7	23.9
Observe wage after 2 years	21.3	19.8
Observe wage after 3 years	16.5	15.2
Monthly wage in SEK after 1 years	19001 (4521)	18952 (4580)
Monthly wage in SEK after 2 years	19616 (4471)	19545 (4493)
Monthly wage in SEK after 3 years	20054 (4409)	19942 (4594)
Full time ($\geq$ 34 hours a week) after 1 year	68.2	67.0
Full time ($\geq$ 34 hours a week) after 2 years	69.5	68.0
Full time ($\geq$ 34 hours a week) after 3 years	69.5	65.9

Notes: Wage after 1 year is the observed wage 1-2 years after the exit, and so on. Full sample is the full sample of all unemployment spells, and our sample the selected sample described in the data section. Standard deviation in parenthesis.

Table 10: *Estimates of long-term job quality sanction effect*

	Exit Hazard		Wage		Hours worked	
	Est.	S.e.				
One year after exit	0.136	0.035	-0.034	0.010	-0.709	0.146
Log-Likelihood	-167,440					
Two years after exit	0.214	0.030	-0.043	0.010	-0.778	0.158
Log-Likelihood	-167,336					
Three years after exit	0.208	0.034	-0.047	0.017	-0.530	0.197
Log-Likelihood	-176,429					

Notes: Each panel (one, two and three years) represents different sets of results. Wage one year after exit is the full-time monthly wage in SEK, and hours worked an indicator variable taking the value on if it is full-time employment and zero otherwise, 1-2 years after the exit from unemployment, and so on. Each model also includes controls for observed and unobserved variables. These estimates can be obtained from the authors upon request. Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors.

6.5 Occupational changes

One can separate out two main explanations for a long run effect. It could either be an effect of the unemployed; (*i*) accepting a job with a lower occupational level, or (*ii*) accepting a less well paid job within the same occupation. From a policy perspective, separating between these two explanations is important. If a sanction forces individuals to switch into a less qualified occupation, it imply that these individuals are not able to utilize all their education and experience. It means that on average, acceptance of a job with a lower occupational level involves a larger loss of human capital than acceptance of a job in the same occupation. This loss becomes irreversible as human capital depreciates over time. It may therefore be more difficult for the individual to move out of a bad job match if the job has a lower occupational level. This makes it important to know whether sanctions often lead to a match in a lower occupational level.

We use two different approaches to test whether a sanction means that the unemployed accepts a job with a lower occupational level. In both approaches we utilize the occupation codes in our wage survey data. In the first approach we use the four official qualification levels. They are based on the "objective" qualifications required to perform each work, and not necessarily on the qualifications the individuals working in each occupation actually have. As described in Section 4, the different occupations are divided into four groups: occupations that require no or limited education, high school competence, short university education, and longer university education (3-4 years or more), respectively. It allows us to rank each occupation from one to four. In the second approach we use register data on the number of years of schooling on every individual in Sweden to classify the occupations. Using this education data and the entire wage survey for 2001, we calculate the mean number of years of schooling among the individuals employed in each occupation. It provides a measure of the qualification level of each occupation. We perform this classification at three different levels: dividing the occupations at one, two and three digit level, respectively.

In order to control for observed and unobserved effects we specify similar models as our regular full model. For the first approach we specify an ordered logit model for the four qualification levels. For the second approach specify a linear model for the mean number of years of schooling for each occupation, and assume that the error term is normally distributed. In order to control for selection on unobserved effect we allow for correlation between the unobservables in model. We take $V_q = \beta_{qe}V_e + \beta_{qs}V_s$, where V_q is unobserved characteristics in the occupation classification measure, and β_{qe} and β_{qs} as for the regular full model measure the correlation between the unobservables in the model.

Table 11: *Sample statistics for occupations*

	Fraction full sample	Fraction our sample	Qualification level	Years of schooling
Professionals	16.9	16.0	4	14.4
Technicians and associate professionals	12.8	12.9	3	13.2
Clerks	8.5	8.5	2	11.7
Service workers and shop sales workers	31.9	33.2	2	11.3
Skilled agricultural workers and fishery	2.0	1.9	2	10.6
Craft and related trade workers	10.0	9.4	2	10.9
Plant and machine operators	9.7	10.2	2	11.0
Elementary occupations	8.3	7.9	1	10.5
Exit to employment observed %	65.7	65.2		
Of which observe occupation code %	33.6	31.6		

Notes: Full sample is the full sample of all unemployment spells, and our sample the selected sample described in the data section. The division of the occupations are based on Statistics Sweden's SSYK classification. Two categories armed forces and managerial occupations are excluded. The qualification level is based on the official classification, based on the qualifications required to perform the tasks associated with each occupation. Years of schooling is the mean years of schooling among all employed in the occupation group in 2001.

Table 11 provides descriptive statistics for the occupation data. We show information for the 8 groups at the one digit level.[9] Column 1 and 2 show the proportion of unemployed entering into each occupation for our sample and the full sample, respectively. Column 3 reports the official qualification level obtained from SSYK for each group. These are the qualification levels used in the first approach. From these classifications it is clear that the occupations are primarily distinguished by the education normally required to perform the work associated with each occupation. Column 4 presents the mean number of years of schooling for the individuals employed in each group. Note that the difference between these groups is quite small. One reason for this is the existence of separate educational tracks.

Table 12 presents the estimation results: the upper panel displays the results from first approach and the lower panel from the second approach. For brevity, we only report the sanction effects. All models indicate a negative effect of a sanction on the qualification level. The effect is, however, not significant in the first approach model. Most likely, this is because these groups are very broadly defined. For the second approach, which utilizes the mean years of schooling to classify the occupations, we find significant effects. A sanction means that the unemployed on average accept employment within an occupation that on average requires 0.036-0.047 less years of schooling. In other words, unemployed who experience a sanction on average switch into

[9]We have excluded work in the armed forces and managerial work. The reason for this is that they are not classified into the four qualification levels in SSYK.

Table 12: *Estimates of sanction effect on type of occupation*

	Exit Hazard		Occupation level.	
	Est.	S.e.	Est.	S.e.
Four level official classification				
Sanction effect	0.136	0.032	-0.030	0.177
Log-Likelihood	-182,041			
Classification by years of schooling				
Sanction effect one digit	0.256	0.030	-0.036	0.016
Log-Likelihood	-183,391			
Sanction effect two digits	0.151	0.029	-0.038	0.020
Log-Likelihood	-175,157			
Sanction effect three digits	0.196	0.028	-0.047	0.026
Log-Likelihood	-182,041			

Notes: The four panels represents different sets of results. Four level official classifications is a ordered logit specification for the official SSYK classification of the occupations. Classification by years of schooling classifies the occupations by the mean years of schooling among all employed in that group of occupations, either at one, two and three digits level. Each model also includes controls for observed and unobserved variables. These estimates can be obtained from the authors upon request. Sample is the selected sample described in the data section. Estimated using WESML with robust standard errors.

a slightly less qualified occupation, resulting in a loss of human capital. Because of the existence of separate educational tracks, this is likely to be a lower bound of the true loss.

6.6 An assessment of the design of the monitoring policy

In Subsection 4.3 we postulated two explanations for the fact that there was no persistent dramatic increase in the occurrence of sanctions after the monitoring policy change. It would be a formidable computational task to estimate a duration model with regime indicators, because the latter are time-varying over the course of a given spell of unemployment. Moreover, as we have seen, there is no uniform moment in time when observed outcomes jump to another level and remain stable afterwards. The occurrence of sanctions displays substantial region-specific fluctuations in the year after the policy change. For these reasons we do not estimate a before/after model. However, note that the calendar time indicators for the inflow moment do not display a significant difference when comparing 2000 to 2001. Because of this, the first-mentioned explanation is the most likely explanation: the policy change was ineffective due to the fact that case workers shun away from issuing sanctions more frequently. This interpretation is consistent with the facts that case workers have substantial discretionary power to implement policy guidelines, and that their primary task is to help the unemployed to find a job. As a result, across our observation window, the monitoring regime does not exert a strong ex ante or threat effect.

One could replace the current Swedish system by a system in which (i) monitoring focuses on job search effort instead of job offer decisions, and (ii) monitoring is carried out by different individuals than the case worker who provides job search assistance. It is plausible that this would lead to a threat effect on the exit rate to work before punishment and as such would lead to a reduction of unemployment. This is both because with (ii) the moral dilemmas that the case workers currently face are avoided, and because with (i) the unemployed cannot avoid sanctions by reducing their search effort to zero. Moreover, in such an alternative system one may expect less adverse effects of sanctions on post-unemployment labor market outcomes than in a system where (i) is not satisfied, like in the current system. Our empirical results show strongly adverse effects on post-unemployment outcomes in the current system. Assuming perfect monitoring after a realized punishment, the system with monitoring of job offer decisions entails that punished individuals now have to accept the jobs they like least, whereas the other system entails that punished individuals have to search harder for any possible job. The jobs they like least are the jobs with a low job quality. If the effects on post-unemployment outcomes are adverse in a system with monitoring of offer decisions then they are also adverse in the other system, because in both systems, the sanction involves a negative income effect. However, the theoretical results imply that the size of these adverse effects is larger in the former system than in the latter.

7 Conclusions

We find that sanctions have adverse effects on post-unemployment outcomes. On average, they cause individuals to accept jobs with a lower hourly wage and less working hours per week. The estimated average reduction in the accepted wage is almost 4%. The probability to move into full-time employment decreases with about 15%. What is more, we provide evidence that post-unemployment outcomes are also affected in the long run. Sanctions causally increase the likelihood of the acceptance of a job at a lower occupational level. Such decisions are to some extent irreversible, in which case they involve a permanent human capital loss. From a present-value point of view, this means that sanctions entail a substantial welfare loss for at least some individuals.

Concerning the effects of sanctions on the transition rate into work, we find a significant positive effect. On average, this involves a 23% increase. Compared to estimates for the job exit rate in other studies, this is a rather small effect. At the same time, the Swedish UI sanction rate is much smaller than in most OECD countries.

We explain our findings by additional and novel empirical and theoretical analyses, and we combine the evidence in order to assess the current Swedish monitoring system. First, our empirical examination of the monitoring policy change in our observation window leads us to conclude that case workers use their substantial discretionary power to keep sanction rates low because they feel uncomfortable initiating punishments to their clients. This finding shows how difficult it is to implement monitoring policies if those who carry out the day-to-day monitoring have discretionary power and have personal contacts to the individuals to whom they are supposed to issue punishments. In our case, the findings implies that across our observation window, the monitoring regime does not exert a strong ex ante (or "threat") effect.

Secondly, our theoretical analysis derives implications of the fact that Swedish monitoring is primarily focused on the prevention of job offer rejections. Such a policy has particularly adverse effects on post-unemployment outcomes. Its emphasis on the acceptance of all job offers means that individuals are pushed to modify their behavior towards the acceptance of low-quality jobs. In addition, this policy induces some individuals to reduce their search effort to zero in order to prevent receiving any job offers at all. The ex ante effect of monitoring is then perverse for some individuals, with more monitoring implying a lower job exit rate. We view this as a potentially important insight. The theoretical analysis is also able to explain also explain why the ex post effect on the job exit rate is not very large. The system of job offer decision monitoring places a natural upper bound on the sanction effect, because even if all offers are accepted, the job exit rate is bounded from above by the job offer arrival rate. And, after a sanction, unemployed workers may reduce their effort to zero in order to prevent further job offers and therefore additional punishments.

We contrast the job offer decision monitoring system to the alternative and more common system of job search effort monitoring. The adverse effects of sanctions on post-unemployment outcomes may be smaller with search effort monitoring, because it pushes individuals to search harder for any possible job and not just the jobs with low quality. Moreover, search effort monitoring is not compatible with the perverse ex ante effect mentioned above, and the ex post effect on the job exit rate is not restricted by the kind of upper bound mentioned above.

Let us consider the potential effects of a switch to a system in which (i) monitoring focuses on job search effort instead of job offer decisions, and (ii) monitoring is carried out by different individuals than the case worker who provides job search assistance. Such a system may lead to a larger threat effect, a larger ex post effect on the job exit rate, and a smaller ex post effect on post-unemployment outcomes. Obviously, a larger threat effect could lead to lower unemployment durations for many individuals. It would be interesting to shed some more light on these issues by studying spatial and temporal variations in institutions and outcomes in more detail, but the currently low occurrence of sanctions precludes this avenue. We should also note that in very recent years the Swedish system has gradually adopted more features of search effort monitoring (OECD, 2007). It is, however, important to point out that the policy changes suggested above cannot be expected to completely rule out adverse effects on post-unemployment outcomes. After all, if those effects are adverse in a system with monitoring of offer decisions then they will also be adverse in the other system, because in both systems, the sanction involves a negative income effect.

Our paper also makes a methodological contribution to the estimation of causal effects of rare endogenous events on duration outcomes. We show that WESML with an endogenously stratified sample containing all treated is a useful estimation method if one has access to population-level register data. It allows for a computationally feasible analysis and provides estimates with high precision.

The finding that individuals move more often to a lower occupational level after a sanction may have implications for the more general issue of how steeply benefits should decline as a function of the elapsed unemployment duration. Theoretical studies of optimal UI design do not distinguish between jobs in the same occupation (with opportunities to mitigate the low starting wage through job-to-job transitions) and jobs with a lower occupational level (where long-run opportunities may be less abundant). Such a distinction may shed a new light on the optimal balance moral hazard with the likelihood that unemployed individuals are driven into sub-optimal job matches. We leave this as a topic for further research.

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Essay 4: Cluster Sample Inference Using Sensitivity Analysis: the Case with Few Groups

1 Introduction

In many studies the analysis sample consists of observations from a number of groups, for example families, regions, municipalities, or schools. These cluster samples impose inference problems, as the outcomes for the individuals within the groups usually cannot be assumed to be independent. Moulton (1990) shows that such intra-group correlation may severely bias the standard errors. This clustering problem occurs in many difference-in-differences (DID) settings, where one usually use variation between groups and over time to estimate the effect of a policy on outcomes at the individual level. As such the DID methodology is compelling, since it has the possibility of offering transparent evidence, which is also reflected in the exploding number of studies using the approach, for surveys see e.g. Meyer (1995) and Angrist & Krueger (2000). Many of these studies use data from only a small number of groups, such as data for men and women, a couple of states, or data from only a few schools or villages. For more examples see e.g. Ashenfelter & Card (1985), Meyer et al. (1995), Card & Krueger (1994), Gruber & Poterba (1994), Eissa & Liebman (1996), Imbens et al. (2001), Eberts et al. (2002), and Finkelstein (2002). The purpose of this paper is to provide a new method of performing inference when the number of groups is small, as is the case in these studies.

The importance of performing correct inference is also reflected in the growing number of studies addressing the inference problem.[1] One key insight from this literature is that the number of groups is important when deciding how to address the clustering problem. If the analysis sample consists of data from a larger number of groups, several solutions to the inference problem are available; the cluster formula developed by Liang & Zeger (1986), different bootstrap procedures (see e.g. Cameron et al. 2008), or parametric methods (see e.g. Moulton 1990). As expected however several Monte Carlo studies

[1] See e.g. Moulton (1986, 1990), Arrelano (1987), Bell & McCaffrey (2002), Wooldridge (2003, 2006), Bertrand et al. (2004), Kezdi (2004), Conley & Taber (2005), Donald & Lang (2007), Hansen (2007*a*, 2007*b*), Ibragimov & Muller (2007), Abadie et al. (2007) and Cameron et al. (2008). Related studies are Abadie (2005) and Athey & Imbens (2006) which study semi-parametric and non-parametric DID estimation.

show that these methods perform rather poorly if the number of groups is small.[2]

To address this problem Donald & Lang (2007) introduce a between estimator based on data aggregated at group level.[3] They show that under certain assumptions, the aggregated error term is i.i.d normal and standard normal inference can be applied even if the sample consists of data from a small number of groups. Their method works as long as the number of groups is not too small. Since their method is based on aggregated data their inference will be conservative in the absence of within group correlation, or if the within group correlation is small. In the limit case when the model is just-identified, i.e. when the number of aggregated observations equals the number of variables varying at group level it is not possible to perform Donald & Lang (2007) inference.[4] An important example of a just-identified model is the two groups and two time periods DID setting. Another alternative is the two-stage minimum distance approach suggested by Wooldridge (2006). One important by-product of this approach is a simple test for the presence of within cluster correlation. However, as for the Donald & Lang (2007) approach the test does not work if the model is just-identified, as it is then based on a chi-square statistic with zero degrees of freedom. A final alternative is to use bias corrected standard errors as suggested by Bell & McCaffrey (2002). The method has two limitations; it does not work if the number of groups becomes too small or if the model includes a dummy variable taking the value one for exactly one cluster and zero otherwise.

As a response this paper proposes to use sensitivity analysis as a new method of performing inference when the number of groups is small. Design sensitivity analysis has traditionally been used to test whether an estimate is sensitive to different kinds of selectivity bias: see e.g. Cornfield et al. (1959) and Bross (1966), further see e.g. Rosenbaum & Rubin (1983), Lin et al. (1998), Copas & Eguchi (2001), Imbens (2003), Rosenbaum (2004) and de Luna & Lundin (2009). In these papers sensitivity analysis is performed with respect to e.g. the unconfoundedness assumption and with respect to the assumption of random missing data. If these assumptions hold, the usual estimators are unbiased and the sensitivity analysis amounts to assessing how far one can deviate from for example the unconfoundedness assumption before changing the estimate by some pre-specified amount, so that one can test if the results are sensitive to a departure from the assumption.

The sensitivity analysis approach proposed in this paper is similar, but nevertheless different in spirit. Under the assumption of no within group correlation standard normal i.i.d. inference based on disaggregated data is applicable.

[2] See e.g. Bertrand et al. (2004), Donald & Lang (2007), Cameron et al. (2008), Hansen (2007*a*), and Ibragimov & Muller (2007).

[3] Under certain assumptions the aggregation can be made on group-time level, instead of group-level.

[4] The inference is then based on a t-statistic with zero degrees of freedom.

If this assumption is violated any standard errors based on the assumption of no within group correlation will be biased downwards. It is shown that under certain assumptions this bias can be expressed in terms of a few parameters, called sensitivity parameters. In the basic case the bias is expressed in terms of a single sensitivity parameter, defined as the ratio between the variance of the group common error term creating within cluster correlation and the variance of the individual error term. The sensitivity analysis then amounts to assessing how much one can deviate from the assumption of no within group correlation before changing the standard error estimate by some pre-specified amount. That is to investigate how sensitive the standard errors are to within group correlation. The test can also be inverted in order to calculate a cut-off value, where higher values of the sensitivity parameter or simply larger variance of the group common shocks renders a certain estimate insignificant. If this cut-off value is unreasonably large one can be confident that the null hypothesis of no effect can be rejected. Optimally one could use information from other sources, for instance data from other countries, other time periods, or for another outcome, in order to assess the reasonable size of the sensitivity parameter. The approach proposed in this paper is therefore similar to standard sensitivity analysis, since it also assesses how much one can deviate from an important assumption, but it is also different in spirit since it is performed with respect to bias in the standard errors and not with respect to bias in the point estimate.

By introducing sensitivity analysis in this way, this paper contributes in several ways. The method is applicable when the analysis sample consists of data from only a small number of groups. It even handles just-identified models. As no other method is applicable in the just-identified case it is the best application of the sensitivity analysis method. If the model is not just-identified but the number of groups is still small the Monte Carlo study in this paper shows that the sensitivity analysis method offers an attractive alternative compared to other commonly used methods. The method is also able to handle different types of correlation in the cluster effects, most importantly correlation within the group over time and multi-way clustering. This is done by introducing several sensitivity parameters.

One key question is of course how to assess whether the sensitivity cut-off value is unreasonably large, that is how to assess the reasonable size of the within group correlation. I believe that this has to be done on a case by case basis. However, one advantage with the approach here is that the basic sensitivity parameter is defined as a ratio between two variances. It gives a sensitivity parameter with a clear economic interpretation, which of course is a basic condition for an informative sensitivity analysis. The next step is the discussion about a reasonable size of the sensitivity parameter. In order to shed more light on this issue two applications are provided. The sensitivity analysis method is applied to data analyzed in Meyer et al. (1995) on the effects of an increase in disability benefits on the duration of the period spent out of work and to Eissa & Liebman (1996) on the effects of an expansion in the earned income tax credit on labor supply. In both these studies key regressions are based on just-identified models. The sensitivity analyses indicate that the conclusion from the first study that the treatment effect is significant is not sensitive to departure from the independence (no-cluster) assumption, whereas the results of the second study are sensitive to the same departure and its conclusion cannot therefore be trusted. It demonstrates that the sensitivity analysis approach is indeed helpful for determining the validity of treatment effects.

The paper is structured as follows. Section 2 presents the basic model and analyzes the asymptotic bias (asymptotic in the number of disaggregated observations) of the OLS standard errors. Section 3 introduces the basic sensitivity analysis approach. Section 4 extends these basic results to more general settings. It is shown that different assumptions about the cluster effects lead to different types of sensitivity analyses. Section 5 presents Monte Carlo estimates on the performance of the sensitivity analysis method. The method is also compared to other commonly used methods of performing inference. Section 6 presents the two applications, and Section 7 concludes.

2 Basic model and bias in the regular OLS standard errors

Consider a standard time-series/cross section model. Take a linear model for the outcome y for individual i in time period t in group g as

$$y_{igt} = x'_{igt}\beta + e_{igt} \tag{1}$$

$$e_{igt} = c_{gt} + \varepsilon_{igt}$$

Here ε_{igt} is an individual time specific error, c_{gt} is a cluster effect which varies across groups and time, and x_{igt} the regressors. Of course individuals can represent any disaggregated unit. The regressors may or may not include fixed group effects and/or fixed time effects. This model covers a wide range of different models, including a "simple" cross-section, with data from for instance a couple of schools or villages. Another important example is the heavily used standard DID model. In a regression framework, a usual DID model is

$$y_{igt} = \alpha_g + \alpha_t + bD_{gt} + c_{gt} + \varepsilon_{igt}, \tag{2}$$

including fixed time, α_t, and fixed group effects, α_g, and where D_{gt} is an indicator function taking the value one if the intervention of interest is implemented in group g at time point t and zero otherwise. The treatment effect is, hence, identified through the variation between groups and over time. In this setting c_{gt} can be given a specific interpretation as any group-time specific shocks.[5]

Define $N = \sum^G \sum^T n_{gt}$, where G is the number of groups, T is the number of time periods, and n_{gt} is the number of individual observations for group g in time period t. If $\mathbb{E}[e_{igt}|x_{igt}] = 0$, the ordinary least square (OLS) estimate of β

$$\widehat{\beta} = (X'X)^{-1}X'Y \tag{3}$$

is an unbiased estimate of β. Here Y is a N-vector collecting all y_{igt}, X is a $N \times K$ matrix containing the observations of the independent variables, and accordingly β a K-vector of the coefficients of interest.

Next consider inference. Assume that

$$\mathbb{E}(ee') = \sigma^2 C,$$

where e is a N-vector collecting all e_{igt}, and $\sigma^2 \equiv 1/N\ tr(ee')$, and C is a positive-definite matrix that captures the correlation in the error terms between the individuals. The true covariance matrix is then

$$V = \sigma^2 (X'X)^{-1} X'CX(X'X)^{-1}, \tag{4}$$

which can be compared with the regular OLS covariance matrix formula

$$\hat{V} = \hat{\sigma}^2 (X'X)^{-1}. \tag{5}$$

The asymptotic bias (asymptotic in the number of individuals (N)) of the regular standard errors has been analyzed extensively: see e.g. Greenwald (1983): other contributions are Campbell (1977), Kloek (1981) and Holt & Scott (1982). Following equation (9)-(11) in Greenwald (1983) and some algebraic manipulations[6] gives the asymptotic bias in the estimated covariance matrix which can be expressed as

$$\mathbb{E}(\hat{V}) - V = \tag{6}$$

$$\sigma^2 \Big(\frac{tr[(X'X)^{-1}X'(I-C)X]}{N-K} (X'X)^{-1} + (X'X)^{-1}X'(I-C)X(X'X)^{-1} \Big).$$

Hence if $C = I$, that is the identity matrix, the estimated covariance matrix is an unbiased estimate of the true covariance matrix, and the estimated standard errors are unbiased. It holds if $c_{gt} = 0$ for all g and all t, and if ε_{igt} is i.i.d. This general formula incorporates the two main reasons for bias in the standard

[5] c_{gt} also captures any differences in the group mean due to changes in the composition of the group over time. If n_{gt} is large this problem is mitigated.

[6] Notice that V and n are defined in a different way here compared to Greenwald (1983). The expression follows from substituting equation (10) and (11) in Greenwald (1983) into equation (9) in Greenwald (1983), breaking out σ^2 and simplifying.

errors into one expression. They are: (*i*) the cluster correlation problem caused by the presence of c_{gt}, highlighted by Moulton (1990), and (*ii*) the policy autocorrelation problem caused by correlation over time in c_{gt}, highlighted by Bertrand et al. (2004). The exact size of these problems depend on the case specific shape of C.

For the model in equation (1) the bias is negative, the OLS standard errors underestimate the true standard errors. It should also be noted that the bias consist of two distinct parts. First, the OLS estimator of the error variance $\hat{\sigma}^2$, is neither an unbiased nor a consistent estimator of the true error variance σ^2, if the error covariance matrix does not satisfy the OLS assumptions. Second, and more obvious, even if the error variance is known, the standard errors are biased since the coefficient covariance matrix is mis-specified.

3 Sensitivity analysis for cluster samples

The discussion in the previous section reveals that whether or not $c_{gt} = 0$ is crucial for how to perform inference. If $c_{gt} = 0$ regular OLS inference can be performed, possibly with control for heteroscedasticity. If $c_{gt} \neq 0$ on the other hand the regular OLS standard errors will be severely biased. As shown by Donald & Lang (2007) this has very important implications when the number of groups is small. They introduce a between estimator based on data aggregated at group level. It creates an all or nothing situation; under the assumption of $c_{gt} = 0$ apparently narrow confidence intervals based on individual data, and under the assumption of $c_{gt} \neq 0$ apparently very wide confidence intervals based on aggregated data. Needless to say arguing that $c_{gt} = 0$ will almost always be very difficult, whereas arguing that the variance of c_{gt} is small is reasonable in many applications. In the end it is the size variance of the within group correlation that matters. This is the key idea behind the new method proposed in this paper.

Formally, the starting point for the sensitivity analysis method is the general formula for the bias in the regular OLS standard errors presented in equation (6). This expression is based on derivations in Greenwald (1983), which among other expressions uses

$$\mathbb{E}(\hat{\sigma}^2) = \sigma^2(N - tr[(X'X)^{-1}X'CX])/(N-K).^7 \tag{7}$$

Combining this expression with the definition of V in equation (4) and the definition of $\hat{V}$ in equation (5), and noting that $\mathbb{E}(\hat{V}) = \mathbb{E}(\hat{\sigma}^2)(X'X)$ gives

$$V = \frac{N-K}{N - tr[(X'X)^{-1}X'CX]}(X'X)^{-1}X'CX\mathbb{E}(\hat{V}). \tag{8}$$

[7] See derivations of equation (A.3) in Greenwald (1983).

Further $\text{plim}(\hat{V}) = \mathbb{E}(\hat{V})$ and thus $\mathbb{E}(\hat{V})$ can be consistently (in terms of number of individuals) estimated by $\hat{V}$.

Starting with this equation the idea behind the sensitivity analysis is straightforward. Faced with a cluster sample with data from only a small number of groups, one can use disaggregated data and estimate β using OLS. Then estimate $\hat{V}$ in equation (4) as if there were no cluster effects. Then notice that $\hat{V}$ only gives correct standard errors if $c_{gt} = 0$ for all g and t. However, as a sensitivity analysis one can use the expression above and express the bias in the covariance matrix in terms of different so called sensitivity parameters, and assess how large they have to be in order to change the variance of a parameter estimate by a certain amount: that is, if the results are insensitive to departures from the assumption of no within group correlation, it indicates that the results can be trusted. As shown below the exact specification of the sensitivity parameters will depend on the assumptions which can be imposed on C.

Let us start with the simplest case. If ε is homoscedastic and if $\mathbb{E}(c_{gt}c_{g't}) = 0$ for all t and all $g \neq g'$, and $\mathbb{E}(c_{gt}c_{gt'}) = 0$ for all g and all $t \neq t'$, then the full error term, $e_{igt} = c_{gt} + \varepsilon_{igt}$, is homoscedastic[8], equi-correlated within the group-time cell and uncorrelated between the group-time cells. Further assume $n_{gt} = n$ and $x_{igt} = x_{gt}$, that is, the regressors are constant within each group, and constant group size. This special case has been analyzed by Kloek (1981).[9] He shows that under these assumptions equation (8) reduces to

$$V = \mathbb{E}(\hat{V})\tau\frac{nGT - K}{nGT - K\tau} \tag{9}$$

with

$$\tau = 1 + (n-1)\frac{\sigma_c^2}{\sigma_c^2 + \sigma_\varepsilon^2}. \tag{10}$$

Here σ_c^2 is the variance of c, and σ_ε^2 the variance of ε. Expressing the ratio between these two variances as $\sigma_c^2 = \gamma\sigma_\varepsilon^2$ gives

$$V = \mathbb{E}(\hat{V})\left(1 + (n-1)\frac{\gamma}{1+\gamma}\right)\frac{nGT - K}{nGT - K(1 + (n-1)\frac{\gamma}{1+\gamma})}. \tag{11}$$

In other words the bias in the covariance matrix is expressed in terms of observables and a single unknown parameter γ, which is interpreted as the relation

[8]The sensitivity analysis throughout this paper is made under the homoscedasticity assumption. The assumption makes it possible to write the bias in terms of single parameters. If one suspect heteroscedasticity, one approach is to use standard errors robust to heteroscedasticity in the spirit of White (1980), and use this covariance matrix instead of $\hat{V}$. The sensitivity analysis based on this specification will then in general be conservative.

[9]Kloek (1981) analyzes the one dimensional case with only a group dimension and no time dimension. A group-time version of his proof is presented in Appendix.

between the variance of the group-time error term and the variance of the individual error term.[10]

Using standard textbook results; if $\gamma = 0$, that is if there is no within group correlation, and $\sum n_{jt}$ is large

$$t = \frac{\hat{\beta}_a}{\sqrt{\mathbb{E}(\hat{V}_{aa})}} \overset{a}{\sim} N(0,1), \tag{12}$$

where $\hat{\beta}_a$ is the ath element of $\hat{\beta}$, and $\hat{V}_{aa}$ the element in the ath column and ath row of $\hat{V}$. Furthermore if $\gamma \neq 0$ and known, that is if there is within group correlation, $c_{jt} \sim N(0, \sigma_c^2)$[11], and $\sum n_{jt}$ is large

$$t = \frac{\hat{\beta}_a}{\sqrt{V_{aa}}} = \frac{\hat{\beta}_a}{\sqrt{\mathbb{E}(\hat{V}_{aa})(1+(n-1)\frac{\gamma}{1+\gamma})\frac{nGT-K}{nGT-K(1+(n-1)\frac{\gamma}{1+\gamma})}}} \overset{a}{\sim} N(0,1). \tag{13}$$

It is then possible to use γ as a sensitivity parameter. After estimating $\hat{\beta}_a$ and consistently estimating $\mathbb{E}(\hat{V}_{aa})$ by $\hat{V}_{aa}$ using the disaggregated data, the sensitivity analysis then amounts to assessing how much γ has to deviate from zero in order to change the standard errors by a pre-specified amount. The sensitivity analysis method is applicable as long as the model is identified. In the present case with variables constant within each group-time cell, this holds if $GT \geq K$, i.e. if the number of group-time cells is larger than or equal to the number of explanatory variables. In other words our sensitivity analysis method even handles just-identified models, for instance the two groups and two time periods DID setting. As no other method is applicable in the just-identified case it is the best application of the sensitivity analysis method. If the model is not just-identified but the number of groups is still small the sensitivity analysis method offers an alternative to other commonly used methods such as the Donald & Lang (2007) approach.

The test can also be inverted in order to calculate the γ value which corresponds to a specific p-value. One could for example be interested in the γ cut-off value which renders the estimated treatment effect statistically insignificant at α% level. This follows from setting $t = Z_{1-\alpha/2}$ and solve for γ in the

[10] Actually γ is only potentially unknown. If the number of groups is larger σ_c^2 can be consistently estimated using the between group variation, and σ_ε^2 can be consistently estimated using the within group variation, and this gives p.

[11] The normality assumption can be replaced by any other distributional assumption, for instance a uniform distribution. However this will complicate the sensitivity analysis, since the combined error term will have a mixed distribution.

equation (13) above

$$\gamma_{c,a} = \frac{(\hat{\beta}_a^2 - Z_{1-\alpha/2}^2 \hat{V}_{aa})(nGT - K)}{(nZ_{1-\alpha/2}^2 \hat{V}_{aa})(nGT - K) - \hat{\beta}_a^2 (nGT - nK)}. \tag{14}$$

Here Z_υ is the υ quantile of the standard normal distribution. Note that I have replaced $\mathbb{E}(\hat{V}_{aa})$ with $\hat{V}_{aa}$ as it is consistently estimated by $\hat{V}_{aa}$. Furthermore, note that $\gamma_{c,a}$ depends on n, the number of observations for each group. This dependence comes both from $\hat{V}$ which decreases as n increases and also directly as n enters the expression for $\gamma_{c,a}$. Taken together these two effects means that $\gamma_{c,a}$ increases as n goes from being rather small to moderately large: however as n becomes large this effect flattens out, and $\gamma_{c,a}$ is basically constant for large n.

If $\gamma_{c,a}$ is unreasonably large, one could be confident that the null-hypothesis about zero effect could be rejected. The key question then becomes: what is unreasonably large? At the end of the day, as with all sensitivity analyses, some judgment has to be made. Since the true γ may vary a lot between different applications, I believe that the assessment has to be done on a case by case basis. However, the sensitivity analysis presented here avoids the common sensitivity analysis pitfall. That is, that one is left with a sensitivity parameter which is hard to interpret and thus hard to relate to economic conditions. Here the basic sensitivity parameter, γ, is defined as the ratio between two variances, which makes it both easier to interpret and easier to discuss. Optimally one could also use information from other sources to make the discussion more informative, for instance, data from another country, other time periods, or for another outcome. In some cases it may also be beneficial to re-scale γ. The two applications presented in Section 6 using data from Meyer et al. (1995) and Eissa & Liebman (1996) further exemplify how γ can be interpreted.

If either the assumption of $n_{gt} = n$ or $x_{igt} = x_{gt}$ is relaxed the sensitivity analysis is still straightforward. Note that the general formula for the bias presented in equation (8) nevertheless holds. In the basic case with $n_{gt} = n$ or $x_{igt} = x_{gt}$ this expression could be simplified considerably. In general under assumption $\mathbb{E}(c_{gt}c_{g't}) = 0$, assumption $\mathbb{E}(c_{gt}c_{gt'}) = 0$, and with the model specified as in equation (1), C has the familiar block-diagonal structure

$$C = \begin{bmatrix} C_1 & \dots & 0 \\ \vdots & \ddots & \vdots \\ 0 & \dots & C_{GT} \end{bmatrix} \tag{15}$$

with $C_{GT} = [(1 - \frac{\gamma}{1+\gamma})I_{gt} + \frac{\gamma}{1+\gamma}J_{gt}]$. Here I_{GT} is an n_{gt} times n_{gt} identity matrix, and J_{gt} is an n_{gt} times n_{gt} matrix of ones: $\gamma_{c,a}$ is then found by numerically solving for γ in

$$Z_{1-\alpha/2} = \frac{\hat{\beta}_a}{\sqrt{V_{aa}}}, \tag{16}$$

with V defined as in equation (8) and C defined as in equation (15) above. From calculations I note that in general $\gamma_{c,a}$ is quite insensitive to violations of $n_{gt} = n$, except when some groups are very large and others are very small.

4 Extended sensitivity analysis

4.1 Correlation over time in the cluster effects

The sensitivity analysis presented in the previous section is applicable under a number of assumptions on c_{gt}. Most notably, $\mathbb{E}(c_{gt}c_{g't}) = 0$ for all t and all $g \neq g'$, and $\mathbb{E}(c_{gt}c_{gt'}) = 0$ for all g and all $t \neq t'$. In many studies $\mathbb{E}(c_{gt}c_{gt'}) = 0$ for all g is a restrictive assumption. In a model with fixed group and fixed time effects, c_{gt} captures any group-time shocks. Consider a study on the effects of minimum wages on employment using variation across regions and over time. The group-time shocks then capture all regional specific shocks in employment. If present they are most likely correlated over time. This problem, often refereed to as the policy autocorrelation problem, was highlighted by Bertrand et al. (2004).

This subsection therefore relax the assumption that $\mathbb{E}(c_{gt}c_{gt'}) = 0$: instead assume an AR(1) structure for c_{gt}

$$c_{gt} = \kappa c_{gt-1} + d_{gt}, \tag{17}$$

where d_{gt} is assumed to be a white noise series with mean zero and variance σ_d^2. Further, assume that $|\kappa| < 1$ and make the natural extension of the basic sensitivity analysis and define $\sigma_d^2 = \gamma\sigma_\varepsilon^2$. It gives two sensitivity parameters, γ and κ, instead of the single sensitivity parameter γ. Then if $\kappa = 0$ the basic sensitivity analysis is applicable. To be clear, κ is interpreted as the first-order autocorrelation coefficient for c_{gt}, and γ as the relation between the variance of the group-time specific shock and the variance of the unobserved heterogeneity.

Consider the case with repeated cross-section data. Assume that data on n_{gt} individuals from group g in time period t are available. The general formula presented in equation (8) for the covariance matrix still holds. However, since c_{gt} is allowed to follow an arbitrary AR(1) process, C will obviously differ from the basic sensitivity analysis. In order to express C in terms of κ and γ I use the well know properties of an AR(1) process. It turns that out if $n_{gt} = n$ and $x_{igt} = x_{gt}$ holds, there is a simple expression for the relation between V

and $\hat{V}$

$$V_{aa} \approx \mathbb{E}(\hat{V}_{aa})(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2}+n\frac{\gamma}{1+\gamma-\kappa^2}H_{aa}) \tag{18}$$

where H_{aa} is the element in the ath column and ath row of H given by

$$H = (\sum_g \sum_t x_{gt}x'_{gt})^{-1} \sum_g \sum_t \sum_{t' \neq t} (\kappa^{|t-t'|} x_{gt}x'_{gt'})$$

The proof can be found in Appendix.

Based on this simple expression for the bias in the regular OLS standard errors, one can assess the sensitivity of the standard errors with respect to both the autocorrelation and the variance of the group-time specific shocks. As for the basic sensitivity analysis one may be interested in the cut-off value which renders an interesting estimate insignificant. In this case with two sensitivity parameters a natural way to proceed is to solve for γ for a range of values of κ. If the interest lies in the effect of variable a, then the cut-off value for γ is

$$\gamma_{c,a} = \frac{(\hat{\beta}_a^2 - Z_{\alpha/2}^2 \hat{V}_{aa})(1-\kappa^2)}{(nZ_{\alpha/2}^2 \hat{V}_{aa})(1+H_{aa}) - \hat{\beta}_a^2}. \tag{19}$$

Again note that $\mathbb{E}(\hat{V}_{aa})$ is replaced with $\hat{V}_{aa}$ as it is consistently estimated by $\hat{V}_{aa}$. If the combinations of $\gamma_{c,a}$ and κ values are unreasonable large, one could be confident in that the null hypothesis about zero effect should be rejected. Also note that $\gamma_{c,a}$ can either increase or decrease with κ, as H_{aa} can either increase or decrease with κ.

If either $n_{gt} = n$ or $x_{igt} = x_{gt}$ do not hold it is not possible to obtain a closed from solution for $\gamma_{c,a}$. But using numerical methods, it is possible to solve for γ in

$$Z_{1-\alpha/2} = \frac{\hat{\beta}_a}{\sqrt{V_{aa}}}, \tag{20}$$

for a range of values of κ and the desired significance level. Here V is defined in equation (8), and C is defined in equation (A.13) presented in appendix.

4.2 Multi-way clustering

Consider an application where one have data from a number of regions and where the region is defined as the group. In the sensitivity analysis presented so far, the assumption of $\mathbb{E}(c_{gt}c_{g't}) = 0$ is crucial. In other words it is assumed that the outcomes for individuals within a region are correlated and that there is no correlation between individuals on different sides of the border between two different regions. Most likely this will be violated in many applications. Here this assumption is relaxed in the situation with cross-section data. Assume that the groups can be divided into group clusters containing one or more groups. Dropping the time dimension, the outcome y for individual i in group g in group-cluster s is

$$y_{igs} = x_{igs}\beta + c_{gs} + \varepsilon_{igs}. \tag{21}$$

Retain the definition of γ from the basic sensitivity analysis as $\sigma_c^2 = \gamma\sigma_\varepsilon^2$. γ is then again interpreted as the relation between the variance of the group-time shocks and the variance of the individual unobserved heterogeneity. Further assume that if $s \neq s'$ then $E(c_{gs}c_{g's'}) = 0$ and if $s = s'$ then $E(c_{gs}c_{g's'}) = \xi\sigma_c^2$.[12] ξ should be interpreted as the relation between the inter-group correlation and the intra-group correlation for groups in the same cluster of groups. This means that it will be far below one in many applications.

Note that the general expression for the covariance matrix presented in equation (8) holds. If the above assumptions hold, and if $n_g = n$ and $x_{igt} = x_{gt}$ hold, the derivations in the appendix show that there is a simple relation between V_{aa} and $\hat{V}_{aa}$

$$V_{aa} \approx \hat{V}_{aa}(1 + (n-1)\frac{\gamma}{1+\gamma} + n\frac{\gamma}{1+\gamma}\xi M_{aa}) \tag{22}$$

where M_{aa} is the element in the ath column and ath row of M given by

$$M = (\sum_s \sum_g x_{gs}x'_{gs})^{-1} \sum_s \sum_g \sum_{g' \neq g} (x_{gs}x'_{g's}).$$

Again there are two sensitivity parameters, γ and ξ. As in the previous case one can proceed to solve for $\gamma_{c,a}$ for a range of values of ξ. Let us that the interest lies in the effect of variable a: then

$$\gamma_{c,a} = \frac{\hat{\beta}_a^2 - Z_{\alpha/2}^2\hat{V}_{aa}}{(nZ_{\alpha/2}^2\hat{V}_{aa})(1+\xi M_{aa}) - \hat{\beta}_a^2}. \tag{23}$$

If these combinations of $\gamma_{c,a}$ and ξ values are unreasonable large, one could be confident that the null hypothesis about zero effect should be rejected. One could also interpret the division of the groups into group clusters as a robustness analysis. The standard errors may be sensitive to some divisions but not to others. Note that introducing multi-way clustering in the way done here increases the standard errors, and thus $\gamma_{c,a}$ decreases with ξ.

[12] It is obviously possible to also allow for an time-dimension, which generally gives sensitivity analysis in three parameters, which would measure the variance, the autocorrelation respectively the between group correlation in the cluster effects.

If either $n_{gt} = n$ or $x_{igt} = x_{gt}$ do not hold it is not possible to obtain a closed from solution for $\gamma_{c,a}$. But it is possible to solve for γ in

$$Z_{1-\alpha/2} = \frac{\hat{\beta}_a}{\sqrt{V_{aa}}}, \tag{24}$$

for a range of values of ξ and the desired significance level. Here V is defined in equation (8), and C is defined in equation (A.25) presented in the appendix.

5 Monte Carlo evidence

This section provides Monte Carlo estimates of the performance of the proposed sensitivity analysis method. The small sample properties of the method and robustness of the method to the choice of reasonable γ are investigated. The sensitivity analysis method is also compared to other commonly used inference methods. I consider a DID set up. The treatment is assumed to vary at group-time level, and the interest lies in estimating the effect of this treatment on individual outcomes.

Assume that the underlying model is

$$y_{igt} = c_{gt} + \varepsilon_{igt}.$$

The group error term, c_{gt}, and the individual error term, ε_{igt}, are both independent normals with variance σ_c^2 and σ_ε^2. Take $\sigma_c^2 = 0.1$ and $\sigma_\varepsilon^2 = 1$. I experiment with different numbers of of groups (G) and different number of time periods (T). Data are generated with a constant group-time cell size, $n_{gt} = n$. In all experiments 50,000 simulations are performed.

I estimate models of the form

$$y_{igt} = \alpha_g + \alpha_t + bD_{gt} + c_{gt} + \varepsilon_{igt}.$$

This represents a general DID setting, with fixed group effects, α_g, fixed time effect, α_t, and a treatment indicator variable, D_{gt}, taking the value one if the treatment is imposed in group g at time point t. b is then the treatment effect. The treatment status is randomly assigned. In the basic case I take two time periods ($T = 2$) and two groups ($G = 2$). The treatment status is then assigned to one of the groups ($G_1 = 1$), and they experience the treatment in the second period. Besides the basic case, other combinations of T,G and D are considered [13]. To be precise, the basic model with $T = 2$, $G = 2$ and $G_1 = 1$ includes two group dummies, one time dummy for the second period, and one treatment dummy taking the value one in the second period for group two. The models for other combinations of T,G and D follow in the same way.

[13] If $T > 2$ the treatment occurs after $T/2 - 0.5$ if T is a odd number and after $T/2$ if T is an even number.

Table 1: *Monte Carlo results for the small sample properties of the sensitivity analysis method*

Group Size (n)	$G = 2$, $T = 2$ $G_1 = 1$	$G = 3$, $T = 2$ $G_1 = 1$	$G = 3$, $T = 3$ $G_1 = 1$	$G = 5$, $T = 5$ $G_1 = 2$
10	0.0503	0.0492	0.0495	0.0504
20	0.0496	0.0489	0.0512	0.0500
50	0.0505	0.0516	0.0494	0.0500
100	0.0484	0.0519	0.0501	0.0505
1000	0.0505	0.0504	0.0495	0.0494

Notes: Monte Carlo results for the treatment parameter which enters the model with a true coefficient of $b = 0$. The model and the data generating process is described in detail in the text. Each cell in the table reports the rejection rate for 5% level tests using the sensitivity analysis γ_c as test-statistic, and γ_t as critical value. Test based on a $t_{nGT-G-T}$. T is the number of time periods, G the number of groups, and G_1 the number of groups who receives the treatment. The number of simulations is 50,000.

5.1 Small sample properties

As shown in Section 3, the sensitivity analysis method can be used to derive a cut-off value, γ_c. This value can be seen as a test-statistic. If one is confident that this value is unreasonably large one should reject the null-hypothesis of zero effect. In other words the critical value is decided by the researcher's knowledge about reasonable values of γ.

If the researcher knows the true relation between σ_c^2 and σ_ε^2, referred to as $\gamma_t = \sigma_c^2/\sigma_\varepsilon^2$, then theoretically if N is large a test for $b = 0$ using γ_c as a test-statistic and using γ_t as the critical value should have the correct size. This should hold for any combination of $T \geq 2, G \geq 2$ and $G > G_1$. This subsection confirms this property. I also examine the small sample properties of this approach. To this end the approach is somewhat modified. Asymptotically (in N) the sensitivity analysis method can be based on a normal distribution, regardless of the distribution of the individual error, ε. If N is small but ε is normally distributed the analysis should be based on a t-distribution with $nGT - G - T$ degrees of freedom. This follows since the t-statistic reported in equation (12) has an exact t-distribution instead of a normal distribution.

Table 1 presents the results from this exercise. Each cell of Table 1 represents the rejection rate under the specific combination of n,T,G,D, and γ_t. As apparent from the table, the sensitivity analysis method works as intended for all sample sizes. It confirms that the derived properties of the sensitivity analysis method are correct. This is not surprising since the sensitivity analysis is based on OLS estimates with well established properties. It does not, however, give evidence for an inferential method in a strict statistical sense as the exact value of the used critical value γ_t is not known in practice. In practice reasonable values of γ_t have to be assessed, for instance, using other data sources.

5.2 Robustness and comparison with other inference methods

The researcher may have information through other data sources, or for other outcomes, which enables a closer prediction of γ_t. However information that enables an exact estimate of γ_t is not likely to be available. The second Monte Carlo study therefore test the robustness of the results with respect to assessing an incorrect γ_t. Distinguish between the true ratio between the two error variances, γ_t and the ratio that the researcher thinks is the correct one, γ_r. If $\gamma_c > \gamma_r$ the sensitivity analysis suggests rejecting the null-hypothesis of zero effect. If $\gamma_t > \gamma_r$ this leads to over-rejection of the null-hypothesis. Here the severity of this problem is tested.

As a comparison the sensitivity analysis method is contrasted with other methods commonly used to perform inference. The other methods include OLS estimates without any adjustment of the standard errors, labeled OLS regular. Furthermore, OLS estimates with the commonly used Eicker-White heteroscedasticity robust standard errors for grouped data. I either "cluster" at group level or "cluster" at the group-time level, i.e. variance matrices which is robust to within group correlation, and robust to within group-time cell correlation, respectively. These inference methods are labeled cluster group and cluster group-time. They are by far the most common ways of correcting for the use of individual data and outcomes that vary only on at group level. The general cluster formula is

$$\hat{V}_{cluster} = \frac{N-1}{N-K}\frac{C}{C-1}\Big(\sum_{c=1}^{C} X_c'X_c\Big)\Big(\sum_{c=1}^{C} X_c'\hat{u}_c\hat{u}_c'X_c\Big)\Big(\sum_{c=1}^{C} X_c'X_c\Big)$$

where c indicates the cluster and C the number of clusters. Further, $\hat{u}_c$ is a vector containing the OLS residuals, and X_c is a matrix containing the observations of the independent variables for the individuals in cluster c. Also, note that a degrees of freedom correction is used. The tests are based on a t_{C-1}, i.e. t_{G-1} for clustering at the group level, and t_{GT-1} for clustering at the group-time level.

The two-step estimator suggested by Donald & Lang (2007) is also considered. In the present case with explanatory variables which vary only at group-level, and in the absence of correlation over time in c_{gt}, the first step is aggregation at the group-time level. This gives

$$\bar{y}_{gt} = \alpha_g + \alpha_t + \beta X_{gt} + c_{gt} + \bar{\varepsilon}_{gt},$$

where $\bar{y}_{gt}$ and $\bar{\varepsilon}_{gt}$ are the group-time averages of y_{igt} and e_{igt}. The second step amounts to estimating this model using OLS. In the present case when both the error terms are independent normals the resulting t-statistic for the hypothesis test of $b = 0$ has a t-distribution with $GT - K$ degrees of freedom.

The upper panel of Table 2 presents the results for the sensitivity analysis method, and the lower panel presents the results for the other four methods.

Size is for 5% level tests for the treatment parameter which enters the model with a true coefficient of $b = 0$. Power is 5% level test versus the alternative that $b = 0.5$. In this analysis I take $n = 200$. Before interpreting these results note that the power should be compared for tests with the same nominal size. Furthermore, the terminology size and power for the sensitivity analysis method is not entirely correct from a statistical point of view. The sensitivity analysis gives a "test-statistic" as a cut-off value, γ_c, but the cut-off value is decided by the researchers assessment of a reasonable size off γ. In that sense it is a test, which makes it reasonable to report the size and power. Also note that the main point of this section is to explore how sensitive the results are to the assessment of a reasonable size off γ.

First, consider the performance of the other methods commonly used to perform inference. The results for the OLS estimates using regular standard errors and the two cluster formulas confirm what has been found in earlier studies, see e.g. Bertrand et al. (2004), Donald & Lang (2007), Cameron et al. (2008), and Hansen (2007*a*). The regular uncorrected OLS estimates have large size distortions. The rejection rate for 5% level tests is 0.256 with $G = 2$, $T = 2$, $G = 1$. The two OLS cluster estimators also suffer from large size distortions. As expected these methods behave poorly if the number of groups is small: after all they were designed for the case with a large number of groups. If the number of groups is only moderately small, say $G = 5$ and $T = 5$, these tests perform somewhat better.[14]

Next, consider the performance of the Donald & Lang (2007) (DL) two step estimator. If the model is just-identified as in the case with $G = 2$ and $T = 2$ the test of the null hypothesis should be done using a t-statistic with zero degrees of freedom. In other words it is not possible to use this test for just-identified models. Next consider how the two step estimator performs if the groups become somewhat larger, but are still very small. The results in Column 2, 3 and 4 show that the DL estimator has correct size if the model is not just-identified. This confirm the results in DL. However, if the number of groups is very small (Column 2 and 3) the power of the DL estimator is low.

Let us compare these results with the results for the sensitivity analysis method, which uses γ_c as the test-statistic and γ_r as the critical value. First, consider the results when $\gamma_t = \gamma_r$, i.e. the researcher is able to correctly assess the size of the within group correlation. As before the test has the correct size. Since the size of the test for the sensitivity analysis method and the DL method are the same for the results in Column 2-4, the power estimates are comparable. The results show that the power is higher in the sensitivity analysis method. If the number of groups is very small, as in Column 2 and 3, the difference is large. For example if $G = 3$, $T = 2$, and $G = 1$ the power is 0.657 for the sensitivity analysis compared to 0.148 for the DL two step estimator. If

[14]Notice that this experiment is set up with no correlation between the groups or over time in c_{gt}. If that were the case one could expect these cluster estimators to perform even worse. The size distortions for $G = 5$ and $T = 5$ would then be likely to also be very large.

Table 2: *Monte Carlo results for the robustness of the sensitivity analysis method and other commonly used methods to perform inference*

	$G=2, T=2$		$G=3, T=2$		$G=3, T=3$		$G=5, T=5$	
	$G_1=1$		$G_1=1$		$G_1=1$		$G_1=2$	
	Size	Power	Size	Power	Size	Power	Size	Power
Sensitivity analysis: $\gamma_t=0.010$								
$\gamma_r=0.005$	0.111	0.671	0.112	0.778	0.108	0.870	0.112	1.000
$\gamma_r=0.008$	0.069	0.587	0.068	0.701	0.066	0.816	0.069	0.999
$\gamma_r=0.009$	0.057	0.562	0.058	0.677	0.058	0.794	0.059	0.999
$\gamma_r=0.010$	0.050	0.531	0.048	0.657	0.050	0.779	0.050	0.998
$\gamma_r=0.011$	0.044	0.504	0.041	0.627	0.044	0.758	0.044	0.998
$\gamma_r=0.012$	0.038	0.481	0.036	0.609	0.038	0.739	0.037	0.998
$\gamma_r=0.015$	0.024	0.411	0.024	0.539	0.024	0.675	0.024	0.996
$\gamma_r=0.020$	0.011	0.314	0.012	0.431	0.011	0.575	0.012	0.992
OLS regular	0.256	0.817	0.257	0.889	0.260	0.945	0.257	1.000
Cluster group-time	1.000	0.978	0.562	0.934	0.370	0.944	0.133	0.973
Cluster group	1.000	0.978	0.276	0.684	0.280	0.750	0.107	0.996
DL two step	n.a.	n.a.	0.051	0.148	0.051	0.462	0.050	0.998

Notes: Monte Carlo results for simulated data. The model and the data generating process is described in detail in the text. γ_t is the true relation between the variance of the group-time error and the individual error, and γ_r the assessed relation between these two variance. Further T is the number of time periods, G the number of groups, G_1 the number of groups who receives the treatment, and n_{gt} the sample size for each group-time cell. Size is for 5% level tests for the treatment parameter which enters the model with a true coefficient of $b=0$. Power is 5% level test versus the alternative that $b=0.5$. The number of simulations is 50,000.

the number of groups becomes somewhat larger as in Column 4 the difference is smaller. In this case the Donald & Lang (2007) two step estimator is likely to be preferable to sensitivity analysis. Also note that even if $G=2$, $T=2$, $G_1=1$ the power of the sensitivity analysis test is high.

The previous comparison was based on the assumption that the researcher is able to assess the correct value of γ. In practice this is unreasonable. It is therefore also interesting to see what happens if $\gamma_t \neq \gamma_r$, i.e. when the researcher is unable to exactly infer γ. These results are also presented in Table 2. These results show that the sensitivity analysis method performs well if the difference between γ_r and γ_t is rather small. For example, the rejection rate for 5% level tests is 0.069 if $\gamma_r=0.008$ and $\gamma_t=0.010$. This is only a small over-rejection of the null-hypothesis. However if the difference between γ_r and γ_t becomes large, there are as expected substantial size distortions.

To summarize, the Monte Carlo simulations have confirmed that the derived properties of the sensitivity analysis method are correct for both large and small sample sizes. They further show that existent inference methods run into problem when the number of groups is very small. Finally, the results show that the sensitivity analysis method is applicable, even if the number of groups is very small, as long as the size of the within group correlation can be reasonably assessed. The two applications provided in the next section show that this can often can be done.

6 Applications

6.1 Application 1: Disability benefits

Meyer et al. (1995)[15] (MVD) study the effects of an increase in disability benefits (workers compensation) in the state of Kentucky. Workers compensation programs in the USA are run by the individual states. Here I describe some of the main features of the system in Kentucky. A detailed description is found in MVD. The key components are payments for medical care and cash benefits for work related injuries. MVD focus on temporary benefits, the most common cash benefit. Workers are covered as soon as they start a job. The insurance is provided by private insurers and self-insurers. The insurance fees that employers pay are experience rated. If eligible the workers can collect benefits after a seven day waiting period, but benefits for these days can be collected retroactively if the duration of the claim exceeds two weeks. The claim duration is decided mainly by the employee and his or her doctor, and there is no maximum claim duration.

The replacement rate in Kentucky before 1980 was $66\frac{2}{3}\%$ and the benefits could be collected up to a maximum of \$131 per week. The reform as of July 15, 1980, analyzed by MVD increased the maximum level to \$217 per week: a 66% increase or 52% over one year in real terms.[16] The replacement rate was left unchanged. Thus workers with previous high earnings (over the new maximum level) experience a 66% increase in their benefits, while the benefits for workers with previous low earnings (below the old ceiling) were unchanged. This creates a natural treatment group (high earners) and a natural control group (low earners). MVD analyze the effect of the increase using a DID estimator, which contrasts the difference in injury duration between before and after the reform for the treatment group and the control group.

The upper panel of 3 restates MVD's results for the outcome mean log injury duration, taken from their Table 4.[17] Column 1-4 present the pre-period

[15]This data has also been reanalyzed by Athey & Imbens (2006). They consider non-parametric estimation, and inference under the assumption of no cluster effects. Meyer et al. (1995) also consider a similar reform in Michigan.

[16]For calculations see Meyer et al. (1995) p 325.

[17]The terminology "mean" is not totally accurate. The outcome used by MVD is censored after 42 months. However, at this duration only about 0.5% of the cases are still open. MVD therefore sets all ongoing spells to 42 months. Meyer et al. (1995) also consider other outcome variables and note that their results are quite sensitive to the choice of specification. Here, the focus is on their preferred outcome.

Table 3: *Sensitivity analysis estimates for application 1 on disability benefits*

	Treated (High earnings)		Non-Treated (Low earnings)		Differences		DID
	Pre period	Post Period	Pre period	Post Period	[2-1]	[4-3]	[5-6]
	[1]	[2]	3]	[4]	[5]	[6]	[7]
Log duration	1.38	1.58	1.13	1.13	0.20	0.01	0.19
	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.07)
Sample Size	1,233	1,161	1,705	1,527			
Sensitivity Analysis:							
γ_c - 5 % [10%]					0.0026 [0.0041]	-	0.00067 [0.00127]
$\sqrt{\gamma_c} * \sigma_\varepsilon$: - 5 % [10%]					0.0629 [0.0787]	-	0.0335 [0.0461]

Notes: The results in the upper panel are taken from Meyer et al. (1995), their standard errors in parentheses. The outcome is mean log duration, censored after 42 months. The sensitivity analysis results in the lower panel is own calculations. γ_c is calculated by numerically solving for γ in equation (16), for the specified significance level.

and post-period averages for the treatment and control group, Column 5 and 6 the difference between the pre- and post-period for the two groups, and Column 7 present the DID estimate. The DID estimate of the treatment effect is statistically significant and suggests that the increased benefits increased the injury duration by about 19%. MVD ignores the cluster-sample issue and use regular OLS standard errors. Thus their standard errors are biased downwards if there are any cluster effects. It is also not possible to perform Donald & Lang (2007) inference, since the model is just-identified.[18] It is also clear that MVD study an interesting question, and one ultimately want to learn something from the reform in Kentucky. The study by MVD is therefore a good example where sensitivity analysis should be applied.

Let us start with the basic sensitivity analysis, applicable under the most restrictive assumptions, namely that the cluster-effects (group-time specific shocks) are uncorrelated between the groups as well as uncorrelated over time. The sensitivity analysis presented in Section 3 is then applicable. The γ_c values for 5% level (10-% in brackets) under these assumptions are reported in the lower panel of Table 3. I report cut-off values for both the difference estimates as well as the DID estimate.[19] The 5% level cut-off value for the DID estimate is 0.00067. The meaning of this estimate is that the variance of the group-time shocks is allowed to be 0.00067 times the variance of the unobserved

[18]The model includes four variables: a constant, a group dummy, a time dummy and a group time interaction.

[19]Notice that no cut-off values are reported for the control group since the difference for this group is already insignificant using the regular standard errors.

individual heterogeneity before the treatment effect is rendered insignificant. At first glance it may seem difficult to assess whether this is a unreasonably large value. Table 3 therefore also reports these values recalculated into cut-off standard deviations for the group-time shocks ($\sqrt{\gamma_c}\sigma_\varepsilon$). These cut-off values show that the standard deviation of the group-shocks is allowed to be 0.034 on 5% level (0.046 10% level). Column 1 and Column 3 show that the mean of the outcome log injury duration are 1.38 and 1.13 for the treatment group and the control group before the reform. Compared to these means the allowed standard deviation of the shocks is quite large. Furthermore, Column 6 show that the change in injury duration in the control group between the two time periods is 0.01. Even if it does not offer conclusive evidence, it suggests that the variance of the group-time shocks is small. Taken together it is therefore fair to say that conclusion from the study by MVD that the treatment effect is significant is not sensitive to departure from the independence (no-cluster) assumption, that is there is an effect on the injury duration.

Next consider an extended sensitivity analysis, which allows for correlation over time in the group-time shocks. In order to take this into account, replace the assumption of no autocorrelation in the cluster effects with an assumption of first order autocorrelation in these shocks. This gives two sensitivity parameters, γ and κ, measuring the size of the cluster effects and the correlation over time in these cluster effects. Since MVD work with repeated cross-section data the results in subsection 4.1 can be applied. The results from this exercise are presented in Figure 1, displaying cut-off values at 10% level for standard deviation of the group-specific time for a range of κ values. In this case with two time periods, a positive autocorrelation in the group-time shocks increases the cut-off values for γ. This extended sensitivity analysis therefore ultimately strengthening the conclusion that there is a statistical significant effect on the injury duration from an increase in disability benefits.

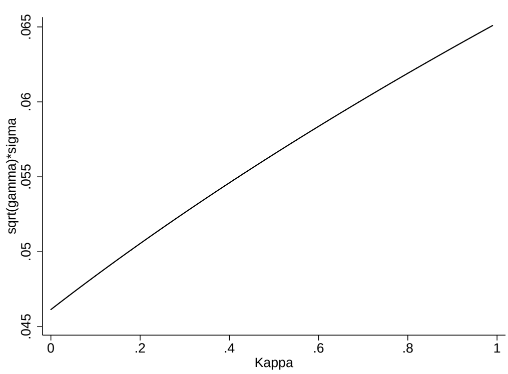

Figure 1: Two parameter sensitivity analysis for the DID estimates in Meyer et al. (1995). Autocorrelation in group-time shocks and allowed standard deviation of the group-time shocks.

6.2 Application 2: Earned income tax credit

Eissa & Liebman (1996) (EL) study the impact of an expansion of the Earned income tax credit (EITC) in the USA on the labor force participation of single women with children. EITC was introduced in 1975. Currently a taxpayer needs to meet three requirements in order to be eligible for the tax credit. The taxpayer need to have positive earned income, the gross income must be below a specified amount, and finally the taxpayer needs to have a qualifying child.[20] The amount of the credit is decided by the taxpayers earned income. The credit is phased in at a certain rate for low incomes, then stays constant within a certain income bracket, and is phased out at a certain rate for higher earnings. High earners are therefore not entitled to any EITC tax credit.

EL study the effects of the 1987 expansion of EITC in USA on labor supply. The reform changed EITC in several ways. The main changes were increases in the subsidy rate for the phase-in of the credit, an increase in the maximum income to which the subsidy rate is applied, and a reduction in the phaseout rate. This resulted in an increase in the maximum credit from \$550 to \$851, and made taxpayers with income between \$11,000 and \$15,432 eligible for the tax credit. All these changes made EITC more generous and the treatment consist of the whole change in the budget constraint. Obviously the reform only changes the incentives for those eligible for the tax credit. One key requirement is the presence of a qualifying child in the family. A natural treatment group is then single women with children, and a natural control group is single women without children. However, some single women with children are high income earners and thus are most likely to be unaffected by the EITC reform. EL therefore further divides the sample by education level. Here I report the results for all single women and single women with less than high-school education, from now on referred to as low educated.

EL use CPS data to estimate the treatment effect. Their outcome variable is an indicator variable taking the value one if the annual hours worked is positive. Similarly to MVD they use a DID approach, which contrast the differences between the post- and pre-reform period labor supply for the treatment and the control group. The main results from their analysis are presented in the upper panel of Table 4, taken from Table 2 in EL. The results from the DID analysis, presented in Column 7, suggest a positive and statistically significant effect of the EITC expansion in both specifications. If all single women are used, EL estimates that the expansion increased the labor force participation with 2.4 percentage points (4.1 percentage points for low educated single women).

The inference issues are very similar to those of the MVD study. In the presence of any group-time effects the standard errors presented by EL are biased downwards. EL have two DID models, which both are just-identified, making sensitivity analysis an attractive alternative. I first consider sensitivity analysis

[20] A qualifying child is defined as a child, grandchild, stepchild, or foster child of the taxpayer.

Table 4: *Sensitivity analysis estimates for application 2 on earned income tax credit*

	Treated (with children)		Non-Treated (without children)		Differences		DID
	Pre period	Post Period	Pre period	Post Period	[2-1]	[4-3]	[5-6]
Sample	[1]	[2]	3]	[4]	[5]	[6]	[7]
All	0.729	0.753	0.952	0.952	0.024	0.000	0.024
	(0.004)	(0.004)	(0.001)	(0.001)	(0.006)	(0.002)	(0.006)
Low education	0.479	0.497	0.784	0.761	0.018	-0.023	0.041
	(0.010)	(0.010)	(0.010)	(0.009)	(0.014)	(0.013)	(0.019)
Sample Size							
All	20,810		46,287				
Low education	5396		3958				
Sensitivity Analysis:							
γ_c - 5 % [10%]							
All					0.00030 [0.00048]	-	0.00022 [0.00034]
Low education					-	-	0.00005 [0.00031]
$\sqrt{\gamma_c} * \sigma_\varepsilon$: - 5 % [10%]							
All					0.0075 [0.0094]	-	0.0053 [0.0066]
Low education					-	-	0.0043 [0.0080]

Notes: The results in the upper panel are taken from Eissa & Liebman (1996), their standard errors in parentheses. The outcome is an indicator variable taking the value one is hours worked is positive, and zero otherwise. Two different samples, all single women and single women with less than high school. The sensitivity analysis results in the lower panel is own calculations. γ_c is calculated by numerically solving for γ in equation (16), for the specified significance level. The calculations are made under the assumption that the sample size is the same before and after the reform in the two groups.

under assumption of no autocorrelation in the group-time shocks, and then I allow for first order autocorrelation in these shocks. The results from the basic sensitivity analysis is presented in the lower panel of Table 4. The 5 percent, γ_c, cut-off value for the two DID estimates is 0.00022 for the full sample and 0.00005 for the sample of low educated mothers. It implies that the variance of the group-time shocks is allowed to be 0.0002 and 0.00005 times the variance of the unobserved individual heterogeneity. It further means that the standard deviation of the group-time shocks is allowed to be about 0.005 for the full sample and about 0.004 for the smaller sample of low educated mothers. In other words even very small shocks render the treatment effect insignificant. It can be compared with the mean labor force participation before the reform, which was 0.73 for all single women with children and 0.48 for low educated single mothers. Single women with children are after all a quite different group compared to single women without children. One can therefore expect quite large group-time specific shocks. Furthermore, there is a large drop of 0.023 in the labor force participation for the control group of low educated single women without children. I therefore conclude that conclusion in EL that the treatment effect is significant is sensitive to departure from the independence (no-cluster) assumption and its conclusion cannot therefore be trusted.

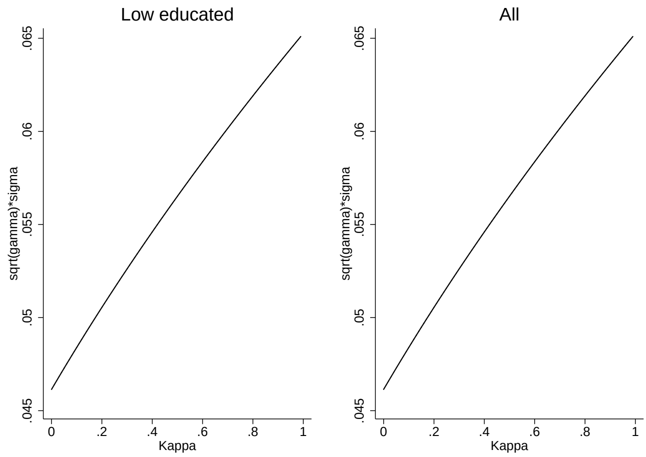

Figure 2: Two parameter sensitivity analysis for the DID estimates in Eissa & Liebman (1996). Left panel: the full sample of single women and right panel: the sample of low educated single women. Autocorrelation in group-time shocks and allowed standard deviation of the group-time shocks.

Next, consider allowing for first order autocorrelation in the group-time effects. As in the previous application I use the results in subsection 4.1 for repeated cross-section data. The cut-off standard deviation of the group shocks at 10% level is displayed for a range of κ values in Figure 2. The left graph display the cut-off values for the full sample and the right graph displays the cut-off values for the smaller sample of low educated mothers. Introducing autocorrelation in the two group two time period case increases the allowed variance of the group specific shocks. However, the variance is still only allowed to be very small before the estimates are rendered insignificant. I therefore conclude based on the estimates presented, that there is no conclusive evidence of any important labor supply effects from the EITC expansion in 1987.

7 Conclusions

In this paper I have derived and implemented a new method to perform inference when the analysis sample consists of observations from small number of groups. Consider for example having data for men and women, for two cities or for a couple of villages. The new method deals with the clustering problem, which is that the grouped structure of the data introduces correlation between the individual outcomes. The proposed sensitivity analysis approach is even able to handle just-identified models, including the often used two group two time period difference-in-differences setting. It therefore offers an alternative to the Donald & Lang (2007) inference. The method has many applications as the clustering problem, for instance, occurs in policy analyses rely which relies on variation at the group level to estimate the effect of a policy at the individual level, a setting used in many studies.

The key feature of the proposed sensitivity analysis approach is that all focus is placed on the size of the cluster effects, or simply the size of the within group correlation. Previously in the applied literature a lot of discussion concerned no within group correlation against non-zero correlation, since these two alternatives imply completely different ways to perform inference. This is a less fruitful discussion. In the end it is the size of the cluster effects that matters. In some cases it is simply not likely to believe that an estimated treatment effect is solely driven by random shocks, since it would require these shocks to have a very large variance. The sensitivity analysis formalizes this discussion by assessing how sensitive the standard errors are to within-group correlation.

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Appendix

Proof (*Equation 9.*)

Start with equation (8)

$$V = \frac{N-K}{N-tr[(X'X)^{-1}X'CX]}(X'X)^{-1}X'CX\mathbb{E}(\hat{V}).$$

First, consider $(X'X)^{-1}X'CX$. Under assumption of $x_{igt} = x_{gt}$ we have

$$X = \begin{bmatrix} x_1 \\ x_2 \\ \vdots \\ x_G \end{bmatrix} \qquad x_g = \begin{bmatrix} l_{g1}x'_{g1} \\ l_{g2}x'_{g2} \\ \vdots \\ l_{gT}x'_{gT} \end{bmatrix}$$

, where l_{gt} is a column vector of n_{gt} ones, G is the number of groups and T is the number of time periods. If $\mathbb{E}(c_{gt}c_{g't}) = 0$ for all t and all $g \neq g'$, and $\mathbb{E}(c_{gt}c_{gt'}) = 0$ for all g and all $t \neq t'$, we further have

$$C = \begin{bmatrix} C_1 & \dots & 0 \\ \vdots & \ddots & \vdots \\ 0 & \dots & C_G \end{bmatrix} C_g = \begin{bmatrix} C_{g1} & \dots & 0 \\ \vdots & \ddots & \vdots \\ 0 & \dots & C_{gT} \end{bmatrix}$$

with

$$C_{gt} = \begin{bmatrix} 1 & p & \dots & p \\ p & 1 & & \vdots \\ \vdots & & \ddots & p \\ p & \dots & p & 1 \end{bmatrix} = [(1-p)I_{gt} + pl_{gt}l'_{gt}]$$

Here I_{gt} is a unit matrix of order n_{gt}, and $p \equiv \frac{\sigma_c^2}{\sigma_c^2+\sigma_\varepsilon^2}$. It follows that

$$X'X = \sum_g \sum_t n_{gt}x_{gt}x'_{gt} \tag{A.1}$$

and

$$X'CX = \sum_g \sum_t x_{gt}l'_{gt}C_{gt}l_{gt}x'_{gt}. \tag{A.2}$$

and

$$x_{gt}l'_{gt}C_{gt}l_{gt}x'_{gt} = x_{gt}l'_{gt} \begin{bmatrix} 1+(n_{gt}-1)p \\ 1+(n_{gt}-1)p \\ \vdots \\ 1+(n_{gt}-1)p \end{bmatrix} x'_{gt} = x_{gt}n_{gt}[1+(n_{gt}-1)p]x'_{gt} \quad \text{(A.3)}$$

Combining equation (A.1), (A.2) and (A.3) gives

$$X'XX'CX = (\sum_g \sum_t n_{gt}x_{gt}x'_{gt})^{-1} \sum_g \sum_t n_{gt}\tau_{gt}x_{gt}x'_{gt} \quad \text{(A.4)}$$

with

$$\tau_{gt} = 1 + (n_{gt}-1)p.$$

Imposing $n_{gt} = n$ we have equation (A.4) as

$$X'XX'CX = \tau I_K \quad \text{(A.5)}$$

with

$$\tau = 1 + (n-1)p.$$

Next consider $\frac{N-K}{N-tr[(X'X)^{-1}X'CX]}$: using the result in equation (A.5) gives

$$tr[(X'X)^{-1}X'CX] = K\tau. \quad \text{(A.6)}$$

Substituting equation (A.5) and equation (A.6) into equation (8) and imposing $n_{gt} = n$ (then $N = nGT$) gives

$$V = \mathbb{E}(\hat{V})\tau \frac{nGT-K}{nGT-K\tau},$$

i.e. equation (9).

Proof (*Equation 18.*)

Again start with equation (8)

$$V = \frac{N-K}{N-tr[(X'X)^{-1}X'CX]}(X'X)^{-1}X'CX\mathbb{E}(\hat{V}).$$

First, consider $(X'X)^{-1}X'CX$. Remember that C is defined as

$$\mathbb{E}(ee') = \sigma^2 C,$$

where e is a vector collecting all $e_{igt} = c_{gt} + \varepsilon_{igt}$, and $\sigma^2 \equiv 1/Ntr(ee')$. In order to express C in terms of κ and γ use the well know properties of an AR(1) process (under the assumption of $|\kappa| < 1$), and the definition of $\sigma_d^2 \equiv \gamma\sigma_\varepsilon^2$ from section 4.1. This gives

$$\sigma_c^2 = \mathbb{E}(c_{gt}c_{gt}) = \frac{\sigma_d^2}{1-\kappa^2} = \frac{\gamma\sigma_\varepsilon^2}{1-\kappa^2} \tag{A.7}$$

and if $t \neq t'$

$$Cov(c_{gt}c_{gt'}) = \mathbb{E}(c_{gt}c_{gt'}) = \kappa^{|t-t'|}\frac{\sigma_d^2}{1-\kappa^2} = \kappa^{|t-t'|}\frac{\gamma\sigma_\varepsilon^2}{1-\kappa^2}. \tag{A.8}$$

Thus if $i = j$

$$\mathbb{E}(e_{igt}e_{jgt}) = \sigma^2 = \sigma_c^2 + \sigma_\varepsilon^2 = \frac{\gamma\sigma_\varepsilon^2}{1-\kappa^2} + \sigma_\varepsilon^2 = \sigma_\varepsilon^2\frac{1+\gamma-\kappa^2}{1-\kappa^2}. \tag{A.9}$$

Further, using (A.7) and (A.9), if $i \neq j$

$$\mathbb{E}(e_{igt}e_{jgt}) = \sigma_c^2 = \frac{\gamma\sigma_\varepsilon^2}{1-\kappa^2} = \sigma^2\frac{\gamma}{1+\gamma-\kappa^2} \tag{A.10}$$

and using (A.8) and (A.9), if $t \neq t'$

$$\mathbb{E}(e_{igt}e_{jgt'}) = \kappa^{|t-t'|}\frac{\sigma_d^2}{1-\kappa^2} = \sigma^2\kappa^{|t-t'|}\frac{\gamma}{1+\gamma-\kappa^2}. \tag{A.11}$$

and under assumption $\mathbb{E}(c_{gt}c_{gt'}) = 0$, if $g \neq g'$

$$\mathbb{E}(e_{igt}e_{jg't'}) = 0 \tag{A.12}$$

Then under (A.9), (A.10), (A.11) and (A.12)

$$C = \begin{bmatrix} C_1 & \dots & 0 \\ \vdots & \ddots & \vdots \\ 0 & \dots & C_G \end{bmatrix} C_g = \begin{bmatrix} C_{11} & \dots & C_{T1} \\ \vdots & \ddots & \vdots \\ C_{1T} & \dots & C_{TT} \end{bmatrix} \tag{A.13}$$

with if $t = t'$

$$C_{tt'} = [(1 - \frac{\gamma}{1+\gamma-\kappa^2})I_{gt} + \frac{\gamma}{1+\gamma-\kappa^2}l_{gt}l'_{gt}] \tag{A.14}$$

and if $t \neq t'$

$$C_{tt'} = \kappa^{|t-t'|}\frac{\gamma}{1+\gamma-\kappa^2}l_{gt}l'_{gs}. \tag{A.15}$$

Define $p_c = \frac{\gamma}{1+\gamma-\kappa^2}$. Then, using equation (A.14), if $t = t'$

$$x_{gt} l'_{gt} C_{tt} l_{gt} x'_{gt} = x_{gt} l'_{gt} \begin{bmatrix} 1+(n_{gt}-1)p_c \\ 1+(n_{gt}-1)p_c \\ \vdots \\ 1+(n_{gt}-1)p_c \end{bmatrix} x'_{gt} = x_{gt} n_{gt} [1+(n_{gt}-1)p_c] x'_{gt}, \tag{A.16}$$

and, using equation (A.15), if $t \neq t'$

$$x_{gt} l'_{gt} C_{tt'} l_{gt'} x'_{gt'} = \kappa^{|t-t'|} x_{gt} l'_{gt} \begin{bmatrix} n_{gt'} p_c \\ n_{gt'} p_c \\ \vdots \\ n_{gt'} p_c \end{bmatrix} x'_{gt'} = \kappa^{|t-t'|} x_{gt} n_{gt} p_c n_{gt'} x'_{gt'} \tag{A.17}$$

Using equation (A.1), (A.16) and (A.17) gives

$$X'XX'CX = (\sum_g \sum_t n_{gt} x_{gt} x'_{gt})^{-1} \tag{A.18}$$

$$(\sum_g \sum_t \sum_{t' \neq t} (\kappa^{|t-t'|} n_{gt} n_{gt'} x_{gt} x'_{gt'}) + n_{gt}[1+(n_{gt}-1)p_c] x_{gt} x'_{gt})$$

Imposing $n_{gt} = n$, substituting for $p_c = \frac{\gamma}{1+\gamma-\kappa^2}$ and simplifying we have equation (A.18) as

$$X'XX'CX = (1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2})I_K + n\frac{\gamma}{1+\gamma-\kappa^2}H \tag{A.19}$$

with

$$H = (\sum_g \sum_t x_{gt} x'_{gt})^{-1} \sum_g \sum_t \sum_{t' \neq t} (\kappa^{|t-t'|} x_{gt} x'_{gt'}).$$

Next consider $\frac{N-K}{N-tr[(X'X)^{-1}X'CX]}$: using the results in (A.19) gives

$$tr[(X'X)^{-1}X'CX] = K(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2}) + n\frac{\gamma}{1+\gamma-\kappa^2}\sum_{a=1}^{K} H_{aa}. \tag{A.20}$$

where H_{aa} is the element in the ath column and ath row of H.

Substituting equation (A.19) and equation (A.20) into equation (8) and noting that under $n_{gt} = n$ we have $N = nGT$ gives

$$V_{aa} = \mathbb{E}(\hat{V}_{aa}) \frac{nGT-K}{nGT-K(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2}) + n\frac{\gamma}{1+\gamma-\kappa^2}\sum_{a=1}^{K} H_{aa}}$$

$$(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2})+n\frac{\gamma}{1+\gamma-\kappa^2}H_{aa}$$

Both the first and the second part of this expression, the two sources of bias in the standard errors are greater than one. However, it will be highly dominated by $(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2})I_K+n\frac{\gamma}{1+\gamma-\kappa^2}H_{aa}$. Thus we have

$$V_{aa}\approx\mathbb{E}(\hat{V}_{aa})(1+(n-1)\frac{\gamma}{1+\gamma-\kappa^2})+n\frac{\gamma}{1+\gamma-\kappa^2}H_{aa}.$$

i.e. equation (18).

Proof (*Equation 22.*)

Again start with equation (8)

$$V=\frac{N-K}{N-tr[(X'X)^{-1}X'CX]}(X'X)^{-1}X'CX\mathbb{E}(\hat{V}).$$

Using the definition $\sigma_c^2\equiv\gamma\sigma_\varepsilon^2$ from section 4.1, if $i=j$ we have

$$\mathbb{E}(e_{igs}e_{jgs})=\sigma^2=\sigma_c^2+\sigma_\varepsilon^2=\sigma_\varepsilon^2(1+\gamma). \tag{A.21}$$

Using this and under assumption $\mathbb{E}(c_{gt}c_{g't})=0$ for all t, and the multiway clustering assumptions if $s\neq s'$ then $E(c_{gs}c_{g's'})=0$ and if $s=s'$ then $E(c_{gs}c_{g's'})=\xi\sigma_c^2$, it gives if $i\neq j$

$$\mathbb{E}(e_{igs}e_{jgs})=\sigma_c^2=\gamma\sigma_\varepsilon^2=\sigma^2\frac{\gamma}{1+\gamma} \tag{A.22}$$

and if $i\neq j$ and $g=g'$ holds

$$\mathbb{E}(e_{igs}e_{jg's})=\xi\sigma_c^2=\xi\gamma\sigma_\varepsilon^2=\sigma^2\xi\frac{\gamma}{1+\gamma} \tag{A.23}$$

and if $s\neq s'$

$$\mathbb{E}(e_{igs}e_{jg's})=0. \tag{A.24}$$

Thus under (A.21), (A.22), (A.23) and (A.24)

$$C=\begin{bmatrix}C_1&\dots&0\\\vdots&\ddots&\vdots\\0&\dots&C_S\end{bmatrix}C_s=\begin{bmatrix}C_{11}&\dots&C_{G_s1}\\\vdots&\ddots&\vdots\\C_{1G_s}&\dots&C_{G_sG_s}\end{bmatrix} \tag{A.25}$$

with if $g=g'$

$$C_{gg'}=[(1-\frac{\gamma}{1+\gamma})I_g+\frac{\gamma}{1+\gamma}l_gl_s] \tag{A.26}$$

and if $g \neq g'$

$$C_{gg'} = \xi \frac{\gamma}{1+\gamma} l_g l_g'. \tag{A.27}$$

Here G_s is the number of groups belonging to group-cluster s. Retain the definition $p = \frac{\gamma}{1+\gamma}$, and define l_{gs} as a column vector of n_{gs} ones. Then, using equation (A.26), if $g = g'$

$$x_{gs} l_{gs}' C_{gg} l_{gs} x_{gs}' = x_{gs} l_{gs}' \begin{bmatrix} 1+(n_{gs}-1)p \\ 1+(n_{gs}-1)p \\ \vdots \\ 1+(n_{gs}-1)p \end{bmatrix} x_{gs}' = x_{gs} n_{gs} [1+(1-n_{gs})p] x_{gs}', \tag{A.28}$$

and, using equation (A.27), if $g \neq g'$

$$x_{gs} l_{gs}' C_{gg'} l_{g's} x_{g's}' = \xi x_{gs} l_{gs}' \begin{bmatrix} n_{g's} p \\ n_{g's} p \\ \vdots \\ n_{g's} p \end{bmatrix} x_{g's}' = \xi x_{gt} n_{gs} p_c n_{g's} x_{g's}' \tag{A.29}$$

Using equation (A.1), (A.28) and (A.29) gives

$$X'XX'CX = (\sum_s \sum_g n_{gs} x_{gs} x_{gs}')^{-1} \tag{A.30}$$

$$(\sum_s \sum_g \sum_{g' \neq g} (\xi n_{gs} n_{g's} x_{gs} x_{g's}') + n_{gs}[1+(1-n_{gs})p] x_{gs} x_{gs}')$$

Imposing $n_{gs} = n$, substituting for $p = \frac{\gamma}{1+\gamma}$ and simplifying we have equation (A.30) as

$$X'XX'CX = (1+(n-1)\frac{\gamma}{1+\gamma}) I_K + n \frac{\gamma}{1+\gamma} \xi M, \tag{A.31}$$

with

$$M = (\sum_s \sum_g x_{gs} x_{gs}')^{-1} \sum_s \sum_g \sum_{g' \neq g} (x_{gs} x_{g's}').$$

Next consider $\frac{N-K}{N-tr[(X'X)^{-1}X'CX]}$, using the results in (A.31) gives

$$tr[(X'X)^{-1}X'CX] = K(1+(n-1)\frac{\gamma}{1+\gamma}) + n \frac{\gamma}{1+\gamma} \xi \sum_{a=1}^{K} M_{aa}. \tag{A.32}$$

where M_{aa} is the element in the ath column and ath row of M.

Substituting equation (A.31) and equation (A.32) into equation (8) and noting that under $n_{gt} = n$ we have $N = nGT$ gives

$$V_{aa} = \mathbb{E}(\hat{V}_{aa}) \frac{nGT - K}{nGT - K(1 + (n-1)\frac{\gamma}{1+\gamma}) + n\frac{\gamma}{1+\gamma}\xi \sum_{a=1}^{K} M_{aa}}$$

$$(1 + (n-1)\frac{\gamma}{1+\gamma} + n\frac{\gamma}{1+\gamma}\xi M_{aa}).$$

Both the first and the second part of this expression, the two sources of bias in the standard errors, are greater than one. However, it will be highly dominated by $(1 + (n-1)\frac{\gamma}{1+\gamma} + n\frac{\gamma}{1+\gamma}\xi M_{aa})$. Thus we have

$$V_{aa} \approx \hat{V}_{aa}(1 + (n-1)\frac{\gamma}{1+\gamma} + n\frac{\gamma}{1+\gamma}\xi M_{aa})$$

i.e equation (22).

Essay 5: Bounds on Treatment Effects on Transitions[1]

1 Introduction

We consider the effect of an intervention where the outcome is a transition from an initial to a destination state. The population of interest is a cohort of units that are in the initial state at the time origin. Treatment is assigned to a subset of the population either at the time origin or at some later time. Initially we assume that the treatment assignment is random. One main point of this paper is that even if the treatment assignment is random, only certain average effects of the treatment are point identified. This is because the random assignment of treatment only ensures comparability of the treatment and control groups at the time of randomization. At later times treated units with characteristics that interact with the treatment to increase/decrease the transition probability leave the initial state first/last, so that these characteristics are under/over represented among the remaining treated relative to the remaining controls and this confounds the effect of the treatment.

The confounding of the treatment effect by selective dropout is usually referred to as dynamic selection. Existing strategies that deal with dynamic selection rely heavily on parametric and semi-parametric models. An example is the approach of Abbring & van den Berg (2003) who use the Mixed Proportional Hazard (MPH) model (their analysis is generalized to a multistate model in Abbring, 2008) . In this model the instantaneous transition or hazard rate is written as the product of a time effect, the baseline hazard, the effect of the intervention and an unobservable individual effect. As shown by Elbers & Ridder (1982) is the MPH model nonparametrically identified, so that if the multiplicative structure is maintained, identification does not rely on arbitrary functional form or distributional assumptions. A second example is the approach of Heckman & Navarro (2007) who start from a threshold crossing model for transition probabilities. Again they establish semi-parametric identification, although their model requires the presence of additional covariates besides the treatment indicator that are independent of unobservable errors and have large support. The identified model is used to undo the confounding due to dynamic selection.

In this paper we ask what can be identified if the identifying assumptions of the semi-parametric models do not hold. We show that even under random as-

[1]Co-authored with Geert Ridder, Department of Economics, University of Southern California.

signment we cannot point identify many average treatment effects of interest, because of dynamic selection. However, we derive sharp bounds on various treatment effects, and show when these bounds are informative. These bounds apply e.g. if random assignment occurs at the time origin, but we want to learn the effect of the treatment on the transition probability after a number of periods, i.e. we are interested in the treatment effect dynamics. Our bounds are general, since beyond random assignment, we make no assumptions on functional form and additional covariates, and we allow for arbitrary heterogenous treatment effects as well as arbitrary unobserved heterogeneity. These bounds could be extended to unconfounded treatment assignment by creating bounds conditional on the covariates (or the propensity score) and then average over the distribution of these covariates. Besides these general bounds we show that additional weak assumptions like monotone treatment response and monotone exit rate may tighten the bounds considerably.

There are many applications in which we are interested in the effect of an intervention on transition probabilities/rates. The Cox (1972) partial likelihood estimator is routinely used to estimate the effect of an intervention on the survival rate of subjects. Transition models are used in several fields. Van den Berg (2001) surveys the models used and their applications. These models also have been used to study the effect of interventions on transitions. Examples are Ridder (1986), Card & Sullivan (1988), Bonnal et al. (1997) ,Gritz (1993), Ham & LaLonde (1996), Abbring & van den Berg (2003), and Heckman & Navarro (2007). A survey of models for dynamic treatment effects can be found in Abbring & Heckman (2007).

An alternative to the effect of a treatment on the transition rate is to consider its effect on the cdf of the time to transition or its inverse, the quantile function. This avoids the problem of dynamic selection. Fredriksson & Johansson (2008) have shown how the effect on the cdf, that is the unconditional survival probability, can be recovered even if the time-varying interventions can start at any time. From the effect on the cdf we can recover the effect on the average duration. From the effect on the cdf we cannot obtain the effect on the conditional transitions probabilities, so that this effect is not informative on the evolution of the treatment effect over time. There are good reasons why we would be interested in the effect of an intervention on the conditional transition probability or hazard rate. First, there is the close link between the hazard rate and economic theory (Van den Berg, 2001). Economic theory often predicts how the hazard rate changes over time. For example, in the application to a job bonus experiment considered in this paper labor supply and search models predict that being eligible for a bonus if a job is found, increases the hazard rate from unemployment to employment. According to these models the positive effect only exists during the eligibility period, and the effect increases shortly before the end of the eligibility period. The timing of this increase depends on the arrival rate of job offers and is an indication of the control that the unemployed has over his/her reemployment time. Any such control has important policy implications. These hypotheses can only be tested by considering how the effect on the hazard rate changes over time.

Other examples of when the evolution of the treatment effect over time is of key interest arise in different fields. For instance, two medical treatments can have the same effect on the average survival time. However, for one treatment the effect does not change over time while for the other the survival rate is initially low, e.g. due to side effects of the treatment, while after that initial period the survival rate is much higher. Research on the effects off active labor market policies (ALMP), often documents a large negative lock-in effect and a later positive effect once the program has been completed, see e.g. the survey by Kluve et al. (2007). In other cases a treatment consist of a sequence of sub-treatments assigned at pre-specified points in time to the survivors in the state. If one is interested in disentangling the sub-treatment effects, the treatment effect over the spell has to be investigated.

In section 2 we define the treatment effects that are relevant if the outcome is a transition. Section 3 discusses their point or set identification in the case that the treatment is randomly assigned. This requires us to be precise on what we mean by random assignment in this setting. In section 4 we explore additional assumptions that tighten the bounds. Section 5 illustrates the bounds for a job bonus experiment data set. Section 6 concludes.

2 Treatment effects if the outcome is a transition

2.1 Parametric outcome models

To set the stage for the definition of a treatment effect for an outcome that is a transition, we consider the effect of an intervention in the Mixed Proportional Hazards (MPH) model. The MPH model specifies the individual hazard or transition rate $\theta(t,d(t),V)$

$$\theta(t,d(t),V) = \lambda(t)\gamma(t-\tau,\tau)^{d(t)}V$$

with t as the time spent in the destination state, $\lambda(t)$, the baseline hazard, $d(t)$, the treatment indicator function in period t, and V, a scalar nonnegative unobservable that captures population heterogeneity in the hazard/transition rate and has a population distribution with mean 1. If treatment starts at time τ then $d(t) = I(t > \tau)$, i.e. we assume that treatment is an absorbing state. The nonnegative function $\gamma(t-\tau,\tau)$ captures the effect of the intervention, an effect that depends on the time until the treatment starts τ and the time treated $t-\tau$. Finally, although γ is common to all units, the effect of the intervention differs between the units, because it is proportional to the individual V. The ratio of the treated and non-treated transition rates for a unit with unobservable V is $\gamma(t-\tau,\tau)$ for $t > \tau$, so that in the MPH model $\gamma(t-\tau,\tau)$ is the effect of the intervention on the individual transition rate.

Let $\overline{d}(t) = \{d(s), 0 \leq s \leq t\}$ be the treatment status up to time t. The MPH model implies that the population distribution of the time to transition $T^{\overline{d}(T)}$ has density

$$f(t|\overline{d}(t)) = \mathbb{E}_V \left[V\lambda(t)\gamma(t-\tau,\tau)^{d(t)} e^{-\int_0^t \lambda(s)\gamma(s-\tau,\tau)^{d(s)} V \mathrm{d}s} \right]$$

and distribution function

$$F(t|\overline{d}(t)) = 1 - \mathbb{E}_V \left[e^{-\int_0^t \lambda(s)\gamma(s-\tau,\tau)^{d(s)} V \mathrm{d}s} \right]$$

The hazard/transition rate given the treatment history is

$$\theta(t|\overline{d}(t)) = \lambda(t)\gamma(t-\tau,\tau)^{d(t)} \mathbb{E}_V \left[V | T^{\overline{d}(T)} \geq t \right].$$

To define treatment effects in the MPH model we can compare units with different treatment histories $\overline{d}(t)$. Let $\overline{d}_0(t)$ and $\overline{d}_1(t)$ be two such histories. Then we can compare either the time-to-transition distribution functions in t, i.e. $F(t|\overline{d}_0(t))$ and $F(t|\overline{d}_1(t))$, or the transition rates in t, i.e. $\theta(t|\overline{d}_0(t))$ and $\theta(t|\overline{d}_1(t))$. The comparison of the transition rates is conditional on survival in the initial state up to time t and the comparison of the distribution functions is not conditional on survival. As a consequence if we compare distribution functions we average over the population distribution of V, but if we compare transition rates we average over the distribution of V for the subpopulation of survivors up to time t.

Let us take $\overline{d}_0(t) = 0$, i.e. the unit is in the control group during $[0,t]$, and $\overline{d}_1(t)$ arbitrary, then $F(t|\overline{d}_1(t)) > F(t|\overline{d}_0(t))$ if and only if

$$\frac{1}{\int_\tau^t \lambda(s)\mathrm{d}s} \int_\tau^t \lambda(s)\gamma(s-\tau,\tau)\mathrm{d}s > 1 \tag{1}$$

holds, i.e. if a λ weighted average of the effect on the individual transition rate is greater than 1. Note that the comparison of the distribution functions is not confounded by the unobservable V. However, if we compare the transition rates in $t > \tau$

$$\theta(t|\overline{d}_0(t)) = \lambda(t)\mathbb{E}_V \left[V | T^{\overline{d}_0(T)} \geq t \right]$$

and

$$\theta(t|\overline{d}_1(t)) = \lambda(t)\gamma(t-\tau,\tau)\mathbb{E}_V \left[V | T^{\overline{d}_1(T)} \geq t \right]$$

then because

$$\mathbb{E}_V \left[V | T^{\overline{d}_0(T)} \geq t \right] > \mathbb{E}_V \left[V | T^{\overline{d}_1(T)} \geq t \right]$$

if and only if (1) holds, we have that under that condition

$$\frac{\theta(t|\overline{d}_1(t))}{\theta(t|\overline{d}_0(t))} < \gamma(t-\tau,\tau).$$

Therefore if the intervention increases the transition rate on average (as in (1), then the ratio of the population treated and control transition rates is strictly smaller than that of the individual treated and control transition rates. If the intervention decreases the transition rate on average, then the population transition rate is strictly larger than the individual rate. Hence, the effect of the intervention on the transition rate is confounded by its differential effect on the distribution of the unobservable among the treated and controls. The intuition behind this result is that the difference of the treated and control transition rates is monotonic in V, so that if (1) holds, treated units with a large value of V are under-represented among the survivors in the initial state, while control units with a small value of V are over-represented among these survivors. This dynamic selection or survivor bias is not just a feature of the MPH model. It is present in any population where the treatment and the individual characteristics interact to increase or decrease the transition probability.

Parametric and semi-parametric models for the transition rate indicate how to correct for the survivor bias in the average treatment effect. If we choose a distribution for V or estimate the distribution as in Heckman & Singer (1984), we can estimate $\mathbb{E}_V\left[V|T^{\overline{d}_0(T)} \geq t\right]$ and $\mathbb{E}_V\left[V|T^{\overline{d}_1(T)} \geq t\right]$ to obtain the correction factor. Because the MPH model is nonparametrically identified this does not depend on untestable distributional assumptions. Of course it requires that the assumption that the hazard is multiplicative in the baseline hazard, the homogenous treatment effect and the spell constant unobserved effect V is maintained. Without these assumptions the correction factor cannot be estimated without additional distributional assumptions.

2.2 Average treatment effects on transitions

In any definition of the causal effect of the treatment on the transition rate we must account for the dynamic selection or survivor bias. If we do not specify a model for the transition rate we need to find another way to make this adjustment. The approach that we take in this paper is to consider average transition rates where the average is taken in the same population for different treatment arms. The MPH model is most often normalized so that the mean of V equals 1. When considering average transition rates one usually average over this population where the mean of V is 1 even in later periods where due to dynamic selection the mean of V is no longer 1 and depends on the treatment arm. The treatment effect identified by the MPH model therefore takes an average over a hypothetical population that at times later than the time origin partly consists of individuals who already left the state of interest and that hypothetical population is the same for every treatment arm. The latter is key in interpreting the effect as causal: by averaging over the same (hypothetical) population we have removed the survivor bias.

In this paper we do not average over the population at the time 0. Instead to define the average effect of the treatment on the transition rate at time t we average over the (hypothetical) population of individuals who would have survived until time t under both treatment arms. The individuals in this population have the same survival experience and any difference between the transition rates must be due to the effect of the treatment. The average is taken over a population that remains in the state of interest. Although we could discuss the definition and identification of treatment effects on transition rates in continuous time the case that time is discrete is conceptually simpler and from now on we assume that transitions occur at times $t = 1, 2, \ldots$.

As before we denote the treatment indicator in period t by d_t and the treatment history up to and including period t by $\overline{d}_t$. Let the potential outcome $Y_t^{\overline{d}_t}$ be an indicator of a transition in period t if the treatment history up to and including t is $\overline{d}_t$. If treatment is an absorbing state, $\overline{d}_t$ is a sequence of 0-s until treatment starts in period τ and the remaining values are 1. It is possible that $\tau = \infty$, the unit is never treated, or $\tau = 1$, the unit is always in the treated state.

As emphasized we are interested in conditional treatment effects, i.e. treatment effects defined for the survivors in t. Let $\overline{d}_{0t}$ and $\overline{d}_{1t}$ be two specific treatment histories. If we average over the hypothetical subpopulation of individuals who would have survived until t under both $\overline{d}_{0t}$ and $\overline{d}_{1t}$, then we define the causal effect of the intervention on the conditional transition rate as

$$\text{ATES}_t^{\overline{d}_{1t},\overline{d}_{0t}} =$$

$$\mathbb{E}\left[Y_t^{\overline{d}_{1t}} | Y_{t-1}^{\overline{d}_{1t-1}} = 0, \ldots, Y_1^{\overline{d}_{11}} = 0, Y_{t-1}^{\overline{d}_{0t-1}} = 0, \ldots, Y_1^{\overline{d}_{01}} = 0\right] -$$

$$\mathbb{E}\left[Y_t^{\overline{d}_{0t}} | Y_{t-1}^{\overline{d}_{1t-1}} = 0, \ldots, Y_1^{\overline{d}_{11}} = 0, Y_{t-1}^{\overline{d}_{0t-1}} = 0, \ldots, Y_1^{\overline{d}_{01}} = 0\right]$$

We call this treatment effect the *Average Treatment Effect on the Survivors* in t (ATES_t). Obvious choices for $\overline{d}_{1t}$ and $\overline{d}_{0t}$ are $\overline{d}_{1t} = (0, \ldots, 0, 1, \ldots, 1)$ with the first 1 at position τ, and $\overline{d}_{0t} = (0, \ldots, 0)$. If we make the usual assumption that there is no effect of the treatment before it starts[2], then $\text{ATES}_t = 0, t = 1, \ldots, \tau - 1$. The differential selection only starts after the treatment begins, so that this property of the ATES_t is consistent with that fact. After the treatment starts there will be dynamic selection and the ATES_t controls for that by comparing the transition rates for individuals with a common (hypothetical) survival experience. Because individuals cannot be observed in both treatment arms, we cannot hope that this treatment effect can be identified using available data.

[2]Abbring & van den Berg (2003) call this the no-anticipation assumption.

3 Identification of treatment effects on transitions under random assignment

We now consider identification of the ATES_t under random treatment assignment. Random assignment of treatment is the most favorable assignment mechanism. However, we need to define what we mean by random assignment in this case. Let D_t be the indicator that treatment is assigned in period t, i.e. the unit is not treated in periods $1,\ldots,t-1$, selected for treatment in period t and, because treatments is assumed to be an absorbing state, remains in the treated state in the subsequent periods. We assume that the treatment is assigned at the beginning of the period, so that the treated responses are observed in periods $t, t+1, \ldots$. We distinguish between three types of randomized assignment

Assumption 1 (Random assignment of the time of treatment) *For all t and $\overline{d}_s, s = 1,2,\ldots$*

$$D_t \perp Y_s^{\overline{d}_s} \quad s = 1,2,\ldots$$

Assumption 2 (Sequential randomization) *For all t and $\overline{d}_s, s = t, t+1, \ldots$ with the first $t-1$ components equal to 0*

$$D_t \perp Y_s^{\overline{d}_s} \quad s = t, t+1, \ldots | D_{t-1} = 0$$

Assumption 3 (Sequential randomization among survivors) *For all t and $\overline{d}_s, s = t, t+1, \ldots$ with the first $t-1$ components equal to 0*

$$D_t \perp Y_s^{\overline{d}_s} \quad s = t, t+1, \ldots | D_{t-1} = 0, Y_{t-1}^0 = \cdots = Y_1^0 = 0.$$

Under assumption 1, the period in which the unit enters the treated state is randomly assigned. Under assumption 2, treatment is assigned randomly in period t to units that have not been treated before, and under assumption, 3 the randomization is among the non-treated survivors. Random assignment of the time of treatment implies sequential randomization, which implies sequential randomization among survivors. In this paper, we focus on identification of average treatment effects under assumption 3.

In the remainder of this paper, we consider the two period case where the transition occurs in period 1, period 2 or after period 2. The reason for this is that all the main points of this paper can be illustrated in that simplified setting. For every member of the population we have a vector of potential outcomes $Y_1^1, Y_1^0, Y_2^{11}, Y_2^{01}, Y_2^{00}$, and vector of treatment indicators D_1, D_2. Let Y_t be the observed indicator of a transition in period t. These observed outcomes Y_1, Y_2 are related to the potential outcomes by the observation rules

$$Y_1 = D_1 Y_1^1 + (1 - D_1) Y_1^0 \tag{2}$$

and

$$Y_2 = D_1 Y_2^{11} + (1 - D_1) D_2 Y_2^{01} + (1 - D_1)(1 - D_2) Y_2^{00}. \tag{3}$$

Because treatment is an absorbing state

$$D_1 = 1 \Rightarrow D_2 = 1.$$

Assumption 3 is in this case

$$D_1 \perp Y_1^1, Y_1^0, Y_2^{11}, Y_2^{01}, Y_2^{00}$$

and

$$D_2 \perp Y_2^{11}, Y_2^{01}, Y_2^{00} | D_1 = 0, Y_1^0 = 0.$$

Hence, under assumption 3 and using the observation rules we can identify from the observed transitions rates the following potential transition probabilities

$$\mathbb{E}(Y_1|D_1 = 1) = \mathbb{E}(Y_1^1|D_1 = 1) = \mathbb{E}(Y_1^1) \tag{4}$$

$$\mathbb{E}(Y_1|D_1 = 0) = \mathbb{E}(Y_1^0|D_1 = 0) = \mathbb{E}(Y_1^0) \tag{5}$$

$$\mathbb{E}(Y_2|Y_1 = 0, D_1 = 1) = \mathbb{E}(Y_2^{11}|Y_1^1 = 0, D_1 = 1) = \mathbb{E}(Y_2^{11}|Y_1^1 = 0) \tag{6}$$

$$\mathbb{E}(Y_2|Y_1 = 0, D_1 = 0, D_2 = 0) = \mathbb{E}(Y_2^{00}|Y_1^0 = 0, D_1 = 0, D_2 = 0) = \tag{7}$$

$$\mathbb{E}(Y_2^{00}|Y_1^0 = 0)$$

$$\mathbb{E}(Y_2|Y_1 = 0, D_1 = 0, D_2 = 1) = \mathbb{E}(Y_2^{01}|Y_1^0 = 0, D_1 = 0, D_2 = 1) = \tag{8}$$

$$\mathbb{E}(Y_2^{01}|Y_1^0 = 0).$$

3.1 Identification of instantaneous treatment effects

The ATES_t defines a number of interesting treatment effects that could be divided into two groups: instantaneous treatment effects and dynamic treatment effects. In the two period setting the two instantaneous treatment effects are

$$\text{ATES}_1^{1,0} = \mathbb{E}(Y_1^1) - \mathbb{E}(Y_1^0)$$

and

$$\text{ATES}_2^{01,00} = \mathbb{E}(Y_2^{01}|Y_1^0 = 0) - \mathbb{E}(Y_2^{00}|Y_1^0 = 0).$$

That is the average instantaneous treatment effect from treatment in the first period, and the average instantaneous treatment effect from treatment in the second period for those who survives the first period. Note that for $\text{ATES}_2^{01,00}$ the treatment in the first period is no treatment in both treatment arms, so that we only need to condition on surviving the first period under no treatment.

From equations (4) and (5) it follow that under assumption 3 we can point identify the instantaneous treatment effect

$$\mathrm{ATES}_1^{1,0} = \mathrm{ATE}_1^{1,0} = \mathbb{E}(Y_1^1) - \mathbb{E}(Y_1^0) = \mathbb{E}(Y_1|D_1 = 1) - \mathbb{E}(Y_1|D_1 = 0), \quad (9)$$

and from equations (7) and (8) we have

$$\mathrm{ATES}_2^{01,00} = \mathbb{E}(Y_2^{01}|Y_1^0 = 0) - \mathbb{E}(Y_2^{00}|Y_1^0 = 0) = \quad (10)$$

$$\mathbb{E}(Y_2|Y_1 = 0, D_1 = 0, D_2 = 1) - \mathbb{E}(Y_1|Y_1 = 0, D_1 = 0, D_2 = 0).$$

3.2 Bounds on dynamic treatment effects on transitions

In the two period setting the dynamic treatment effect of interest is

$$\mathrm{ATES}_2^{11,00} = \mathbb{E}(Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0) - \mathbb{E}(Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0),$$

that is the average treatment effect in the second period from treatment in the first period for those who survive under both treatment and no treatment in the first period. It follows directly from equations (4)-(8), which hold under assumption 3, that $\mathrm{ATES}_2^{11,00}$ in general is not point identified. This is because the random assignment of treatment only ensures comparability of the treatment and control groups at the time of randomization. At later times treated units with characteristics that interact with the treatment to increase/decrease the transition probability leave the initial state first/last, so that these characteristics are under/over represented among the remaining treated relative to the remaining controls and this confounds the effect of the treatment. Without out any further assumption we cannot uncover this dynamic selection and point identify the average dynamic treatment effect.

It is, however, clear that the observed transitions rates place restrictions on the potential transition probabilities. We therefore turn to the second main point of this paper and derive sharp bounds on $\mathrm{ATES}_2^{11,00}$. Sharp bounds in the sense that there exists a feasible joint distribution of the potential outcomes which is consistent with both the upper bound and the lower bound. The sharp bounds are derived by considering the joint distribution of the potential outcomes. The upper (lower) bound is found by constructing a joint distribution of the potential outcomes which, given the restrictions from the observed quantities, maximize (minimize) $\mathrm{ATES}_2^{11,00}$.

In order to simplify the derivations define

$$\begin{aligned}
p(y_1^1, y_1^0) &= \Pr(Y_1^1 = y_1^1, Y_1^0 = y_1^0) \\
p(y_2^{01}, y_2^{00}|1,0) &= \Pr(Y_2^{01} = y_2^{01}, Y_2^{00} = y_2^{00}|Y_1^1 = 1, Y_1^0 = 0) \\
p(y_2^{11}|0,1) &= \Pr(Y_2^{11} = y_2^{11}|Y_1^1 = 0, Y_1^0 = 1) \\
p(y_2^{11}, y_2^{01}, y_2^{00}|0,0) &= \Pr(Y_2^{11} = y_2^{11}, Y_2^{01} = y_2^{01}, Y_2^{00} = y_2^{00}|Y_1^1 = 0, Y_1^0 = 0)
\end{aligned}$$

We consider an absorbing state, so that Y_2^{10} is not defined. In addition as discussed above if $Y_1^1 = 1$ Y_2^{11} is not defined, and if $Y_1^0 = 1$, neither Y_2^{01} nor Y_2^{00} is defined. The parameters of the joint distribution of the potential outcomes are then

$$\begin{array}{rl} p(y_1^1, y_1^0) & y_1^1, y_1^0 = 0,1 \\ p(y_2^{01}, y_2^{00}|1,0) & y_2^{01}, y_2^{00} = 0,1 \\ p(y_2^{11}|0,1) & y_2^{11} = 0,1 \\ p(y_2^{11}, y_2^{01}, y_2^{00}|0,0) & y_2^{11}, y_2^{01}, y_2^{00} = 0,1. \end{array}$$

We consider bounds on

$$ATES_2^{11,00} = \sum_{y_2^{00}=0,1} \sum_{y_2^{01}=0,1} p(1, y_2^{01}, y_2^{00}|0,0) - \sum_{y_2^{11}=0,1} \sum_{y_2^{01}=0,1} p(y_2^{11}, y_2^{01}, 1|0,0) \tag{11}$$

and

$$\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0] = \sum_{y_2^{00}=0,1} \sum_{y_2^{01}=0,1} p(1, y_2^{01}, y_2^{00}|0,0) \tag{12}$$

and

$$\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0] = \sum_{y_2^{11}=0,1} \sum_{y_2^{01}=0,1} p(y_2^{11}, y_2^{01}, 1|0,0). \tag{13}$$

The observed fractions, in the first period, with D_1, Y_1 give

$$\Pr(Y_1 = y_1|D_1 = 1) = \sum_{y_1^0=0,1} p(y_1, y_1^0) \tag{14}$$

and

$$\Pr(Y_1 = y_1|D_1 = 0) = \sum_{y_1^1=0,1} p(y_1^1, y_1) \tag{15}$$

, and the observed fractions, in the second period, with D_2, Y_2 give

$$\Pr(Y_2 = y_2|D_1 = 1, Y_1 = 0) = \tag{16}$$

$$\frac{\sum_{y_2^{01}=0,1} \sum_{y_2^{00}=0,1} p(y_2, y_2^{01}, y_2^{00}|0,0) p(0,0) + p(y_2|0,1) p(0,1)}{\sum_{y_1^0=0,1} p(0, y_1^0)}$$

and

$$\Pr(Y_2 = y_2|D_1 = 0, D_2 = 0, Y_1 = 0) = \tag{17}$$

$$\frac{\sum_{y_2^{11}=0,1} \sum_{y_2^{01}=0,1} p(y_2^{11}, y_2^{01}, y_2|0,0) p(0,0) + \sum_{y_2^{01}=0,1} p(y_2^{01}, y_2|0,1) p(1,0)}{\sum_{y_1^1=0,1} p(y_1^1, 0)}$$

and

$$\Pr(Y_2 = y_2|D_1 = 0, D_2 = 1, Y_1 = 0) = \tag{18}$$

$$\frac{\sum_{y_2^{11}=0,1}\sum_{y_2^{00}=0,1} p(y_2^{11}, y_2, y_2^{00}|0,0)p(0,0) + \sum_{y_2^{00}=0,1} p(y_2, y_2^{00}|0,1)p(1,0)}{\sum_{y_1^{1}=0,1} p(y_1^1, 0)}.$$

The bounds are obtained by minimizing and maximizing (11)-(13) under the restrictions (14)-(18), and obviously with the additional restriction that all probabilities by definition lie between zero and one. Both the outcomes in equations (11)-(13) and the restrictions are linear, so that the bounds are the solution to a LP problem.

Our main results are

Proposition 1 (Bounds on conditional transition probabilities) *Suppose that assumption 3 holds. Then*

$$max(0, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 1) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)})$$

$$\leq \mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0] \leq$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)})$$

and

$$max(0, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 0) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)})$$

$$\leq \mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0] \leq$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}),$$

and

$$max(0, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 1) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)})-$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)})$$

$$\leq ATES_2^{11,00} \leq$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}) -$$

$$max(0, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 0) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}).$$

Proof see Appendix A. □

Proposition 1 provides a closed form solution for the sharp bounds on $\text{ATES}_2^{11,00}$. These bounds impose no assumptions beyond sequential random assignment among survivors. In fact, we make no assumptions on functional form and additional covariates, and we allow for arbitrary heterogeneous treatment effects as well as arbitrary unobserved heterogeneity. From these general results follow two important results on point identification and on the informativeness of the bounds

Corollary 2 (Point identification) *1. Suppose that assumption 3 and* $\Pr(Y_1 = 0|D_1 = 0) = 1$ *hold. Then* $\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0]$ *is point identified and equal to* $\Pr(Y_2 = 1|Y_1 = 0, D_1 = 1)$.

2. Suppose that assumption 3 and $\Pr(Y_1 = 0|D_1 = 1) = 1$ *hold. Then* $\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0]$ *is point identified and equal to* $\Pr(Y_2 = 1|Y_1 = 0, D_2 = 0, D_1 = 0)$.

3. Suppose that assumption 3, $\Pr(Y_1 = 0|D_1 = 1) = 1$ *and* $\Pr(Y_1 = 0|D_1 = 0) = 1$ *hold. Then* $ATES_2^{11,00}$ *is point identified and equal to* $\Pr(Y_2 = 1|Y_1 = 0, D_1 = 1) - \Pr(Y_2 = 1|Y_1 = 0, D_1 = 0, D_2 = 0)$.

Proof see Appendix A. □

Corollary 3 (Informative bounds) *Define* $A \equiv max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)$. *Suppose that assumption 3 hold. In addition if either*

$$\frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{A} < 1$$

or

$$1 - \frac{[1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{A} > 0$$

or

$$1 - \frac{[1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{A} > 0$$

or

$$\frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{A} < 1$$

hold. Then the bounds in proposition 1 are informative on $ATET_2^{11,00}$.

Proof see Appendix A. □

Corollary 2 shows that if there is no dynamic selection, i.e. if $\Pr(Y_1 = 0|D_1 = 1) = 1$ and $\Pr(Y_1 = 0|D_1 = 1) = 1$, the dynamic treatment effect $ATES_2^{11,00}$ is point identified. If everyone survive the first period we have under random treatment two directly comparable groups even in the second period. The corollary also includes two results which may seem counterintuitive: $\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0]$ is point identified if $\Pr(Y_1 = 0|D_1 = 0) = 1$, and $\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0]$ is point identified if $\Pr(Y_1 = 0|D_1 = 1) = 1$. That is the counterfactual outcome under treatment (no treatment) is point identified if no one exits in the control (treatment) group. The intuition behind these results are that we consider the average treatment effect for those who survive the first period under both treatment and control. If $Y_1^1 = 0$ for everyone and under random assignment we have

$$\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0] = \mathbb{E}[Y_2^{00}|Y_1^0 = 0] = \mathbb{E}[Y_2|Y_1 = 0, D_1 = 0, D_2 = 0].$$

Together with similar reasoning for $\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0]$ give the results in the corollary.

Corollary 3 tells us that the bounds are informative as long as $\Pr(Y_1 = 0|D_1 = 1) = 1$ and $\Pr(Y_1 = 0|D_1 = 1) = 1$ are not too small. Even though the bounds often are informative they can be quite wide in many situations. If $\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) \geq 1$ it follows from proposition 1 that the width of the bounds on $ATES_2^{11,00}$ are

$$\frac{2 - \Pr(Y_1 = 0|D_1 = 1) - \Pr(Y_1 = 0|D_1 = 0)}{\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1}.$$

In other words, the width of the general bounds is directly related to the size of $\Pr(Y_1 = 0|D_1 = 1)$ and $\Pr(Y_1 = 0|D_1 = 0)$, i.e. how large fraction that leaves the state of interest in the first period.

4 Identification of treatment effects on conditional transitions under additional weak assumptions

The sharp bounds in the previous section did not impose any assumptions beyond random assignment. In this section, we explore the identifying power of additional weak assumptions. To try to make the intuition behind the assumptions clear, we discuss our assumptions in the context of a medical example and relate them to the assumptions made in the popular MPH model. Again, the MPH model specifies the individual hazard rate for individual i as

$$\theta(t, d(t), V_i) = \lambda(t)\gamma(t - \tau, \tau)^{d(t)} v_i \tag{19}$$

or, in a regression-type expression, the integrated hazard function for individual i as

$$\log \int_0^t \lambda(t) = -\log \gamma(t-\tau,\tau)^{d(t)} - \log v_i + \varepsilon_i, \qquad (20)$$

where ε is $EV1$ extreme value type 1 distributed. The time to a transition $T_i^{d(T)}$ for individual i is fully determined by: the baseline hazard $\lambda(t)$, the treatment path $d(T)$, the homogenous[3] treatment effect γ, the nonnegative spell constant unobservable heterogeneity for individual i v_i, and the specific draw of ε_i. The MPH model builds on several assumptions, most notably: proportionality of the hazard function, homogenous treatment effect, spell constant unobserved individual heterogeneity, and a single one dimensional shock which given the hazard function determines the realized time to a transition. In some applications, these assumptions are harmless and in other applications they are very restrictive. Obviously, our general bounds are sometimes wide as we impose neither one of these assumptions.

In many applications one weak assumption is monotone treatment response (MTR). The assumption has been explored by e.g. Manski (1997) and Manski & Pepper (2000). In a transition framework the assumption has to be modified. Most often, there will not be a single MTR assumption in a transition framework. For instance, one may assume positive treatment effect for all individuals in some time period and negative treatment effect for all individuals in another time period. Let $Y_{it}^{\overline{d}_t}$ be the indicator of a transition in period t for individual i if the treatment history up to and including t is $\overline{d}_t$. In our two period example, we define three MTR assumptions appropriate for that setting: MTR with observed sign in the first period, and either negative or positive MTR in period 2 from treatment given in time period 1, as[4]

[3]By homogenous we mean that given the time of treatment and the time elapsed since treatment there is one homogenous treatment effect for all individuals.

[4]Another more subtle difference compared to Manski & Pepper (2000) is that we phrase the assumptions in terms of something that is most accurately described as the average individual treatment effect. Manski & Pepper (2000) states their assumption in the form $Y_{it}^1 > Y_{it}^0$ with one single individual treatment effect. In a transition framework $Y_{it}^1 - Y_{it}^0$ could either be -1,0, or 1. It is thus reasonable to focus on the average individual treatment effect, for instance $\mathbb{E}(Y_{i1}^1) - \mathbb{E}(Y_{i1}^0)$.

Assumption 4 (Monotone treatment response in period 1) *For t=1 and all i*

$$\Pr(Y_1^1 = 1) \geq \Pr(Y_1^0 = 1) \Rightarrow \Pr(Y_{i1}^1 = 1) \geq \Pr(Y_{i1}^0 = 1)$$

and

$$\Pr(Y_1^1 = 1) \leq \Pr(Y_1^0 = 1) \Rightarrow \Pr(Y_{i1}^1 = 1) \leq \Pr(Y_{i1}^0 = 1)$$

Assumption 5 (Positive MTR in period 2 from treatment in period 1) *For t=2 and all i*

$$\Pr(Y_{i2}^{11} = 1 | Y_{i1}^1 = 0, Y_{i1}^0 = 0) \geq \Pr(Y_{i2}^{00} = 1 | Y_{i1}^0 = 0, Y_{i1}^1 = 0)$$

Assumption 6 (Negative MTR in period 2 from treatment in period 1) *For t=2 and all i*

$$\Pr(Y_{i2}^{00} = 1 | Y_{i1}^1 = 0, Y_{i1}^0 = 0) \geq \Pr(Y_{i2}^{11} = 1 | Y_{i1}^0 = 0, Y_{i1}^1 = 0)$$

For intuition behind these assumptions let us consider a medical example. The set up is as follows: time of origin is the date when the patient is diagnosed with cancer. The treatment is chemotherapy, which can start directly after the patient has been diagnosed with cancer, i.e. in time period 1, or at some later time period t. The transition state is death. In this context, assumption 4 means that if we observe a positive (negative) effect on average from being instantly treated with chemotherapy we conclude that all patients benefit (suffer) from being instantly treated with chemotherapy. Assumption 5 (assumption 6) implies that we assume that all patients who survive the first period benefit (suffer) in the second period from chemotherapy started in the first period.

Another source of heterogeneity in our general setting is that we have not placed any restrictions on the unobserved heterogeneity in the model. In the MPH model unobserved heterogeneity is introduced by v_i, the spell constant unobserved heterogeneity in the transition rate and by ε_i, the one dimensional idiosyncratic shock which given the transition rate determines if a transition is realized or not. Needless to say, this places restrictions on the types of unobserved heterogeneity that is plausible. One could, for instance, imagine that the shocks are multidimensional, with one shock under treatment and one shock under no treatment. As an illustration, return to the medical example, and assume that we know that chemotherapy on average is beneficial for a certain patient and that this patient receives chemotherapy and dies in time period one. One question then is what can be inferred about what would have happened to this patient if the patient would not have received chemotherapy, i.e. what can we say about $\Pr(Y_{i1}^0 = 1 | Y_{i1}^1 = 1)$. In the MPH model the answer is straightforward: as the effect of the treatment is positive on average ($\gamma(t-\tau,\tau)^{d(t)} > 1$) and we have the single shock ε, it implies that we know that the patient would have died also under no treatment.

The problem with identification without the MPH model assumptions can be seen by noticing that

$$\Pr(Y_{i1}^1 = 1) < \Pr(Y_{i1}^0 = 1)$$

implies that

$$\Pr(Y_{i1}^1 = 1 | Y_{i1}^0 = 1)\Pr(Y_{i1}^0 = 1) + \Pr(Y_{i1}^1 = 1 | Y_{i1}^0 = 0)\Pr(Y_{i1}^0 = 0) <$$

$$\Pr(Y_{i1}^0 = 1 | Y_{i1}^1 = 1)\Pr(Y_{i1}^1 = 1) + \Pr(Y_{i1}^0 = 1 | Y_{i1}^1 = 0)\Pr(Y_{i1}^1 = 0).$$

As easily seen, without any further assumptions, one cannot say much about these conditional probabilities using only information on the marginal probabilities $\Pr(Y_{i1}^1 = 1)$ and $\Pr(Y_{i1}^0 = 1)$. If one nevertheless infer information from the marginal probabilities one have placed restrictions on the types of unobserved heterogeneity that is possible in the model. In fact, it may be the case that $\Pr(Y_{it}^1 = 1 | Y_{it}^0 = 1) = 0$ and $\Pr(Y_{it}^0 = 1 | Y_{it}^1 = 1) = 0$ even if $\Pr(Y_{it}^1 = 1) \neq 0$ and $\Pr(Y_{it}^0 = 1) \neq 0$.

In this paper we explore the identifying power of the one shock assumption made in the MPH model and weaker versions of it. For presentation reasons define for two treatment histories $\overline{d}_{st}$ and $\overline{d}_{kt}$

$$A(0) \equiv \mathbf{1}(Y_{t-1}^{\overline{d}_{st-1}} = 0, \ldots, Y_1^{\overline{d}_{s1}} = 0, Y_{t-1}^{\overline{d}_{kt-1}} = 0, \ldots, Y_1^{\overline{d}_{k1}} = 0)$$

as an indicator function taking the value one if the expression in the parenthesis is true. We explore the two assumptions

Assumption 7 (Positively correlated shocks) *For all t and i and each pair of treatment histories, denoted by $\overline{d}_{st}$ and $\overline{d}_{kt}$. If*

$$\Pr(Y_{it}^{\overline{d}_{st}} = 1 | A(0) = 1) \geq \Pr(Y_{it}^{\overline{d}_{kt}} = 1 | A(0) = 1)$$

holds then

$$\Pr(Y_{it}^{\overline{d}_{st}} = 1 | Y_{it}^{\overline{d}_{kt}} = 1, A(0) = 1) \geq \Pr(Y_{it}^{\overline{d}_{st}} = 0 | Y_{it}^{\overline{d}_{kt}} = 1, A(0) = 1)$$

$$\Pr(Y_{it}^{\overline{d}_{kt}} = 0 | Y_{it}^{\overline{d}_{s,t}} = 0, A(0) = 1) \geq \Pr(Y_{it}^{\overline{d}_{kt}} = 1 | Y_{it}^{\overline{d}_{st}} = 0, A(0) = 1),$$

, and if

$$\Pr(Y_{it}^{\overline{d}_{st}} = 1 | A(0) = 1) \leq \Pr(Y_{it}^{\overline{d}_{kt}} = 1 | A(0) = 1)$$

holds then

$$\Pr(Y_{it}^{\overline{d}_{kt}} = 1 | Y_{it}^{\overline{d}_{st}} = 1, A(0) = 1) \geq \Pr(Y_{it}^{\overline{d}_{kt}} = 0 | Y_{it}^{\overline{d}_{st}} = 1, A(0) = 1)$$

$$\Pr(Y_{it}^{\overline{d}_{st}} = 0 | Y_{it}^{\overline{d}_{kt}} = 0, A(0) = 1) \geq \Pr(Y_{it}^{\overline{d}_{st}} = 1 | Y_{it}^{\overline{d}_{kt}} = 0, A(0) = 1).$$

Assumption 8 (Single dimensional shock) *For all t and i and each pair of treatment histories, denoted by $\overline{d}_{st}$ and $\overline{d}_{kt}$*

$$\Pr(Y_{it}^{\overline{d}_{st}} = 1|A(0) = 0) \geq \Pr(Y_{it}^{\overline{d}_{kt}} = 1|A(0) = 0) \Rightarrow$$

$$(Y_{it}^{\overline{d}_{st}} \geq Y_{it}^{\overline{d}_{kt}}|A(0) = 0)$$

and

$$\Pr(Y_{it}^{\overline{d}_{st}} = 1|A(0) = 0) \leq \Pr(Y_{it}^{\overline{d}_{kt}} = 1|A(0) = 0) \Rightarrow$$

$$(Y_{it}^{\overline{d}_{st}} \leq Y_{it}^{\overline{d}_{kt}}|A(0) = 0).$$

For intuition behind these assumptions consider the medical example. Assumption 7 allows for different shocks under treatment and no treatment, but it assumes that these shocks are positively correlated. More precisely, if a randomly induced flu causes the patient to die in the first period we expect the same patient to also be exposed to the flu under no treatment. There is, however, some randomness involved, so that it may not be an exactly equally severe flu. Assumption 8 implies that all random events like exposure to a flu are the same no matter if the patient receives the treatment or not.

Combining assumption 4 with assumption 7 give

Proposition 4 (Bounds under MTR and positively correlated shocks) *Define* $A \equiv max(-\frac{1}{2} + \Pr(Y_1 = 0|D_1 = 0) + \frac{1}{2}\Pr(Y_1 = 0|D_1 = 1), \frac{\Pr(Y_1=0|D_1=0)}{2})$ *and* $B \equiv max(-\frac{1}{2} + \Pr(Y_1 = 0|D_1 = 1) + \frac{1}{2}\Pr(Y_1 = 0|D_1 = 0), \frac{\Pr(Y_1=0|D_1=1)}{2})$*. Suppose assumption 3, 4, and 7 hold. Then if* $\Pr(Y_1 = 1|D_1 = 1) < \Pr(Y_1 = 1|D_1 = 0)$

$$max(0, \frac{A - [1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{A}) -$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{A})$$

$$\leq ATES_2^{11,00} \leq$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{A}) -$$

$$max(0, \frac{A - [1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{A})$$

and if $\Pr(Y_1 = 1|D_1 = 1) > \Pr(Y_1 = 1|D_1 = 0)$

$$max(0, \frac{B - [1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{B}) -$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{B})$$

$$\leq ATES_2^{11,00} \leq$$

$$min(1, \frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{B})-$$

$$max(0, \frac{B-[1-\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)]\Pr(Y_1=0|D_1=0)}{B})$$

Proof see Appendix A. □
and combining assumption 4 and assumption 8 give

Proposition 5 (Bounds under MTR and a single shock) *Suppose assumption 3, 4, and 7 holds. Then if* $\Pr(Y_1=1|D_1=1) < \Pr(Y_1=1|D_1=0)$

$$max(0, \frac{\Pr(Y_1=0|D=0)-[1-\Pr(Y_2=1|D_1=1,Y_1=0)]\Pr(Y_1=0|D_1=1)}{\Pr(Y_1=0|D_1=0)})-$$

$$\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)$$

$$\leq ATES_2^{11,00} \leq$$

$$min(1, \frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{\Pr(Y_1=0|D=0)})-$$

$$\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)$$

and if $\Pr(Y_1=1|D=1) > \Pr(Y_1=1|D_1=0)$

$$\Pr(Y_2=1|D_1=1,Y_1=0)-$$

$$min(1, \frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{\Pr(Y_1=0|D_1=1)})$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2=1|D_1=1,Y_1=0)-max(0, \frac{\Pr(Y_1=0|D=1)}{\Pr(Y_1=0|D_1=1)}-$$

$$\frac{[1-\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)]\Pr(Y_1=0|D_1=0)}{\Pr(Y_1=0|D_1=1)})$$

Proof see Appendix A. □

These expressions show that these weak assumptions may have strong identifying power. This will be further illustrated in our application to re-employment bonus experiment. If $\Pr(Y_1=1|D=1) \approx \Pr(Y_1=1|D=0)$ the bounds under the MTR assumption and the single shock assumption are very narrow, as we have assumed that the treated and non treated who exit during the first period have similar characteristics. Note that, if either assumption 4 or assumption 8 do not hold the bounds on the $ATES_2^{11,00}$ may be wide even if $\Pr(Y_1 = 1|D = 1) \approx \Pr(Y_1 = 1|D = 0)$.

The two assumptions of positive respectively negative treatment response in period 2 will effectively bound away negative respectively positive average treatment effects. For completeness are these bounds presented in Appendix B.

A third major source of heterogeneity in our general setting is that we have not placed any restrictions on the relation between $\Pr(Y_2^{11} = 1|Y_1^1 = 0, Y_1^0 = 0)$ and $\Pr(Y_2^{11} = 1|Y_1^1 = 0, Y_1^0 = 1)$, and no restrictions on the relation between $\Pr(Y_2^{00} = 1|Y_1^1 = 0, Y_1^0 = 0)$ and $\Pr(Y_2^{00} = 1|Y_1^1 = 1, Y_1^0 = 0)$. In fact, we allow for the extreme case that those who survives under both treatment and no treatment in the first period all exit under no treatment in the second period, whereas none of those who exit under treatment and survives under no treatment in the first period exit under no treatment in the second period. It means that some individuals exit relatively faster in one time period and relatively slower in another time period. The corresponding assumption in the MPH model of fixed unobserved heterogeneity obviously rules out any such heterogeneity.

We explore a weaker assumption compared to fixed unobserved heterogeneity, and explore the assumption that some individuals are inherently "weaker" than others under both treatment and no treatment as well as in all time periods. We call this monotone exit rate and define it as

Assumption 9 (Monotone exit rate) *For two individuals $i \neq j$, either*

$$\mathbb{E}[Y_{it}^{\overline{d}_s}|Y_{it-1}^{\overline{d}_s} = 0, \dots, Y_{i1}^{\overline{d}_s} = 0] \leq \mathbb{E}[Y_{jt}^{\overline{d}_s}|Y_{jt-1}^{\overline{d}_s} = 0, \dots, Y_{j1}^{\overline{d}_s} = 0]$$

or

$$\mathbb{E}[Y_{it}^{\overline{d}_s}|Y_{it-1}^{\overline{d}_s} = 0, \dots, Y_{i1}^{\overline{d}_s} = 0] \geq \mathbb{E}[Y_{jt}^{\overline{d}_s}|Y_{jt-1}^{\overline{d}_s} = 0, \dots, Y_{j1}^{\overline{d}_s} = 0].$$

hold for all t and all treatment histories $\overline{d}_s$.

Let us once again return to the medical example. If patient A has larger chance of dying without chemotherapy compared with patient B in time period one, the monotone exit assumption implies that patient A also has larger chance of dying with chemotherapy in period one. It further means that if both patients survive until time period t, patient A has larger chance of dying under both chemotherapy and without chemotherapy in time period t. In other words, patient A is assumed to be inherently more fragile compared to patient B. In the two period case the monotone exit assumption implies that $\Pr(Y_2^{11} = 1|Y_1^1 = 0, Y_1^0 = 0) \leq \Pr(Y_2^{11} = 1|Y_1^1 = 0, Y_1^0 = 1)$ and $\Pr(Y_2^{00} = 1|Y_1^1 = 0, Y_1^0 = 0) \leq \Pr(Y_2^{00} = 1|Y_1^1 = 1, Y_1^0 = 0)$. We then have

Proposition 6 (Bounds under monotone exit rate) *Define* $A \equiv max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)$. *Suppose that assumption 3 and assumption 9 holds. Then*

$$max(0, \frac{A - [1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{A} -$$

$$\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0))$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) -$$

$$max(0, \frac{A - [1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{A}).$$

Proof see Appendix A. □

For completion we report the bounds under monotone exit rate combined with MTR and positively correlated shocks, and the bounds under monotone exit rate combined with MTR and single shocks in Appendix B.

5 Application to the Illinois bonus experiment

5.1 The re-employment bonus experiment

Between mid-1984 and mid-1985, the Illinois Department of Employment Security conducted a controlled social experiment.[5] The goal of the experiment was to explore, whether bonuses paid to Unemployment Insurance (UI) beneficiaries (treatment 1) or their employers (treatment 2) reduced the unemployment of beneficiaries relative to a randomly selected control group. In this paper we focus primarily on the effect of treatment 1.

Both treatments consisted of a \$ 500 bonus payment, which was about four times the average weekly unemployment insurance benefit. In the experiment, newly unemployed claimants were randomly divided into three groups:

1. The *Claimant Bonus Group*. The members of this group were instructed that they would qualify for a cash bonus of \$500 if they found a job (of at least 30 hours) within 11 weeks and, if they held that job for at least 4 months. 4186 individuals were selected for this group, of those 3527 (84%) agreed to participate.

2. The *Employer Bonus Group*. The members of this group were told that their next employer would qualify for a cash bonus of \$500 if they, the claimants, found a job (of at least 30 hours) within 11 weeks and, if they held that job for at least four months. 3963 were selected for this group and 2586 (65%)

[5] A complete description of the experiment and a summary of its results can be found in Woodbury & Spiegelman (1987).

agreed to participate.
3. The *Control Group*, i.e. all claimants not assigned to one of the other groups. This group consisted of 3952 individuals. The individuals assigned to the control group were excluded from participation in the experiment. In fact, they did not know that the experiment took place.

The descriptive statistics in Table 2 in Woodbury & Spiegelman (1987) confirm that the randomization resulted in three similar groups.

5.2 Results of previous studies

Woodbury & Spiegelman (1987) concluded from a direct comparison of the control group and the two treatment groups that the claimant bonus group had significantly smaller average unemployment duration. The average unemployment duration was also smaller for the employer bonus group, but the difference was not significantly different from zero. In the USA, UI benefits end after 26 weeks, meaning that all unemployment durations are censored at 26 weeks. Therefore note that the response variable is insured weeks of unemployment, and not weeks out of employment.

Meyer (1996) analyzed the same data but focused on the treatment effects on conditional transition rates. Besides taking care of censoring, Meyer focuses on the conditional transitions rates because labor supply and search theories suggest interesting dynamic treatment effects. The bonus is only given to the unemployed if (s)he finds a job within 11 weeks and retains it for four months. The cash bonus is also the same for all unemployed. Based on these features theory gives some interesting predictions, all investigated by Meyer (1996). The first prediction is that the transition rate during the eligibility period (first 11 weeks) will be higher in the two treatment groups compared with the control group. A second prediction is that the transition rate in the treatment groups should rise just before the end of the eligibility period, as the unemployed are in a hurry to collect the bonus.

In order to analyze these predictions, Meyer (1996) estimates a proportional hazard (PH) model with a flexible specification of the baseline hazard. He uses the treatment indicator as an explanatory variable. Since, there was partial compliance with treatment his estimator can be interpreted as a intention to treat (ITT) estimator.[6] In his analysis Meyer (1996) controls for age, the logarithm of base period earnings, race, sex and the logarithm of the size of the unemployment insurance benefits. He finds a significantly positive effect of the claimant bonus and positive but insignificant effect of the employer bonus.

[6]The non full compliance is addressed in detail by Bijwaard & Ridder (2005). They introduce a new method to handle the selective compliance in the treatment group. If there is full compliance in the control group, their two-stage linear rank estimator is able to handle the selective compliance in the treatment group even for censored durations. In order to achieve this they assume a MPH structure for the transition rate. Their estimates indicate that the ITT estimates by Meyer (1996) underestimate the true treatment effect.

A more detailed analysis of the effects for the claimant group reveals positive effect on the transition rate during the first 11 weeks in unemployment, an increased effect during week 9 and 10, and no significant effect on the transition rate after week 11. All these results are in line with the predictions from labor supply models and search theories.

5.3 Set identification

Meyer (1996) heavily relies on the proportionality of the hazard rate to investigate the hypothesis suggested by labor supply models and search theories. We now ask what can be said about these hypothesis if the assumptions imbedded in the MPH (PH) model do not hold, that is what can be identified relying solely on random assignment and additional weak assumptions. We follow Meyer (1996) and estimate the ITT effect. We divide time into 12 discrete periods: week 1-2, week 3-4, ... , week 23-24. The reason for this is that there is a pronounced even-odd week effect in the data, with higher transition rate during odd weeks. In this setting the theoretical predictions we wish to test could be expressed as; (*i*) positive treatment effect during the period when the bonus could be claimed (period 1-5)

$$\text{ATES}_1^{1,0}, \ldots, \text{ATES}_5^{1\ldots1,0\ldots0} > 0,$$

(*ii*) no effect once the bonus offer have expired (period 6-12)

$$\text{ATES}_6^{1\ldots1,0\ldots0}, \ldots, \text{ATES}_{12}^{1\ldots1,0\ldots0} = 0,$$

and (*iii*) intensified effect of the bonus offer at the end of the eligibility period (period 5)

$$\text{ATES}_5^{1\ldots1,0\ldots0} > \text{ATES}_4^{1\ldots1,0\ldots0}.$$

From section 3 we have that under random assignment $\text{ATE}_1^{1,0}$ is point identified, and that $\text{ATES}_2^{11,00}$ in general is not point identified. We also wish to consider bounds on $\text{ATES}_t^{1\ldots1,0\ldots0}$ for $t > 2$. It is clear that when deriving such bounds one would end up with a sequence of restrictions: one for the treatment group and one for the control group in each time period. We consider a simpler version of these bounds. Consider the bounds for time period t: one way of constructing such bounds is to redefine the time periods into considering $t = 0$ to $t-1$ as the new first period and period t as the new second period. The two period bounds, derived in this paper, are then directly applicable. Note that, this procedure gives conservative bounds as we have aggregated some restrictions.

Our bounds are expressed in terms of population moments, but they could be estimated by replacing the population moments with their sample analogs, for instance

$$\Pr(Y_1 = 0|D_1 = 1) = \frac{\sum_{i=1}^{N} \mathbf{1}(D_{1i} = 1)Y_{1i}}{\sum_{i=1}^{N} \mathbf{1}(D_{1i} = 1)}.$$

Here N is the number of individuals in the sample and $\mathbf{1}(\cdot)$ is an indicator function taking the value one if the expression in the parenthesis is true and zero otherwise. Inferences for set identified models have been discussed in a series of recent papers, see e.g. Chernozhukov et al. (2007) for an insightful overview of this literature. Imbens & Manski (2004) have shown how to construct confidence intervals when the identified set is an interval whose upper and lower endpoints are means (or behave like means). Our general bounds are of that type. In order to construct confidence intervals we first bootstrap (399 replicates) the variance of the two endpoints, and then apply the Imbens & Manski (2004) confidence intervals. We also apply that method to the bounds under additional restrictions.[7]

Table 1 presents the upper and the lower bound on ATES_t (and their confidence intervals) for the claimant group under random assignment and combinations of additional assumptions. Figure 1 displays the same bounds, and the confidence intervals. The general bounds, which impose no assumptions beyond random assignment are labeled no. The instantaneous treatment effect on the transition rate (week 1-2) is point identified and indicates a positive treatment effect of being offered the possibility to claim a bonus. The transition rate is about 2 percentage points higher in the claimant group compared to the control group. From week 3-4 and onwards the bounds are quite wide. In fact, without further assumptions we cannot rule out that the bonus actually has a negative impact on the conditional transition rate from week 3 and onwards. However, note that until week 20 the bounds are nevertheless informative on the average treatment effect.

Next, consider what can be identified under additional weak assumptions. First, consider the plausibility of the assumptions considered in section 4. The average treatment effect is positive during the first period. Assumption 4, monotone treatment response, then implies that being offered a job bonus has positive or zero effect on the transition rate from unemployment to employment for all unemployed. It is hard to imagine that any individual would suffer from a bonus offer, so that assumption 4, most likely, is fulfilled. Assumption 8, a single shock, means that being offered a bonus does not affect the ran-

[7]Note that for some of these bounds intervals the upper (lower) bound is constructed by taking the maximum (minimum) value of two or more restrictions. This means that the Imbens & Manski (2004) inference in a strict sense is not applicable, see e.g. Pakes et al. (2007) and Romano & Shaikh (2008). The complication arises since with a finite sample there is some uncertainty about which restriction that is binding. One alternative is to apply the subsampling method proposed in Romano & Shaikh (2008). However, we have noticed that in our application there is little uncertainty about which of the restrictions that are binding. We therefore feel confident in applying the Imbens & Manski (2004) confidence intervals.

Table 1: *Bounds on conditional transition probabilities for the Illinois job bonus experiment (claimant bonus)*

Assumptions	No [1]	MTR+PS [2]
Week		
1-2	[0.008 (0.023 : 0.023) 0.037]	[0.007 (0.023 : 0.023) 0.038]
3-4	[-0.107 (-0.097 : 0.111) 0.120]	[-0.081 (-0.068 : 0.102) 0.111]
5-6	[-0.106 (-0.095 : 0.100) 0.111]	[-0.090 (-0.081 : 0.086) 0.095]
7-8	[-0.114 (-0.102 : 0.121) 0.133]	[-0.089 (-0.080 : 0.095) 0.105]
9-10	[-0.128 (-0.113 : 0.127) 0.143]	[-0.090 (-0.080 : 0.090) 0.100]
11-12	[-0.142 (-0.123 : 0.140) 0.159]	[-0.086 (-0.076 : 0.087) 0.097]
13-14	[-0.192 (-0.166 : 0.162) 0.188]	[-0.099 (-0.086 : 0.084) 0.096]
15-16	[-0.233 (-0.193 : 0.206) 0.244]	[-0.090 (-0.077 : 0.082) 0.095]
17-18	[-0.414 (-0.316 : 0.316) 0.406]	[-0.100 (-0.086 : 0.086) 0.100]
19-20	[-1.152 (-0.865 : 0.809) 1.107]	[-0.116 (-0.100 : 0.093) 0.107]
21-22	[-1.000 (-1.000 : 1.000) 1.000]	[-0.157 (-0.138 : 0.095) 0.111]
23-24	[-1.000 (-1.000 : 1.000) 1.000]	[-0.135 (-0.116 : 0.112) 0.129]
Assumptions	MTR+SS [3]	ME [4]
1-2	[0.006 (0.023 : 0.023) 0.039]	[0.008 (0.023 : 0.023) 0.037]
3-4	[0.000 (0.011 : 0.038) 0.056]	[-0.088 (-0.081 : 0.094) 0.103]
5-6	[-0.007 (0.004 : 0.046) 0.067]	[-0.075 (-0.068 : 0.075) 0.083]
7-8	[0.002 (0.013 : 0.063) 0.085]	[-0.070 (-0.063 : 0.078) 0.086]
9-10	[-0.004 (0.008 : 0.070) 0.084]	[-0.065 (-0.058 : 0.070) 0.077]
11-12	[-0.003 (0.008 : 0.063) 0.071]	[-0.057 (-0.051 : 0.063) 0.070]
13-14	[-0.013 (-0.002 : 0.057) 0.065]	[-0.061 (-0.053 : 0.057) 0.064]
15-16	[-0.008 (0.003 : 0.051) 0.059]	[-0.051 (-0.044 : 0.051) 0.059]
17-18	[-0.012 (0.000 : 0.050) 0.058]	[-0.052 (-0.045 : 0.050) 0.057]
19-20	[-0.015 (-0.003 : 0.050) 0.057]	[-0.126 (-0.048 : 0.050) 0.128]
21-22	[-0.034 (-0.021 : 0.047) 0.056]	[-1.285 (-1.000 : 1.000) 1.289]
23-24	[-0.015 (-0.002 : 0.056) 0.066]	[-1.000 (-1.000 : 1.000) 1.000]
Assumptions	ME+MTR+PS [5]	ME+MTR+SS [6]
1-2	[0.008 (0.023 : 0.023) 0.037]	[0.008 (0.023 : 0.023) 0.037]
3-4	[-0.071 (-0.059 : 0.094) 0.103]	[0.002 (0.014 : 0.038) 0.055]
5-6	[-0.075 (-0.068 : 0.075) 0.082]	[-0.004 (0.007 : 0.046) 0.068]
7-8	[-0.070 (-0.063 : 0.078) 0.086]	[0.005 (0.016 : 0.063) 0.085]
9-10	[-0.065 (-0.058 : 0.070) 0.078]	[0.001 (0.012 : 0.070) 0.083]
11-12	[-0.057 (-0.051 : 0.063) 0.071]	[0.002 (0.012 : 0.063) 0.071]
13-14	[-0.061 (-0.053 : 0.057) 0.064]	[-0.007 (0.004 : 0.057) 0.065]
15-16	[-0.051 (-0.044 : 0.051) 0.059]	[-0.003 (0.007 : 0.051) 0.059]
17-18	[-0.052 (-0.045 : 0.050) 0.058]	[-0.005 (0.005 : 0.050) 0.057]
19-20	[-0.055 (-0.048 : 0.050) 0.058]	[-0.009 (0.002 : 0.050) 0.058]
21-22	[-0.070 (-0.062 : 0.047) 0.055]	[-0.026 (-0.014 : 0.047) 0.055]
23-24	[-0.061 (-0.053 : 0.056) 0.065]	[-0.009 (0.003 : 0.056) 0.066]

Notes: Bounds in parenthesis and confidence intervals in brackets. Raw indicates the difference in the raw hazard rate, and no the bounds under random assignment. MTR stands for assumption monotone treatment response, SS a single shock, PS positively correlated shocks, and ME monotone exit rate.

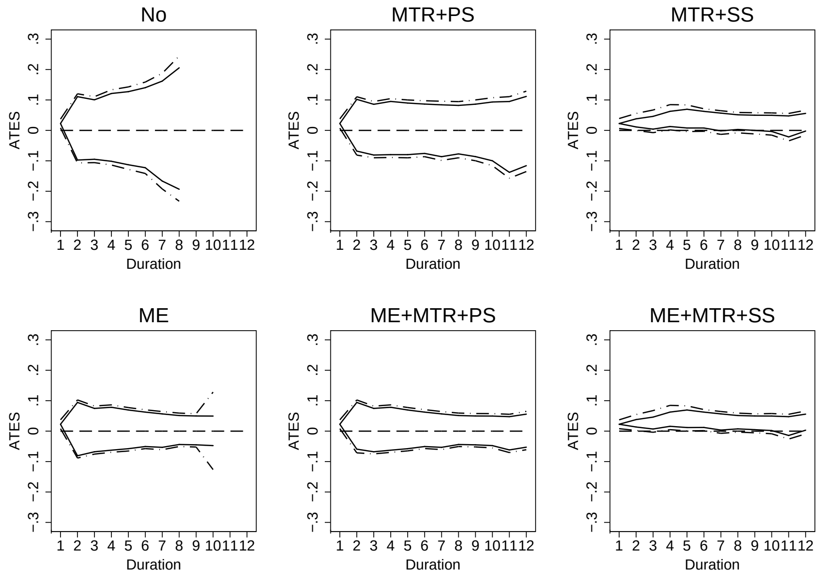

Figure 1: Bounds on conditional transition probabilities for the Illinois job bonus experiment (claimant bonus)

dom events influencing the arrival of employment. All random events that is not caused by the bonus offer should occur under both treatment and no treatment. We have no strong beliefs to doubt this assumption. Since assumption 7, positively correlated shocks, is weaker than assumption 8 we also explore the identifying power of this assumption. Assumption 9, monotone exit rate, implies that some unemployed individuals have a higher probability of finding employment compared to other unemployed when being offered the job bonus as well when not being offered the bonus. This assumption is fulfilled if the ranking of the individuals in terms of the characteristics that determines job offers, such as experience and job search effort, stays the same during the entire job search period. We are confident in that this is a quite good description of reality.

As expected, when imposing additional weak assumptions the bounds are tightened considerably. Assumption 7 and assumption 4 allow us to rule out very large negative and very large positive average dynamic treatment effects. Assumption ME has the same effect. Imposing assumption 8 and assumption 4 further tightens the bounds. If these assumptions hold we can rule out that the bonus offer has a negative effect on the conditional transition rates. These two assumptions together with assumption 9 give even more narrow bounds. Let us return to the three hypotheses suggested by labor models and search theories, and consider our most restrictive bounds as of model 6. We conclude that there is a positive effect of the bonus offer on the conditional transition rate during all periods up until week 11. It confirms the first hypothesis. The upper bound increases in time period 5 (week 9-10), but the lower bound is lower than the upper bound for period 4. Hence, we cannot rule out that there is an intensified effect shortly before the bonus offer expires, but we cannot either rule out the opposite. Now consider the third hypothesis: that there is no effect on the transition rate after week 11. Obviously, as more time has passed the dynamic selection is more severe in this time period. We conclude that there actually may be a substantive positive effect on the conditional transition probabilities also after week 11. As our results diverge from the results of previous studies of the re-employment experiment we conclude that previous results based on semi-parametric models heavily rely on the imposed structure.

6 Conclusions

In this article, we have derived and implemented sharp bounds on conditional transitions probabilities under random assignment. We have shown that even under random assignment only instantaneous average treatment effects is point identified. Dynamic treatment effects, which requires that one study conditional transitions probabilities, are in general not point identified. Because our bounds impose no assumptions beyond the random assignment they are not sensitive to arbitrary functional form assumptions made in semi-parametric models. We have also derived bounds under additional weak assumptions such as monotone treatment response and monotone exit rate.

Our re-analysis of data from the Illinois re-employment bonus experiment shows that our bounds are informative about average treatment effects. It also demonstrates that previous semi-parametric methods to deal with dynamic selection heavily rely on structure that is imposed, as it restricts the possible types of dynamic selection. The application further shows that imposing weak assumptions may lead to quite narrowly identified bounds.

The bounds that have been derived in this paper are for a two time period setting. In future research we intend to generalize these bounds into a setting with more than two time periods. We also intend to show how our bounds, that are applicable under random treatment assignment, could be applied under unconfounded treatment assignment. In that case one way to proceed is to create bounds conditional on the covariates (or the propensity score) and then average over the distribution of these covariates.

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Appendix A

Introduce the following notation

$$\begin{aligned} p^1(y_1^1) &= \Pr(Y_1^1 = y_1^1) \\ p^0(y_1^0) &= \Pr(Y_1^0 = y_1^0) \\ p^{11}(y_2^{11}|y_1^1, y_1^0) &= \Pr(Y_2^{11} = y_2^{11}|Y_1^1 = y_1^1, Y_1^0 = y_1^0) \\ p^{00}(y_2^{00}|y_1^1, y_1^0) &= \Pr(Y_2^{00} = y_2^{00}|Y_1^1 = y_1^1, Y_1^0 = y_1^0). \end{aligned}$$

Proof (*Proposition 1.*) First, consider bounds on $\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0] = p^{00}(1|0,0)$. Start with equation (13), average out y_2^{01}, and rearrange gives

$$p^{00}(1|0,0)$$

$$= \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0) - p^{00}(1|1,0)p(1,0)}{p(0,0)}.$$

Use equation (15) to substitute for $p(1,0) = \Pr(Y_1 = 0|D_1 = 0) - p(0,0)$

$$p^{00}(1|0,0) = \tag{A.1}$$

$$\frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{p(0,0)} -$$

$$\frac{p^{00}(1|1,0)(\Pr(Y_1 = 0|D_1 = 0) - p(0,0))}{p(0,0)}.$$

The LP problem then consists of maximizing and minimizing equation (A.1) in $p^{00}(1|0,1)$ and $p(0,0)$. We have

$$\frac{\partial p^{00}(1|0,0)}{\partial p^{00}(1|0,1)} = \frac{-(\Pr(Y_1 = 0|D_1 = 0) - p(0,0))}{p(0,0)} = -\frac{p(1,0)}{p(0,0)} \leq 0,$$

i.e. the objective function we want to maximize/minimize is non-increasing in $p^{00}(1|0,1)$ for all values of $p(0,0)$. Then for the maximization (minimization) problem take the minimum (maximum) value of $p^{00}(1|0,1)$, and notice that $0 \leq p^{00}(1|0,1) \leq 1$ gives

$$max\ p^{00}(1|0,0) = \frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{p(0,0)}, \tag{A.2}$$

with

$$\frac{\partial max\ p^{00}(1|0,0)}{\partial p(0,0)} =$$

$$-\frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{p(0,0)^2}\leq 0,$$

and

$$min\ p^{00}(1|0,0)= \tag{A.3}$$

$$\frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{p(0,0)}-$$

$$\frac{(\Pr(Y_1=0|D_1=0)-p(0,0))}{p(0,0)},$$

with

$$\frac{\partial min\ p^{00}(1|0,0)}{\partial p(0,0)}=$$

$$\frac{(1-\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0))\Pr(Y_1=0|D_1=0)}{p(0,0)^2}\geq 0.$$

So that both for the maximization problem and for the minimization problem take $p(0,0)$ as small as possible. This unknown joint distribution is bounded by the known marginal distributions as given by equation (14) and (15). From the results in Hoeffding (1940) and Fréchet (1951) we have

$$max(\Pr(Y_1=0|D_1=1)+\Pr(Y_1=0|D_1=0))-1,0)\leq p(0,0)\leq \tag{A.4}$$

$$min(\Pr(Y_1=0|D_1=1),\Pr(Y_1=0|D_1=0)).$$

Substitute the minimum value from equation (A.4) into equation (A.2)

$$max\ p^{00}(1|0,0)= \tag{A.5}$$

$$min(1,\frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{max(\Pr(Y_1=0|D_1=1)+\Pr(Y_1=0|D_1=0)-1,0)}),$$

and into equation (A.3)

$$min\ p^{00}(1|0,0)= \tag{A.6}$$

$$max(0,\frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{max(\Pr(Y_1=0|D_1=1)+\Pr(Y_1=0|D_1=0)-1,0)}-$$

$$\frac{\Pr(Y_1=0|D_1=0)-max(\Pr(Y_1=0|D_1=1)+\Pr(Y_1=0|D_1=0)-1,0)}{max(\Pr(Y_1=0|D_1=1)+\Pr(Y_1=0|D_1=0)-1,0)}).$$

Note that, we in equation (A.5) and (A.6) made it explicit that the probability $p^{00}(1|0,0)$ by definition lies between zero and one.

Second, consider bounds on $\mathbb{E}[Y_2^{11}|Y_1^1=0,Y_1^0=0]=p^{11}(1|0,0)$. Start with equation (12), average out y_2^{01}, rearrange, and use equation (14) to substitute for $p(0,1)$ gives

$$p^{11}(1|0,0)= \tag{A.7}$$

$$\frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{p(0,0)}-$$

$$\frac{p^{11}(1|0,1)(\Pr(Y_1=0|D_1=1)-p(0,0))}{p(0,0)}.$$

Then

$$\frac{\partial p^{11}(1|0,0)}{\partial p^{11}(1|1,0)}=\frac{-(\Pr(Y_1=0|D_1=1)-p(0,0))}{p(0,0)}=-\frac{p(0,1)}{p(0,0)}\leq 0,$$

gives using similar reasoning as for the bounds on $\mathbb{E}[Y_2^{00}|Y_1^1=0,Y_1^0=0]$

$$max\ p^{11}(1|0,0)=\frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{p(0,0)}, \tag{A.8}$$

with

$$\frac{\partial max\ p^{11}(1|0,0)}{\partial p(0,0)}=-\frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{p(0,0)^2}\leq 0,$$

and

$$min\ p^{11}(1|0,0)= \tag{A.9}$$

$$\frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)-\Pr(Y_1=0|D_1=1)+p(0,0)}{p(0,0)}$$

with

$$\frac{\partial min\ p^{11}(1|0,0)}{\partial p(0,0)}=$$

$$\frac{-\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)+\Pr(Y_1=1|D_1=0)}{p(0,0)^2}\geq 0.$$

So that again take $p(0,0)$ as small as possible. Substitute the minimum value from equation (A.4) into equation (A.8)

$$max\ p^{11}(1|0,0)= \tag{A.10}$$

$$min(1, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}),$$

and into equation (A.9)

$$min\ p^{11}(1|0,0) = \tag{A.11}$$

$$max(0, \frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 1) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}).$$

Third, consider bounds on $ATETS_2^{11,00}$. After substitutions the only variable appearing in equation (A.1) in the derivations of the bounds for $\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0, D_1 = 1]$ and in equation (A.7) in the derivations of the bounds for $\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0, D_1 = 1]$ is $p(0,0)$. However, in both cases the optimal value of $p(0,0)$ is the minimum value so that the bounds for $\mathbb{E}[Y_2^{00}|Y_1^1 = 0, Y_1^0 = 0, D_1 = 1]$ and $\mathbb{E}[Y_2^{11}|Y_1^1 = 0, Y_1^0 = 0, D_1 = 1]$ can be used directly when constructing bounds for $ATETS_2^{11,00}$. We then have

$$max\ ATETS_2^{11,00} = max\ p^{11}(1|0,0) - min\ p^{00}(1|0,0)$$

and

$$min\ ATETS_2^{11,00} = min\ p^{11}(1|0,0) - max\ p^{00}(1|0,0),$$

which give the results in proposition 1.

Proof (*Corollary 2.*) $p^{11}(1|0,0)$ is point identified if $max\ p^{11}(1|0,0) = min\ p^{11}(1|0,0)$, using proposition 1 this holds if

$$\frac{\Pr(Y_1 = 0|D_1 = 1) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

equals zero, i.e. if $\Pr(Y_1 = 0|D_1 = 0) = 1$. In the same way $p^{00}(1|0,0)$ is point identified if $max\ p^{00}(1|0,0) = min\ p^{00}(1|0,0)$, using proposition 1 this holds if

$$\frac{\Pr(Y_1 = 0|D_1 = 0) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

equals zero, i.e. if $\Pr(Y_1 = 0|D_1 = 1) = 1$. $ATETS_2^{11,00}$ is point identified if both $p^{00}(1|0,0)$ and $p^{11}(1|0,0)$ are point identified, i.e. if $\Pr(Y_1 = 0|D_1 = 1) = 1$ and $\Pr(Y_1 = 0|D_1 = 0) = 1$ hold.

Proof (*Corollary 3.*) The bounds on $ATETS_2^{11,00}$ are informative if they exclude either 1 or -1, which hold if *max* $p^{11}(1|0,0) < 1$, or *min* $p^{11}(1|0,0) > 0$, or *max* $p^{00}(1|0,0) < 1$, or *min* $p^{00}(1|0,0) > 0$ hold. Using proposition 1 it immediately follows that this hold if

$$\frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

smaller than 1, or if

$$\frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 0) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

largen than zero, or if

$$\frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)} -$$

$$\frac{\Pr(Y_1 = 0|D_1 = 1) - max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

larger than zero, or if

$$\frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0) - 1, 0)}$$

smaller than one. This gives the result in the corollary.

Proof (*Proposition 4.*) Assumption 4 and 7 only restrict $p(0,0)$. Following the derivations of the general bounds we then we end up with the equations (A.2),(A.3),(A.8) and (A.9). Moreover, again take $p(0,0)$ as small as possible for both the maximization and the minimization problem. The restrictions as implied by assumption 4 differs whether $p^1(1) > p^0(1)$ or $p^1(1) < p^0(1)$ hold. First consider $p^1(1) > p^0(1)$. From assumption 4 we have

$$\Pr(Y_1^1 = 1) \geq \Pr(Y_1^0 = 1) \Rightarrow \Pr(Y_{i1}^1 = 1) \geq \Pr(Y_{i1}^0 = 1), \forall i$$

and from assumption 7

$$\Rightarrow \Pr(Y_{i1}^1 = 1|Y_{i1}^0 = 1) \geq \Pr(Y_{i1}^1 = 0|Y_{i1}^0 = 1), \forall i$$

which lead to

$$\Rightarrow \Pr(Y_{i1}^1 = 1, Y_{i1}^0 = 1) \geq \Pr(Y_{i1}^1 = 0, Y_{i1}^0 = 1), \forall i$$

and thus

$$\Rightarrow \Pr(Y_1^1 = 1, Y_1^0 = 1) \geq \Pr(Y_1^1 = 0, Y_1^0 = 1).$$

So that expressed in the short hand notation that

$$p^1(1) > p^0(1) \Rightarrow p(1,1) \geq p(0,1). \tag{A.12}$$

In the same way under assumption 7 and assumption 4

$$\Pr(Y_1^1 = 1) \geq \Pr(Y_1^0 = 1) \Rightarrow \Pr(Y_{i1}^1 = 1) \geq \Pr(Y_{i1}^0 = 1)$$

$$\Rightarrow \Pr(Y_{i1}^0 = 0 | Y_{i1}^1 = 0) \geq \Pr(Y_{i1}^0 = 1 | Y_{i1}^1 = 0)$$

$$\Rightarrow \Pr(Y_{i1}^1 = 0, Y_{i1}^0 = 0) \geq \Pr(Y_{i1}^1 = 0, Y_{i1}^0 = 1)$$

$$\Rightarrow \Pr(Y_1^1 = 0, Y_1^0 = 0) \geq \Pr(Y_1^1 = 0, Y_1^0 = 1).$$

So that expressed in the short hand notation that

$$p^1(1) > p^0(1) \Rightarrow p(0,0) \geq p(0,1). \tag{A.13}$$

Using equation (14) and (15) we can rewrite equation (A.12) and (A.13) as

$$p(1,1) \geq p(0,1) \Leftrightarrow p^1(1) - p(1,0) =$$

$$p^1(1) - (p^0(0) - p(0,0)) \geq p^1(0) - p(0,0) \Leftrightarrow$$

$$2p(0,0) \geq p^1(0) - p^1(1) + p^0(0) = 2p^1(0) - 1 + p^0(0) \Leftrightarrow$$

$$p(0,0) \geq -\frac{1}{2} + \Pr(Y_1 = 0 | D_1 = 1) + \frac{1}{2}\Pr(Y_1 = 0 | D_1 = 0) \tag{A.14}$$

and

$$p(0,0) \geq p(0,1) \Leftrightarrow p(0,0) \geq p^1(0) - p(0,0) \Leftrightarrow$$

$$p(0,0) \geq \frac{p^1(0)}{2} = \frac{\Pr(Y_1 = 0 | D_1 = 1)}{2}. \tag{A.15}$$

Combining equation (A.14) and (A.15) and noticing that equation (A.14) is more restrictive than $p(0,0) \geq max(\Pr(Y_1 = 0|D_1 = 1) + \Pr(Y_1 = 0|D_1 = 0)) - 1, 0)$ gives

$$p(0,0) \geq \tag{A.16}$$

$$max(-\frac{1}{2}+\Pr(Y_1=0|D_1=1)+\frac{1}{2}\Pr(Y_1=0|D_1=0),\frac{\Pr(Y_1=0|D_1=1)}{2}).$$

Substituting the minimum value of $p(0,0)$ given by equation (A.16) into equations (A.2),(A.3),(A.8) and (A.9) and using

$$max\,ATETS_2^{11,00}=max\;p^{11}(1|0,0)-min\;p^{00}(1|0,0) \quad (A.17)$$

and

$$min\,ATETS_2^{11,00}=min\;p^{11}(1|0,0)-max\;p^{00}(1|0,0), \quad (A.18)$$

gives the second result in the proposition.

Second consider $p^1(1)<p^0(1)$, by similar reasoning as above under assumption 7 and assumption 4

$$p^1(1)>p^0(1)\Rightarrow p(1,1)\geq p(1,0). \quad (A.19)$$

and

$$p^1(1)>p^0(1)\Rightarrow p(0,0)\geq p(1,0). \quad (A.20)$$

Further derivations as above using equation (14), (15), (A.19) and (A.20) gives

$$p(0,0)\geq$$

$$max(-\frac{1}{2}+\Pr(Y_1=0|D_1=0)+\frac{1}{2}\Pr(Y_1=0|D_1=1),\frac{\Pr(Y_1=0|D_1=0)}{2}),$$

and the first result in the proposition follows by substituting this into equations (A.2),(A.3),(A.8) and (A.9), and using equations (A.18) and (A.17), gives the first result in the proposition.

Proof (*Proposition 5.*) Assumptions 4 and 7 only restrict $p(0,0)$. Again, following the derivations of the general bounds we then we end up with the equations (A.2),(A.3),(A.8) and (A.9), so that again take $p(0,0)$ as small as possible for both the maximization and the minimization problem. First consider $p^1(1)>p^0(1)$. From assumption 4 we have

$$\Pr(Y_1^1=1)\geq\Pr(Y_1^0=1)\Rightarrow\Pr(Y_{i1}^1=1)\geq\Pr(Y_{i1}^0=1),\forall i$$

and from assumption 8 we have

$$\Pr(Y_{i1}^1=1)\geq\Pr(Y_{i1}^0=1)\Rightarrow Y_{i1}^1\geq Y_{i1}^0,\forall i$$

further

$$Y_{i1}^1\geq Y_{i1}^0,\forall i\Rightarrow\Pr(Y_1^1=0,Y_1^0=0)=\Pr(Y_1^1=0)=\Pr(Y_1=0|D=1)$$

or in the short hand notation that

$$p^1(1) > p^0(1) \Rightarrow p(0,0) = \Pr(Y_1 = 0|D = 1) \tag{A.21}$$

By similar argument under assumption 4 and assumption 8

$$p^1(1) < p^0(1) \Rightarrow p(0,0) = \Pr(Y_1 = 0|D = 0). \tag{A.22}$$

Substituting A.21 and A.22 for $p(0,0)$ into equations (A.2),(A.3),(A.8) and (A.9), and using equations (A.18) and (A.17), gives the result in the proposition.

Proof (*Proposition 6.*) First, consider $p^{00}(1|0,0)$. In deriving the general bounds we have after substitutions equation (A.1). It still holds here so that for the maximization (minimization) problem we wish to take the minimum (maximum) value of $p^{00}(1|0,1)$. In comparison with the general bounds assumption 9 places the additional restriction that $p^{00}(1|1,0) \geq p^{00}(1|0,0)$, so that for the maximization problem we have $p^{00}(1|0,1) = p^{00}(1|0,0)$, and for the minimization problem we have $p^{00}(1|0,1) = 1$. It further means that equation (A.6) still holds for the minimization problem, and for the maximization problem substituting for $p^{00}(1|0,1) = p^{00}(1|0,0)$ gives

$$p^{00}(1|0,0) =$$

$$\frac{\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 0)}{p(0,0)} -$$

$$\frac{p^{00}(1|0,0)(\Pr(Y_1 = 0|D_1 = 0) - p(0,0))}{p(0,0)},$$

and thus after rearranging

$$max\ p^{00}(1|0,0) = \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0). \tag{A.23}$$

Second, consider $p^{11}(1|0,0)$. By similar reasoning equation (A.7) still holds, and for the maximization problem $p^{11}(1|0,1) = p^{11}(1|0,0)$ and for the minimization problem we have $p^{11}(1|0,1) = 1$. So that equation (A.11) still holds, and for the maximization problem substituting for $p^{11}(1|0,1) = p^{11}(1|0,0)$ gives

$$p^{11}(1|0,0) =$$

$$\frac{\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)\Pr(Y_1 = 0|D_1 = 1)}{p(0,0)} -$$

$$\frac{p^{11}(1|0,0)(\Pr(Y_1=0|D_1=1)-p(0,0))}{p(0,0)}.$$

and thus after rearranging

$$max\ p^{11}(1|0,0)=\Pr(Y_2=1|D_1=1,Y_1=0). \tag{A.24}$$

Combining equation (A.6),(A.11),(A.23) and (A.24),and using equations (A.18) and (A.17), gives the result in the proposition, gives the result in the proposition.

Proof (*Proposition 9.*) From the proof of proposition 6 we have that assumption 9 restricts $p^{00}(1|0,0)$ and $p^{11}(1|0,0)$. The proof further shows that after imposing assumption 9 $p(0,0)$ do not appear for the maximization solutions for $p^{00}(1|0,0)$ and $p^{11}(1|0,0)$. So that we have *max* $p^{00}(1|0,0)$ from equation (A.23) and *max* $p^{11}(1|0,0)$ from equation (A.24). Further assumption 9 do not restrict $p(0,0)$. However, assumption 4 and assumption 7 restrict $p(0,0)$, that is the same assumption as in the derivations of proposition 4. We can therefore use the minimization solutions for *min* $p^{00}(1|0,0)$ and *min* $p^{11}(1|0,0)$ from the proofs for proposition 4. Combining these results and noting that minimum solution for $p(0,0)$ depends on whether $\Pr(Y_1=1|D_1=1)<\Pr(Y_1=1|D_1=0)$ or $\Pr(Y_1=1|D_1=1)<\Pr(Y_1=1|D_1=0)$ give the result in the proposition.

Proof (*Proposition 10.*) Following similar reasoning as in the previous proof gives the result.

Appendix B

Proposition 7 (Bounds under positive treatment response in period2) *Suppose assumption 3 and assumption 5 holds. Then*

$$max(0, max(0, \frac{A-[1-\Pr(Y_2=1|D_1=1,Y_1=0)]\Pr(Y_1=0|D_1=1)}{A})-$$

$$min(1, \frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{A}))$$

$$\leq ATES_2^{11,00} \leq$$

$$max(0, min(1, \frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{A})-$$

$$max(0, \frac{A-[1-\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)]\Pr(Y_1=0|D_1=0)}{A}))$$

Proposition 8 (Bounds under negative treatment response in period2) *Suppose assumption 3 and assumption 6 holds. Then*

$$min(0, max(0, \frac{A-[1-\Pr(Y_2=1|D_1=1,Y_1=0)]\Pr(Y_1=0|D_1=1)}{A})-$$

$$min(1, \frac{\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)\Pr(Y_1=0|D_1=0)}{A}))$$

$$\leq ATES_2^{11,00} \leq$$

$$min(0, min(1, \frac{\Pr(Y_2=1|D_1=1,Y_1=0)\Pr(Y_1=0|D_1=1)}{A})-$$

$$max(0, \frac{A-[1-\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)]\Pr(Y_1=0|D_1=0)}{A}))$$

Proposition 9 (Bounds ME, MTR response and positively correlated shocks) *Suppose assumption 3, 4, 7 and 9 holds. Define* $A \equiv max(-\frac{1}{2} + \Pr(Y_1=0|D_1=0) + \frac{1}{2}\Pr(Y_1=0|D_1=1), \frac{\Pr(Y_1=0|D_1=0)}{2})$ *and* $B \equiv max(-\frac{1}{2} + \Pr(Y_1=0|D_1=1) + \frac{1}{2}\Pr(Y_1=0|D_1=0), \frac{\Pr(Y_1=0|D_1=1)}{2})$ *.Then if* $\Pr(Y_1=1|D_1=1) < \Pr(Y_1=1|D_1=0)$

$$max(0, \frac{A-[1-\Pr(Y_2=1|D_1=1,Y_1=0)]\Pr(Y_1=0|D_1=1)}{A})-$$

$$\Pr(Y_2=1|D_1=0,D_2=0,Y_1=0)$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) -$$

$$max(0, \frac{A - [1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{A})$$

and if $\Pr(Y_1 = 1|D_1 = 1) > \Pr(Y_1 = 1|D_1 = 0)$

$$max(0, \frac{B - [1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{B}) -$$

$$\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) -$$

$$max(0, \frac{B - [1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{B})$$

Proposition 10 (Bounds ME, MTR response and single shocks) *Suppose assumption 3, 4, 8 and 9 holds. Then if* $\Pr(Y_1 = 1|D_1 = 1) < \Pr(Y_1 = 1|D_1 = 0)$

$$max(0, \frac{\Pr(Y_1 = 0|D = 0) - [1 - \Pr(Y_2 = 1|D_1 = 1, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 1)}{\Pr(Y_1 = 0|D_1 = 0)}) -$$

$$\Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)$$

and if $\Pr(Y_1 = 1|D = 1) > \Pr(Y_1 = 1|D_1 = 0)$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)$$

$$\leq ATES_2^{11,00} \leq$$

$$\Pr(Y_2 = 1|D_1 = 1, Y_1 = 0) -$$

$$max(0, \frac{\Pr(Y_1 = 0|D = 1)}{\Pr(Y_1 = 0|D_1 = 1)} -$$

$$\frac{-[1 - \Pr(Y_2 = 1|D_1 = 0, D_2 = 0, Y_1 = 0)]\Pr(Y_1 = 0|D_1 = 0)}{\Pr(Y_1 = 0|D_1 = 1)})$$

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