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Essays on welfare dependency and the privatization of welfare services

Kajsa Hanspers

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Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: (with Matz Dahlberg and Eva Mörk) This paper investigates if mandatory activation programs for welfare recipients have effects on welfare participation, employment and disposable income. In contrast to earlier studies, we are able to capture both entry and exit effects. The empirical analysis makes use of a Swedish welfare reform in which the city districts in Stockholm gradually implemented mandatory activation programs for individuals on welfare. On average, we find that mandatory activation of welfare recipients increases employment as well as disposable income. However, the sizes of the estimated effects must be considered relatively small. There are some indications of larger effects for single-headed households.

Essay 2: (with Karin Edmark) This study tests whether individuals who grow up with parents on welfare benefits are themselves more (or less) likely to be welfare recipients as young adults, compared to individuals who grow up in non-welfare households. We use the sibling difference method to identify causal effects separately from the effects of correlated factors. While a descriptive analysis reveals a fairly high positive intergenerational correlation, especially in the late teens and conditional on a large set of household level factors, the sibling analysis provides no support for a causal effect of parents' welfare benefit receipt on children's future welfare use.

Essay 3: Spending on health care makes up a large proportion of the GDP in Sweden as in most developed countries. The introduction of private alternatives and more competition in the market have been advanced as a way to increase efficiency and patient choice, but the previous literature contain conflicting evidence regarding the quality impact of market reforms in health care. This paper examines the impact on health care quality of reforms aimed at introducing more competition in the market for primary health care. The analysis is performed using cross-county variation in private supply and the

financial incentives provided by the health care organization in Sweden 1998 to 2010. The analysis separates between measures of quality that are easily observed by patients and measures intended to capture medical quality, which is more difficult for patients to assess. The results indicate that the reforms intended to increase competition do not seem to improve the overall quality of primary health care. Increased competition in the market is associated with more visits to the primary health care, but otherwise, the results give no support for effects on availability or patient satisfaction. The results on clinical quality do not indicate any consistent evidence of any impact of competition either.

Essay 4: This paper investigates differences in worker absence between privately and publicly employed workers in Swedish primary schools and preschools. Using hive-offs, i.e., ownership changes from a public to a private owner, I analyze the effects of private versus public employment on sickness absence, parental leave, and temporary leave for care of children. The empirical analysis suggests a negative effect on sickness absence in preschools, but not in primary schools. For parental leave and temporary parental leave for care of children, no significant effects are found in either industry. The fact that parental leave, which is voluntary to a greater extent than sickness absence, seems to be unaffected by ownership opposes the argument of stronger incentives for attendance in the private sector. However, the finding of a negative effect on sickness absence suggests that private employment may be associated with better work conditions, at least for workers in the preschool sector.

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Stockholm, August 2013.
Kajsa Hanspers

Contents

Introduction

This thesis consists of four self-contained essays in empirical economics. They can be divided into two general topics: the first two essays treat labor market programs for welfare recipients and the intergenerational transmission of welfare dependency and the last two essays deal with the effects of private ownership and competition in the provision of welfare services. I discuss the two topics in each section below.

Prevention and transmission of welfare dependency

An important objective of the welfare state is to guarantee all inhabitants a reasonable standard of living. To this end, the state insures against loss of income in times of unemployment, sickness or in other situation when individuals are unable to support themselves. At the same time, it is important that the social security systems do not distort the incentives, so that labor supply is negatively affected. The challenge of legislators is hence to find ways to provide an adequate safety net, without distorting the incentives to work. One way of achieving low participation rates in the unemployment insurance system is to increase the disutility received from supporting oneself by unemployment benefits. Limiting the duration of benefit payments, monitoring combined with sanctions, and conditioning benefits on participation in active labor market programs, are examples of policies aimed at counteracting the adverse incentive effects without reducing the replacement rate. Not all individuals are entitled to unemployment insurance. In Sweden, individuals must be a member of the unemployment insurance and have worked for at least six months during a year to qualify for unemployment insurance.

Welfare assistance is only directed to individuals or households unable to support themselves through work or the social insurance system. It hence constitutes the final safety net, for instance for unemployed individuals who are not qualified for unemployment insurance benefits.

In order to reduce welfare participation, US and many European countries have implemented active labor market policies, activation programs, directed to unemployed welfare recipients. The first essay (joint with Matz Dahlberg and Eva Mörk) studies the impact of the introduction of mandatory activation programs directed to unemployed welfare recipients in Sweden. We

investigate if the policies for activation directed to welfare recipients led to a reduction in welfare dependency, increased labor supply and how the policies affected the economic well-being. The program entailed more effort from welfare recipients as it demanded daily, or almost daily, attendance at a job center as well as participation in other work-related activities. The intention of the program was to increase search intensity and the human capital of participants and hence increase the probability to find employment. Since the participation in the programs was time consuming, they also worked as a screening device by separating those in need of assistance from those who were not.

The programs were gradually implemented in city districts of Stockholm between 1998 and 2004. We use the gradual implementation of these programs to identify the effects of the introduction of the programs in a difference-in-differences setup. In the empirical analysis we find positive effects of activation on employment and disposable income, but the effects are relatively small. We also investigate whether the effects were different for vulnerable groups. Of these, we find small effects for Swedish born and single headed households.

While policies aimed at reducing welfare dependency can be effective for the targeted individuals, it is also possible that the policies have long run effects. This could, for example, be the case if the behavior of individuals is transmitted to future generations. Intergenerational mobility has been an interest for researchers for a long time. For example, the literature has documented intergenerational transmission of educational attainment, earnings and poverty from parents to children (for overviews of the literature see e.g. Solon, 1999 and Black and Devereux, 2011).

Essay 2 (co-authored with Karin Edmark) investigates the intergenerational transmission of welfare receipt in Sweden during the 1990s. As the previous literature has shown, children who grow up in poor families are more likely to end up in poverty as adults (see e.g. Duncan et al., 1998 and Airio et al., 2005). Our objective is to find out how parents' participation in the welfare benefit system affects children's outcomes as young adults. The welfare participation of parents may affect the welfare dependency of children in several ways. First, parent's receipt of welfare may affect the children's attitude towards welfare participation. Growing up in a household with welfare benefits can be associated with a "welfare stigma" (Moffit, 1983) and it is possible that children of welfare receiving parents become accustomed with the stigma and welfare use hence become associated with less disutility. Second, children of welfare receiving parents have the opportunity to learn how the welfare system works. Their information advantage means that the cost of welfare participation is lower. Finally, children of parents with welfare benefits may be affected by the poor labor market attachment of parents. Parents who depend on welfare benefits for their livelihood are likely to have a weaker attachment to the labor market, which may

affect children as they do not get to learn from the labor market experience of their parents and have fewer informal job contacts.

To establish the causal link between the welfare dependency of parents and their children is difficult since the parents' welfare history is likely to be correlated with other factors, such as human capital, attitude towards work and emotional well-being, which are also likely to have an impact on the future outcomes of children. We try to identify a causal effect by comparison of siblings, where one lived in the household when the parents received welfare benefits and the other when the parents did not. The sibling comparison approach is useful as it allows researchers to account for unobservable household characteristics, but the approach requires that one of the siblings is untreated. Hence, our approach only allows us to identify the effect in families which received welfare benefits during a limited time period. The results should therefore be interpreted as effect of temporary welfare receipt. In the empirical analysis we find a large positive correlation in welfare receipt between parents and children. The correlation persists even after controlling for a large set of household level characteristics. When we control for unobservable household characteristics in the sibling analysis we find no support of a causal effect of parents' welfare benefit receipt on children's welfare benefits use as young adults. The positive correlation that we observe to begin with is hence due to unobservable factors.

The implications of competition private ownership in welfare service production

The second part of this thesis analyzes the market for welfare services, which have been provided primarily by the public sector in many countries. In the past few decades the predominant role of the public sector in the production of welfare services has been challenged. During the 1980s and 1990s the ideas of New Public Management (NPM) gained foothold in Sweden as well as internationally. A central feature of the NPM agenda was the introduction of market oriented reforms in the public sector. The proponents of the NPM reforms argued that private provision and competition would lead to more efficiency in production, and thus better or, equal, quality at a lower cost. The ideas had large influence in Sweden which traditionally has had a large public welfare sector. For example, the deregulation of primary and secondary schools is considered one of the most far-reaching in the world.

Competition is generally perceived by economists as a way of achieving an efficient outcome. Firms facing competition from other firms will attempt to lower their prices, increase quality and engage in innovation to get a competitive advantage over other firms. However, there are some notable exceptions when the market forces fail to achieve an efficient outcome. In situa-

tions like this, the negative aspects of market regulation may be seen as worthwhile to counteract the drawbacks of a free market. For example, regulation may be preferred in markets with severe information asymmetries, problems of externalities or natural monopolies. The information asymmetries present in the market for welfare services is one of the arguments that can motivate government intervention. Markets for publicly funded welfare services are often labeled as quasi-markets. The typical feature of quasi-markets is that providers cannot compete in terms of prices, since prices are regulated or since goods or services are funded by the government. Since providers in a quasi-market cannot compete in terms of prices, they must try to attract customers in other ways. One way is to supply better services. For example, empirical evidence from the school sector indicate that increased competition from privately owned schools had a positive effect on educational quality (Böhlmark and Lindahl, 2012). However, the conditions in the market and the nature of the services are crucial for whether competition is beneficial for the quality provided. As argued by Le Grand and Bartlett (1993), transparent information is an important factor for competition to come to effect in a quasi-market. School quality may be difficult to observe in advance, but in contrast to education, health care quality can be difficult to observe even after consuming the services. Health care is therefore often mentioned as a typical example of what is labeled as credence goods. Credence goods entail services where the providers of services have expert knowledge which consumers have small opportunities to match. Providers can take advantage of their information advantage to provide unnecessarily high quality at a higher expense (for instance car repairs) or to provide less quality than needed to gain profits. If the patient cannot observe the quality of the health care they receive, the providers' incentives to compete in terms of quality are low. It is hence uncertain if competition will yield a better provision of quality. There is also a risk that the problem is enhanced when private providers enter the market, as they may be more motivated to slack on quality than public firms. As pointed out by Hart, Schleifer and Vishny (1997), private firms have stronger motives to cut costs as they are profit generating. Even if the quality provided in private and public is similar, the information asymmetries may restrict the scope for quality competition.

Essay 3 deals with the effect of competition in the market for primary health care services, which has been subject to deregulatory reform in the past two decades. Unlike in many other countries, primary health care has primarily been organized by the public sector in Sweden since the 1970s. Since the 1990s it has been subject to several reforms than have led to an increasing share of privately provided services. Apart from a short period of free entry in 1994–1995, the increase in private provision has mainly occurred through out-sourcing. More recently out-sourcing has been replaced by patient choice combined with reimbursement based on the choices of patients in order to attain stronger incentives for quality competition among

providers. The empirical analysis utilizes the variation across counties in the implementation of these reforms to investigate the effects of competition and private provision on primary health care quality.

The results do not show any striking evidence of an effect of competition. Throughout the analysis a positive effect on the number of doctor's visits in primary health care is found, but since this finding is not supported by other measures of quality it is difficult to draw the conclusion that there is a general impact on health care quality. Neither quality aspects for which information asymmetries are small, such as availability, or measures of quality that are more difficult for patients to assess seem to be affected by competition.

The theme of essay 4 is related to the topic of essay 3, but instead of competition, the essay focuses on the effects of ownership in markets for welfare services and instead of focusing on the services per se, its focus is on the outcomes of employees. In this essay I investigate the effect on worker absence of private ownership in welfare services.

Private provision of welfare services has increased rapidly in Sweden as a consequence of the introduction of reforms in the 1990s, which allowed private providers into the market. In some cases privatization has occurred through hive-offs, i.e. situations in which a public units is taken over by a private firm. I use the variation in ownership caused by hive-offs in preschools and primary schools to estimate the effects on sickness absence, parental leave, and temporary parental leave for care of sick children. The hive-offs provide a nice source of variation in ownership, since the ownership switch is not initiated by the employees themselves. By using hive-offs, the problem of workers self-selecting into private ownership can be circumvented and hence the variation in ownership caused by these hive-offs allow me to account for unobserved worker characteristics that may be correlated with the decision to work in the private sector, as well as their absence behavior.

While a number of studies investigate the effects of privatization on wages, the effects on worker effort and absence behavior is an unexplored area. Investigating the effects of ownership contributes to a better understanding of how privatization can affect costs and quality in welfare service production. It also contributes to the literature on the incentives in private and public employment. Earlier empirical studies on the effects of private/public ownership on worker outcomes have found that ownership can matter for wages, which also suggests that worker productivity is affected (see e.g. Pendelton, 1997). The theoretical literature suggests that worker effort is likely to be higher in the private than the public sector due to the differences in the incentives to exert high effort. The property rights literature discusses a number of reasons to why the incentives to efficiency in private firms are stronger. First, private ownership is usually concentrated to a few owners, whereas public ownership is dispersed among the whole population. Second,

while private owners are entitled to residual rights, public owners cannot access their share of financial gains directly. Finally, owners of private firms are entitled to make decisions about the firm whereas the political process makes it difficult for voters to directly influence the performance of public firms. Together, the concentrated ownership, residual rights, and the larger possibilities to monitor the operation of a firm makes it easier and more worthwhile for private owners to optimize the performance of their firm.

If private ownership involves stricter incentives for worker effort, workers are likely to respond to the incentives by reducing voluntary absence. For parental leave, there is usually a possibility to divide the responsibility with the other parent. If absence can result in foregone promotions and wage increases or even be sanctioned by discharge, voluntary absence as parental leave could be expected to decrease. Theoretically, the direction of the effect on sickness absence is ambiguous. If sickness absence is voluntary to some extent, it may also be negatively affected. However, ownership might also affect the work environment. On the one hand, higher demands on efficiency could mean that private firms invest in a better work environment and hence reduce sickness absence. On the other hand, demands on high performance could also translate into a poor work environment, with greater workloads and more stress, which could increase sickness absence. The results on parenal leave and temporary parental leave does not support the hypothesis of an incentive effect of private employment. For these outcomes no robust effect can be found. The results on sickness absence in preschools indicate that differences in the work environment could matter. However, these findings are not supported by the corresponding results for primary schools, where no effect on sickness absence is found.

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Essay 1: Mandatory Activation of Welfare Recipients – Evidence from the City of Stockholm[1]

Coauthored with Matz Dahlberg and Eva Mörk

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1 Introduction

Starting in the late 1980's, both the US and several countries in Europe have experienced a shift in policies targeted at welfare recipients. Partly motivated by increased caseloads in years of economic downturn, focus has shifted from passive benefits towards increased use of mandatory activation programs, some taking the form of public employment programs. In the US, the shift was made definite with the implementation of the PRWORA legislation in 1996[2], but already before 1996, a number of states, through state waivers, had implemented different types of mandatory welfare-to-work programs (see, e.g., Hamilton, 2002). Several European countries, such as the UK, France, Germany, The Netherlands, Sweden and Denmark followed the US path and implemented a number of legislative changes that introduced compulsory work-for-benefit programs, in many countries especially targeted at young people (see Lødemel and Trickey, 2000, for an overview of European workfare programs during the 1990s). From a Scandinavian perspective, conditioning benefits on participation in different activities is nothing new, but active labor market programs have long been one of the cornerstones in the Scandinavian welfare states.[3] However, these programs have mainly been targeted at unemployed individuals receiving unemployment insurance benefits. What was new during the 1990's was that these types of programs also to a larger extent were targeted at social assistance recipients.

The idea behind mandatory activation programs is that conditioning welfare on requirements to work or to engage in work-related activities, such as education, training or job search, will work as a screening device, separating the truly needy from those who are not, and at the same time, the activation itself will increase the productivity of those who are unable to get a job.[4] Besley and Coate (1992) formalize the mechanism behind activation requirements and show theoretically that activation may have both short- and long-run effects on welfare caseloads.[5]

[2] For good overviews of this reform, see Blank (2002), Grogger and Karoly (2005), and Moffitt (2007).

[3] Organizations like EU and OECD have been very positive towards the Scandinavian type of programs.

[4] The idea has a long tradition in societal program design dating back to, e.g., the English Poor Laws, according to which "no able-bodied person was to receive money or other help from the Poor Law authorities except in a workhouse".

[5] A theoretical model in the same vein, where mandatory activation programs reduce both welfare use and welfare payments, is presented in Grogger and Karoly (2005).

In this paper, we will use quasi-experimental data from a Swedish welfare reform in order to empirically investigate to what extent conditioning welfare on participation in work-related activities reduces the number of people on welfare. As opposed to earlier studies, we are able to observe both entry and exit effects, although we will not distinguish between the two.[6] In addition, we will also analyze the effects on employment and economic well-being. Through the reform, mandatory activation programs were implemented gradually in the city districts in Stockholm over the period 1998 to 2004. We will use this gradual implementation in a difference-in-differences setup. Using data from city districts within a single local labor market has large advantages, since it makes it possible to control for macroeconomic shocks, something that is difficult when using, e.g., data on U.S. states. The reform was "clean" in the sense that the activation programs for welfare recipients were implemented in isolation, hence not accompanied by, e.g., financial incentives, like the EITC, or time limits. Finally, having access to very rich individual-level register data (on all individuals living in Stockholm over the period 1993–2003), we can also investigate whether the effects are heterogeneous with respect to, e.g., age, country of birth and marital status.

Most of the available empirical evidence of the effect of activation programs comes from randomized experiments conducted in North America. Hamilton (2002) summarizes the evidence from 11 projects that were implemented under the National Evaluation of Welfare-to-Work Strategies (NEWWS) Program, whereas Grogger and Karoly (2005) summarize lessons from the US both before and after PRWORA.[7] Among the programs that involved mandatory activation, there existed two types of programs, those with an employment-focused approach and those with an education-focused approach. In addition, there were also some programs that applied mixes of the two approaches. The evidence from this research indicates that the programs did increase employment and decrease welfare benefits among participants, but had no net effect on the participants' economic well-being. According to Grogger and Karoly, the average reduction in welfare use was 5.1 percentage points. Also, programs that emphasized short-term job search assistance and encouraged participants to find jobs quickly had positive effects on employment already after one year, whereas programs that emphasized longer-term skill-building activities took some time to have effects. After five years, however, the second type of program had caught up with

[6] Persson and Vikman (2013), in a project that was initialized after the first WP-version of the current paper, study entry and exit effects of the same activation programs that we analyze in this study. They find that the reduction in caseloads of welfare participants was mainly driven by an increase in welfare exits. However, for people younger than 26, they also find entry effects.

[7] See also Bloom and Michalopoulus (2001), who present an overview of the results from 29 welfare reform initiatives in the US and Canada.

the job-first programs (see Hamilton, 2002). Most successful were the programs that combined the two approaches.

Empirical evidence from European countries is much scarcer, which is unfortunate given the big differences between the working of the American and the European labor markets.[8] Furthermore, to our knowledge there exists no randomized trials in Europe, but the existing studies rely on propensity score matching or other econometric techniques to handle selection into programs. Below we summarize the available European evidence.[9]

Using data from the Danish municipality of Aarhus, Bolvig et al. (2003) analyze how active social policy in the form of employment or training programs affect exit rates out of welfare as well as the duration of subsequent employment spells. Their duration analyses point at positive post-program effects of employment programs (different types of subsidized employment in a private or public enterprise), whereas training programs have lock-in effects as well as negative post-program effects. The lock-in effects seem more present for women than for men. In addition, employment spells resulting from program participation seem to be shorter than other employment spells. Also relying on Danish data, Graversen and Jensen (2010) estimate the relative employment effect of private sector employment programs for welfare benefit recipients compared to other programs applied for this group. Also, they allow for heterogeneous treatment effects. They do not find any average effects of these private sector employment programs, but do find substantial heterogeneity. In particular, those most likely to participate in a program are those that are less likely to benefit from these programs, which implies that there is scope for improved matching.

Dahl (2003) analyzes the Norwegian workfare program for welfare recipients. These programs typically take the form of "work-first" where the educational component is lacking to a large extent. He finds that it is those recipients with weakest attachment to the labor market that typically are assigned to these programs and that the programs do not have any effects on employment or the self-sufficiency of the participants. Lorentzen and Dahl (2005) instead focus on the active labor market programs ran by the state, as opposed to programs ran by the municipality, and find that the selection for these programs are typically positive, and that the programs seem to increase employment among participants receiving social assistance.

Hohmeyer and Wolff (2012) investigate the effects on regular employment of the German "one-euro jobs". Implemented in 2005, these jobs are temporary (often 6 months or less), part-time jobs that should be in the public's interest and not compete with regular jobs. Caseworkers can assign

[8] Of course, also European countries differ from each other and so does the type of activation programs that are in place in the different countries.

[9] There are also some earlier Swedish studies with less credible identification strategies, see, e.g. Giertz (2004) and Milton and Bergström (1998). These studies find no, or very tiny, effects of activation on caseloads.

welfare recipients to these jobs, and if refused, the individual will face benefit sanctions. Participants in one-euro jobs do not receive regular wages. Instead they continue to receive their welfare benefits, but with an additional one or two euro per hour (thus the name). One-euro jobs are the most important active labor market policy for welfare participants in Germany with 600,000 or more participants per year. Using propensity score matching, Hohmeyer and Wolff find that one-euro-jobs seem to have initial lock in effects, but that after 20 months there are some positive effects, especially for West German women.[10] There are however no employment effects for men from East Germany. For participants younger than 25, lock-in effects dominate and there are no long-run positive effects. Finally, the effects are larger for participants with a weaker connection to the labor market. Hohmeyer (2012) finds, analyzing the same data, that lock-in effects are largest for jobs with longer planned duration. However, the better short-term performance of shorter programs does not make up for the poorer post-program effects.

Huber et al. (2011) study the German Hartz IV-reform, of which the one-euro-jobs made up the largest component, using both register and survey data. They separate between one-euro-jobs, short basic training including basic job-search assistance, work tests and minor adjustment of generals skills, and further short training lasting up to three months focusing on occupation related skills. The only component for which they find average positive effects on employment is short training. When looking for heterogeneous effects, they find that there are indeed some groups for which the programs work better. For example, one-euro jobs seem to work for West German males that are not lone parents and/or migrated to Germany. The findings of Huber et al. differ somewhat from those found by Hohmeyer and Wolff (2012), something that could be explained by the fact that Hohmeyer and Wolff focus on early participants and consequently have a longer follow-up period, whereas Huber et al. look at the programs when some time has passed since the implementation of the German welfare reform, since they argue that there are some data problems with the early participants.

All of the above mentioned studies have focused on the effects for program participants, i.e. on the likelihood that a welfare recipient leaves welfare as a consequence of the mandatory activation program. Beside affecting exits from welfare, mandatory activation can also potentially affect entry rates into welfare. Some individuals may choose not to apply for welfare given that it is conditioned on participation in programs. In fact, Grogger et al. (2003) and Moffitt (2007), among others, argue that much of the decline in welfare use and caseloads following the U.S. welfare reform in 1996 was due to decreased entry rather than to increased exit. Forslund and

[10] The control group of matched welfare recipients that did not have a one-euro job was still obliged to actively search for jobs with the assistance of the public employment service office.

Skans (2006) and Rosholm and Svarer (2008) show that such threat-effects of active labor market programs do exist for young unemployed in Sweden and Denmark. Also, relying on a randomized experiment in Kentucky where unemployment insurance claimants were randomized into mandatory employment and training services, Black et al. (2003) find that people in the treatment group leave UI upon receiving notice of the programs, rather than during or after participation in these programs. Hence, entry effects are likely to be considerable, and not taking these into account risk underestimating the effects of mandatory activation programs. As mentioned above, we investigate the total effects on caseloads, thus considering both entry and exit effects.

We find that the activation programs implemented in Stockholm from 1998 and onwards increased employment (defined by being employed in November and by labor income) and increased the individuals' disposable income, but that both these effects are small. However, we do not find any statistically (or economically) significant effects on welfare. When examining the effects for different sub-groups, we find that the programs seem to work better for Swedish born, and for single headed households.

The remainder of the paper is organized as follows: the next section describes the Swedish welfare system and the activation programs in Stockholm. In section 3, the data used are described, and in section 4, we present the empirical strategy that is employed. The main results are presented in section 5, results for vulnerable groups are presented in section 6 and finally section 7 summarizes the paper and concludes.

2. Welfare and workfare in Sweden

Sweden was early in building up a system for social security. In fact, already in 1913 Sweden, as the first country in the world, introduced a law on public pension that provided a guaranteed pension for everyone over the age of 67. The Swedish social security system is often considered to be one of the most extensive and generous systems in Western welfare states. During the beginning of the twentieth century, the traditional poor relief was to a large extent replaced by an extensive, mostly publicly financed, social insurance system, including e.g. an occupational accident insurance, unemployment insurance, a national old age pension, universal health coverage insurance, and an income related sickness benefit. However, in order to qualify for unemployment insurance benefits, the individuals must be members of an unemployment insurance fund and have worked at least six months during the year preceding the unemployment spell. This implies that individuals without earlier labor market experiences do not qualify for benefits. If needing financial aid they instead have to turn to the municipalities applying for social assistance, that is, a means tested benefit making up the final safety net in Sweden. The Social Services Act states that all Swedish and foreign citizens living in Sweden have the right to apply for social assistance in the absence of other means of economic support. The benefit level should ensure a reasonable standard of living, and depends on the number and age of household members. The municipalities are free to set the exact level as long as it excesses the minimum level set by The National Board of Health and Welfare. As opposed to the situation in many other countries (e.g., the US and UK), receiving welfare is not dependent on having children. However, in order to be eligible for welfare benefits, all other means, including savings and valuable assets, must be exhausted, and it is assigned at the household level.

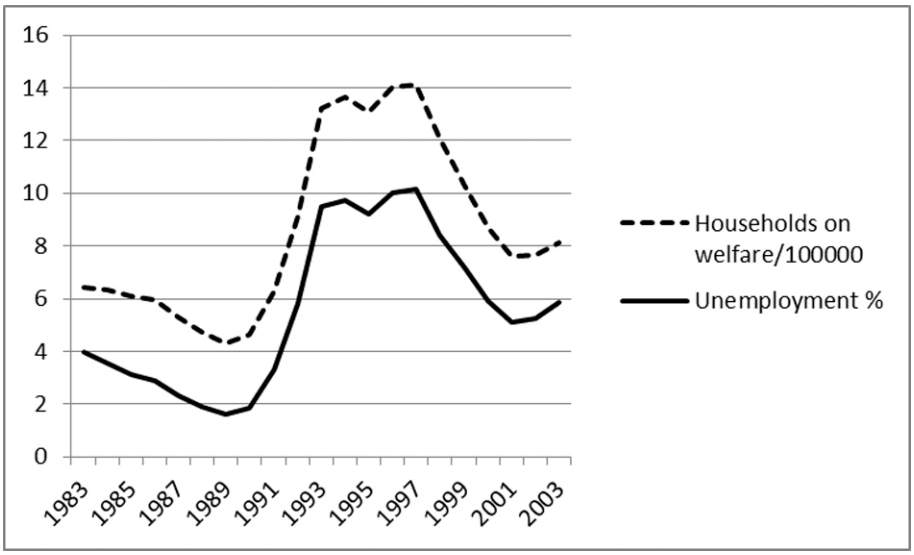

Figure 1. Unemployment and caseloads, 1983–2003.

Source: Statistics, Sweden.

Figure 1 shows the number of households receiving social assistance, as well as the unemployment level in Sweden during the studied period. As is clear from the figure, caseloads follow the unemployment level over time. Consequently, during the good economic years in the 1980s, those in need of social assistance due to unemployment were relatively few. According to the Social Services Act they were supposes to be "available for work" in order to receive financial assistance, which was defined by The National Board for Health and Welfare as searching for jobs and not turning down any "suitable offers". A "suitable offer" was perceived as a job matching the skills and qualifications of the individual and in line with collective agreements. Typically, unemployed welfare recipients were also supposed to register at the Public Employment Services (PES) and take part in suitable labor market programs.[11]

The economic recession of the 1990s and the accompanying rise in unemployment led to financial distress for the municipalities that experienced increased costs for social assistance, as well as diminishing tax revenues. Probably because of the rising caseloads, the municipalities expressed the view that the PES was not doing enough for job-seekers that were not entitled to benefits from the unemployment insurance. As a consequence, the

[11] Sweden has a long tradition of active labor market programs, where the responsibility for labor market policies lays at the central level. See, e.g., Calmfors et al. (2001) for a description of the Swedish labor market.

municipalities started to build up their own active labor market programs and conditioned receiving social assistance on participation in these programs.[12] The right to require participation in activation programs by welfare recipients was formally introduced by a change in the Social Services Act in 1998.[13] The new law made it possible for municipalities and city districts to demand participation in work-related activities, such as internships and supervised job searches, in return for welfare benefits. In this paper, we will focus on the activation programs implemented between 1998 and 2003 in the city districts of the city of Stockholm. The city of Stockholm is by far Sweden's largest municipality. It makes up the central part of a much larger labor market area. Next, we will turn to a description of the programs in place in Stockholm.

During the period studied (1993–2003), the municipality of Stockholm was divided into 18 city districts (see Map in Appendix A).[14] The city districts are responsible for the majority of the municipality's services within their geographical areas.[15] However, it is still the municipal council that sets taxes and allocates funds between the city districts.[16] In addition, the municipal council defines overall goals through their guidelines. The political composition in the District Councils is equivalent to that of the Municipal Council, which is elected every fourth year. Hence, there are no elections at the city district level, and the political majority is the same all over the city of Stockholm.

The earliest examples of activation programs in Stockholm are from 1998 and 1999, when Rinkeby and Skärholmen introduced programs intended to enroll all unemployed welfare recipients in job searching activities.[17] They were followed by Kista and Farsta in 2001 and by many other city districts since then. In fact, since 2004 there have been mandatory activation programs in force in all city districts.

[12] For a discussion of the welfare system during the 1990s, see Johansson (2000, 2001) and Bergmark (2000).

[13] Many of the changes prescribed by the 1998 law reflected trends that had been in practice earlier; Salonen and Ulmestig (2001) show that many municipalities seem to have applied rules similar to the new policy even before 1998. Also, the rule has been used in a wider sense, for example, applied to groups other than youths.

[14] On January 1, 2007, the number of city districts decreased to 14.

[15] The districts' responsibilities include refugee reception services, recreational programs for children and youth, pre-school, income support, budgetary counseling and debt restructuring, consumer advisory services, local business and labor market initiatives, local urban environment issues, maintenance of parks, services and care for the disabled, social services, family law, and elderly services.

[16] In Sweden, municipalities have the right to collect revenues from a local, proportional, income tax. They are also allowed to charge user fees for some of the services they provide.

[17] It is noteworthy that, as opposed to in some other Swedish municipalities, there did not exist any large scale activation programs in any of the Stockholm city districts before 1998 when the Social Service Act was changed.

These programs have been known under the name "activation programs" and typically require a number of hours' attendance each week. According to the official descriptions, the aim of the programs is to facilitate job searches for the unemployed and to "coach" the participants to become self-supporting. However, in a case study by Thorén (2005), it is concluded that "municipal activation policy in its practical form will not necessarily improve client's prospects to find employment since its primary function rather is as a method to control clients' entitlement to social assistance".

In order to determine when the different city districts launched mandatory activation, we have disseminated a questionnaire addressed to the heads of the welfare administration in each city district.[18] The questionnaire was complemented with telephone interviews whenever it was difficult to categorize a program based on the information given in the questionnaire. Based on the information from the questionnaire and the interviews, we can determine in which year a mandatory program was launched in each city district.[19] In order to be labeled as "a mandatory program", it must be directed toward all unemployed individuals receiving welfare benefits and require attendance for some hours per week. The programs all use a common reporting system in which the participants' attendance is recorded daily. Most importantly, the register is open to social workers, which means that absence is immediately detected and will in many cases lead to reduced benefits. Some of the programs are extensions of previous programs, but the ambitions of the current programs are much higher.[20] Table 1 shows when the activation programs subject to this study were implemented.[21]

[18] The questionnaire is given in Appendix B.

[19] A valid question is of course whether we can trust the answers given by the welfare administrators. Do the programs really include *all* individuals receiving welfare, and are they as harsh as the administrator claims? Without conducting thorough implementation studies, we can of course never be 100 percent certain. However, as far as we know, there are no reasons for the administration not to tell the truth. Also, it is worth noting that if the programs are in fact not as compulsory and as "tough" as stated by the heads of the welfare administration, we would get estimates that, if anything, are biased towards zero. Hence, the effect that we find in the paper should be seen as a lower bound of the effects of general activation programs. In addition, in the interviews we ask about programs that have actually been in place for a number of years, making it likely that it is the actual program, not just the ambitions of the program, which we capture.

[20] In the earlier years, job seeking activities were often limited to occasional contacts with an employment counselor, whose role mostly consisted of discussing the client's situation and possibly arranging labor market training. Cooperation between the social administration and consultants was scarce, and a common view is that the follow-up was insufficient.

[21] In one district (Skarpnäck) it is impossible to establish when the "ambitious" program began, and Skarpnäck is therefore excluded. In addition, the most central city districts are excluded from the sample altogether as the share of recipients of welfare benefits is very low in this part of the city and their methods are difficult to categorize.

Table 1. *Starting years for activation programs in Stockholm city districts.*

District	Year
Rinkeby	1998
Skärholmen	1999
Farsta	2001
Kista	2001
Älvsjö	2002
Hägersten	2003
Liljeholmen	2003
Spånga-Tensta	2003
Bromma	2004
Enskede-Årsta	2004
Hässelby-Vällingby	2004
Vantör	2004

In order to provide a better understanding of the programs, we will describe the program in Skärholmen in more detail. The program in Skärholmen is one of the most documented programs (see Ekström, 2005 and Thorén, 2005 for a more detailed description), and it is to a large extent comparable to other, less documented programs in other parts of the city.[22] For example, three other city districts (Hägersten, Liljeholmen and Älvsjö) have joined the project, and during our study period, the four districts shared the facilities in Skärholmen.

In 1998, the city district of Skärholmen began to apply a method that has since become known as "the Skärholmen model". During the first year, the activities were only directed to students who were unemployed during the summer, but in 1999, the program was extended to include all unemployed recipients of welfare benefits. When welfare applicants enter the welfare services office, those whose main motivation for applying for welfare is categorized as “unemployment” are immediately sent to “The Job Center” (the local employment agency that administers the job-seeking activities for welfare recipients). Usually, the applicants must meet with Job center personnel before their application is processed. Sometimes the applicant is given suggestions on jobs to seek or other activities on their first visit to the Job center. As long as a person has not found a job or an activity to participate in, the program requires three hours of daily attendance at the Job Center, either in the morning or in the afternoon. Every second week the schedule rotates in order to prevent black market work. The central component in the model is job-seeking activities. These are facilitated by providing job

[22] Blomberg et al. (2006) study the activation programs implemented in six city districts (Vantör, Skärholmen, Kista, Hässelby-Vällingby, Rinkeby and Spånga-Tensta) and conclude that the programs are similar in many respects. For example, all districts have reception offices from which the welfare applicants are directed to activation centers. At these centers, a mix of the following activities takes place: unassisted job search, assisted job search, internships, work practice, and job guidance.

seekers with an individual labor market coach and material that may be helpful in the job search, such as computers, telephones and stationery. In addition to job-seeking activities, the program involves participation in internships, short-term education such as computer courses, and other activities arranged by the city district, such as gardening or cleaning in the community. As noted by Thorén (2005), many of the activities aim at testing the participants' willingness to work. There is also a large amount of cooperation between the welfare office and the coaches at the Job Center. Not participating actively at the Job Center will be reported to the welfare administrator, who can decline to provide the recipients their welfare benefits.

3. Data

The register data used in this paper comes from Statistics Sweden and contains yearly information on all individuals aged 18–64 living in the municipality of Stockholm during the years 1993 through 2003.[23] Table 2 reports summary statistics on the variables used in this paper. In order to measure the effects on welfare participation, we use a dummy (*Welfare recipient)* that indicates whether the individual lives in a household that received welfare during the year.[24] We see from Table 2 that this is true for almost ten percent of all individuals in our sample. A potential problem with this measure of welfare participation is that it is quite crude in the sense that an individual is considered as being a welfare participant if he or she has received some welfare benefits at some point during a year. However, the amount received differs substantially between individuals, and it is therefore also interesting to investigate the effect on the amount of welfare money received during a year (*Welfare benefits*).[25] The average amount received is approximately 2,000 SEK per year. This might seem like a low figure, but note that individuals receiving no welfare are included. For those individuals who did receive some welfare, the average amount received is approximately 22,300 SEK.

Since we are interested in what happens to individuals who potentially leave welfare or refrain from entering into welfare, we will also investigate the effects on employment. We use four different measures of employment: A dummy indicating whether the individual worked at least 1 hour in November (*Employed in November*), a variable that measures how many months the individual was employed in the year (*Months employed*), a dummy indicating whether the individual was employed all 12 months (*Employed all year*) , and income earned from employment (*Income from employment*). In the variables *Employed all year* and *Months Employed*, an individual was defined as employed if the work performed that month generated an income larger than 25 percent of the minimum wage of workers in

[23] We exclude newly arrived immigrants since they all receive social assistance the two first years in Sweden.

[24] Welfare benefits are directed to households, not individuals. For simplicity, we will in the rest of the paper write as if it was the individual who received welfare. What we mean is, however, whether the individual lived in a household that received welfare.

[25] The variable "Welfare benefits" is the individual's share of the household's welfare benefits.

the hotel and restaurant sector. Summary statistics for the different employment measures are reported in Table 2. Approximately 73 percent of the population is employed according to the first definition.

Table 2. *Summary statistics.*[26]

Variable	Mean	Std. Dev.	Min	Max
Outcome variables				
The probability of receiving welfare	0.096	0.295	0	1
Welfare benefits	2,195	9,975	0	510,800
Employed in November	0.728	0.445	0	1
Months employed	8.437	5.185	0	12
Employed all year	0.641	0.480	0	1
Income from employment	161,327	169,667	0	25,977,500
Disposable income*	156,519	262,956	-1,551,500	223,910,80
Control variables				
Woman	0.498	0.500	0	1
Age 18–25	0.152	0.359	0	1
Age 26–35	0.263	0.440	0	1
Age 36–45	0.357	0.479	0	1
Age 46–64	0.228	0.420	0	1
Born in Sweden	0.760	0.427	0	1
Born in Nordic country	0.047	0.212	0	1
Born in Western country	0.027	0.163	0	1
Born in East European country	0.037	0.189	0	1
Born in other country	0.133	0.340	0	1
Elementary school< 9 years	0.211	0.408	0	1
Elementary school 9 years	0.258	0.438	0	1
High school	0.196	0.397	0	1
College/University<2 years	0.1652	0.369	0	1
College/University>2 years	0.163	0.370	0	1
Ph D	0.009	0.095	0	1
Immigration 2–4 years ago	0.019	0.138	0	1
Immigration 5–9 years ago	0.055	0.228	0	1
Immigration 10–14 years	0.048	0.214	0	1
Immigration>15 years ago or not at all	0.877	0.328	0	1
With young children (<7 years)	0.186	0.389	0	1
One child	0.201	0.401	0	1
More than one child	0.206	0.404	0	1

* Only available for the years 1995–2003. For some individuals, disposable income takes on a negative value, which can be explained by the fact that some individuals make quite substantial capital losses.

[26] Exact definitions of all variables as well as data sources are given in Appendix C.

Finally, we will investigate what happens to the economic well-being of individuals by investigating effects on disposable income. As we can see from the table above, disposable income varies substantially between individuals. In the empirical analysis, we will also control for a number of individual specific characteristics; summary statistics for those variables are also provided in Table 2.

The city districts that we analyze are rather heterogeneous with respect to demographic composition and outcome variables, as illustrated by Table 3, which presents summary statistics from 1993 on some of the outcome variables as well as the share of the population that was born outside Sweden. Notably, Rinkeby stands out with its high share of welfare recipients and people born outside Sweden and its low share of employment.

Table 3. *City district characteristics in 1993.*

	Share welfare recipients	Average welfare benefits	Share employed (Nov.)	Average disposable income*	Share born outside Sweden
Bromma	0.06	1,047	0.76	149,986	0.11
Enskede-Årsta	0.08	1,400	0.74	130,543	0.15
Farsta	0.12	2,277	0.71	126,121	0.16
Hägersten	0.08	1,449	0.73	131,553	0.15
Hässelby-	0.08	1,210	0.74	138,319	0.14
Kista	0.18	3,561	0.68	122,213	0.40
Liljeholmen	0.10	1,847	0.72	123,935	0.15
Rinkeby	0.33	6,587	0.45	93,622	0.76
Skärholmen	0.12	1,902	0.67	121,044	0.30
Vantör	0.13	2,431	0.69	121,569	0.19
Spånga-Tensta	0.16	2,890	0.66	126,920	0.39
Älvsjo	0.07	979	0.77	141,893	0.13

* Only available for the years 1995–2003.

Comparing the figures in Table 3 with the year of program implementation shown in Table 1, it is worth noting that the city districts with the highest welfare participation seem to have implemented the policy first. In the next section, we will discuss how this is taken into account in the empirical analysis.

4. Econometric strategy

When investigating the effect of a specific policy on individual behavior, the econometric challenge is to separate effects of the policy from other factors that may also affect individual behavior. If one only compares the behavior of an individual before and after a policy change, there is a major risk that one also captures differences in the behavior that depend on factors other than the policy. One way to isolate the effect of the policy from all other things that may affect individual behavior is to compare the changes in behavior of individuals residing in a city district that has implemented the policy with changes in the behavior of individuals residing in a city district that has *not* implemented the policy, thereby netting out other factors that may affect individual behavior. We will use this difference-in-differences approach in this paper.

The identifying assumption for this model is that if the policy had not been implemented, welfare caseloads in the city district that implemented the policy would have changed in the same way as in the city districts that did not implement the policy. As mentioned above, the city districts implemented the policy at different times. The labor market in these years (1998–2003) was somewhat turbulent, with decreasing unemployment rates until 2001 followed by a small increase. Barth et al. (2004, 2006) have shown that labor market conditions matter differently for different groups; i.e., the weaker the group is with respect to labor market attachment, the more sensitive the group is to fluctuations in labor market conditions. Given that the city districts with the potentially weakest groups were those that implemented mandatory activation first, one might worry that not taking this into account would put the identifying assumption at risk. In order to avoid this potential problem, we will control for a number of specific individual characteristics and also, in the richest specifications, allow the coefficients for these characteristics to have different effects over time. By doing this, we control for the fact that a specific demographic structure in the early years may affect welfare caseloads differently than having the same demographic structure in the later years, when the labor market conditions differ.

Even after controlling for demographics in the flexible way described above, there might be different time trends in the different city districts. We will therefore also allow for linear, city-district-specific time trends in some of the specifications. The equation that constitute the richest specification in our empirical analysis is then given by

$$Y_{ijt} = \alpha_j + t_t + \beta program_{jt} + \theta_t X_{ijt} + trend_j + \varepsilon_{ijt}, \quad (1)$$

where Y_{ijt} is the outcome of interest for individual i in city district j in time period (year) t, α_j are city-district-specific fixed effects, t_t are time-specific fixed effects that are common for all city districts, $program_{jt}$ is an indicator variable that takes the value 1 if the policy is implemented in city district j in year t (and all years thereafter), X_{ijt} is a vector of demographic covariates, $trend_j$ are city-district-specific time trends, and ε_{ijt} are error terms.

One thing that equation (1) does not control for is unobserved city-district-specific shocks that might vary over time. If such shocks exist, they might cause two different kinds of problems. First, if the shocks are correlated with the timing of the reform, β might capture these shocks rather than true program effects. Second, such shocks might imply that the standard errors of individuals within the same city district will be correlated, making the estimated standard errors biased and thereby invalidating inference.

Since we focus on city districts within a close geographical distance that also make up the center of a much larger labor market region, we believe that we are likely to capture any such shocks with the common time effect together with the time-varying coefficient on the control variables. However, to examine whether the results are robust to serial correlation across city districts we apply the wild bootstrap procedure suggested by Cameron, Gelbach and Miller (2008) to calculate critical values for inference. Because of its good small sample properties the method is preferred over computation of cluster robust standard errors, which assumes a large number of clusters whereas our sample only contains 12 clusters. Inference is based on the sampling distribution obtained via the Wild cluster bootstrap with 1000/500 replications.

As a further sensitivity check, we will also conduct a placebo experiment where we pretend that the programs took place five years before their actual implementation and then estimate the effects of these placebo programs using data from the pre-reform period, i.e., before any city district had implemented any program. If we find an effect of the true timing of the reform, but no effect for the placebo reform or pre-program effects, we will be more confident that we have in fact captured relevant differences in the city-districts with our model specification, thus finding the true program effect.

A final threat to identification would be if welfare-prone individuals move between city districts depending on whether or not the districts have implemented strict mandatory activation programs. If this is the case, we might be worried that equation (1) captures these effects rather than effects on welfare participation. However, Edmark (2009), using the introduction of the same activation program that we analyze in this paper, does not find that the moving patterns of welfare-prone individuals differ from the moving patterns of non-welfare-prone individuals.

5. Average effects of mandatory activation

In this section we present the difference-in-differences estimates of the effects of mandatory activation on welfare, employment and disposable income. As model specification checks we also present results from placebo experiments.

5.1 Effects on welfare participation

The theoretical prediction from the Besley and Coate (1992) model implies that welfare participation should decrease as a consequence of the introduction of mandatory activation programs. Table 4 presents the effect of mandatory activation on the probability for an individual to receive welfare sometime during a year (upper panel) as well as the amount of welfare received during a year (including zeros) (lower panel). We estimate the linear model given in equation (1), controlling for several observed as well as unobserved characteristics of the city districts. We estimate three variants of equation (1) to examine how sensitive the results are to the inclusion/exclusion of some of the covariates. In the first column, we estimate the model without city-district-specific time trends (i.e. $trend_j = 0$ in equation (1)) and without time-varying covariates (i.e. $q_t = q$ in equation (1)). In the second column, we add city-district-specific time trends and in the last column we also add time-varying covariates.

In each panel and for each specification (i.e. each column) we present the results using two different data sets; first we present the difference-in-differences estimates of the effect of activation and second we present estimates from placebo experiments. In the placebo experiments, we use data from the period 1993–98, i.e., the period before any mandatory activation program had been put in place in any city district. In order to create placebo reforms, we pretend that the programs were implemented five years before they actually were. Hence, we pretend that Rinkeby implemented the program in 1993, Skärholmen in 1994, Farsta and Kista in 1996, etc. We then estimate the difference-in-differences specification given in equation (1) on the pre-reform data. If we do not find any effect of the placebo reform, we will be more confident that we have a reasonable model specification and that any estimated effect is in fact a program effect and not just an unobserved city-district-specific shock. For all estimates we present both robust

standard errors clustered at the household level (within parentheses) and p-values obtained from the wild bootstrap procedure (within brackets).

Table 4. *Effects on welfare participation.*

	(1)	(2)	(3)
Probability of receiving welfare			
Activation	-0.026	-0.005	-0.002
	(0.001) ***	(0.001) ***	(0.001)
	[0.028] **	[0.604]	[0.57]
Placebo	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Welfare benefits			
Activation	-420.4	-129.7	-6.582
	(34.34) ***	(36.71) ***	(36.40)
	[0.106]	[0.678]	[0.93]
Placebo	18.43	-20.21	1.448
	(32.34)	(32.80)	(32.83)
	[0.976]	[0.904]	[0.912]
City-district trends	No	Yes	Yes
Time-variant Xs	No	No	Yes

Notes: Robust standard errors clustered at the household level within parentheses. P-values from Wild Bootstrap within brackets (Activation: 1000 repl, Placebo: 500 repl). ***, **, and * denotes statistical significance at the 1, 5 and 10 percent level respectively. The included covariates are the following: dummies for sex, age, education, country of origin, number of years in Sweden, marital status as well as for the presence of kids in the household (any kid, several kids, young kids).

Turning to the results, it is clear that the point estimates are sensitive to the inclusion of trend variables and time-varying covariates. For example, the estimated probability that a household receives welfare changes from an estimated decrease of 2.6 percentage points when mandatory activation is implemented in the most parsimonious specification (c.f. column (1)) to an estimated decrease of 0.5 percentage points when city-district-specific time trends are added to the model (c.f. column (2)). However, the p-value obtained from the wild bootstrap procedure also indicates that the point estimate becomes statistically insignificant. In the richest specification, the point estimate further decreases to -0.2 percentage points and is statistically insignificant (c.f. the last column). A similar pattern is also observed for the amount of welfare benefits received; it changes from an estimated decrease by 420 SEK to an estimated decrease by approximately 6 SEK per year. None of the effects on welfare benefits are statistically significant when using the wild bootstrap critical values.

Hence, we cannot reject that the activation did not affect the households' probability of receiving welfare or the amount of welfare that they received,

even though the placebo estimates are closer to zero than the estimated effects. Furthermore, the estimated point estimates suggest that any potential effects are small.

5.2 Effects on employment

Next we turn to the effects of the activation programs on employment.[27] We use four different variables to capture effects on employment. The first is a dummy taking the value one if the individual was employed in November in a given year and zero otherwise. The second is the number of months that the individual has been employed during a year. The third is a dummy indicating whether an individual has been employed all 12 months of the year, and the fourth is income from employment. The results are presented in Table 5. A first observation from the table is that the most parsimonious specification does not seem to provide reliable results; the point estimates changes quite drastically when turning to the richer specifications, and the placebo estimates are significant (always when using standard confidence intervals, for the probability of employment in November when using wild bootstrap confidence intervals). However, for the richer specifications, the placebo estimates look perfectly fine; they are close to zero (and always closer to zero than the effects estimates) and insignificant. For the richest specification (c.f. column (3)), it seems like the activation programs significantly affected the households' employment status in November and their income from employment.

We find that mandatory activation increases the individual's probability of being employed in November by 0.4 percentage points, which corresponds to an increase of 0.5 percent of the mean value. Income from employment increases by 1,079 SEK per year, which corresponds to an increase of 0.7 percent of the mean value in the sample. We also get positive point estimates for the number of months that the individual is employed and the probability that the individual is employed for the full year, but these estimates are not statistically significant (at least not when using p-values from the wild bootstrap distribution). It must however be noted that even if some effects are statistically significant, they are all relatively small in size.

[27] The predictions from the Besley and Coate (1992) model are not explicit about outcomes other than welfare participation, but implicitly there is an understanding that mandatory activation should have a positive effect on the employment rate and, possibly, other labor market outcomes.

Table 5. *Effects on employment.*

	(1)	(2)	(3)
Probability of being employed in November			
Activation	0.007	0.007	0.004
	(0.001) ***	(0.001) ***	(0.001) ***
	[0.048] **	[0.01] ***	[0.036] **
Placebo	-0.007***	-0.001	-0.000
	(0.001)	(0.001)	(0.001)
	[0.080] *	[0.536]	[0.888]
Number of months employed			
Activation	0.062 ***	0.063***	0.029*
	(0.015)	(0.015)	(0.015)
	[0.088] *	[0.242]	[0.290]
Placebo	-0.077***	0.007	0.008
	(0.015)	(0.016)	(0.016)
Probability of being employed for the full year			
Activation	0.001	0.003	0.002
	(0.001)	(0.001) **	(0.001)
	[0.706]	[0.144]	[0.53]
Placebo	-0.007	0.002	0.001
	(0.001 ***	(0.002)	(0.002)
	[0.188]	[0.384]	[0.332]
Income from employment			
Activation	-8,355***	891.4**	1,079***
	(418.9)	(369.0)	(372.9)
	[0.03] **	[0.558]	[0.10] *
Placebo	-3,474***	243.6	76.70
	(321.9)	(298.4)	(299.6
	[0.188]	[0.472]	[0.636]
City-district trends	No	Yes	Yes
Time-variant Xs	No	No	Yes

Notes: Robust standard errors clustered at the household level within parentheses. P-values from Wild Bootstrap within brackets (Activation: 1000 repl, Placebo: 500 repl). ***, **, and * denotes statistical significance at the 1, 5 and 10 percent level respectively. The included covariates are the following: dummies for sex, age, education, country of origin, number of years in Sweden, marital status as well as for the presence of kids in the household (any kid, several kids, young kids).

5.3 Effects on economic well-being

Another interesting question is how well the individuals are doing in "overall" economic terms. Thanks to reliable register-based information on individuals' disposable income[28], we are able to analyze this, something that has not been done in earlier studies on U.S. welfare reform when relying on observational data.[29] From the results, presented in Table 6, it seems like the introduction of mandatory activation lead to a significant net increase in disposable income of 2,358 SEK (c.f. the results from the richest specification given in column (3)). This amounts to an increase by 1.5 percent of the mean value. For the richest specifications, the estimates from the placebo reform are statistically insignificant; the point-estimates are small and close to zero. These findings strengthen our belief that mandatory activation do indeed affect the individuals' disposable income, although, just as for the effects on employment, the economic significance is marginal.

Table 6. *Effects on disposable income.*

	(1)	(2)	(3)
Disposable income			
Activation	-4,388***	1,922***	2,358***
	(409.4)	(697.6)	(681.7)
	[0.056] *	[0.114] *	[0.25] *
Placebo	-1,939***	237.8	340.0
	(409.1)	(451.3)	(453.7)
	[0.40]	[0.840]	[0.556]
City-district trends	No	Yes	Yes
Time-variant Xs	No	No	Yes

Notes: Robust standard errors clustered at the household level within parentheses. P-values from Wild Bootstrap within brackets (Activation: 1000 repl, Placebo: 500 repl). ***, **, and * denotes statistical significance at the 1, 5 and 10 percent level respectively. The included covariates are the following: dummies for sex, age, education, country of origin, number of years in Sweden, marital status as well as for the presence of kids in the household (any kid, several kids, young kids).

[28] Disposable income is defined as all income received (from work, social security systems, transfers, etc.) minus taxes and other payments (such as study loan payments).

[29] The income data available in the U.S. are self-reported and, as is discussed in Meyer and Sullivan (2003), income therefore tends to be underreported, especially by welfare recipients. Using consumption data instead, Meyer and Sullivan (2004) examine the material conditions of single mothers and their families to assess the net effect of the U.S. welfare reforms on the well-being of these families. They find that the material conditions of single mothers have not declined either in absolute terms or relative to different comparison groups (such as single childless women).

6. Does mandatory activation affect vulnerable groups differently?

So far we have estimated average effects. However, as is shown by Table 7, there are certain groups for whom welfare participation is especially high, i.e., younger people, those born outside Sweden (in particular, those born in non-Western areas, i.e., Asia, Africa and Latin America), and single households. It is therefore of interest to investigate whether the mandatory activation programs have different effects for these groups. Also, welfare might be extra harmful for young people or immigrants due to, e.g., scarring effects, making it especially important to understand how to decrease welfare participation in these groups.[30] In this section, we will investigate whether the effects of mandatory activation are heterogeneous with respect to family status (single households), age (18-25) and country of origin (born in Sweden and born in a non-western country respectively). We do this by estimating the model in equation (1) for the different subgroups (defined by family status, age, or country of origin). The results, presented in Table 8, are those obtained from the richest model specification (i.e., with city-district specific time trends and time-varying covariates).

Table 7. *Welfare, employment and income, by group.*

	18-25	Swedish born	Non-western	Single households
Pr. of receiving welfare	0.15	0.056	0.307	0.221
Welfare benefits	2,728	1,130	7,557	4,036
Employed in November	0.559	0.783	0.499	0.716
Months employed	6.20	9.08	5.72	8.23
Employed all year	0.372	0.697	0.398	0.634
Income from empl.	77,568	180,614	80,810	139,203
Disposable income	83,134	167,025	114,059	173,194
No. of obs.	397,571	1,986,564	347,940	211,946

[30] Skans (2011) shows that experiencing unemployment subsequent to graduation from high school has negative effects on both unemployment and earnings at least five years after graduation, whereas Åslund and Rooth (2007) show that exposure to high local unemployment rates affects immigrants for at least ten years after entry to Sweden.

The results from Table 8 indicate the effects on employment are mostly present for Swedish born, but the economic significance is low. For example, the likelihood of being employed in November increases with approximately 0.4 percentage points, responding to one half percent of the average mean. Also, there are some indications that the probability of receiving welfare decreases for single households; the presence of activation programs decrease the likelihood that a household receive welfare with 1.4 percentage points, which corresponds to a decrease with around six percent. In addition, the likelihood of being employed in November increases with approximately 1.0 percentage points for singles, responding to 14 percent of the average mean.

Table 8. *Heterogeneous effects.*

	18-25	**Swedish born**	**Non-western**	**Single households**
Probability of receiving welfare				
Activation	-0.009***	-0.005***	0.006	-0.014***
	(0.003)	(0.001)	(0.004)	(0.004)
	[0.252]	[0.436]	[0.692]	[0.048]**
Welfare benefits				
Activation	-81.57	-68.92**	237.0*	-172.2
	(92.62)	(30.06)	(127.6)	(120.8)
	[0.712]	[0.296]	[0.332]	[0.336]
Employed in November				
Activation	-0.002	0.004**	0.001	0.009**
	(0.004)	(0.002)	(0.003)	(0.004)
	[0.768]	[0.020]**	[0.852]	[0.040]**
Months employed				
Activation	-0.047	0.035**	-0.00302	0.0660
	(0.045)	(0.017)	(0.038)	(0.0517)
	[0.368]	[0.300]	[0.960]	[0.216]
Employed full year				
Activation	-0.002	0.002	-0.002	0.005
	(0.004)	(0.002)	(0.003)	(0.005)
	[0.744]	[0.424]	[0.816]	[0.452]
Income from employment				
Activation	20.12	1,321***	143.1	1,731
	(704.2)	(478.3)	(642.6)	(1,133)
	[0.948]	[0.156]	[0.816]	[0.268]
Disposable income				
Activation	47.10	3,625***	324.9	-231.8
	(481.7)	(885.3)	(1,458)	(1,252)
	[0.836]	[0.424]	[0.648]	[0.852]
City-district trends	Yes	Yes	Yes	Yes
Time-variant Xs	Yes	Yes	Yes	Yes

Notes: See Table 4.

7. Concluding remarks

In this paper, we have examined whether the introduction of mandatory activation programs has any effects on welfare participation, employment, and disposable income. The theoretical prediction from the Besley and Coate (1992) model is that mandatory activation decreases welfare participation and, implicitly, increases employment. As far as we know, this is the first time that a clear empirical test of the hypothesis that this type of program implies fewer people on welfare has been carried out, taking both entry and exit effects into account.

In order to identify causal effects, we made use of a variation that was generated by the gradual implementation of mandatory activation in the city districts in the municipality of Stockholm. The data are very suitable for examining this question for several reasons. First, the reform was clean in the sense that no other instruments, like time limits or tax credits, were introduced at the same time, allowing us to estimate the direct effects of the programs. Second, the reform was initiated at different points in time in different city districts, making identification easier. Finally, by using data from city districts within a single local labor market, we were able to control for common macroeconomic shocks.

On average, we found a positive, but small effect on employment (the probability that an individual is employed increases with the introduction of the programs). Also, disposable income increases marginally when activation programs are introduced. However, we do not find any statistically (or economically) significant effects on welfare.

Looking for heterogeneous effects for different groups, we find that the programs seem to work better for Swedish born, and that the effects on employment are largest for single headed households. Finally, the latter group is the only group for which we find positive effects of any importance on the probability of receiving welfare, which reduces with six percent when activation programs are in place.

In the analysis, we have investigated the effects of the whole package of activation programs. What we have not done is to analyze the different components of the programs, such as work training and education. In addition, we are not able to say whether it is the threat of activation that affects potential welfare recipients, or if it is the programs themselves that help program participants. It might be the case that zero effects on welfare caseloads are a result of some programs having positive program effects while others have

lock-in effects. Analyzing this would require more detailed data on the program participants as well as more institutional knowledge of the exact components of the different programs. Such an analyzes must also be able to handle selection into different programs. Given the extensive use of activation programs all over Europe and the US, this type of analysis is highly called for. Also, given earlier evidence that it is not always those that are likely to benefit the most from activation programs that are those assigned to these programs calls for further analysis.

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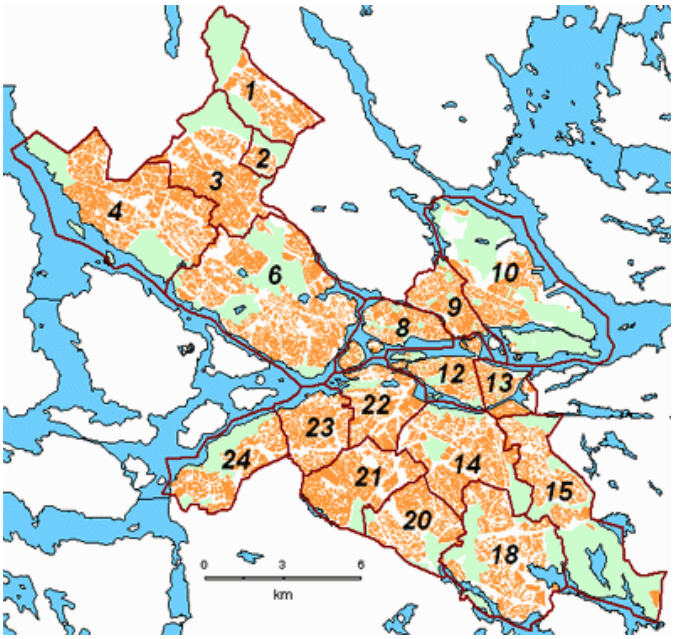
Appendix A: Map – city districts of Stockholm.



1. Kista
2. Rinkeby
3. Spånga-Tensta
4. Hässelby-Vällingby
6. Bromma
8. Kungsholmen
9. Norrmalm
10. Östermalm
12. Maria-Gamla stan
13. Katarina-Sofia
14. Enskede-Årsta
15. Skarpnäck
18. Farsta
20. Vantör
21. Älvsjö
22. Liljeholmen
23. Hägersten
24. Skärholmen

Appendix B: Survey to the social service unit of the city districts of Stockholm

(Note that the original version is in Swedish and that this is a translated version.)

The survey refers to information on activities for unemployed individuals, capable of working, who receive welfare benefits.

1. Does your city district currently have any activation/labor market related programs for unemployed individuals, capable of working, who receive welfare benefits?

Yes

No

If no, turn to question 9 of the survey.

If yes, please name the program(s):

2. Since what year has the program or programs existed in their current form (under the same or a different name)?

3. Do the program(s) encompass all individuals, capable of working, who are unemployed and receive welfare benefits?

Yes

No

4. If you responded "No" to question 3:

- What percentage of all individuals, capable of working, who are unemployed and receive welfare benefits are served by the program(s)?

- Which groups of individuals are targeted by the program(s)?

5. Please specify how and to what extent the following activities are being used in the program(s):

a. Job-seeking activities

b. Job training activities

c. Other assigned work (for example, within the municipal services)

d. Other activities – please specify which:

6. What is the minimum number of hours of weekly attendance that is required in the program(s)?

7. Is absence/non-attendance systematically reported to the social service officials?

Yes

No

Comments:

8. Can absence/non-attendance (without acceptable motives) lead to rejection of the welfare benefit application?

Yes

No

Comments:

In the following part of the survey, we ask for information on programs that were targeted to unemployed individuals, capable of working, who receive welfare benefits, before the current program/programs started.

9. Which programs have been in place during the period from 1990 until the start of the current program/programs? Under each heading below, please specify the name of the program, or the main activity if you do not know/there was no name for the program (for example, "Meeting with job counselor"). Please also specify during what years the program/activity was in place.

Program 1:

Name: _____

Time period:_____

Program 2:

Name: _____

Time period: _____

[..etc..]

Below follows a set of questions about the programs/activities that were in place before the current program(s). Please answer the questions about each program under the heading that corresponds to the list above.

Program/Activity 1:

1. Which groups were targeted by the program/activity?

2. How large a share of all individuals, capable of working and receiving welfare benefits, were encompassed by the program/activity?

3. Please specify to what extent the following activities were used in the program/activity:

a. Job-seeking activities

b. Job-training activities

c. Other assigned work (for example, within the municipal services)

d. Other activities (please specify which):

7. Was absence/non-attendance systematically reported to the social service officials?

Yes

No

If yes, in what way:

8. Could absence/non-attendance (without acceptable motives) lead to refusal/rejection of the welfare benefit application?

Yes

No

Comments:

Program/Activity 2:

[The same questions were repeated for all programs/activities listed]

Appendix C: Register data

The data used in this paper come from three databases (all of them part of the IFAU database): LOUISE, syss and anst.

LOUISE: A longitudinal database containing information on education, income and employment for the whole population older than 16 in Sweden. It contains data for all years since 1990.

Syss: Syss is part of RAMS (registered labor market statistics) and contains data on employers, income from employment and employment from 1985 to 2000. For later years, see LOUISE.

Anst: Anst is part of RAMS (registered labor market statistics) and contains information about when the employee began work and when the employment was terminated.

Table C1. *Definition of variables*

Variable	Database and name	Description
Dependent variables:		
Welfare recipient	LOUISE: socbidp1*	Indicator variable that takes value 1 if socbidp1>0.
Welfare benefits	LOUISE: socbidp1	The individual's share of the household's welfare benefits. Includes zeros.
Employed in November	sys: syss*	Indicator variable that takes the value 1 if an individual is employed for at least 1 hour in November.
Employed all year	anst: mantill and manfran	The variable takes the value 1 if an individual has been employed a full year in a position that has generated more than 25 percent of the minimum wage for a worker within the hotel and restaurant sector.
Months employed	anst: mantill and manfran	The number of months an individual has been employed during the year in a position that has generated more than 25 percent of the minimum wage for a worker within the hotel and restaurant sector.
Income from employment	LOUISE: loneink	The sum of gross earnings from an employer during the year.
Disposable income	LOUISE: dispink	All income from work and social security systems, transfers minus taxes, study loan payments, etc. For details, see SCB (2005, p. 190).
Variables used for heterogeneous effects:		
Two parent household with young children (<7 years)	LOUISE: barn0003 and barn0406, famstf	Indicator variable that takes the value 1 if a household is headed by two adults and has children less than 7 years in the household.
Single-parent household with young children	LOUISE: barn0003 and barn0406, famstf	Indicator variable that takes the value 1 if a household is headed by one adult and has children less than 7 years in the household.
Born in Sweden	sys: fland	Indicator variable for Sweden as country of birth.
Born in Nordic country	sys: fland	Indicator variable for any of the Nordic countries as country of birth.
Born in Western country	sys: fland	Indicator variable for any of the Western countries as country of birth (Western Europe, U.S. and Canada).
Born in Eastern Europe	sys: fland	Indicator variable for any of the Eastern European countries as country of birth.
Born in other country	sys: fland	Indicator variable for any other country of birth.
Other control variables		
Woman	LOUISE: kon	Indicator variable that takes value 1 if an individual is a woman.
Households with young children (<7 years)	LOUISE: barn0003 and barn0406	Indicator variable for the presence of children under 7 years in the household.
18–25	LOUISE: fodar*	Indicator variable that takes the value 1 if an individual is within the age interval 18–25.
26–35	LOUISE: fodar*	Indicator variable that takes the value 1 if an individual is within the age interval 26–35.
36–45	LOUISE: fodar*	Indicator variable that takes the value 1 if an individual is within the age interval 36–45.

Table C1 continued.

46–64	LOUISE: fodar*	Indicator variable that takes the value 1 if an individual is within the age interval 45–64.
Children=1	LOUISE: barn0003, barn0406, barn0715, barn1617*	Indicator variable for the presence of one child under 18 years in the household.
Children>1	LOUISE: barn0003, barn0406, barn0715, barn1617*	Indicator variable for the presence of more than one child under 18 years in the household.
Elementary school< 9 years	LOUISE: hsun*	Indicator variable that takes the value 1 if the individual's highest education is elementary school < 9 years.
Elementary school 9 years	LOUISE: hsun*	Indicator variable that takes the value 1 if the individual's highest education is elementary school 9 years.

Notes: * Variable/s used to generate the variable used.

Essay 2: Is welfare dependency inherited? Estimating the causal welfare transmission effects using Swedish sibling data[31]

Coauthored with Karin Edmark

[31] Constructive and useful comments from Eva Mörk, Markus Jäntti, Per-Anders Edin, Erik Grönqvist and seminar participants at Uppsala University, Umeå University, IFAU, the 2008 IIPF Annual Congress in Maastricht, and the 8th Journées Louis-André Gérard-Varet in Marseille. Research grants from the Swedish Council for Working Life and Social Research (FAS), and from the Jan Wallander and Tom Hedelius Foundation are gratefully acknowledged.

1 Introduction

Welfare benefit policy is an area that has received considerable political interest in many countries during the last decades. Starting with the US welfare reform of 1996, several countries have sought to reform their welfare benefit systems to decrease caseloads and increase employment. One motivation for these policies, in particular in the US, is the fear that having a large population on welfare may produce "welfare cultures"; i.e., a situation in which individuals are trapped in poverty and the use of welfare spreads through social interactions. Such interactions may occur between individuals in various social networks, for example between parents and children.

In this paper we focus on the long term effects of welfare use, and test whether the use of welfare benefits is transmitted from parents to their children. This is an important topic as it does not only reveal something about the mechanisms of welfare use, but it may also shed light on the potential long term effects of policies aimed at reducing welfare use. More precisely, this paper estimates the direct causal effects of growing up in a household that receives welfare benefits, using a sibling difference comparison on Swedish households during the 1990s.

There are several ways in which parental welfare benefit receipt could affect the children's welfare use as adults: i) children of welfare recipients may develop less of a natural connection to work life and have less access to work-related networks[32], which may make them more likely to receive welfare as adults; ii) children of welfare recipients may learn how the welfare benefit system works, and how life on welfare is, which may make them either more or less likely to use welfare; and iii) children of welfare recipients may experience welfare receipt as more or less stigmatizing. The effects we consider are hence related to viewing the parents as role models, to attitudes to welfare benefits, and to access to employment-related networks through the parents.

The challenge is to separate these effects of parental welfare use from other factors that are correlated with the welfare use of both parents and children. There are two types of such factors: i) the welfare receipt of parents may be correlated with other household level characteristics, which affect future welfare benefit receipt of children through the home environment; and

[32] That parental networks can be important in Sweden is confirmed by Kramarz and Nordström-Skans (2011).

ii) parents and children may be similar in personal characteristics that affect their likelihood of receiving welfare benefits, such as attitudes to or skills for work. Although there are a fairly large number of studies on intergenerational welfare transmission, especially studies based on US data, there is no clear evidence of whether the commonly observed positive intergenerational correlation in welfare benefit receipt reflects a causal effect of parental welfare benefits, or whether this correlation is the result of other factors. [33]

Only a few of the previous studies have attempted to separately identify the causal effects. Gottschalk (1996) attempts to capture unobserved heterogeneity by adding parental welfare benefit status measured when the child is an adult to the regression model. This approach will capture the effect of any similarities in the propensity to become a welfare benefit recipient, to the extent that they stay fixed over time and enter the model linearly. It does not, however, solve the problem of how to control for other factors – related to the home environment etc. – which are correlated with parental welfare benefits during childhood but are not fully captured by the parents' current welfare receipt.

Another approach is to instrument for parental welfare benefit receipt as the child grows up (see e.g., Pepper, 2000; Siedler,2004 and Maloney et al.,2003). The idea here is to use only the variation in parental welfare benefit receipt that is exogenous – i.e., uncorrelated with other factors that affect children's future welfare benefit receipt. The problem is to find instruments that have sufficiently strong effect on parents' welfare benefit receipt as the child grows up but that have no direct effects on the child's future welfare benefit receipt. Some of the instruments that were used in the previous studies are questionable from a methodological point of view. For example, Maloney et al. (2003) use parent's educational and civil status as instruments, in spite of the fact that these may well have a direct effect on children's future welfare benefit status. The more convincing studies (see e.g., Pepper, 2000) use the local unemployment rate as instrument for parental welfare use.

To the best of our knowledge, the only previous study on intergenerational welfare benefit transmission in Sweden is Stenberg (2000), in which the intergenerational correlations for a sample of approximately 12,000 individuals born in 1952 are estimated. The study includes an extensive set of control variables, and the results suggest a positive correlation in the welfare benefit receipt of children and parents in households where other social problems are also present, such as behavioral problems in school or having a father with a criminal record, but not in households without such problems.

[33] See e.g. Antel (1992), Gottschalk (1990) and (1996), Pepper (2000), Siedler (2004), Maloney et al. (2003), and Stenberg (2000).

The sibling-based approach that is used in this paper relies on using a difference-in-difference (DID) estimator to control for the influence of omitted variables that are correlated with the welfare benefit receipt of both parents and children. The findings in Stenberg (2000) highlight the fact that family level factors, which are often not detectable to the researcher and are likely to be correlated with welfare benefit receipt, may be important to control for to isolate the causal effects of welfare. This makes the sibling difference analysis particularly appealing, because it in effect controls for all observed and unobserved family level characteristics that stay fixed over time. While previous studies investigate the effect of the welfare benefit receipt of parents in general, our use of the sibling-difference-method means that we focus on the effects of *temporary* welfare receipt.[34] Since a large share of the welfare case load is made up of relatively short-term spells (see Dahlberg et al., 2008), this is an interesting group to study, but it shall be kept in mind that the results may very well differ from the effects of long-term welfare dependency.

We have access to detailed register data on all Swedish individuals aged 16–64, including information on welfare benefit payments and family connections, and several socio-economic and demographic background variables. Using these rich data, we are able to construct a data set that meets our requirements for the sibling difference analysis. Specifically, we extract a sample of two types of families with (at least) two children: i) in which the parents received no welfare benefits until the older sibling was 24, but started to receive welfare benefits after that; and ii) in which the parents never received welfare benefits until both siblings had turned 24. This sample is used to estimate the effect of parental welfare benefit receipt on the likelihood that the child receives welfare at the age of 24.[35]

The fact that we use a DID-specification, means that we rely on variation between younger and older siblings, and between types of families, to identify the effect. First, we take the difference in welfare benefit receipt at age 24 between the older and younger siblings, in families in which the younger but not the older sibling was exposed to parental welfare receipt before age 24. That is, we use only families in which the parents started to receive welfare benefits after the older sibling had turned 25. Second, we subtract the corresponding between-sibling difference among the families where the parents never received welfare. The appeal of this specification is that the first difference controls for family level factors to the extent that they stay fixed over time and affect older and younger siblings in a similar manner, while the second difference controls for factors that are specific to the time period

[34] The methodology relies on finding an untreated control group (i.e. siblings without experience of welfare benefit receipt), which implies limiting the analysis to families with welfare benefits for a limited time period.

[35] As will be discussed further in section 4.2, we hence use a specific sample of individuals for the analysis, and this affects the generalizability of the results.

during which we measure the outcome variable, as long as these factors have a similar effect on children from the two types of families.

To this DID-specification, we add a set of time-varying covariates, and we add time trends interacted with a set of predetermined factors that are correlated with welfare benefit propensity. This helps control for household level and other factors that are not captured by the DID-specification. The estimation details will be further discussed in section 3.

The sibling difference method has been used in several areas, such as returns to schooling (Ashenfelter and Zimmerman, 1997), the effects on children's educational attainment of parental separation (Björklund and Sundström, 2006), and intergenerational effects in unemployment (Ekhaugen, 2005). The study that is most closely related to this, Levine and Zimmerman (2005), applies the sibling difference method to test the effects of maternal welfare benefit receipt on children's developmental outcomes, such as educational attainment. Their results show no evidence of a causal link between maternal welfare receipt and children's outcomes.

Our study is also related to the literature on intergenerational income mobility. The aggregate evidence from these studies suggests a positive correlation between parents' and children's income, although the estimated magnitudes of this correlation vary.[36] Björklund and Jäntti (1997), for example, show that the income mobility seems to be larger in Sweden than in the US. Other studies investigate the intergenerational transmission of poverty, such as Duncan et al. (1998) and Airio et al. (2005), and find that the risk of poverty in adulthood is larger for individuals growing up in poor families.

The results of our analysis confirms a positive correlation between children's and parents' welfare benefit receipt in the data, even after we control for a large number of *observable* household level characteristics. However, our DID-based sibling analysis, which conditions on unobserved heterogeneity, yields no support for a *causal* effect of temporary parental welfare benefit receipt on children's future welfare benefit use.

The organization of the remaining sections is as follows: section 2 describes the data used, section 3 presents and discusses the empirical model, and section 4 contains a descriptive analysis. Section 5 presents and discusses the results of the sibling comparison, and section 6 concludes.

[36] See e.g. Solon (1992), Corak and Heisz (1999), Chadwick and Solon (2002).

2 Data

We are fortunate to have access to register data on incomes and demographic variables for all Swedes aged 16–64 from 1990–2007. The information on welfare benefits is in the form of the total yearly amount (in SEK currency) of welfare benefits received by the household. The data also contains a number of socio-economic factors, such as disposable income, other types of benefits (sickness benefits, sickness pension benefits and benefits for disabled) and information on family characteristics and educational attainment. Importantly, we are able to link parents and children (biological and adopted), and from age 16, we observe whether the children and parents live in the same household or whether the child has moved out. [37]

We use these data to construct a data set consisting of all families with children born in 1973 and 1981, in 1974 and 1982, or in 1975 and 1983.[38] As will be explained further in section 3, using the sibling-difference-method implies focusing on families in which there are siblings that are 8 years apart, *and* where the oldest of these sibling-pairs did not experience that the parents received welfare benefits when he or she was 17–19 years old. This is admittedly a long period, but it is necessary as we want to be able to measure the outcome variable – welfare benefit receipt as a young adult – of the older sibling before the start of a potential parental welfare benefit spell.[39] In order to get an idea of whether this group of families is very different (also in other aspects) from the general population, Table A1 in the Appendix reports the characteristics of the sibling-sample families, as well as characteristics of the general population of families. The general pattern is that the families in the sibling sample are on average economically better off than the average families: they are in general less likely to experience parental welfare benefit receipt, have a bit higher income, and are less likely to have unemployed parents. The parents are also more often Swedish-born, and the average education level of the parents is slightly higher, although not by much. The differences are however on average not very large; so the descriptive statistics in Table A1 does not indicate that the families in the sibling sample are dramatically different from the rest of the population.

[37] We include only children who live with their mother at age 17 and who do not live with their parents at age 24.
[38] Twins and other siblings that are born during the same year are excluded from the sample.
[39] The families included in the sample may also have children born in between the births of the siblings in the sample, but these siblings are not included in the analysis.

We measure the children's exposure to parental welfare when they are 17-19.[40] This means that we omit sibling pairs in which the parents received welfare when the older sibling was 17-19. This provides approximately 4500 families to use for the estimations. The outcome variable, children's welfare use, is measured as the children are 24 years old. This age was chosen as it was the latest age at which we could observe all the cohorts studied.

Welfare benefits in Sweden are provided by the municipalities to households not able to support themselves by other means. The welfare benefits are means-tested and can be received by all types of households, including single households and households without children. The aim is to ensure a reasonable living standard to those who lack sufficient income from employment or the general social insurance system[41], and the benefits are to cover housing costs as well as living costs. The exact amount is decided by the municipalities, but a minimum amount is established in national law since 1998.

Figure 1 shows the aggregate unemployment and welfare benefit rates in 1980–2006 (with welfare benefits starting from 1983 on).[42] Before 1993-94, the welfare benefit levels were relatively constant at approximately 6 percent, but between 1992 and 1994, they rose to more than 8 percent and remained high until the late 1990s. The shaded areas in the figure show when the older (dark gray) and the younger (light gray) siblings are 17–19 years old, which is the age when the younger siblings are potentially exposed to parental welfare benefits. The circles denote the years in which the outcome is measured for the three cohorts of older (dark gray) and younger (light gray) siblings. It is worth pointing out that the outcome of the older sibling is always measured one year before the start of the younger sibling's exposure to parental welfare receipt to ensure that the outcome of the older control cohorts is not directly affected by the welfare benefit receipt of their parents.

[40] The fact that the first year of the data set is 1990 means that the first year we observe the family characteristics is when the children in cohorts 1981–83 are 7–9 years old, and when the children in cohorts 1973–75 are 15–17. How this might affect the results is discussed in section 3.

[41] The benefits levels in the unemployment and sickness insurance systems, as well as parental benefits, are strongly related to the individual's previous income from employment. Individuals with a loose connection to the labor market are hence strongly over-represented as recipients of welfare benefits – not only due to low or non-existent labor income, but also due to low or non-existing income from the ordinary social insurance system.

[42] The aggregate welfare benefit rate in Figure 2 includes economic support to recent immigrants. Recent immigrants are however excluded from the regression analysis of the paper.

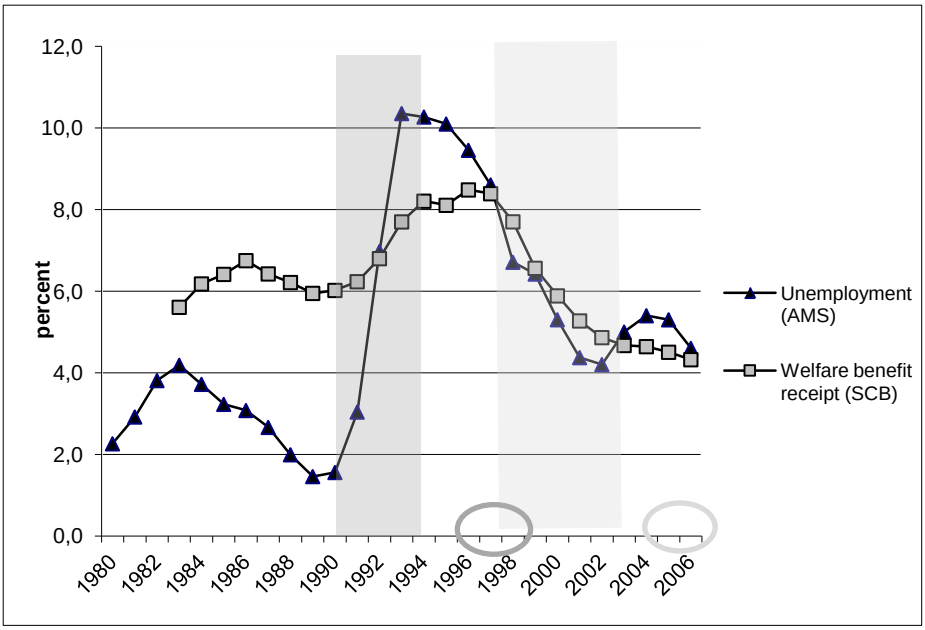


Figure 1. Unemployment and welfare benefit rates 1980-2007.[43]

Source: The National Labor Market Board (AMS) and Statistics Sweden (SCB).

The variables that are included in the regression analysis are summarized in Table 1. All monetary variables have been deflated to year 2005's price level. Table 1 contains the average values of the main variables by welfare status of the families. We can see that both young and old siblings from families that have received welfare are more likely to receive welfare as adults. Welfare benefit receipt is also more common if the parents in these families are born outside of Sweden, have lower education, are not employed and have received sickness benefits. Finally, we observe that old siblings are generally more likely to receive welfare benefits than young siblings, which is a fact that probably mirrors the tougher labor market situation facing the adult older siblings (see Figure 1).

As shown in Table 1, we use several measures of parental benefit receipt. First, a dummy variable, *Parental Welfare 17–19*, indicates whether the parent received welfare benefit at any time as the child grew up, defined as when the child is 17–19 years old. Second, we generate variables for the share of years during ages 17–19 that the parents received welfare benefits, *Parental welfare share of years 17–19*, as well as for the average yearly amount of benefits paid out during this period, *Parental welfare/year*. To measure the welfare use of the children as young adults, we generate a

[43] Welfare benefit rates refers to the share of individuals all ages that live in households that received welfare benefits at some point during the year.

dummy variable, which equals one if the child received any welfare benefits at the age of 24.

Table 1 also shows the large number of family level characteristics measured as the child grows up (age 17–19), which will be used in the analysis. These include variables measuring the following: whether the parents got divorced during this period, *Divorce 17–19;* the share of years the household was a single parent household, *Single mother share yrs 17–19*; the number of children ages 0–17 in the household measured as the child is 19, *Nr children 0-17 at age 19*; the age of the mother as the child is born, *Age of mother at child's birth*; a set of dummy variables measuring the parents' education levels as the child is 19; and their region of birth. Furthermore, we include measures of the share of years, measured as the child is 17–19, that the parents received sickness benefits and disability pension, respectively, and we include measures of the share of years the parents were outside employment. Family disposable income will be included in some of the regression specifications[44], and we include the unemployment rate in the municipality where the child resides at age 24.

[44] It can be discussed whether the income level should be controlled for, since being low income household is an inherent part of being a welfare recipient. This will be discussed further in section 4.

Table 1. *Family characteristics of the sibling sample, by welfare status.*

	Families with welfare		Families without welfare	
	Younger sibling	Older sibling	Younger sibling	Older Sibling
Indicators of welfare benefit status:				
Child welfare at 24	0.14	0.27	0.03	0.06
Parental welfare during ages 17–19	0.48	0.00	0.00	0.00
Parental welfare share of years 17–19	6.20	0.00	0.00	0.00
Parental welfare SEK/ year 17–19	6.20	0.00	0.00	0.00
Household characteristics during childhood:				
Divorce during ages 17–19	0.06	0.09	0.03	0.03
Single mother share of years 17–19	0.35	0.17	0.13	0.07
Disposable family income SEK, total 17–19	0.65	0.97	0.74	1.05
Sickness benefits mother share of years 17–19	0.26	0.47	0.20	0.34
Sickness benefits father share of years 17–19	0.14	0.41	0.12	0.28
Sickness pension mother share of years 17–19	0.18	0.07	0.07	0.02
Sickness pension father share of years 17–19	0.12	0.05	0.05	0.01
Disability pension mother share of years 17–19	0.02	0.01	0.00	0.00
Disability pension father share of years 17–19	0.00	0.00	0.00	0.00
Mother not employed share of years 17–19	0.32	0.19	0.10	0.10
Father not employed share of years 17–19	0.25	0.13	0.08	0.07
Parental characteristics:				
Age of mother at child's birth	31.35	23.35	32.08	24.08
Mother's age at immigration (=0 if native)	2.17	1.90	0.92	0.66
Father's age at immigration (=0 if native)	3.16	2.05	1.19	0.76
Mother born in Sweden	0.90	0.90	0.96	0.96
Mother born in Nordic country	0.05	0.05	0.03	0.03
Mother born in Western country	0.01	0.01	0.00	0.00
Mother born in East Europe	0.01	0.01	0.00	0.00
Mother born outside Europe/Western country	0.04	0.04	0.00	0.00
Father born in Sweden	0.88	0.89	0.96	0.96
Father born in Nordic country	0.05	0.04	0.02	0.02
Father born in Western country	0.01	0.02	0.01	0.01
Father born in East Europe	0.01	0.01	0.01	0.01

Table 1 continued.

Father born outside Europe/Western country	0.04	0.04	0.01	0.01
Mother finished primary school at age 19 of the child	0.24	0.31	0.17	0.19
Mother finished high school at age 19 of the child	0.53	0.48	0.50	0.50
Mother finished college at age 19 of the child	0.23	0.21	0.33	0.31
Father finished primary school at age 19 of the child	0.33	0.37	0.26	0.29
Father finished high school at age 19 of the child	0.45	0.41	0.43	0.42
Father finished college at age 19 of the child	0.23	0.22	0.31	0.29
Children in the household at age 19 of the child	0.40	2.11	0.23	1.82
Local characteristics at age 24 of the child:				
Local unemployment	3.97	6.10	3.71	5.64
Observations	164	164	4273	4273

3 Empirical methodology

This section describes the details of the empirical specification. As a starting point, we define the baseline regression equation as follows:[45]

$$S_{im}^{C} = a + bS_{i}^{P} + fZ_{i}^{P} + lU_{m} + e_{im}, \qquad (1)$$

where S_{im}^{C} measures the welfare benefit use of child *i*, living municipality *m*, as a young adult. The explanatory variable of interest is S_{i}^{P}, which measures the welfare benefits of the parents as the child grows up.[46] Z_{i}^{P} denotes a set of other parental and family variables as the child grows up; U_{m} is the unemployment rate in the municipality of residence of the adult child; and e_{im} is a random error term.

While correlations resulting from a regression model, such as equation (1), are certainly interesting in themselves, they may not, as previously discussed, reveal the causality of the effects. Is an observed correlation between parental welfare participation and the child's outcome really the effect of the welfare participation, or is it due to some omitted family characteristic, or to similarities between parents and children, which are also correlated with welfare use?

To control for unobserved family heterogeneity, we turn to the sibling difference analysis. Because this method uses within-family variation, i.e., variation in exposure to parental welfare benefit receipt between siblings in the same family, it will in effect control for the influence of all family level heterogeneity that is fixed over time. The household level covariates will hence now only enter in the regression to the extent that they change over time. Estimating the sibling difference equation amounts to adding a family fixed effect, a_{j}, as well as a time cohort effect[47], T_{i}, to equation (1):

[45] We use a linear probability model in this paper but all regressions have also been estimated by a probit model, which gives a similar result. The results from these regressions are available upon request.

[46] Only two parent and single mother households are included in the analysis.

[47] This captures differences in the outcomes of the young and the old siblings that are due to the fact that the outcomes for the old and young siblings are measured at different points in time, with different labour market opportunities etc.

$$S_{ijm}^{C} = \alpha_j + \beta S_{ij}^{P} + \phi Z_{ij}^{P} + \gamma T_i + \lambda U_m + \varepsilon_{ijm}. \quad (2)$$

Under the assumption that all unobserved family heterogeneity that is correlated with parental welfare benefit receipt and the outcome variable is captured by the inclusion of the family fixed effects, the β-coefficient in equation (2) will capture the causal effects of being exposed to parental welfare benefit receipt. Therefore, the crucial assumption for our method to work is that there are no trends in unobservable factors, which affect the children's future welfare benefit receipt and differ systematically between families with and without welfare benefits. To make this assumption more plausible, we allow for the time effect, T_i, to differ with respect to a set of predetermined household level factors that are likely to be correlated with the welfare benefit propensity of parents.[48] In this manner, we control for heterogeneous time trends between groups that are likely to differ in terms of the risk of receiving welfare benefits. In addition, we allow for the effect of local unemployment measured when the child is 24, to differ between children from the two family types; one type in which the parents do not receive welfare benefits when any of the siblings are 17–19 years, and the other in which the parents receive welfare benefits as the younger, but not the older, child is 17–19. The resulting estimating equation is:

$$S_{ijm}^{C} = \alpha_j + \beta S_{ij}^{P} + \phi Z_{ij}^{P} + \gamma T_i + \psi \left(T_i \times W_j\right) + \lambda U_m + \eta\left(U_m \times B_j\right) + \varepsilon_{ijm}, \quad (3)$$

where W_j is a set of predetermined household levels characteristics that tend to be correlated with welfare benefit receipt[49], and B_j equals one if the parents receive welfare benefits as the younger sibling is a 17–19 years old, and is zero otherwise.

Before we turn to the estimation of the regression equations, it is illuminating to discuss in more detail the sources of bias that could potentially affect the result. For this purpose, we first note that the β-coefficient in equation (2) can be rewritten as the expected difference in the differences in adult welfare use between young siblings S_y^C and old siblings S_o^C, in fami-

[48] These are the following: Mother born in Nordic country, mother born in Western country, mother born in East European country, mother born in other country, father born in Nordic country, father born in Western country, father born in East European country, father born in other country, indicators for whether the father or mother were unemployed at any time during 1985-1990, indicators for the education level of the father and mother (primary school, high school and college) and an indicator for the mother being single any time during 1985-1990.

[49] Note that W_j is a subset of the variables in Z_{ij}, for which we have access to predetermined information either in the form of information from previous years (1985–90), or in the form of variables that are constant over time, such as country of birth.

lies that differ in whether the parents received welfare benefits as the younger sibling was a teenager or not, S_y^P=1 or S_y^P=0. This expression is shown in equation (4), where for simplicity, we have omitted all other covariates:

$$\hat{b} = E\left((S_y^C - S_o^C \mid S_y^P = 1) - (S_y^C - S_o^C \mid S_y^P = 0)\right) \quad (4)$$

Equation (4) is useful to have in mind as we discuss the different types of bias that may affect the results.

First, the fact that the data start in 1990 means that we lack information on the early childhood of the siblings – for the older siblings for the period up until they were 14–16 years old, and for the younger siblings until they were 6–8 years old. However, we do have information on a set of economic and demographic variables that are correlated with welfare benefit receipt for 1985–89, and we use these variables to predict welfare benefit receipt for these years.[50] According to our predictions, none of the households in our sibling-sample is predicted to have received welfare benefits during 1985-89, which is reassuring.

We still, however, lack information until 1984, when the older siblings were 9–11 years old. How might this affect the results? To the extent that the children from the different types of families in our sample, in which parents either received or did not receive welfare benefits as the younger sibling was 17–19 years old, were equally exposed to previous parental welfare spells, the b-coefficient in equation (4) remains unbiased. However, if previous welfare spells differ systematically across families, this might affect the results. Perhaps the most likely case to consider is one in which parental welfare participation during the unobserved period was more common in families in which the parents also received welfare benefits as the younger sibling was a teenager, i.e., where S_y^P =1. This would give rise to a bias towards zero because now not only the young sibling, but also the older sibling, would to some extent have experienced, and been affected by, parental welfare receipt as a child. The fact that we do not observe the early childhood of the older siblings is therefore likely to understate the effect of parental welfare use on child welfare use.

Another scenario that could give rise to bias towards zero is the potential existence of unobserved factors, which affects the welfare benefit receipt of both parents and children, but where the timing is such that it first affects welfare receipt of the older siblings, and then affects the welfare receipt of

[50] Specifically, we use the following variables to predict the probability to use welfare benefits during the year: employment status, work-related annual incomes, local unemployment conditions, education level and type, and region of birth, and we use the estimated coefficients from 1991 to for the predictions. The predictions for this year correctly predict welfare benefit receipt in 86 percent of cases.

the parents. In such a situation, the welfare benefit of parents and older siblings would be positively correlated, and we would again risk understating the effect of parental welfare benefit receipt.

A third issue to keep in mind when interpreting the results is that by construction, the sibling sample will not include the most severe cases of parental welfare benefit receipt, i.e., those in which parents receive welfare benefits year after year. This is because, as described in the previous section, the sibling sample only includes families that received no welfare benefits until the older sibling was 24 years old and the younger sibling was 16 years old. The effect that is captured by estimating our DID-specification is hence the effect of being exposed to parental welfare benefits in the late teenage years but not before that. Because the larger share of welfare caseloads are in fact made up of shorter spells[51], this is an interesting group to study. It is however important to keep in mind that the results may be different in families that are dependent on welfare benefits over longer periods of time.

[51] Dahlberg et al. (2008) show that only a small share of the total welfare case load is made up by individuals who remain on welfare year after year.

4 Descriptive analysis

Before the estimation of the sibling-based DID-analysis of the causal intergenerational effects, we will start by providing a detailed description of the intergenerational pattern of welfare benefit receipt, using data on all individuals born in years 1981–83 and their parents. This means that we use the full cohorts born in the same years as the younger siblings of the sibling analysis. For these cohorts, we observe a large number of household level socioeconomic factors starting from age 9.

The reason for starting off with a descriptive section is partly that we think that the intergenerational correlations that are estimated using our large set of data and covariates are interesting in themselves and partly that they can be useful for comparison with other studies.

A first look at the raw data reveals that children's welfare use is much higher if their parents received welfare benefits as the child grew up: 15.0 percent of all children whose parents received welfare at any point while the child was 9–19 years of age, received welfare themselves at the age of 24, compared to 2.8 percent in families in which the parents received no welfare. The risk that the child received welfare benefits at age 24 further increases with the share of years that the parents received welfare benefits: among children whose parents received welfare during up to half of the years during the period, approximately 14 percent received welfare themselves at age 24, while among those whose parents received welfare more than three quarters of the years, the share of welfare benefit recipients is almost the double, 26 percent.

This correlation can, however, stem either from the welfare use itself or from other factors. Table 2 therefore shows the results from estimating the regression equation (1), where we condition on our observable background information on cohorts 1981–83. The results in Table 2 show how welfare benefit receipt at the age of 24 correlates with exposure to parental welfare participation when the child is 9–19 years old, which is defined in column (1), as a dummy for whether the household received welfare benefits at any point in time during this period; in column (2), as the share of years the household received welfare; and, in column (3), as the average annual amount of welfare received during the period. The dependent variable, child

welfare use as a young adult, is defined as a dummy that equals one if the household received welfare benefits at the age of 24.[52]

The regressions in Table 2 contain a large set of household-level and parental background covariates, as well as the municipal unemployment rate facing the child as an adult.[53] Table 2 only shows the results for the variable of main interest, parental welfare, but the full results can be found in the appendix, see Table A2.

Table 2. *LPM for parental welfare use and child welfare use.*

	Child welfare dummy variable at age 24		
	(1)	(2)	(3)
Parental welfare dummy 9–19	0.066***		
	(0.002)		
Parental welfare share of years 9–19		0.201***	
		(0.006)	
Parental welfare/year 9–19 (1000 SEK)			0.002***
			(0.000)
Household level covariates	Yes	Yes	Yes
Municipality fixed effects[¤]	No	No	Yes
Observations	182,200	182,200	182,200
R-squared	0.064	0.072	0.065

Robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels respectively.

[¤] Municipality fixed effects are included in the regression using the average yearly amount of welfare benefits received as the child is 9–19, to control for the fact that cost of living, especially housing, differs between municipalities. This affects the amounts of welfare benefits granted.

The results in Table 2 suggest that parental welfare benefit receipt is positively correlated with children's welfare use as young adults after we control for a large number of household level characteristics and the local unemployment rate measured at age 24 of the child. The coefficient in column (1) indicates that growing up in a household that receives welfare benefits at any point during ages 9–19, is correlated with a 6.6 percentage point increase in the probability to receive welfare as a young adult; in addition, the coefficient in column (2) suggests that an increase in the share of the years that the parents received welfare benefits while the child was 9–19 from zero to 100 percent, is correlated with a 20 percentage point increase in the same proba-

[52] Note that only children who no longer live with their parents at age 24 are included in the sample.

[53] Household disposable income is included in the set of household level variables in the specification of column (2). Whether or not this variable should be included can be discussed, since having a lower income can be seen as an inherent part of being a recipient of welfare. We have chosen to include the variable since we want to isolate the role-model-; net-work-; and attitude-related effects of exposure to parental welfare benefits. It should however be pointed out that excluding disposable income from the regressions does not affect the results for parental welfare benefit receipt.

bility. The coefficient in column (3) indicates that a 1000 SEK increase in parent's annual average welfare benefit is correlated with less than a 0.002 percentage point increase in the welfare probability of the child as an adult.[54]

Table 3 shows the results for the same regression specification as in Table 2, but shows the estimated coefficients for parental welfare receipt when the child was 9–12 years, 13–16 years and 17–19 years separately.[55] This is interesting both as an indication of at which age the intergenerational correlation is the strongest, and as an indication of whether using exposure to parental welfare benefits in the late teens, as we do in the sibling-analysis, is reasonable. (The results for the full set of covariates are available upon request.)

[54] We also estimated Table 3 with a logit-specification. The resulting average marginal effects were also positive and statistically significantly different from zero, but were a bit smaller than the coefficients in Table 3. The results are available upon request.

[55] To account for that the potential length of the welfare spell is longer for older children we also add interaction terms between all combinations of welfare receipt at different age categories.

Table 3. *LPM for parental welfare at different ages and child welfare use.*

	Child welfare dummy variable at age 24		
	(1)	(2)	(3)
Parental welfare 9–12	0.026***		
	(0.003)		
Parental welfare 13–16	0.033***		
	(0.003)		
Parental welfare 17–19	0.099***		
	(0.006)		
Parental welfare share of years 9–12		0.063***	
		(0.007)	
Parental welfare share of years 13–16		0.080***	
		(0.008)	
Parental welfare share of years 17–19		0.186***	
		(0.011)	
Parental welfare/year 9–12			0.002***
(1000 SEK)			(0.000)
Parental welfare/year 13–16			0.002***
(1000 SEK)			(0.000)
Parental welfare/year 17–19			0.005***
(1000 SEK)			(0.000)
Household level covariates	Yes	Yes	Yes
Municipal fixed effects[¤]	No	No	Yes
Observations	175,861	175,861	175,861
R-squared	0.071	0.072	0.074

Robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels respectively. All regressions include interactions terms between all combinations of parental welfare receipt at the given age categories. For example, the regression in column (1) include parental welfare 9-12*parental welfare 13-16, parental welfare 9-12*parental welfare 17-19, parental welfare 13-16*parental welfare 13-16 and parental welfare 9-12*parental welfare 13-16* parental welfare 17-19.

[¤] Municipality fixed effects are included in the regression using the average yearly amount of welfare benefits, to control for the fact that cost of living, especially housing, differs between municipalities. This affects the amounts of welfare benefits granted.

As shown in Table 3, the correlation between parental and child welfare participation is the strongest if the parents received welfare benefits when the child was a teenager: the coefficients in all specifications are statistically significantly larger for exposure to parental welfare when the child is 17–19 years old compared with the younger age intervals. The estimated coefficients in column (1) suggest that parental welfare benefits is correlated with a 10 percentage point increase in the probability that the child received welfare benefits at age 24 if measured when the child is 17–19 years old, compared to an approximately 3 percentage point increase when the child is 13–16, and 9–12, respectively. A similar pattern is given for the alternative

measures in column (2) and (3). The stronger correlation in the late teens might be due to for example role-model or net-work related effects being particularly strong at these formative years.

Therefore, our descriptive analysis shows that even after conditioning on a large set of household-level covariates, there is a strong positive intergenerational correlation in welfare benefit receipt in our data. We have also observed that this correlation is stronger for parental welfare spells that occur during the late teenage years of the children. Taken together, this suggests that it is interesting to study the causal effects of parental welfare receipt, especially during the late teens, which is what we will do in the sibling-analysis in the following section.

5 Estimating the causal effects using a sibling comparison approach

To test whether the positive correlations that were estimated in the previous section in fact reflect a causal relationship between parental and child welfare benefit receipt, we turn to the estimation of the sibling DID-regression in equations (2)–(3). The results are presented in Table 4.

The specification in column (1) includes only family fixed effects and a dummy variable for being a young sibling, in addition to the main explanatory variable. In column 2, we include parental characteristics[56], local unemployment and the interaction between coming from a welfare receiving family and local unemployment. In column 3, we exclude the interaction with local unemployment but instead include an interaction between a set of predetermined parental background characteristics correlated with welfare benefit use and the time dummy variable (i.e., if the sibling is young). Finally, column 4 combines all covariates from column 2 and 3. This specification corresponds to equation (3).

For comparison, we have added an additional column (column 5), which contains the results obtained when we run the OLS correlation regression of equation (1) using only the sample of younger siblings that are included in the sibling analysis.[57] As shown in column 5, these results are fairly similar to the results obtained when we used the entire cohorts in Table 2.[58]

The coefficients for the three different measures of parental welfare benefit receipt during ages 17-19 are shown in panels *A*–*C*; parental welfare at any time is shown in panel *A*; share of years with welfare in panel *B*; and the amount of welfare benefits in panel *C*.[59] (The results for the full set of covariates can be found in Tables A3-A5 in the Appendix.)

[56] Parents' country of birth, indicators for whether the father or mother were unemployed at any time during 1985-1990, indicators for the education level of the father and mother (primary school, high school and college) and an indicator for the mother being single any time during 1985–1990.

[57] Note that the older siblings are not included in this regression, since the parents' welfare receipt is zero for all of the older siblings and there is hence no identifying variation among them to use to identify effects of parental welfare benefits. In the sibling-DID they are however needed as an untreated control group.

[58] The full set of results for the correlations in column 5 can be obtained from the authors upon request.

[59] We have also estimated specification (2) in Table 4 using conditional fixed effects logit (clogit-command in STATA). This yielded statistically insignificant effects for all three

Table 4. *Sibling fixed effects model for parental welfare and child welfare use at the age of 24.*

	Child welfare at 24				
	Sibling fixed effects model				Correlation young siblings
	(1)	(2)	(3)	(4)	(5)
Explanatory variable A:					
Parental welfare during ages 17–19	-0.091**	-0.066	-0.077**	-0.057	0.093***
	(0.040)	(0.075)	(0.039)	(0.075)	(0.026)
R-squared	0.020	0.029	0.033	0.034	0.034
Explanatory variable B:					
Parental welfare share of years 17–19	-0.211***	-0.205*	-0.188**	-0.189	0.164***
	(0.081)	(0.120)	(0.080)	(0.121)	(0.054)
R-squared	0.022	0.030	0.035	0.035	0.033
Explanatory variable C:					
Parental welfare/year 17–19 (1000 SEK)	-0.007	-0.004	-0.007	-0.004	0.005*
	(0.005)	(0.006)	(0.005)	(0.006)	(0.002)
R-squared	0.083	0.092	0.095	0.097	0.086
Dummy variable young sibling	Yes	Yes	Yes	Yes	-
Family fixed effects	Yes	Yes	Yes	Yes	-
Household level covariates and local unemployment at age 24	No	Yes	Yes	Yes	Yes[†]
Interactions household characteristics*young	No	No	Yes	Yes	No
Interaction local unemployment*welfare family	No	Yes	No	Yes	No
Observations	8,938	8,938	8,938	8,938	4,469
Number of groups	4,461	4,461	4,461	4,461	-

Robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels respectively.. All regressions with parental welfare/year as explanatory variable include municipal fixed effects at age 19.
[†] The household level covariates in column (5) also include the time invariant variables that are naturally excluded in the fixed effects analysis.

measures of parental welfare benefit receipt. (Inclusion of the trend-variables in specifications (3) and (4) led to convergence-problems, so these specifications were not estimated.)

As shown in column (1) in Table 4, the effect of parental welfare becomes negative for all parental welfare measures, as we control for unobserved family level heterogeneity by including sibling fixed effects in the regressions. For the two former measures, *A* and *B*, of parental welfare, the coefficient size decreases as we add additional covariates to the regression, in specifications (2)–(4). With the inclusion of the interaction term between welfare family and local unemployment in column 2 the coefficient of measure A become negative, while measure B only is insignificant when including both this interaction and the interactions between the age dummy and parental characteristics. For the third measure, average yearly amounts of parental welfare benefits, the coefficient is always insignificant. Since we believe that controlling for different trends between families with different risk to be recipients of welfare, and also to allow for the effect of local unemployment to differ between different types of families (as discussed in section 3), the estimations in column 4 are our preferred specifications. The results of these suggest insignificant estimates for parental welfare use, for all three measures of parental welfare.[60]

Among the control variables (displayed in Appendix A), we find that the age of the father at immigration has a significant positive effect on the risk of receiving welfare benefits as an adult. The local unemployment rate also seems to be an important factor: a one percentage point increase in the local unemployment level is associated with a 1 percent percent increase in the probability of receiving welfare. Finally, on average, the younger siblings have a 3-5 percentage point lower risk of using welfare benefits as an adult, which is consistent with the general decreasing trend in welfare benefit levels during the first half of the 2000s, as was indicated in Figure 1.

The overall picture that emerges from the sibling analysis is hence that including a family fixed effect in welfare use eliminates the positive correlation between children's and parents' welfare benefit receipt that was found in the initial correlation analysis. As suggested by our use of predicted values for welfare benefit receipt for the period 1985–89, this result does not seem to be driven by a negative bias due to unobserved early welfare use.

The results suggest that the large and positive intergenerational correlation in welfare benefit receipt that was observed in the descriptive analysis, especially when parents received welfare benefits in the child's late teens,

[60] It can be noted that the estimations in Table 5 were calculated using only families for which we do have information on the set of predetermined variables, observed in 1985–90, which are used to create the time trends in estimations column (3) and (4). When we rerun the regressions in column (1) and (2) using the full sample of siblings, we find that the point estimate of specification (1) goes from being marginally insignificant and negative, to being significant and negative, while the point estimate of specification (2) is negative and insignificant, as in Table 6. The major difference between the two samples is that the sample used in the main analysis does not include families in which the father and/or mother immigrated later than 1985.

was not due to the welfare benefit receipt per se, but rather to unobservable factors correlated with welfare benefit receipt. It is interesting to note that this is consistent with the findings of Stenberg (2000), who reports evidence of positive intergenerational correlations in welfare benefit receipt only when other social problems that are usually not observable to the researcher, such as behavioral problems in school or having a father with a criminal record, were present.

There are, however, a couple of potential alternative explanations for the insignificant results of the sibling analysis that should be noted.

First, as discussed in section 3, the estimates in Table 4 are identified using only the families in which the parents did not receive welfare benefits until the younger sibling turned 17. The results shall hence be interpreted as the effects of *temporary* parental welfare spells when the children are in their late teens, on children's welfare participation. We cannot rule out that the effects are different for longer spells of parental welfare benefit receipt, or that the effect might be different for younger children.

Second, as also noted in section 3, the welfare benefit receipt of both the parents and the older siblings could be correlated with some latent variable that affects the welfare benefit receipt among the older siblings before it affects that of the parents. This would lead to the coefficient being biased towards zero.

Third, our sample size is admittedly small; when restricting the sample to only include siblings that meet the requirement for our sibling-based analysis, we are left with 164 families that received welfare. We cannot therefore rule out that the results reflect a lack of variation in our sample. However, the cross-sectional OLS-results in column (5) of Table 4 are statistically significant and are also similar in size to the estimates in the analysis using the full 1981-1983 cohorts in section 4, so the sibling sample is at least rich enough to detect the same correlation pattern as when using the full data set.

Fourth, the fact that the older siblings faced a tougher labor market situation at the time when we measure their outcomes, compared to the younger siblings, as indicated by the unemployment rates in Figure 1, could contribute to the non-significant effect of the sibling analysis, if this means that the older siblings were more likely to receive welfare benefits due to unemployment. This is something we tried to address in the empirical analysis by including the local unemployment rate, but is it possible that this was not sufficient to solve the issue.

A further complicating factor is that changes in labor market policy could have different effects on individuals from different family backgrounds. For example, starting from the mid-1990s, there has been a trend of stronger emphasis on mandatory activation schemes, such as job search training and work practice, for the unemployed. It is likely that the younger siblings were subject to more of these requirements as young adults, and this could be a

contributing factor to the negative, although non-significant estimates of the sibling analysis.

Finally, it is worth mentioning that our results are similar to the results in Ekhaugen (2009), who finds positive correlations in unemployment between parents and children when not accounting for unobserved family heterogeneity but negative, albeit insignificant, effects as sibling-fixed effects are included.

6 Conclusions

To conclude, the results of our study suggest that children who grow up in households in which the parents received welfare benefits are themselves more likely to receive welfare benefits. This correlation increases with the period spent on welfare by the parents and is present also when we control for a number of household socio-economic controls. Using the sibling comparison approach to identify causal effects, we cannot, however, refute the zero hypothesis of no relationship between parental welfare use and child welfare participation, at least for children in families which received welfare benefits for a limited period of time. This suggests that the positive relationship found in the descriptive analysis was driven by other factors than by welfare use per se. These results are consistent with the previous literature on welfare benefit transmission, which has generally found no or weakly positive evidence of causal intergenerational welfare benefit transmission.

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Appendix A: Supplementary descriptive statistics and estimates

Table A1 compares characteristics of the sibling sample families, where the youngest siblings are all born in 1981–1983, with characteristics for all households with children born in 1981–1983. The characteristics are measured at the time when the child was 17–19 years if age.

Before looking at the pattern in Table A1, one should keep in mind that the sibling sample differs from the general population not only in that there are siblings that are 8 years apart, but also in that the parents did not receive welfare benefits when the older sibling was 17–19 years of age.

Keeping these aspects in mind, Table A1 shows that the families in the sibling sample are on average economically better off than the average families: they are in general less likely to experience parental welfare benefit receipt, have a bit higher income, and are less likely to have unemployed parents. The parents are also more often Swedish-born, and the average education level of the parents is slightly higher, although not by much.

Table A1. *Characteristics of sibling sample compared to all families.*

	Families in sibling sample		All families	
	Mean	Std. dev.	Mean	Std. dev.
Parental welfare during ages 17–19	0.037	0.189	0.123	0.328
Disposable family income (1000 SEK), total 17–19	737.356	830.527	689.312	1742.798
Mother not employed share of years 17–19	0.110	0.283	0.165	0.336
Father not employed share of years 17–19	0.086	0.252	0.143	0.319
Age of mother at child's birth	32.056	3.449	27.940	5.128
Mother born in Sweden	0.959	0.199	0.852	0.355
Mother born in Nordic country	0.029	0.167	0.047	0.212
Mother born in Western country	0.004	0.067	0.010	0.097
Mother born in East Europe	0.003	0.058	0.037	0.188
Mother born outside Europe/Western country	0.005	0.070	0.054	0.226
Father born in Sweden	0.956	0.205	0.862	0.345
Father born in Nordic country	0.021	0.142	0.038	0.192
Father born in Western country	0.009	0.093	0.015	0.123
Father born in East Europe	0.007	0.082	0.031	0.174
Father born outside Europe/Western country	0.008	0.087	0.053	0.224
Mother finished primary school at age 19 of the child	0.168	0.374	0.179	0.383
Mother finished high school at age 19 of the child	0.502	0.500	0.495	0.500
Mother finished college at age 19 of the child	0.330	0.460	0.327	0.469
Father finished primary school at age 19 of the child	0.266	0.441	0.251	0.433
Father finished high school at age 19 of the child	0.430	0.495	0.464	0.499
Father finished college at age 19 of the child	0.305	0.450	0.285	0.451
Children <18 years in the household at age 19 of the child	0.238	0.570		
Observations	4,461		183027-198359*	

* Depending on variable.

Table A2. *LPM for parental welfare use and child welfare use*.

	Child welfare at 24		
	(1)	(2)	(3)
Parental welfare during ages 9-19	0.066***		
	(0.002)		
Parental welfare share of years ages 9-19		0.201***	
		(0.006)	
Parental welfare yearly amount ages 9-19			0.002***
			(0.000)
Sickness benefits mother share of years 9-19	0.036***	0.037***	0.054***
	(0.003)	(0.003)	(0.003)
Sickness pension mother share of years 9-19	-0.003	0.008*	0.004
	(0.005)	(0.004)	(0.005)
Disability pension mother share of years 9-19	0.006	0.009	0.004
	(0.011)	(0.011)	(0.011)
Sickness benefits father share of years 9-19	0.026***	0.032***	0.039***
	(0.003)	(0.003)	(0.003)
Sickness pension father share of years 9-19	0.005	0.015***	0.013**
	(0.006)	(0.006)	(0.006)
Disability pension father share of years 9-19	-0.007	-0.008	-0.006
	(0.011)	(0.011)	(0.011)
Mother not employed share of years 9-19	0.052***	0.026***	0.056***
	(0.003)	(0.003)	(0.003)
Father not employed share of years 9-19	0.054***	0.034***	0.050***
	(0.004)	(0.004)	(0.004)
Mother finished high school at age 19	-0.012***	-0.008***	-0.011***
	(0.002)	(0.002)	(0.002)
Mother finished college at age 19	-0.014***	-0.011***	-0.016***
	(0.002)	(0.002)	(0.002)
Father finished high school at age 19	0.002	0.002	0.001
	(0.001)	(0.001)	(0.001)
Father finished college at age 19	0.001	0.001	-0.003**
	(0.002)	(0.002)	(0.002)
Disposable family income total 9-19 (1 000 000's of SEK)	-3.15e-10***	-4.04e-10***	-1.76e-07*
	(1.03e-10)	(1.15e-10)	(1.01e-07)
Single mother share of years 9–19	0.032***	0.028***	0.047***
	(0.002)	(0.002)	(0.002)

	Child welfare at 24		
	(1)	(2)	(3)
Divorce during ages 9–19	0.011***	0.012***	0.016***
	(0.002)	(0.002)	(0.002)
Nr children 0-17 in the household at age 19	0.003***	0.002**	0.003***
	(0.001)	(0.001)	(0.001)
Mother born in Nordic country	0.005	0.011***	0.011***
	(0.004)	(0.004)	(0.004)
Mother born in Western country	-0.003	0.005	0.003
	(0.006)	(0.006)	(0.006)
Mother born in East Europe	-0.006	-0.007	-0.009
	(0.006)	(0.006)	(0.006)
Mother born outside Europe/Western country	-0.001	-0.008	-0.007
	(0.007)	(0.007)	(0.007)
Father born in Nordic country	0.005	0.013***	0.013***
	(0.004)	(0.004)	(0.004)
Father born in Western country	-0.005	0.005	0.003
	(0.005)	(0.005)	(0.005)
Father born in East Europe	-0.015**	-0.017***	-0.015**
	(0.006)	(0.006)	(0.006)
Father born outside Europe/Western country	-0.007	-0.003	0.002
	(0.006)	(0.006)	(0.006)
Mother’s age at child’s birth	-0.001***	-0.001***	-0.001**
	(0.000)	(0.000)	(0.000)
Mother’s age at immigration	0.000	-0.0003**	-0.000
	(0.000)	(0.000)	(0.000)
Father’s age at immigration	0.0003**	0.000	0.000
	(0.000)	(0.000)	(0.000)
Local unemployment at age 24 of child	0.001***	0.001***	0.0004
	(0.000)	(0.000)	(0.000)
Constant	0.036***	0.040***	0.058***
	(0.005)	(0.005))	(0.010)
Municipality fixed effects	No	No	Yes
Observations	182,200	182,200	182,200
R-squared	0.064	0.072	0.065

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A3. *Sibling fixed effects model for parental welfare during ages 17-19 and child welfare use at the age of 24, siblings born in 1973-75 and 1981-83.*

	Child welfare at 24			
	(1)	(2)	(3)	(4)
Parental welfare 17-19	-0.091**	-0.066	-0.077**	-0.057
	(0.040)	(0.075)	(0.039)	(0.075)
Young	-0.036***	-0.046**	-0.043*	-0.042**
	(0.004)	(0.020)	(0.022)	(0.022)
Sickness benefits mother share of years 17-19		-0.005	-0.005	-0.004
		(0.012)	(0.012)	(0.012)
Sickness pension mother share of years 17-19		0.021	0.019	0.019
		(0.024)	(0.024)	(0.024)
Disability pension mother share of years 17-19		-0.031	-0.020	-0.018
		(0.076)	(0.079)	(0.079)
Sickness benefits father share of years 17-19		0.008	0.007	0.007
		(0.014)	(0.014)	(0.014)
Sickness pension father share of years 17–19		0.015	0.015	0.015
		(0.026)	(0.026)	(0.026)
Disability pension father share of years 17–19		0.118	0.120	0.120
		(0.079)	(0.080)	(0.080)
Mother not employed share of years 17–19		-0.031*	-0.034*	-0.034*
		(0.018)	(0.018)	(0.018)
Father not employed share of years 17–19		0.007	0.008	0.007
		(0.019)	(0.019)	(0.019)
Mother finished high school at age 19		-0.038	-0.045	-0.045
		(0.031)	(0.032)	(0.032)
Mother finished college at age 19		-0.021	-0.028	-0.028
		(0.040)	(0.041)	(0.041)
Father finished high school at age 19		0.012	0.009	0.009
		(0.023)	(0.023)	(0.023)
Father finished college at age 19		-0.002	-0.005	-0.005
		(0.030)	(0.030)	(0.030)
Disposable family income total 17–19 (1 000 000's of SEK)		-0.001	-0.001	-0.001
		(0.003)	(0.003)	(0.003)
Single mother share of years 17–19		0.023	0.021	0.021
		(0.018)	(0.018)	(0.018)
Divorce during ages 17–19		0.001	0.001	0.001
		(0.022)	(0.022)	(0.022)

Table A3 continued.

Nr children 0-17 in the household at age 19	0.007	0.005	0.005
	(0.007)	(0.007)	(0.007)
Father born in Nordic country	-0.090	-0.120	-0.120
	(0.111)	(0.112)	(0.112)
Father born in Western country	-0.011	-0.016	-0.019
	(0.079)	(0.078)	(0.078)
Father born in East Europe	0.075	0.077	0.077
	(0.173)	(0.190)	(0.190)
Father born outside Europe/Western country	0.323	0.342	0.343
	(0.237)	(0.231)	(0.231)
Age of mother at birth	0.004	0.004	0.004
	(0.004)	(0.004)	(0.004)
Age of father at immigration	-0.003**	-0.003*	-0.003*
	(0.002)	(0.002)	(0.002)
Average local unemployment	0.008**	0.008**	0.008**
	(0.003)	(0.003)	(0.003)
Mother born in Nordic country * young		-0.034	-0.034
		(0.032)	(0.032)
Mother born in Western country * young		0.100*	0.100*
		(0.053)	(0.053)
Mother born in East Europe * young		0.052*	0.053*
		(0.030)	(0.031)
Mother born outside Europe/Western country * young		-0.137	-0.136
		(0.097)	(0.097)
Father born in Nordic country * young		0.049	0.049
		(0.046)	(0.046)
Father born in Western country * young		0.015	0.015
		(0.050)	(0.050)
Father born in East Europe * young		-0.018	-0.018
		(0.066)	(0.066)
Father born outside Europe/Western country * young		-0.076	-0.077
		(0.077)	(0.077)
Father unemployed during 1985-1990* young		0.008	0.008
		(0.020)	(0.020)
Father only primary school in 1985-1990* young		-0.009	-0.009
		(0.010)	(0.010)

Table A3 continued.

Mother only primary school in 1985-1990* young			0.021*	0.021*
			(0.011)	(0.011)
Single mother in 1990* young			-0.010	-0.010
			(0.026)	(0.026)
Mother unemployed during 1985-1990* young			-0.012	-0.012
			(0.011)	(0.011)
Welfare family*local unemployment		0.008		0.009
		(0.032)		(0.032)
Observations	8,938	8,938	8,938	8,938
Number of groups	4,461	4,461	4,461	4,461
R-squared	0.020	0.029	0.033	0.034

*Robust standard errors in parentheses. * significant at 5%; ** significant at 1%.*

Table A4 *Sibling fixed effects model for parental welfare share of years 17-19 during ages 17-19 and child welfare use at the age of 24, siblings born in 1973-75 and 1981-83.*

	Child welfare at 24			
	(1)	(2)	(3)	(4)
Parental welfare 17-19	-0.007	-0.004	-0.007	-0.004
	(0.005)	(0.006)	(0.005)	(0.006)
Young	-0.038***	-0.042**	-0.041*	-0.039*
	(0.005)	(0.021)	(0.024)	(0.023)
Sickness benefits mother share of years 17-19		-0.009	-0.009	-0.009
		(0.013)	(0.013)	(0.013)
Sickness pension mother share of years 17-19		0.009	0.008	0.007
		(0.024)	(0.024)	(0.025)
Disability pension mother share of years 17-19		-0.076	-0.077	-0.076
		(0.091)	(0.090)	(0.090)
Sickness benefits father share of years 17-19		0.010	0.010	0.009
		(0.014)	(0.014)	(0.014)
Sickness pension father share of years 17–19		0.019	0.020	0.019
		(0.027)	(0.027)	(0.027)
Disability pension father share of years 17–19		0.099	0.098	0.100
		(0.085)	(0.086)	(0.086)
Mother not employed share of years 17–19		-0.035*	-0.040**	-0.038**
		(0.019)	(0.019)	(0.019)
Father not employed share of years 17–19		0.003	0.003	0.004
		(0.020)	(0.020)	(0.020)
Mother finished high school at age 19		-0.030	-0.042	-0.037
		(0.031)	(0.032)	(0.032)
Mother finished college at age 19		-0.013	-0.026	-0.019
		(0.041)	(0.041)	(0.042)
Father finished high school at age 19		0.012	0.010	0.011
		(0.024)	(0.024)	(0.024)
Father finished college at age 19		0.008	0.005	0.006
		(0.032)	(0.032)	(0.032)
Disposable family income total 17–19 (1 000 000's of SEK)		-0.003	-0.002	-0.002
		(0.004)	(0.004)	(0.004)
Single mother share of years 17–19		0.035*	0.031	0.033*
		(0.020)	(0.019)	(0.019)
Divorce during ages 17–19		-0.002	-0.001	-0.000
		(0.023)	(0.023)	(0.023)

Table A4 continued.

Nr children 0-17 in the household at age 19	0.005	0.005	0.004
	(0.007)	(0.007)	(0.007)
Father born in Nordic country	-0.077	-0.104	-0.102
	(0.114)	(0.114)	(0.114)
Father born in Western country	-0.031	-0.037	-0.045
	(0.047)	(0.046)	(0.046)
Father born in East Europe	0.204*	0.234*	0.235*
	(0.123)	(0.128)	(0.128)
Father born outside Europe/Western country	0.250	0.272	0.271
	0.198	0.207	0.203
Age of mother at birth	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)
Age of father at immigration	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)
Average local unemployment	0.007**	0.009**	0.008**
	(0.003)	(0.003)	(0.003)
Mother born in Nordic country * young		-0.035	-0.036
		(0.032)	(0.032)
Mother born in Western country * young		0.095*	0.097*
		(0.054)	(0.053)
Mother born in East Europe * young		0.071**	0.074**
		(0.028)	(0.030)
Mother born outside Europe/Western country * young		-0.132	-0.129
		(0.103)	(0.102)
Father born in Nordic country * young		0.040	0.041
		(0.044)	(0.043)
Father born in Western country * young		0.024	0.025
		(0.053)	(0.053)
Father born in East Europe * young		-0.057	-0.059
		(0.061)	(0.061)
Father born outside Europe/Western country * young		-0.087	-0.087
		(0.079)	(0.079)
Father unemployed during 1985-1990* young		0.018	0.017
		(0.020)	(0.020)
Father only primary school in 1985-1990* young		-0.008	-0.007
		(0.010)	(0.010)

Table A4 continued.

Mother only primary school in 1985-1990* young			0.020*	0.021*
			(0.011)	(0.011)
Single mother in 1990* young			-0.026	-0.023
			(0.028)	(0.028)
Mother unemployed during 1985-1990* young			-0.014	-0.013
			(0.011)	(0.011)
Welfare family*local unemployment		0.030		0.029
		(0.019)		(0.019)
Observations	8,938	8,938	8,938	8,938
Number of groups	4,461	4,461	4,461	4,461
R-squared	0.083	0.092	0.095	0.097

*Robust standard errors in parentheses. * significant at 5%; ** significant at 1%.*

Table A5. *Sibling fixed effects model for parental welfare/year during ages 17-19 and child welfare use at the age of 24, siblings born in 1973-75 and 1981-83.*

	Child welfare at 24			
	(1)	(2)	(3)	(4)
Parental welfare 17-19	-0.007	-0.004	-0.007	-0.004
	(0.005)	(0.006)	(0.005)	(0.006)
Young	-0.038***	-0.042*	-0.041*	-0.039*
	(0.005)	(0.021)	(0.024)	(0.023)
Sickness benefits mother share of years 17-19		-0.008	-0.008	-0.008
		(0.013)	(0.013)	(0.013)
Sickness pension mother share of years 17-19		0.009	0.007	0.006
		(0.025)	(0.025)	(0.025)
Disability pension mother share of years 17-19		-0.077	-0.078	-0.077
		(0.091)	(0.090)	(0.090)
Sickness benefits father share of years 17-19		0.008	0.008	0.008
		(0.014)	(0.014)	(0.014)
Sickness pension father share of years 17–19		0.020	0.020	0.020
		(0.027)	(0.027)	(0.027)
Disability pension father share of years 17–19		0.099	0.098	0.099
		(0.085)	(0.086)	(0.086)
Mother not employed share of years 17–19		-0.034*	-0.039**	-0.037*
		(0.019)	(0.019)	(0.019)
Father not employed share of years 17–19		0.004	0.004	0.005
		(0.020)	(0.020)	(0.020)
Mother finished high school at age 19		-0.030	-0.041	-0.037
		(0.031)	(0.033)	(0.033)
Mother finished college at age 19		-0.013	-0.025	-0.019
		(0.042)	(0.042)	(0.042)
Father finished high school at age 19		0.010	0.008	0.009
		(0.023)	(0.024)	(0.024)
Father finished college at age 19		-0.003	-0.006	-0.006
		(0.031)	(0.032)	(0.031)
Disposable family income total 17–19 (1 000 000's of SEK)		-0.002	-0.002	-0.002
		(0.004)	(0.004)	(0.003)
Single mother share of years 17–19		0.033*	0.030	0.031
		(0.020)	(0.019)	(0.020)
Divorce during ages 17–19		-0.006	-0.006	-0.005
		(0.023)	(0.023)	(0.023)

Table A5 continued.

Nr children 0-17 in the household at age 19	0.005	0.004	0.004
	(0.007)	(0.007)	(0.007)
Father born in Nordic country	-0.070	-0.099	-0.096
	(0.114)	(0.114)	(0.114)
Father born in Western country	-0.026	-0.033	-0.041
	(0.047)	(0.046)	(0.046)
Father born in East Europe	0.207*	0.237*	0.237*
	(0.123)	(0.128)	(0.129)
Father born outside Europe/Western country	0.255	0.277	0.276
	(0.232)	(0.225)	(0.225)
Age of mother at birth	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)
Age of father at immigration	-0.003*	-0.003	-0.003
	(0.002)	(0.002)	(0.002)
Average local unemployment	0.008**	0.009***	0.008**
	(0.003)	(0.003)	(0.003)
Mother born in Nordic country * young		-0.035	-0.036
		(0.033)	(0.033)
Mother born in Western country * young		0.097*	0.099*
		(0.053)	(0.053)
Mother born in East Europe * young		0.069**	0.073**
		(0.028)	(0.030)
Mother born outside Europe/Western country * young		-0.132	-0.129
		(0.103)	(0.103)
Father born in Nordic country * young		0.041	0.043
		(0.044)	(0.044)
Father born in Western country * young		0.026	0.028
		(0.053)	(0.053)
Father born in East Europe * young		-0.056	-0.058
		(0.061)	(0.061)
Father born outside Europe/Western country * young		-0.087	-0.087
		(0.080)	(0.080)
Father unemployed during 1985-1990* young		0.019	0.018
		(0.020)	(0.020)
Father only primary school in 1985-1990* young		-0.007	-0.007
		(0.010)	(0.010)

Table A5 continued.

Mother only primary school in 1985-1990* young			0.020*	0.020*
			(0.011)	(0.011)
Single mother in 1990* young			-0.021	-0.018
			(0.028)	(0.028)
Mother unemployed during 1985-1990* young			-0.013	-0.012
			(0.011)	(0.011)
Welfare family*local unemployment		0.030		0.029
		(0.019)		(0.019)
Observations	8,938	8,938	8,938	8,938
Number of groups	4,461	4,461	4,461	4,461
R-squared	0.082	0.091	0.095	0.096

*Robust standard errors in parentheses. * significant at 5%; ** significant at 1%.*

Essay 3: Does competition improve quality? Empirical evidence from Swedish primary health care[61]

[61] I am grateful for valuable comments and suggestions from Eva Mörk, Anders Forslund, Henrik Jordahl, Erik Lindqvist Mikael Lindahl and participants at the UCLS Brown bag seminar. I would also like to thank Eric Sätterström and Jakob Ask for providing me with data.

1 Introduction

Competition is generally perceived as beneficial for consumers as it contributes to higher quality and lower prices. Over the past few decades several governments have sought to reap the benefits of competition in publicly funded welfare services, breaking the dominant role of the public sector in this field. Sweden, like several other countries, has opened up traditionally public institutions to private ownership and more competition with the aim of improving service provision and cost efficiency and increasing choice possibilities for the users of welfare services. The reforms have been backed up with arguments about the higher efficiency of private firms and that more competition will lead to better performance among public providers as well (Shleifer, 1998).

However, the market for welfare services differs from conventional markets in many aspects. Most importantly, the markets for welfare services are quasi markets, i.e., markets where the government acts as purchaser of services from competing providers either on a contract basis or through a voucher system. This means that there is no room for price competition among providers. Instead providers can compete for customers with quality. But, the scope for quality competition may differ across markets. A key contributing factor in quality competition is that quality can actually be observed. Among the criteria of well-functioning quasi markets, Le Grand and Bartlett (1993) point out accurate information for purchasers and providers. When quality is difficult for buyers to monitor, the incentives for providers to offer good quality may be taken over by other priorities, such as reducing costs.[62] A similar argument can be found in the literature on outsourcing of public services: the contractibility of services is seen as highly dependent on the opportunities to observe and verify quality (see for instance Andersson and Jordahl, 2011). Voucher systems are often believed to solve some of the problems involved in contracting of services, but with information asymmetries present, it may be equally difficult for users of these services to assess quality and performance as for public contractors.

[62] Profit is a clear motive to shirk on quality, but also public providers can fail to provide the highest quality, for instance by not engaging in innovation or not using resources efficiently (Hart, Shleifer and Vishny, 1997, Shleifer, 1998).

The contribution of this paper is to provide empirical evidence on the impact of increased private provision and competition on primary health care quality in Sweden. Health care is often regarded as a market with pronounced information asymmetries as consumers' ability to assess the quality provided by care givers is limited. Health care personnel are generally more aware of the appropriate diagnostics and treatments than are patients. Not only does the information asymmetry complicate the choice of a provider, but determining the quality of health care services is also difficult *ex post*. Health care services therefore belong to the group of goods labeled as credence goods (see e.g. Darby and Karni, 1973 and Dulleck and Kerschbamer, 2006). The common trait for this type of goods or services is that the provider or seller is an expert who knows more about the quality of the good or service than the consumer, and hence can abuse their information advantage to supply an excess amount of services at an unnecessarily high cost or services of poor quality.

On the other hand, some aspects of health care quality may be easy to observe: waiting times and telephone availability are observable for patients either through their own experience, the provider's reputation or Web pages comparing availability. If a health care provider does not satisfy patients' demands in terms of these aspects, the patients are likely to look for another provider. This clearly provides incentives to practices that want to stay in the market to satisfy patients' demands. As suggested in the multitasking model by Holmstrom and Milgrom (1991), when certain tasks or aspects of a task are rewarded, providers may react by giving higher priority to performance of these, resulting in less attention being given to less rewarding activities. In the present setting, it may be more rewarding for providers to focus on observable quality components, such as opening hours, than to invest in better routines or in being more generous with medical tests, aspects of which patients may be unaware.

This paper investigates how primary health care quality is affected by competition from private providers. An additional aim is to analyze how the institutional features in the market affect the conditions for competition. First, I investigate the impact of competition in the market. This part of the analysis utilizes local variation in the expansion of private provision in Sweden since 1998, caused by the increasing prevalence of outsourcing of primary health care in many counties. Second, I investigate how the organization of primary health care in terms of the conditions for competition between units matter for quality. Some regions have introduced financing systems that entail stronger incentives for competition. The principle by which the regulator reimburses health care providers is an important instrument for imposing the desired incentives. For instance, capitation systems are designed to provide the market participants with incentives for competition whereas the traditional budget transfer system gives providers weaker incentives to compete with each other. To investigate this issue the variation in the

reimbursement systems across counties and over time is utilized to determine the extent to which such institutions matter for quality competition. In addition, I examine the effects of a patient choice reform implemented during the period of 2007-2010. Since 2010 all counties are required to have implemented patient choice systems in primary health care, but at this point several counties had already introduced the central features of the reform. The reform aimed at increasing competition between health care providers by introducing free entry for providers and financing by capitation for the counties that had not introduced it earlier. The purpose of the reform was to strengthen the link between the patient´s choice and the allocation of resources between health care providers, hence letting patient demand decide which providers should survive in the market. I also examine the impact of introducing patient choice on quality using the variation in the timing of across counties.

Using a comprehensive set of measures of primary health care quality, the impact of private provision is estimated. Since the effect is identified by changes in the share of private providers and the organization of health care on regional level, the empirical strategy accounts for permanent cross county differences that could be correlated with the provision of quality. For instance, large city regions may be able to recruit better physicians.

The analysis makes use of several outcome variables, of which a first group of measures is intended to capture aspects of quality that are easy for patients to observe, and a second group of measures, which focus on the medical quality, are intended to capture aspects less observable for patients. To evaluate the effect on observable quality three different measures are considered, most of which focus on the extent that primary health care is available and easily accessible. First, the effect on the number of visits to a primary health care physician per capita is considered. This measure is intended to capture to what extent care givers satisfy the population's demands for frequent and immediate visits to health care. A second measure is the prescription of antibiotics. The use of antibiotics may reflect good availability and high quality care, but supplying unnecessary antibiotics can also be a way to attract and retain patients (Bennet et al., 2011). Thirdly, the results of a survey of the population's view of health care are used. With this data it is possible to evaluate patient satisfaction with health care in terms of how long patients have to wait to see a physician and how they rate their visit.

Whereas patients can easily observe quality measures such as waiting times and the behavior of doctors and nursing staff, some other aspects of quality such as medical expertise and the quality of a specific treatment may be harder for patients to assess. To capture this dimension I also analyze the effects on granted insurance claims and avoidable hospitalizations. The granted insurance claims provide a source of information on the risk of injury in the primary health care system, which most likely reflects medical quality. A second measure of unobservable quality is the number of avoida-

ble hospitalizations. The incidence of avoidable hospitalizations has been widely used as an indicator of the quality of diagnosis and treatment in primary care (see for instance Fleming, 1995). The idea behind the measure is that if primary health care can provide timely and effective care, hospitalization is unnecessary for some conditions.

This paper adds to the literature on the effect of competition and outsourcing in the market for health care. The empirical literature on the quality effects of competition in health care markets focuses mainly on competition in the hospital sector. The market for primary health care has been investigated to a much more limited extent. Since the organization of primary health care in Sweden differs from most other OECD countries due to the dominant role of the Swedish public sector, it is an interesting setting to investigate the effects of introducing market reforms in primary health care. There are few studies analyzing the impact of competition on quality in primary health care, partly because it is regarded as a market with little market concentration since in most countries these services are provided by small private firms (Gaynor and Vogt, 2000). Another reason is that access to good data is limited. Among the few empirical studies from primary care, Pike (2010) examines the effects of competition on quality in the British market for general practitioners. He finds a positive relationship between competition and patient satisfaction and clinical quality, measured as unnecessary referrals to hospitals. Since the analysis is based on cross sectional data it is difficult to separate a causal effect from unobservable factors in the areas with more competition which also could affect patient satisfaction and the number of preventable hospital visits. Bennett, Hung and Lauderdale (2011) examine whether market concentration has implications for quality competition in terms of the prescription of antibiotics in Taiwan. They find that competition increases the prescription of antibiotics. Both these studies concern competition between small private GP practices, whereas this paper also addresses how institutional features such as private or public ownership and the conditions for market competition are related to the provision of quality. In a related study on Swedish data, Fogelberg and Karlsson (2012) investigate the effect of the introduction of patient choice in primary health care on the prescription of antibiotics. Their findings suggest that the increased competition resulting from the reform had a positive and significant effect on antibiotics prescription.

Most of the earlier empirical evidence on the effects of competition on quality originates from hospital markets.[63] For example, Kessler and McClellan (2000) find that market concentration is associated with higher

[63] Since the theoretical implications of quality competition differ between markets where prices are regulated and markets where prices are set by firms, I discuss only the studies on markets where prices are regulated as they are in Sweden. The studies involving regulated prices with US data focus mainly on Medicare patients.

mortality for heart attack patients. In contrast, Gowrisankaran and Town (2003) find higher mortality rates for heart attacks and pneumonia in hospitals located in more competitive markets. The difference between these studies is that whereas Kessler and McClellan use national data and measure market concentration on the regional level, in the latter study the data cover only the county of Los Angeles and that it uses hospital specific measures of market concentration. There are also a handful of studies using variation from reforms introducing more competition in the hospital market in UK. Among these, Propper at al. (2008) find that competition reduces quality in terms of heart attack mortality. Their identification is based on differences in the scope for increased competition following a deregulatory reform across different areas. In a later study, Bloom et al. (2010) find that competition led to improved management quality, which was positively associated with lower mortality rates.

The empirical results of this paper give no unanimous evidence of the impact of competition and market reform on quality. The theoretical prediction of improved quality, especially for aspects of quality which are easy for patients to assess, following competition is not confirmed. Even though the results suggests that competition in the market for primary health care is associated with more primary health care visits, the results for waiting times give no support for an effect on availability. On the other hand, clinical quality, measured as the number of granted insurance claims, seems to be higher in markets with capitation and a large share of private providers. However, this result is not supported by the results for avoidable hospitalizations. Hence, no clear conclusions for the measures of less observable qualiy can be drawn.

The introduction of patient choice is associated with an increase in the number of doctor's visits and antibiotics prescriptions as well as shortened waiting times, but the relationship is not necessarily an effect of increased competition as the costs for primary health care seem to be positively affected by the reform.

The rest of the paper is organized as follows: The following section describes the organization of the Swedish health care system. The third section presents an overview of the main theoretical arguments. Section 4 presents the data and section 5 the empirical strategy. In section 6 the results are presented, followed by some final comments in section 7.

2 Institutional background

Health care is a central part of the Swedish welfare state: In 2010 health care costs corresponded to 9.7 percent of GDP. The guiding principles for health care are established at the central level, although the production of health care is administered on the county level. Counties are governed by 21 regional governments, county councils, appointed by the county electorate. County councils levy regional taxes to finance their responsibilities, which encompass, besides health care, public transport and culture. Health care is mainly funded through these local taxes and state grants constitute the second largest source of funding. The remainder is financed by patient fees. Although most of the privately provided care is publicly financed a market for private health insurance exists, but its size is negligible.

The Swedish model for primary health care distinguishes itself in an international perspective. The largest part of primary care is performed by public providers, unlike in most OECD countries where private providers dominate. Private health care is limited almost exclusively to outpatient care, however, privatizations of hospitals have occurred.[64] Outpatient care consists mainly of primary care and outpatient specialist care, as opposed to inpatient care which is the care performed on patients admitted to a hospital.

Primary care constitutes the largest part of out-patient care. Its main function is to provide care by a general practitioner (GP), but it also includes visits to children's clinics and antenatal clinics. It typically constitutes the first contact with the health care in case of illness, whether it be a matter of chronic disease or a temporary condition. Outpatient care also encompasses specialist care, such as visits to a gynecologist, dermatologist, or ear, nose and throat specialist. Specialist care clinics are often, but not always, situated in hospitals, but visits do not count as inpatient care as long as the patient is not admitted to the hospital. To visit a specialist, a doctor's referral (most often from a primary care facility) is often required, which means that GPs fill the role of gatekeeper for more advanced care. Admission to hospitals can also occur through visits to the hospital emergency department in the case of acute illness.

[64] For instance, since 1999 one of the largest hospitals operating in the capital city, Stockholm, is run by a private firm.

2.1 Privatization and patient choice

In the 1960s and 1970s, public health care expanded in Sweden. Up to this point, a large proportion of health care was provided by physicians in private practice. The rapid expansion public health care and the introduction of the 1970 reform referred to as the Seven-Crown reform (due to the maximum fee instituted at this time) made it difficult for private practices to operate in the market. The reform imposed limitations on physicians' possibilities to run a private clinic while in public employment and the new reimbursement system favored public facilities, but private facilities could still freely exist in and enter the market.

In 1985 a reform known as "the Dagmar reform" was introduced. The reform restricted free entry of private practices, since they would need a contract with the county to be able to operate with public funding. The contracts also included a maximum number of patients that the private practices could see per year.

Since the beginning of the 1990's the Swedish healthcare system has undergone extensive deregulation and privatization. This development began with the decentralization of regulatory power to the county councils in 1991, which made it legally possible for county councils to decide how to organize the provision of health care. Following decentralization, many counties introduced purchaser-provider models, creating regional quasi markets, in which private health care providers could coexist with public ones. As a result private providers became increasingly common in some counties, mainly in outpatient care, leading to increased competition in a market previously dominated by public providers.

In 1989 the Swedish Association of Local Authorities and Regions (SALAR) recommended to the counties to introduce free choice of primary care provider, a recommendation which all counties follow since 1991. The recommendation implied that patients have the full right to choose their permanent health care contact, private or public, and that it should not be directed to a specific location. Since 1999, the recommendation has been inscribed in law.

There have also been initiatives towards privatization and consumer choice from the central governmental level. In 1994 the Primary Doctor Reform was implemented, giving all residents the right to choose their own house doctor. In the same year free entry of private GPs was introduced, and private general practitioners could then establish themselves in the market with public funding based on the number of listed patients they had. These reforms were abolished in 1995 and 1996, respectively, although they led to a permanent increase in private practices and some counties preserved some of the features of the reform, such as listing, even after the abolishment. Following the repeal of the house doctor system, private sector house doctors could continue to operate with compensation from the National Insurance

Board as long as they had a contract with the county council. However, they represent a small share of primary health care, in 2002 365 GPs received compensation and in 2010 the number had fallen to 316 (Swedish Association of Local Authorities and Regions, 2003 and 2010).

Table 1. *Deregulatory reforms in Swedish primary health care.*

Year	Reform
1989	Recommendation of free choice of primary health care provider.
1991	All counties have introduced free choice of health care provider
1994	Primary doctor reform and introduction of free entry
1995	Primary doctor reform abolished
1996	Free entry abolished
1999	Free choice of primary health care provider fixed by law
2007	The first county introduces patient choice (Halland).
2009	The law concerning patient choice in primary health care and care for the elderly (LOV) is implemented.
2010	Patient choice has been introduced in all counties

After the abolition of free entry and the house doctor system, private provision of health care continued to increase, but mainly through outsourcing of outpatient care (Anell, 2011). In 2000 about 85 percent of the counties practiced outsourcing of health care to private firms to some extent at least. Since free entry was removed, private firms could only enter the market if contracted by the county council. The contracts needed to be subject to public procurement. Counties were required by law (LOU 2007:1091) to publicly advertise contracts and provide details of the services to be out-sourced and the requirements on the provider. Firms responded with offers and counties choose which provider to contract. The contract specified the quantities of health care to be performed by the provider. In this system the competition between firms only occurred during the procurement process, and was thereafter limited until the next procurement occasion. The extent to which competition in health care has been introduced varies between counties. The last three columns of Table 2 show that the share of privately provided services increased through outsourcing in many counties from 1998 through 2006. In other counties, the share of services provided by private firms has remained low.

Primary health care have traditionally been funded by yearly budgetary transfers from the county councils. The budget for a particular year is based on the costs of the previous year. Since providers cannot increase their revenues by increasing supply, this system give providers weak incentives to provide high quality health care to attract and retain patients. In the past few decades, reimbursement through capitation has become increasingly popular. The first column of Table 2 shows the year in which capitation-based reimbursement was introduced in each county. Capitation is a reimbursement

principle by which providers' compensation depends on the number of residents listed for their practice. Since a practitioner's revenue depends on the number of patients that choose a practice as their primary caregiver, capitation is associated with stronger incentives to attract and retain patients by providing good quality. On the negative side, providers may be compelled to avoid patients with extensive care needs, a problem that some counties have tried to overcome by weighting reimbursement by age, socioeconomic status or past health. Also, in some counties capitation payments are supplemented with a fixed amount and it is common that practices may retain the patient fee. As Table 2 shows, of the counties that introduced capitation before it became obligatory (as described in the next section), some counties have used capitation for a long time, while others did not introduce it until quite recently.

In 2009 a law called "the Patient Choice reform" (LOV) was implemented. Since January the 1st in 2010 the Health and Medical Service Act (1982:763) states that patient choice paired with free entry of private health care providers is obligatory in all counties.[65] In three counties patient choice was implemented before the LOV was enacted: In Halland a patient choice reform came into effect 2007, followed by Stockholm and Västmanland in 2008. Column 2 of Table 2 shows the introduction dates for all counties. As described above, free choice of primary health care provider has been applied since the beginning of the 1990s, and hence the name if the reform is somewhat misleading. Instead, the novelty of the reform was that that the patients' choice shall decide the allocation of resources between health care providers, as the means follow the patient, combined with increased competition in the market by the introduction of free entry. With the enactment of the patient choice reform in 2010, all counties have turned to capitation based payments.

[65] In spite of this, three counties failed to do so. By June 2010 all counties had adopted patient choice.

Table 2. *Primary health care reforms and private provision by county.*

County	Capitation	Patient choice	Share of physician visits in private primary health care		
			1998	2006	2010
Norrbotten	2010	January 2010	9.6	9.6	14.3
Västerbotten*	<1998	January 2010	10.5	16.3	19.7
Jämtland	2006	January 2010	3.8	9.3	9.6
Västernorrland	2004	January 2010	2.0	14.2	25.4
Dalarna	2010	May 2010	5.6	6.5	5.7
Gävleborg	<1998	January 2010	11.4	18.2	26.9
Värmland	2005	January 2010	9.6	31.3	18.9
Örebro	<1998	January 2010	19.4	20.2	19.9
Västmanland	<1998	January 2008	44.2	43.7	57.2
Uppsala	2006	January 2010	20.5	12.2	32.0
Stockholm	<1998	January 2008**	31.0	50.1	59.6
Sörmland	2010	January 2010	15.3	20.4	30.1
Östergötland	2001	September 2009	8.4	10.9	13.9
Västra Götaland*	<1998	October 2009	17.0	33.2	39.0
Jönköping	2010	June 2010	12.3	15.0	27.3
Kalmar	<1998	January 2010	15.4	16.6	19.1
Halland	2007	January 2007	24.7	39.3	n.a.
Kronoberg	2009	March 2009	0	15.0	26.7
Blekinge	2010	January 2010	17.0	19.3	22.7
Skåne	2002	May 2009	33.2	37.0	41.6
Gotland	2002	March 2010	0.1	0	25.6

* Capitation was not in use county-wide until 2004 in Västerbotten and 2002 in Västra Götaland but the most populated areas had introduced it by 1998. ** Stockholm county includes the municipality of Norrtälje, which did not introduce patient choice until November 2010.

Although patients could choose which provider to turn to and also to list themselves at a particular practice even before the implementation of the reform the introduction of patient choice strengthened the patient's right to choose a health care provider. For instance, in patient choice systems there is no ceiling in how many patients that can register themselves to a specific doctor or practice (although practices can apply for a ceiling in special cases). An exception is Stockholm county where health care providers may limit the number of listed patients (Konkurrensverket 2010:2). In addition, the patient's possibilities to choose are improved in the patient choice system, as information about the various health care providers is readily available through leaflets sent to households and through webpages listing practices and since active choice is encouraged. For the counties which had not introduced it before, patient choice has also been associated with a transition from a grant based compensation system to capitation.[66] The major change

[66] In Stockholm reimbursement by capitation is supplemented by a fee-for-service system which amounts to about half of the total reimbursement. In other counties the fee-for-service consists of the patient fee, which is around SEK 100–200 per visit.

in the terms of competition comes from the free entry regulation. Free entry implies that providers are allowed to establish themselves in the market if authorized by the county and there is no limit for how many health care units the county can authorize.[67] Before the introduction of patient choice, entry to the market was decided by county councils, which chose which health care units that should be contracted out. As can be seen by comparing the last two columns of Table 1 the patient choice reform clearly has changed the terms of competition by increasing the share of privately provided services.

To sum up, in the recent years the institutional setting for competition in Swedish primary health care has changed from public procurement and grant financing to a market of free entry patient choice and capitation based reimbursement. Together, these factors have led to stronger incentives for competition in the market. The next section discusses the theoretical implications of the reforms in the market for primary health care described above.

[67] To obtain authorization firms must meet specific requirements, such as demands on the personnel qualifications, financial stability and compliance with the health care program of the county.

3 Theoretical framework and related literature

This paper seeks to investigate the effect of market reforms on the quality of primary health care provision. The inflow of private firms has led to less market concentration in a market previously dominated by public providers and also to an increase in private provision of services. In addition, the incentives for competition vary between counties as a result of differences in the reimbursement system and the free entry regulation. An additional contribution of this paper is therefore to investigate how the market reacts to financial incentives in the reimbursement system and how these incentives interact with the share of private privately provided services. Below, I discuss the theoretical arguments to why competition induced by more private actors in the market and by the financial incentives could matter for quality.

3.1 Competition

As described in the previous section, many counties have allowed an inflow of private firms into the market for primary health care through outsourcing during the 1990s. The increase in private provision has introduced more competition in the market, since the publicly run health care facilities have been complemented by private firms. To understand how this aspect of deregulation can be expected to affect quality I turn to a model of competition and quality in a regulated price setting. The theoretical literature on competition and quality under regulated prices suggest that when the price is set above the marginal cost, competition leads to higher quality (Gaynor, 2006). When providers cannot compete in terms of prices, firms will try to attract patients by providing better quality. If patients can freely choose their primary health care provider, their choices will reflect their perception of the quality of treatment. For this reason patient demand is often suggested as a regulatory tool for achieving quality competition in health care.

Depending on the institutional features of health care provision the market can still be more or less competitive. An important factor for competition to come to effect is the incentives induced by the reimbursement principles used. There is a large body of literature (see for instance Barnum et al, 1995 and Liu and Mills, 2007) on the incentives provided by the different types of compensation systems used in health care provision. Financing through budgetary transfers, has been one of the most common forms of provider

payment to public sector units (Barnum et al, 1995). Budgetary transfers give government officials large control over spending although the incentives for the provider to keep costs down will be low when the budget exceeds the actual costs. However, since revenues are not based on the number of patients that chose a particular provider, financing through budgetary transfers provides low incentives for improving quality, even when there are a large number of providers in the market. Often the budget of year t is based on costs in year t-1, but large costs do not necessarily represent provision of high quality, but could also be result of shirking.

In capitation systems providers are paid a fixed amount per patient tied to their practice. Since the total cost can be predicted by the authorities, payment per capitation also allows for cost control. The difference to budget financing is that capitation gives providers incentives to compete to attract and retain patients as the payment follow the choice of the users. Capitation payment is similar to fee for service in this respect, but whereas capitation give providers incentives to avoid patients with large health care needs, fee-for-service give doctors incentives to provide unnecessary medical services.

In the Swedish system two main principles have been used, budgetary transfers and capitation. With the introduction of patient choice all counties have adopted capitation systems. Since the patient choice reform free entry also prevails in the market for primary care. Free entry implies that the risk of being driven out of the market increases and hence the stakes of providing bad quality increases.

Prediction: In markets with more providers there is larger scope for quality competition. As long as the regulated price exceeds the cost of providing a baseline level of quality, firms will increase quality to increase their market share. The large inflow of firms with the introduction of free entry in connection with the patient choice reform in 2010 suggests that the profitability in the market was perceived as good. Hence, the inflow of new firms to the Swedish primary care market could be expected to increase quality competition. Capitation based reimbursement systems and free entry increases the incentives for quality competition, and hence a larger effect from competition is to be expected, when capitation is in place.

3.2 Different aspects of quality

The model above assumes that quality is observable for patients. As emphasized since Arrow (1963), patients may not necessarily be good judges of health care quality. In fact, the quality of health care services is often difficult to assess even after the actual treatment have taken place. If the patient's condition persists, it is hard to know if it is due to insufficient quality of the treatment or other factors, such as lifestyle or just bad luck. This particular feature is common for all goods and services labeled credence goods (see for

instance Darby and Karni, 1973 and Dulleck and Kerschbamer, 2006). Since consumers of credence goods are not aware of the quality they need, providers have an information advantage over the patient and can hence chose to provide too high or too low quality without the risk of being detected. When providers receive payment per service the incentives for them to overstate the problem increases, which can lead to excessive use of medical services at a higher cost is situations when a more simple and inexpensive treatment would have been sufficient. In Swedish publicly funded primary health care, the incentives to provide too much treatment are low, as reimbursement mainly occurs though fixed payments per practice or patient. Instead, providers may be compelled to provide lower quality.

However, some aspects of quality, such as opening hours and telephone availability may be easy to evaluate, whereas aspects such as the use of new technical equipment, diagnostics tests and up to date medical knowledge among the staff are aspects of quality that may be impossible for the patient to assess. The multitasking model of Holmstrom and Milgrom (1991) suggests that rewards to certain aspects of quality may harm the unrewarded dimensions of quality. According the model, providers allocate effort to multiple tasks. When purchasing the service, the principal can direct rewards to certain tasks to increase the performance in this dimension. The problem is that it is difficult to tie rewards to aspects that are hard to measure, and hence the result might be that the provider puts disproportionate effort on the rewarded elements, at the cost of lower effort on non-measurable aspects. Patient choice is sometimes regarded as a regulatory tool for ensuring quality, but in the presence of information asymmetries for some, but not other aspects of quality the same problem may arise. In this case multitasking theory predicts that providers will divert resources away from tasks which are not rewarded by increases in patient demand from aspects that patients are unaware of and which therefore do not affect consumer demand. If rewarded and unrewarded aspects are related to each other in some way it may be expected that both aspects will improve by a spillover effect (Werner et al., 2005). An example is increased availability which is easily observed and can be convenient for the patient, but it may also affect the chances of successful treatment, which is an outcome that the patient cannot observe in advance.

Prediction: Competition will lead to higher quality in dimensions that are observable for the contractor or patient, whereas for less observable quality measures competition will have no or a negative effect, unless they are positively correlated with the more easy observed quality measures.

3.3 Ownership

Except for changing the terms for competition in the market, deregulation of primary health care have also led to an inflow of private firms and hence a change in the composition of the firms operating in the market. One of the key arguments in the property rights literature is that private ownership is more efficient than public ownership. Private firms are more concerned with financial costs as they internalize profits, which give them incentives to engage in more efficient production.[68] Outsourcing is often motivated by this argument, but it has also been suggested that when problems of contractual incompleteness are present, outsourcing might have a negative effect on quality. This standpoint is formalized in an influential paper by Hart, Shleifer and Vishny (1998). They show that, due to differences in incentives, private contractors devote more attention to cost reduction at the cost of lower quality than public managers. Private firms are also more motivated to engage in quality innovation than are public managers since they can reap the benefits of their innovations. The situation arises because a manager of a private firm has residual control rights and can realize the net gains from cost reductions, whereas quality improvement is less rewarding as long as it is not contracted upon. The key insight from the model is that whereas costs are always lower with a private contractor running the facility, the quality may be either lower or higher, depending on the contractibility of quality and the opportunities for quality innovation. In situations where quality is incontractible to a large extent and the scope for quality innovations is low, the benefits of in-house provision are larger. The case for private provision is stronger when the risk for reduction in quality from cost reductions is low compared to the scope for quality innovations.

Prediction: Which category health care belongs to is difficult for the authors to decide upon. Hart et al. suggest that the possibilities of quality innovation in health care are large, but so is the risk of cost reductions damaging quality. It is therefore difficult to predict the potential effect of an increase in private provision of primary health care.

[68] The higher efficiency of private health care has not been consensually proven empirically. See Hollingsworth (2003) for a review of the literature.

4 Data

The analysis is based on data from several different sources. The measure of private provision and many of the quality measures originate from the Swedish Association of Local Authorities and Regions. Control variables are collected from Statistics Sweden's registers. Since there are no registers over primary health care consumption on individual level, the variables are measured as aggregated on county and year. A detailed description of the variables can be found in Table A2 in the Appendix.

In the literature on quality effects of hospital competition it is common to use measures of market concentration such as the Herfindahl index as explanatory variable. Unfortunately, most of the quality measures used are not available on a lower level than county. Since the most appropriate level for measuring market concentration would be municipality or neighborhood, such an approach is not possible in this case. Instead the share of services performed by private practices is used to capture to what extent competition from other providers than the public sector matter for quality. In addition, to measure the terms of competition in the market two additional variables are used to define the reimbursement system in place in each county (capitation or budgetary transfers) and whether the patient choice reform was implemented. Figure 1 illustrates the variation in private provision divided by reimbursement system during the period 1998-2006.[69] As the graph shows, in counties with capitation systems private providers have a larger market share than in counties with a budgetary transfer system. The third line in the graph represents counties which changed from budgetary transfers to capitation during the period 1998-2006. The graph indicates that a change in financing system does not seem to be associated with increasing outsourcing—the growth in share of private primary care services in these counties follow the same pattern as in counties which practices budgetary transfers during the full period.

[69] To be able to separate the effects of private provision, capitation and free entry the analysis is divided into two separate parts: one concerning the introduction of private provision and capitation and a second part which concerns the introduction of patient choice. The graph, illustrates the period of the first part of the empirical analysis, which focus on the time prior to the introduction of patient choice.

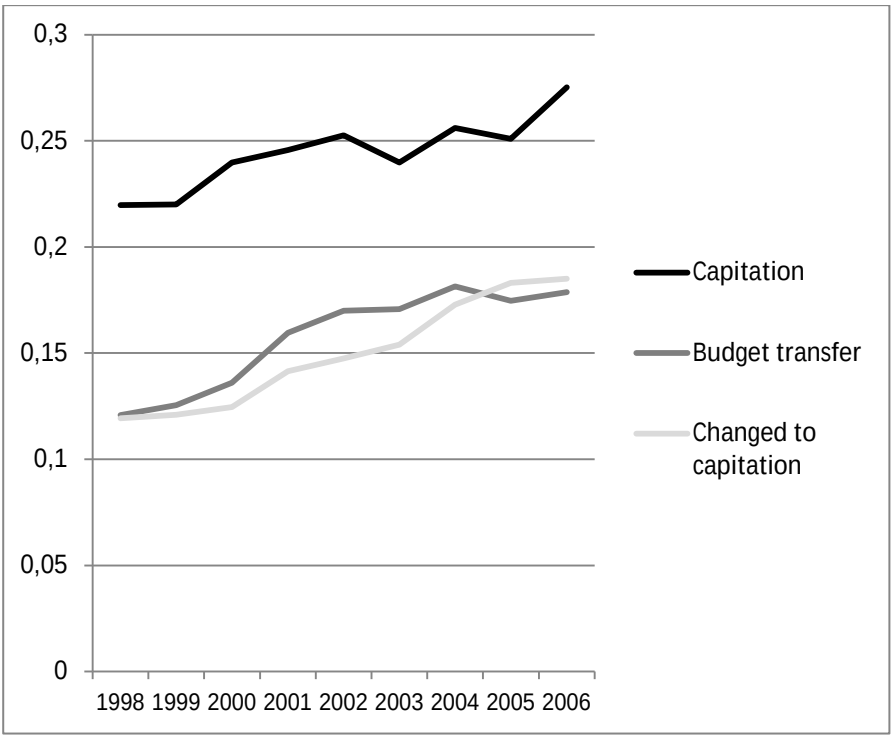

Figure 1. Share of private visits to a primary health care physician by reimbursement system, 1998-2006.

4.1 Quality

Measuring quality in primary health care is not straightforward. There are no registers encompassing all patients and treatments in primary care as is the case with in-patient care performed in hospitals. Instead, in this study outcome variables measured on the county level are collected from four different sources. Table A2 in the Appendix describe the measures used and provides the data source of each measure.

The first outcome variable to be used in the analysis is the number of visits to a primary care physician per capita. The measure can be interpreted as an indicator of availability since more doctor´s visits suggests that it is easier for people to reach their primary care facility instead of waiting for recovery or turning to the emergency. It could also represent quality if it for instance means seeing a physician instead of a nurse.[70] The second measure is the

[70] The availability of health care can affect long run health outcomes. Some studies suggest that higher primary health care availability is associated with lower mortality and less hospital utilization, whereas other studies find no such effects (Rosano et al., 2012). As several evaluations of primary health care show, there are problems with access to doctors and it happens

number of doses[71] of antibiotics prescribed in out-patient care per capita. From the patient's perspective, an antibiotics prescription is a measure of high quality either for actually reducing the pain and length of their illness or due to that receiving a prescription is interpreted as being taken seriously and listened to, regardless of the actual need of antibiotics.[72] Another approach to measure availability is to use patient experiences. Once a year the Swedish Association of Local Authorities and Regions (SALAR) produces a survey covering at least 1000 randomly chosen individuals per county.[73] The survey questions concern the population's experience of health care availability and how they rate the services performed. Due to a change in the questionnaire in 2010 I will only use the version of the survey that was used up to and including 2009.[74] The following questions are used:

1. How long time did you have to wait until the visit could take place?
2. What summarized rating would you like to give to your health care visit (GP practice)?

The questions are only directed to respondents who have previously stated that they had visited the primary health care during the year and refer to the most recent visit.[75] Since the interviews are performed during the whole year the responses can refer to experiences during the former year.

The choices for question 1 were "the same day," "within 7 days," "more than 7 days" and "don't know." For the second question the respondents could rate their visit on a scale of 1–5 where 5 was the highest grade, or answer "don't know."

From these questions I construct the following variables:

1. Share of patients who could visit their GP practice on the same day.
2. Share of patients who had to wait more than 7 days.

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that individuals do not visit primary care due to the difficulty in reaching a doctor (SOU 2012:2).

[71] Doses are defined as "Defined Daily Doses" (DDD), which is a measure of drug consumption defined by WHO. It corresponds to the assumed average maintenance dose per day for a drug used for its main purpose in adults.

[72] Several studies have found that doctors face situations in which they feel the need to prescribe antibiotics to satisfy patients' expectations even when they know antibiotics are not useful or necessary (Brody, 2005, Butler et al., 1998). However, from the societal perspective increased use of antibiotics is not necessarily an indicator of high quality care, since unnecessary antibiotics use contributed to antibiotics resistance.

[73] In some counties the number of respondents exceed 1000.

[74] During 2001-2009 the survey questions remained almost unchanged; however, since 2010 they have been designed to capture the public's general view of health care and many of the more precise questions that are appropriate for measuring primary health care quality have been omitted entirely.

[75] The share of respondents who state that they had visited health care is around 70 percent and of these about 50 percent had visited a primary health care unit.

3. Share of patients who gave their visit a grade of 4-5.

The shares are calculated from the total number of respondents, including those who responded "don't know."

The dropout rate of the surveys is around 30 percent, but since drop outs are replaced by new respondents the number of respondents is still at least 1000 per county and year. Since the background characteristics of the respondents may differ from those of the general population, the background information is used to construct control variables added in the analysis for the outcomes from the survey. The outcome measures from the surveys are based on patient experience, but as discussed above it can be difficult for patients to assess medical quality. It is therefore interesting to contrast the outcomes above with other types of measures. The prevalence of care injuries[76] is one source of information on the medical quality of primary health care. There are three instances to which patients can address complaints of errors in health care provision: the Medical Responsibility Board (MRB)[77], the regional patient rights committees and the patient insurance system. In addition, care providers should report themselves to the National Board for Health and Welfare when errors have been made resulting in serious risk for the patient. Of these various complaint types, only claims made to the patient insurance provide a satisfactory source of information.[78] When care injuries occur, patients can apply for compensation from the patient insurance, which covers all health care injuries in publicly funded health care. The data on patient insurance cases cover all instances in which primary care patients have applied for compensation for their injuries from the care provider's insurance. Data on the number of granted claims is also available. All injuries that occur in publicly financed health care units can be subject to compensation from the patient insurance. Advantages of the patient insurance data is that these data include errors reported to all other instances, as long as the patient is interested in compensation and that they are reported by the date that the error occurred. As compensation can be claimed until two years after the injury is detected, the data does not include all cases. Table A3 shows the distribution of the year of injury for complaints made in 2011. The table shows that the lag is most prominent up to three years back in time, and that it is smaller for granted complaints. Only 9 percent of claims received by the patient insurance originated from injuries that occurred in 2008 or

[76] Care injuries are defined as "suffering, physical or psychological injury or illness and death that could have been avoided if adequate measures had been taken during the patient's contact with health and medical care" by the National Board for Health and Welfare.

[77] As of 2011 patients must address their complaints to the National Board of Health and Welfare instead of to the Patient Responsibility Board.

[78] Reports to the MRB are unsuitable due to categorization of errors by error reporting date and not by error occurrence date; moreover, self-reported errors are not available on the county level until 2010.

earlier. The delay in the reporting of injuries should not be a problem, since it is unlikely that the lag is correlated with the introduction of competition.

In the analysis the number of claims is weighted against the population of each county. The alternative would be to calculate the number of claims per doctor's visit, but if the number of physician visits increases due to shorter and more frequent or unnecessary visits as a consequence of the reforms, the results could be underestimated.

A weakness in using patient complaints is that the willingness to report errors may be directly influenced by the reform, for instance if patients become less tolerant of errors or want to express dissatisfaction with the reform. This is less of a problem since the analysis focuses on approved reports, but even the number of well-warranted complaints could increase.

A measure of primary health care quality that has been used in several contexts is avoidable hospitalizations. The measure consists of a number of conditions that are likely to not have required hospitalization if the patients have had access to timely and effective primary care. The measure has been used by the National Board of Health and Welfare (NBHW) as an indicator of Swedish primary health care since 2005 and contains conditions such as anemia, asthma, diabetes and chronic obstructive pulmonary disease. A complete list of the diagnoses included in the measure can be found in Table A4 in the Appendix. Since the measure is not available until 2005, I have used the Swedish National Inpatient Register to construct a measure similar to that used as indicator at present.[79]

Avoidable hospitalizations is also used as a measure of performance in care for the elderly, in particular nursing homes which also provide basic medical services. Since care for the elderly has been subject to deregulation similar to that in primary health care, there is a risk for exaggeration of the results. In the analysis of avoidable hospitalizations observations for individuals over 65 are therefore excluded, since this group is more likely to contain individuals living in nursing homes for elderly.[80]

In addition to the quality measures above I estimate the effect on the counties' costs for primary health care. They consist of the net cost of primary health care provided by practices owned by the county as well as services provided by private practices which are contracted or authorized by the county council.

Figures 2 and 3 illustrate the development of the main outcome variables. Figure 2, which contains the measures of observable quality, shows that the

[79] However, there are some discrepancies in the conditions included since the NBHW measure is based on a finer division of conditions than the diagnosis codes available from the inpatient register. The final column of Table A4 shows how the two measures differ from each other.

[80] It is rare for patients under this age to be living in a nursing home. In 2011, among people between the ages of 0–64, 4700 were receiving this type of service, whereas among those older than 64, the corresponding number was 92,000 (Socialstyrelsen, 2012).

number of doctor's visits as well as the prescription of antibiotics decreased in the beginning of the period, 1998 to 2004. Both variables show an increasing trend since 2004, except for a drop in antibiotics prescriptions during 2009–2010. The share of primary care patients who give their visit a high grade increased over the whole period. The share of patients who could visit their primary care facility on the same day did not changed significantly, while there was a decrease in the share of patients who had to wait for more than 7 days. As for measures of less observable quality (hereafter "unobservable quality") illustrated in Figure 3, the number of avoidable hospitalizations decreased over the whole period, except for a small increase in 2010. The number of insurance claims remained on approximately the same level until 2007. Thereafter the number of granted claims has fallen, which to some extent depend on the lag in reported claims, described above and in Table A3 in Appendix A.

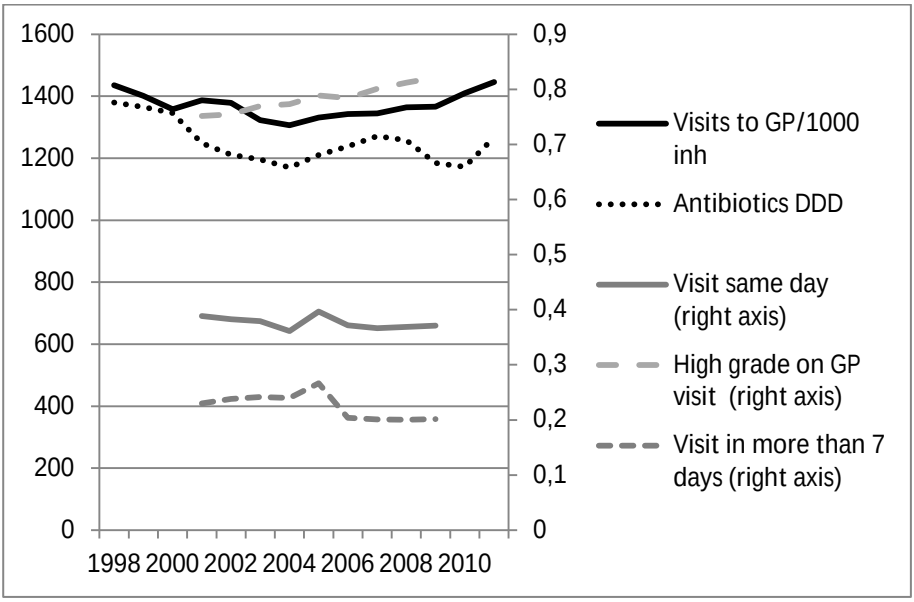

Figure 2: Measures of observable quality, 1998–2011

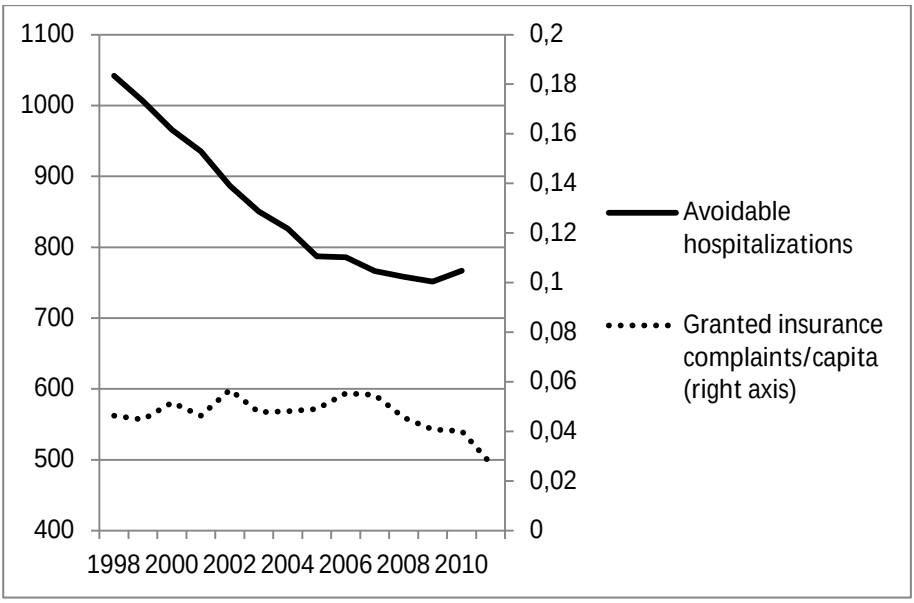

Figure 3: Measure of unobservable quality, 1998-2011

5 Empirical strategy

To investigate the impact of the share of privately provided primary health care I exploit the regional variation in the private market's share of doctor's visits in publicly funded primary health care. The empirical specification is given by:

$$Y_{jt} = \beta_0 + \beta_1 P_{jt} + \gamma X_{jt} + \delta_j + \delta_t + \varepsilon_{jt} \quad (1)$$

where *j* indexes county and *t* time. Y_{jt} is the outcome of interest, quality of primary health care in county *j* at time *t*. P_{jt} is a continuous measure of the share of doctor's visits performed by private providers in primary health care.

By including county-fixed effects (δ_j), the model absorbs all persistent unobserved county characteristics that may be correlated with the private share of doctor's visits and also with quality. The time-fixed effects, δ_t, remove national trends in health care quality that are common to all counties.

One concern is that there are county characteristics that vary over time and regions that are correlated with both health care quality and the introduction of more competition. These might be, for example, shocks in the demographic composition or in the regional economy that affect the health care needs, and also the introduction of more competition. To control for this kind of factors the model also includes a number of control variables, X_{jt}, which consist of time varying economic and demographic characteristics: age (share of population aged 0–6 years, 7–19 years, 20–59 years, 60–79 years and over 80 years), share of women, share of foreign citizens[81], share with only primary education, share with welfare benefits, political majority, tax base and unemployment.

Prior to the patient choice reform, two different reimbursement systems were in place across counties: financing through budgetary transfers and financing through capitation.[82] To investigate if the compensation system

[81] A preferable measure would have been the share of the population born outside of Sweden, but this measure is not available for the full time period. To use foreign-born population with a shorter time period does not change the results.

[82] To classify the counties according to compensation structure information from three surveys (Bergman and Dahlbäck,2001;, Lundberg and Rydnert, 2003 and Olsson and Thorling, 2005) for the years 2001, 2003 and 2006 was used, as well as telephone interviews with county officials.

matters for the impact of competition, I interact the share of private doctor's visits with an indicator variable, cap_{jt}, for whether a capitation system was in place in in county *j* at time *t*. The second model is specified as:

$$Y_{jt} = \beta_0 + \beta_1 P_{jt} + \beta_2 cap_{jt} + \beta_3 \left(P_{jt} * cap_{jt}\right) + \gamma X_{jt} + \delta_j + \delta_t + \varepsilon_{jt} \quad (2)$$

To separate the effect of competition, private provision and reimbursement principle from the effects of the more comprehensive patient choice reform, I begin by estimating equation 1 and 2 on data from the period 1998-2006, i.e. the period before patient choice had been implemented in any county.

In the second part of the analysis I investigate whether the introduction of the patient choice reform, described in section 2, seems to have affected quality. In this part of the analysis I estimate the following model:

$$Y_{jt} = \beta_0 + \beta_1 patient\ choice_{jt} + \gamma X_{jt} + \delta_j + \delta_t + \varepsilon_{jt}, \quad (3)$$

where $patient\ choice_{jt}$ is an indicator variable set to unity if the patient choice reform had been implemented in county *j* at time *t*. The analysis is based on the full time period, 1998–2010, and is conducted for two different samples separately: counties that had introduced capitation before introducing patient choice and counties for which patient choice also entailed a change in reimbursement system, from budget financing to capitation.

The crucial assumption of the empirical strategy employed is that trends in quality would have been similar in counties that introduced private competition, capitation or patient choice reforms and counties that did not. However, if the introduction of reforms aimed at increasing competition was a response to politicians' concerns regarding a future increase in population demand on health care, the estimated effect would represent a spurious relationship. The demographic structure, especially a growing population of elderly, is probably the most important factor for predicting the future need for primary health care. These kinds of factors are controlled for in the regressions, but it is possible that the decision to let more competition into the market is associated with a change in the general budget for primary health care. If, for instance, county councils decide to allocate more funds to primary health care at the same time as they increase competition, a positive relationship between competition and quality would be spurious. In order to investigate this issue I also estimate the regressions above for the net costs of primary health care.

As an additional robustness check I include linear county-specific trends in the regressions to control for any linear trends in quality for each county. Any effect on quality is therefore identified from the residual variation around the linear time trend in quality in each county. The analysis is presented in section 6.3.

6 Results

6.1 Effects of competition from private providers and reimbursement[83]

This section presents the results from the analysis between competition in primary health care and quality. The analysis encompasses the period 1998–2006[84], i.e. the period before any of the counties had introduced a patient choice system. I begin by analyzing the impact of competition from private providers on the "observable" quality measures and then move forward to discussing the effects on "unobservable" quality. The first column of each table show the effect of competition from private health care providers and the last column present the estimates from equation 2, where the private share of primary health care is interacted with a dummy for capitation financing. The estimates should be interpreted as the effect on quality of a 10 percentage point increase in the share of privately provided services. The sample used in the analysis is arguably small. Since it can be interesting to see how the efficiency is affected when reducing the amount of control variables I also estimate the regressions and in turn exclude both control variables and county fixed effects (column 1 and 4), only county fixed effects (column 2 and 5) and finally control variables (column 3 and 6). However, in most cases the exclusion of control variables does not increase the efficiency of the results. The results are presented in the Appendix, Table B1-B3 for reference.

The results in Table 3 suggest that the number of doctor's visits increase as a result from an increase in the share of services provided by private providers. The point estimate in column 1 suggests that a 10 percentage point increase in the share of privately provided services is associated with 29 more visits to primary care physicians per capita, which corresponds to approximately 2 percent of the sample mean. Also, the estimate for antibiotics prescriptions suggests a positive, although statistically insignificant, relationship to the share of privately provided services. The estimate of 0.055 in column 1 suggests that a 10 percent increase in private share is associated

[83] Since two counties, Västra Götaland and Västerbotten, did not introduced capitation in the entire county at the same time all regressions have been run excluding these counties, but with approximately the same results.

[84] The time period for the analysis of the quality measures derived from the patient survey is 2001–2006, and for costs for primary health care 2000–2006.

with a 0.4 percent increase in the average number of daily doses per inhabitant. The increase in the number of physician visits could be interpreted as an increase in availability, but the results from the patient survey do not support such a conclusion. Both of the estimated coefficients on the likelihood of obtaining a visit on the same day or the risk of having to wait more 7 days are small and statistically insignificant. The found effects on the number of doctor's visits but not on patient experiences of availability could also reflect that availability has improved due to the increase in private provision, but that it has also created an increased demand, which has had an adverse effect on waiting times. The final panel shows the estimated coefficient for the effect of competition from private providers on the share of patients who give their primary care visit a high grade. If competition from private providers is associated with improvements in service and conduct towards patients, a positive effect on how patients rate their visit might be expected. The estimated coefficient is positive, but of small magnitude and statistically insignificant. The coefficient suggests that an increase in private provision of 10 percentage points is associated with a 1.2 percentage point, or 2 percent of the sample mean, increase in the share of patients who give their primary care visit a high grade.

Table 3: *The effect of private provision and capitation on observable quality.*

	(1)	(2)
Doctor's visits/1000 inh.		
Private share	28.726*	31.161*
	(15.095)	(17.372)
Capitation		39.421
		(42.923)
Capitation*Private share		–14.028
		(14.612)
Observations	189	189
R^2	0.904	0.904
Mean of dependent variable	1362	1362
Antibiotics DDD/100 inh.		
Private share	0.055	0.069
	(0.085)	(0.115)
Capitation		0.243
		(0.188)
Capitation*Private share		–0.084
		(0.113)
Observations	189	189
R^2	0.976	0.976
Mean of dependent variable	12.63	12.63
Visit same day		
Private share	–0.006	0.003
	(0.013)	(0.019)
Capitation		–0.017
		(0.044)
Capitation*Private share		0.007
		(0.027)
Observations	111	111
R^2	0.719	0.724
Mean of dependent variable	0.380	0.380
Visit in more than 7 days		
Private share	–0.012	–0.017
	(0.012)	(0.019)
Capitation		0.015
		(0.036)
Capitation*Private share		0.003
		(0.024)
Observations	111	111
R^2	0.737	0.740
Mean of dependent variable	0.238	0.238

Table 3 continued.

High grade on primary health care visit		
Private share	0.012	0.001
	(0.009)	(0.014)
Capitation		–0.035
		(0.032)
Capitation*Private share		0.022
		(0.020)
Observations	111	111
R^2	0.681	0.690
Mean of dependent variable	0.771	0.771
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at county level are shown in parentheses. In addition to the fixed effects indicated in the table, all regressions control for the following county level variables: share of households with welfare benefits, population density, share of foreign citizens, per capita tax base, unemployment rate, share of women, share of population aged 0–6 years, 7–19 years, 20–59 years, 60–79 years and over 80 years. Regressions for the outcome variables high grade on primary care visit, visit same day and visit in more than 7 days also include controls for the characteristics of the respondents: share of women, share born in a Nordic country, share with primary education only, share of respondents under 39 years, 40–69 years and older than 70 years.

For the measures used to capture "unobservable quality" presented in Table 4, avoidable hospitalizations and granted insurance claims, there is no statistically significant support for any effects of private share. The estimate on the number of avoidable hospitalizations per 100,000 inhabitants in column 1 is negative and insignificant. The magnitude of the estimated coefficient represents a reduction of 2 percent in avoidable hospitalization in relation the sample mean. The estimate on granted insurance claims is very close to zero and hence indicates no effect from increased private provision.

As discussed in section 3, the expected effect from increased private provision in primary health care is ambiguous, partly because it is uncertain whether the ownership effect is positive or negative, and partly because a negative effect could be counteracted by the competition effect. Taken together, the results provide no striking evidence of any impact on quality of competition from private providers in the market for primary health care. The number of visits to a primary health care physician seems to be positively related to increased private provision, but the rest of the results do not provide any support for improved availability or changes in quality in either direction.

Table 4: *The effect of private provision and capitation on unobservable quality.*

	(1)	(2)
Avoidable hospitalizations/100 000 inh.		
Private share	–18.970	–15.005
	(15.843)	(16.629)
Capitation		62.531
		(45.142)
Capitation*Private share		–22.393
		(16.474)
Observations	189	189
R^2	0.886	0.889
Mean of dependent variable	898.3	898.3
Granted insurance claims/capita		
Private share	0.000	0.003
	(0.004)	(0.004)
Capitation		0.026***
		(0.009)
Capitation*Private share		–0.010***
		(0.003)
Observations	189	189
R^2	0.733	0.746
Mean of dependent variable	0.050	0.050
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at county level are shown in parentheses. In addition to the fixed effects indicated in the table, all regressions control for the following county level variables: share of households with welfare benefits, population density, share of foreign citizens, per capita tax base, unemployment rate, share of women, share of population aged 0–6 years, 7–19 years, 20–59 years, 60–79 years and over 80 years.

In some counties the financial incentives for quality competition are weaker, since revenues are not tied to the number of listed patients. If competition is associated with higher quality, it might be expected that the effect would be stronger where payment per number of listed persons, capitation, is used. To investigate this issue I estimate the model including a dummy for capitation-based reimbursement, and interact it with the share of privately provided services. Since the incentives for quality competition are stronger with capitation, the expected sign of the interaction term in equation 2 is positive, at least for the observable measures of quality (not including the share of patients that wait more than seven days to visit their primary care facility). If quality competition is limited to observable measures of quality, no effect on the provision of unobservable quality could be expected from either more private provision or from competition inducing reimbursement. Column 2 of Tables 3 and 4 presents the result of this analysis.

The results do not give any conclusive support for these hypotheses, however. The introduction of capitation does not seem to affect any of the measures of observable quality. For the number of visits to a primary care physician, the relationship with private share is positive and of the same magnitude as before. No statistically different effect is found in counties with capitation, however. For antibiotics, the point estimate for the introduction of capitation on the prescription of antibiotics is positive although not statistically significant. The results concerning waiting times yield no significant results either. If anything, the point estimates suggest that waiting times are longer in more competitive markets.

Turning to the measures of unobservable quality in column 2 of Table 4, capitation does not seem to have any statistically significant effect on avoidable hospitalizations. For granted insurance claims, the results suggest that more private provision is positively related to compensated injuries in markets without capitation, whereas in markets with capitation systems and high shares of private provision granted compensation claims are less frequent. The point estimates in column 2 suggests that a 10 percentage point increase in private provision in counties with reimbursement through budgetary transfer is associated with a 6 percent increase in granted insurance claims. The introduction of capitation is associated with a 50 percent increase in insurance claims, but the positive effect is counteracted by more competition—a 10 percentage point increase in the share of private providers is associated with a statistically significant reduction of 20 percent in the number of granted insurance claims. These results suggest that capitation could be associated with worse medical quality, but also that the incentives to improve medical quality rise when there are more providers in the market. This result fits well with the theories of quality competition—capitation itself does not give providers incentives to improve quality, but more competition from other providers strengthens the incentive to provide high quality services. The fact that effects are found for insurance claims, which is intended to capture unobservable quality, and not for the observable measures is quite the contrary to the prediction from theory.

The estimates in Tables 3 and 4 provide no comprehensive support for improved primary health care quality due to increased competition from private providers or capitation. The only effects that are statistically significant are the effect of private share on the number of doctor's visits in counties with budget transfers and the results on insurance claims. The rest of the estimation results are highly imprecise, which could be due to the fact that there is little variation in the data. Also, the small magnitude of the point estimates suggests that if there is an impact on quality from competition it is marginal for most outcomes.

A potential explanation to the observed pattern is that the introduction of more competition is associated with an ambition to cut the costs for primary health care. Fewer resources allocated to primary health care could counter-

act a potential quality increase caused by more competition. To investigate this issue I also estimate the effect on the counties' net costs for primary health care. The point estimates in column 2 of Table 5 suggest that a 10 percentage point increase in the private share is associated with an increase in cost of approximately 100 SEK per capita in counties with budgetary transfers, which corresponds to 3.8 percent of the sample mean. The introduction of capitation is also positively associated with higher costs, whereas the costs are lower in counties with capitation and a large share of private provision. These results are, however, imprecisely estimated, and it is therefore difficult to draw any strong conclusions. However, the signs of the point estimates in column 2 fit well with the results for the number of doctor's visits in Table 3 – the effects of competition from private providers and capitation are positive, and these variables are also associated with higher costs.

Table 5: *The effect of private provision and capitation on primary health care costs.*

	(1)	(2)
Net costs for primary health care/capita		
Private share	39.875	103.681
	(92.901)	(100.314)
Capitation		71.281
		(190.661)
Capitation*Private share		–89.841
		(80.341)
Observations	145	145
R^2	0.850	0.854
Mean of dependent variable	2723	2723
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: see Table 3.

During the period studied above, county councils had full discretion to decide the number of primary health care units that could operate within the county and their locations. It is likely that county councils did not allow more production than the level that they thought would be enough to meet for the population's needs, resulting in a supply of services that is lower or perfectly matched to demand. Even when the means follows the patient, the scope for competition will be limited if there is a shortage of primary health care in the market. Next, I turn to the analysis of patient choice, a reform that increased competition pressure in the market by also introducing free entry for primary health care providers.

6.2 Patient choice

As described in section 2, patient choice has gradually been introduced in Swedish counties since 2007. These reforms have led to larger organizational changes in some counties than others, as many counties had already introduced capitation payment and listing before 2007. For these counties, patient choice has mainly led to the introduction of free entry in the market. Combined with the capitation system, free entry implies a higher risk of going out of business if a practice is unable to attract and retain its patients. For counties with a more traditional organization of primary health care prior to the reform, patient choice has been associated with both free entry and a new reimbursement system.

Tables 6 and 7 present the results of the effect of patient choice on quality. As in the previous section, results from regressions excluding control variables and county effects are presented in Tables B4–B6 of Appendix B. The analysis is based on the full period 1998–2010[85], although the observations for the years with budget financing in counties that switched to capitation are omitted. The first column contains the results from using the sample of counties that had capitation-based reimbursement before the reform, while the second column contain the results for counties for which patient choice also entailed a change in the reimbursement system.

As Table 6 shows, many of the estimates on observable quality have the expected signs. The patient choice reform seems to be associated with more visits to a primary care physician and shorter waiting times, both for counties that practiced capitation and those that did not. In counties with capitation the point estimate for doctor's visits in column 1 represents a 2 percent increase in relation to the sample mean. The corresponding effect in counties with budget financing prior to the reform is 6 percent. Only the estimate for the number of doctor's visits in counties with budget financing is statistically significant however.[86] Also, the point estimates for waiting times suggest improved availability as a consequence of the patient reform, although the estimates for the share of patients who could visit their primary care facility the same day are imprecisely estimated. In counties with capitation there is a statistically significant decrease in the share of patients who had to wait for a visit for more than a week of 10 percent in relation the sample mean. Moreover, the estimates of the prescription of antibiotics are positive for both types of counties. The point estimate in column 1 is statistically significant on a 10 percent level and suggests a 1.3 percent increase in relation to the

[85] The time period for the analysis of the quality measures derived from the patient survey is 2001–2009, and for costs for primary health care, 2000–2010.

[86] Excluding county effects from the regression for counties with capitation yield significant results as the first panel of Table B4 in the appendix shows, but the magnitude of the estimate is also smaller when accounting for county effects, which suggest that they are important to include.

sample mean in counties with capitation, while the point estimate for counties with budget financing in column 2 is statistically insignificant.

Table 6: *The effect of patient choice on observable quality.*

	Capitation	Budgetary transfers
	(1)	(2)
Doctor's visits/1000 inh.		
Patient choice	26.773	86.351**
	(28.780)	(29.486)
Observations	138	91
R^2	0.963	0.941
Mean of dependent variable	1360	1381
Antibiotics DDD/100 inh.		
Patient choice	0.167*	0.060
	(0.092)	(0.172)
Observations	138	91
R^2	0.986	0.973
Mean of dependent variable	12.51	12.32
Visit same day		
Patient choice	0.006	0.087
	(0.016)	(0.046)
Observations	97	59
R^2	0.769	0.863
Mean of dependent variable	0.364	0.401
Visit in more than 7 days		
Patient choice	–0.023**	–0.065
	(0.009)	(0.045)
Observations	97	59
R^2	0.842	0.772
Mean of dependent variable	0.225	0.216
High grade on primary health care visit		
Patient choice	–0.012	0.021
	(0.006)	(0.016)
Observations	97	59
R^2	0.729	0.842
Mean of dependent variable	0.784	0.795
Time effects	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: see Table 2.

Whereas there are some indications of improved observable quality, especially in counties that practiced capitation prior to the reform, there seems to be no improvement in terms of unobservable quality. Neither of the estimated coefficients for avoidable hospitalizations or granted insurance claims are significant, but both point estimates show positive signs for counties that had implemented capitation prior to patient choice. The point estimates for

counties with capitation in column 1 corresponds to a 2 percent increase in avoidable hospitalizations and a 15 percent increase in granted insurance claims in relation to the sample mean. For counties with budgetary transfers, the estimate on granted insurance claims indicates a positive impact of patient choice, while the estimate for avoidable hospitalizations is negative.

Table 7. *The effect of patient choice on unobservable quality.*

	Capitation	Budgetary transfers
	(1)	(2)
Avoidable hospitalizations/100 000 inh.		
Patient choice	16.137	-21.950
	(17.829)	(21.401)
Observations	138	91
R^2	0.903	0.953
Mean of dependent variable	824.0	866.4
Granted insurance claims/capita		
Patient choice	0.007	0.004
	(0.008)	(0.004)
Observations	138	91
R^2	0.632	0.651
Mean of dependent variable	0.047	0.045
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: see Table 3.

As discussed in the previous section reforms intended to increase competition in the market could be associated with changes in resources allocated to primary health care. Table 8 presents the results from estimating the effect on costs of patient choice. The point estimate for counties with capitation in column 1 shows an increase in costs per capita of 188 SEK, which corresponds to 6 percent of the sample mean. The corresponding estimate for counties with budget financing is a decrease in net costs of SEK 199, or 7 percent of the sample mean. Since it is in counties with capitation that indications of improved observable quality were found, it is not possible to rule out that this was a result of increased allocation of financial resources to primary care.

Table 8. *The effect of patient choice on costs for primary health care.*

	Capitation	Budgetary transfers
	(1)	(2)
Net costs for primary health care/capita		
Patient choice	187.578*	-199.383**
	(99.655)	(70.730)
Observations	126	77
R^2	0.937	0.958
Mean of dependent variable	3093	3023
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes

Notes: see Table 3.

6.3 Sensitivity analysis

As a robustness check I have also estimated all regression including linear county specific trends to account for heterogeneous trends in quality across counties. To conserve space, the results are presented in Appendix B. The results for the analysis of competition through the inflow of private providers and the financing system are presented in Table B7 in the appendix and the results from the patient choice reform can be found in Table B8. From column 1 in Table B7, it is evident that the overall results from the main analysis are not altered when including county specific trends. The statistically significant effect of private share on the number of visits to a primary care physician remains significant. Also the results from the regressions separating the effect by the financing system in column 2 are approximately unchanged with the inclusion of county specific trends. The statistically significant increase in the number of doctor's visits from an increase in private providers remains as do the earlier found pattern of a significant negative relationship between medical injuries and capitation combined with a high rate of private provision. The positive estimate on antibiotics prescriptions is of the same magnitude, but still insignificant.

The results in column 1 in Table B8 suggest that the effects of patient choice found in counties which practiced capitation prior to introducing the patient choice reform are quite robust to the inclusion of county-specific trends. The estimate for antibiotic prescriptions is no longer significant, although of similar magnitude, whereas the reduction in patients waiting more than a week for a visit to a primary care facility remains significant. Also, the estimated effect on net costs turns insignificant with the inclusion of county specific trends, but the magnitude is similar to the estimates in Table 7. Column 2 shows the results for counties which used reimbursement

by budgetary transfers before the introduction of patient choice. The positive result on the number of doctor's visits as well as the negative effect on net costs remain with the inclusion of country effects.

7 Discussion

The impact of private providers and competition on health care quality has been an issue of considerable debate in the recent years. Proponents of increased private provision and competition in the market for health care emphasize better service and more innovation in private firms as well as stronger incentives for quality competition. Critics point out the risk for deterioration of quality due to the profit goal of private firms and the difficulties for patients to evaluate quality. Both economic theory and the available empirical evidence are inconclusive when it comes to the effect of competition and private provision on quality.

In this paper I have estimated the effect of initiatives for increased competition and private provision on quality in Swedish primary health care. To investigate if health care providers are more inclined to compete for patients by improving aspects of quality for which information asymmetries are less pronounced than aspects of quality which patients are unaware of, the analysis separates across observable and unobservable measures of quality.

The first part of the analysis concerns the impact of competition through an inflow of private providers in the market and the differences in the response to competition in counties with capitation and budget financing. The results suggest an increase in the number of visits to a primary care physician following an increase in private provision, but the introduction of capitation does not seem to have any effect on the number of visits. Although primary health care practices could be expected to be more motivated to improve observable quality as a response to more competition, the analysis yields no evidence of an effect on observable quality, such as waiting times. Instead the results suggest a negative effect on medical quality, measured as the number of granted insurance claims. In counties with reimbursement by capitation and a large share of private provision, the number of granted insurance claims was lower. The results are not supported by the analysis for the second measure of medical quality, avoidable hospitalizations. For this outcome no effects are found.

The second part of the analysis investigates the introduction of patient choice. The introduction of patient choice seems to be associated with an increase in the number of doctor's visits. Moreover, the results give some support for a positive effect on the prescription of antibiotics in out-patient care, at least for counties which had previously practiced reimbursement by capitation. This finding is in line with results found by Fogelberg and

Karlsson (2013). They show that following the introduction of patient choice, antibiotic prescriptions increased more in markets with large scope for competition compared to markets where the possibilities of active choice of practice is limited. Also, waiting times seems to have improved slightly in counties with capitation prior to the reform. However, it cannot be out ruled that the quality improvements are to some extent a result from more spending on primary health care rather than due to increased incentives for quality resulting from a more competitive market.

The evaluation of privatization and competition on quality in primary health care has been obstructed by the lack of data and the difficulties of measuring quality accurately. This paper is the first study to thoroughly investigate the consequences, in terms of quality, of the deregulatory reforms in Swedish primary health care. To get a comprehensive measure of general quality a wide range of measures intended to capture quality in two different dimensions are used. Unfortunately, the sample in the analysis and the variation in data is small, which makes it difficult to draw strong conclusions. Nevertheless, the unique setting of primary health care predominantly organized by the public sector, makes it interesting to investigate the issue on Swedish data. In spite of its limitations, the empirical analysis gives an indication of what happens to health care quality when private providers enter the market. The results do not indicate any large effects on the quality provided. No effect is found for most of the quality measures, and where effects are found, the size is relatively small. The analysis hence suggests that the expected improvements in quality are can be difficult to reach and that the fear of deterioration of quality may be unwarranted, at least in the short run. One caveat is that the patient choice reform was implemented only recently, and it is possible that the consequences of the reform will be other in the long run. It should also be noted that the estimated effects in this paper are average county effects. If quality has improved in some areas and deteriorated in other, the results could still suggest a zero effect. It is hence impossible to know whether the effects were the same across all areas and all individuals, or if the effects represent averages of heterogeneous effects. Since one of the fears of the introduction of competition and capitation is that health care will be unevenly distributed this is an important question. Hopefully, future research will contribute to get a deeper understanding on issues like this. Access to data on primary health care may be better in the future as incorporating primary care in the Swedish patient register is currently discussed. If these plans are implemented it would for instance be possible to study the effects on individuals of different socio-economic background and to increase precision by utilizing variation across municipalities or city districts instead of counties.

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Appendix A: Data and descriptive statistics

Table A1: *Descriptive statistics*

	Obs	Mean	Std. Dev.	Min	Max
Outcome variables					
Doctor's visits/1000 inh.	273	1364.8	150.21	987	1921.9
Antibiotics DDD/1000 inh.	294	12.51	1.53	9.50	17.40
Visit same day	171	0.38	.06	0.222	0.54
Visit in more than 7 days	171	0.23	.05	0.127	0.36
High grade on primary health care visit	171	0.79	.036	0.673	0.88
Avoidable hospitalizations/1000 inh.	273	855.99	134.64	578.32	1289.69
Granted insurance claims	294	0.05	0.0264693	0.01	0.19
Insurance claims	294	0.12	0.0557715	0.04	0.39
Net costs for primary health care	250	3108.23	674.9379	1450.66	5067.78
Control variables					
Share with primary education	294	0.26	0.04	0.17	0.38
Share with welfare benefits	294	0.05	0.01	0.02	0.09
Population density	294	43.88	60.88	2.5	320.5
Share foreign citizens	294	0.04	0.02	0.02	0.10
Tax base	273	129403.3	23128.87	83072	203089
Unemployment	294	4.03	0.99	1.94	7.51
Share > 80 years	294	0.06	0.01	0.04	0.07
Share 0-6 years	294	0.07	0.01	0.06	0.09
Share 7-19 years	294	0.16	0.01	0.14	0.18
Share 60-79	294	0.19	0.02	0.14	0.24
Share women	294	0.50	0.003	0.49	0.51

Table A2: *Data*

Measure	Description	Period	Source
Explanatory variables			
Share of physician visits in private primary health care	Share of visits to a physician in private primary care.	1998-2010	SALAR
Reimbursement system	An indicator variable which takes the value 1 of capitation was or came into effect a certain year and 0 if reimbursement was through budgetary transfers.	1998-2010	Earlier studies and complementary interviews with county officials.
Patient choice	An indicator variable which takes the value 1 of patient choice was or came into effect a certain year and 0 otherwise.	1998-2010	Swedish Competition Authority
Outcome variables			
Observable quality			
Visits to p.c. physician/1000 inh.	The number of visits to a physician in primary health care.	1998-2010	The Swedish Association of Local Authorities and Regions (SALAR)
Antibiotics DDD/100 inh.	The number of defined daily doses (DDD) of antibiotics prescribed in out-patient care	1998-2010	Swedish Institute of Communicable Disease Control
High grade on primary health care visit	The share of respondents who rated their visit to a primary care facility a 4 or 5 of a 5 graded scale where 1 is the lowest.	2001-2009	SALAR
Visit same day	The share of respondents who stated that they could visit their primary care facility the same day.	2001-2009	SALAR
Visit in more than 7 days	The share of respondents who stated that they waited more than 7 days to visit their primary care facility.	2001-2009	SALAR
Unobservable quality			
Avoidable hospitalizations/100 000 inh.	The number of hospitalizations due to conditions that should not require hospital care. For the diagnoses included see Table A4.	1998-2010	National Board of Health and Welfare
Granted insurance claims/capita	The number of insurance claims for which compensation was granted.	1998-2010	The Patient Insurance (Patientförsäkringen LÖF)
Additional outcomes			
Net costs for primary health care/capita	Net cost for primary health care excl. home care and pharmaceuticals covered by the pharmaceutical benefit scheme.	2000-2010	SALAR
Control variables			
Share of households with welfare benefits		1998-2010	Statistics Sweden
Population density		1998-2010	Statistics Sweden
Share of foreign citizens		1998-2010	Statistics Sweden
Per capita tax base		1998-2010	Statistics Sweden

Unemployment rate	The share of the population registered at the unemployment office.	1998-2010	Swedish Public Employment Service
Share of women		1998-2010	Statistics Sweden
Share of population aged 0-6 years		1998-2010	Statistics Sweden
Share of population aged 7-19 years		1998-2010	Statistics Sweden
Share of population aged 20-59 years		1998-2010	Statistics Sweden
Share of population aged 60-79 years		1998-2010	Statistics Sweden
Share of population aged over 80 years		1998-2010	Statistics Sweden
Additional controls for survey sample			
Share of female respondents		2001-2009	SALAR
Share of respondents born in Nordic country (including Sweden)		2001-2009	SALAR
Share of respondents with primary education		2001-2009	SALAR
Share of respondents aged under 40 years		2001-2009	SALAR
Share of respondents aged 40-69 years		2001-2009	SALAR
Share of respondents aged over 69 years		2001-2009	SALAR

Table A3: *Claims of compensation from the patient insurance 2011, by year of injury*.

	Number of claims			Compensated claims		
Year of injury	Claims	Share	Cumulative share	Claims	Share	Cumulative share
2011	382	31%	31%	148	34%	34%
2010	371	30%	61%	158	36%	70%
2009	197	16%	76%	57	13%	83%
2008	96	8%	84%	36	8%	91%
2007	42	3%	88%	8	2%	93%
2006	26	2%	90%	8	2%	95%
2005	30	2%	92%	4	1%	96%
2004	19	2%	94%	4	1%	97%
2003	11	1%	95%	5	1%	98%
2002	16	1%	96%	1	0%	98%
2001	16	1%	97%	3	1%	99%
2000	7	1%	98%	2	0%	99%
-1999	29	2%	100%	3	1%	100%

Table A4: *Diagnoses in avoidable hospitalizations*

Diagnosis	National Board for Health and welfare	Analysis	Difference
Anemia	D501, D508, D509	D50	D50 also includes D500, anemia due to chronic bloodloss.
Asthma	J45, J46	J45, J46	-
Diabetes	E101-E108, E110-E118, E130-E138, E140-E148	E10, E11, E13, E14	E10, E11, E13 and E14 also contain diabetes without complications: E109, E119, E139, E149.
Cardiac insufficiency	I50, I110, J81	I 50, I11, J81	I11 also contain I119, which also belong to the measure
High blood-pressure	I10, I119	I10, I119	I11 also contain I110, which also belong to the measure.
Chronic obsructive pulmonary disease	J41, J42, J43, J44, J47 main diagnosis J20 with J41, J42, J43, J44, J47 as side diagnosis	J41, J42, J43, J44, J47	Excludes J20 with J41, J42, J43, J44, J47 as side diagnosis since side diagnoses cannot be identified in data.
Vascular spasms	I20, I240, I248, I249	I20, I24	I24 also include I241, "post-infarct syndrome.
Bleeding ulcers	K250-K252, K254-K256, K260-K262, K264-K266, K270-K272, K274-K276, K280-K282, K284-K286	K24, K26, K27, K28	Also contain ulcers without bleeding or perforation.
Diarrhea	E86, K522, K528, K529	E86, K52	K52 also includes K520, K521.
Epileptic seizures	O15, G40, G41, R56	O15, G40, G41, R56	-
Inflammatory diseases of the female pevic organs	N70, N73, N74	N70, N73, N74	-
Renal pelvis infection	N390, N10, N11, N12, N136	N10, N11, N12	N390, N136 excluded.
Ear, nose and throat infections	H66, H67, J02, J03, J06, J312	H66, H67, J02, J03, J06	J312 excluded.

Appendix B: Additional results

Table B1: *The effect of private provision and capitation on observable quality.*

	(1)	(2)	(3)	(4)	(5)	(6)
Doctor's visits/1000 inh.						
Private share	46.813***	33.097	58.660***	33.391	27.260	41.901
	(13.592)	(21.788)	(15.494)	(32.865)	(23.594)	(24.732)
Capitation				-80.118	-14.894	-74.680
				(79.178)	(49.996)	(57.283)
Capitation*Private share				27.599	9.680	34.967
				(37.390)	(17.572)	(25.313)
Observations	189	189	189	189	189	189
	0.280	0.869	0.612	0.306	0.869	0.628
Mean	1362	1362	1362	1362	1362	1362
Antibiotics DDD/100 inh.						
Private share	0.588**	0.042	0.177	0.710*	0.121	0.216
	(0.251)	(0.104)	(0.169)	(0.358)	(0.146)	(0.283)
Capitation				0.227	0.269*	-0.308
				(0.964)	(0.147)	(0.566)
Capitation*Private share				-0.181	-0.143	0.032
				(0.406)	(0.091)	(0.252)
Observations	189	189	189	189	189	189
	0.405	0.971	0.777	0.409	0.972	0.780
Mean	12.63	12.63	12.63	12.63	12.63	12.63
Visit same day						
Private share	0.001	-0.001	-0.017*	0.012	0.013	-0.010
	(0.009)	(0.012)	(0.009)	(0.011)	(0.012)	(0.013)
Capitation				-0.011	0.003	0.021
				(0.043)	(0.027)	(0.023)
Capitation*Private share				-0.010	-0.021	-0.013
				(0.016)	(0.015)	(0.013)
Observations	111	111	111	111	111	111
	0.045	0.682	0.484	0.106	0.692	0.490
Mean	0.380	0.380	0.380	0.380	0.380	0.380
Visit in more than 7 days						
Private share	-0.001	-0.005	0.003	0.002	-0.008	0.006
	(0.004)	(0.009)	(0.004)	(0.007)	(0.011)	(0.009)
Capitation				0.010	0.022	-0.008
				(0.028)	(0.019)	(0.022)
Capitation*Private share				-0.004	-0.000	-0.002
				(0.010)	(0.012)	(0.011)
Observations	111	111	111	111	111	111
	0.209	0.668	0.540	0.212	0.674	0.546
Mean of dependent variable	0.238	0.238	0.238	0.238	0.238	0.238

Table B2 continued.

High grade on primary health care visit						
Private share	0.000	0.001	0.003	0.011*	-0.005	0.007
	(0.003)	(0.009)	(0.003)	(0.006)	(0.011)	(0.004)
Capitation				0.013	-0.048*	0.004
				(0.013)	(0.028)	(0.016)
Capitation*Private share				-0.013**	0.022	-0.005
				(0.006)	(0.018)	(0.006)
Observations	111	111	111	111	111	111
	0.176	0.505	0.494	0.255	0.530	0.499
Mean	0.771	0.771	0.771	0.771	0.771	0.771
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B2: *The effect of private provision and capitation on unobservable quality.*

	(1)	(2)	(3)	(4)	(5)	(6)
Avoidable hospitalizations/100 000 inh.						
Private share	-32.171**	2.129	4.102	-44.281**	-11.940	-14.268
	(13.199)	(18.610)	(6.334)	(17.609)	(16.691)	(10.147)
Capitation				-12.738	-11.932	-40.733
				(46.924)	(43.437)	(24.102)
Capitation*Private share				16.570	19.240	28.695**
				(22.824)	(14.711)	(10.491)
Observations	189	189	189	189	189	189
R-squared	0.526	0.851	0.801	0.534	0.855	0.810
Mean	898.3	898.3	898.3	898.3	898.3	898.3
Granted insurance claims/capita						
Private share	-0.011***	0.002	-0.002	-0.012*	0.006**	-0.000
	(0.003)	(0.003)	(0.002)	(0.006)	(0.003)	(0.003)
Capitation				-0.000	0.024***	0.001
				(0.013)	(0.006)	(0.008)
Capitation*Private share				0.002	-0.008***	-0.002
				(0.006)	(0.002)	(0.003)
Observations	189	189	189	189	189	189
R-squared	0.237	0.714	0.553	0.243	0.733	0.555
Mean	0.0496	0.0496	0.0496	0.0496	0.0496	0.0496
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B3: *The effect of private provision and capitation on costs for primary health care.*

	(1)	(2)	(3)	(4)	(5)	(6)
Net costs for primary health care/capita						
Private share	-85.031	-15.748	-8.452	-110.940	86.643	33.401
	(60.143)	(86.961)	(63.676)	(121.322)	(99.421)	(88.668)
Capitation				-25.791	207.728	148.488
				(305.332)	(305.693)	(193.799)
Capitation*Private share				33.455	-158.395	-78.557
				(117.809)	(113.448)	(87.414)
Observations	189	189	189	189	189	189
R-squared	0.526	0.851	0.801	0.534	0.855	0.810
Mean	898.3	898.3	898.3	898.3	898.3	898.3
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B4: *The effect of patient choice on observable quality.*

	Capitation			Budgetary transfers		
	(1)	(2)	(3)	(4)	(5)	(6)
Doctor's visits/1000 inh.						
Private share	251.584*	126.443	95.243**	361.164***	171.031***	155.311**
	(136.216)	(76.228)	(42.332)	(85.932)	(24.059)	(42.395)
Observations	138	138	138	91	91	91
	0.119	0.846	0.706	0.370	0.897	0.898
Mean	1360	1360	1360	1381	1381	1381
Antibiotics DDD/100 inh.						
Private share	1.686**	0.342	0.230	1.258**	-0.061	0.300
	(0.582)	(0.204)	(0.214)	(0.491)	(0.279)	(0.200)
Observations	138	138	138	91	91	91
	0.186	0.960	0.904	0.272	0.934	0.927
Mean	12.51	12.51	12.51	12.32	12.32	12.32
Visit same day						
Private share	0.011	0.003	0.014	0.049	0.010	0.066
	(0.019)	(0.013)	(0.015)	(0.036)	(0.026)	(0.037)
Observations	97	97	97	59	59	59
	0.049	0.653	0.635	0.145	0.683	0.786
Mean	0.364	0.364	0.364	0.401	0.401	0.401
Visit in more than 7 days						
Private share	-0.018	-0.008	-0.012	-0.038*	-0.021	-0.043**
	(0.014)	(0.009)	(0.011)	(0.018)	(0.013)	(0.015)
Observations	97	97	97	59	59	59
	0.291	0.722	0.690	0.343	0.628	0.743
Mean of dependent variable	0.225	0.225	0.225	0.216	0.216	0.216
High grade on primary health care visit						
Private share	-0.011	-0.011	-0.014**	0.036*	0.004	0.010
	(0.007)	(0.009)	(0.006)	(0.017)	(0.010)	(0.012)
Observations	97	97	97	59	59	59
	0.433	0.588	0.663	0.432	0.731	0.797
Mean	0.784	0.784	0.784	0.795	0.795	0.795
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B5: *The effect of patient choice on unobservable quality*.

	Capitation			Budgetary transfers		
	(1)	(2)	(3)	(4)	(5)	(6)
Avoidable hospitalizations/100 000 inh.						
Private share	-13.076	37.360*	57.432**	-85.082**	-14.321	-19.295
	(51.459)	(19.497)	(24.500)	(25.437)	(30.136)	(30.600)
Observations	138	138	138	91	91	91
R-squared	0.471	0.870	0.821	0.693	0.888	0.913
Mean	824.0	824.0	824.0	866.4	866.4	866.4
Granted insurance claims/capita						
Private share	-0.016*	0.007	0.005	-0.006	0.008	0.007
	(0.009)	(0.009)	(0.011)	(0.008)	(0.006)	(0.004)
Observations	138	138	138	91	91	91
R-squared	0.069	0.544	0.406	0.088	0.532	0.567
Mean	0.0468	0.0468	0.0468	0.0450	0.0450	0.0450
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B6: *The effect of patient choice on costs for primary health care.*

	Capitation			Budgetary transfers		
	(1)	(2)	(3)	(4)	(5)	(6)
Net costs for primary health care/capita						
Private share	-53.328	215.403*	211.825*	-101.255	10.061	28.314
	(204.600)	(115.800)	(118.636)	(293.580)	(115.352)	(150.557)
Observations	126	126	126	77	77	77
R-squared	0.563	0.912	0.812	0.542	0.919	0.903
Mean	3093	3093	3093	3023	3023	3023
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	Yes	No	No	Yes	No
Control variables	No	No	Yes	No	No	Yes

Table B7: *The effect of private provision and capitation on quality, including county specific trends.*

	(1)	(2)
Doctor's visits/1000 inh.		
Private share	37.355**	30.479*
	(14.499)	(17.246)
Capitation		-25.624
		(36.994)
Capitation*Private share		14.623
		(16.832)
Observations	189	189
R^2	0.936	0.936
Mean of dependent variable	1362	1362
Antibiotics DDD/100 inh.		
Private share	0.023	-0.003
	(0.108)	(0.096)
Capitation		0.254
		(0.228)
Capitation*Private share		-0.023
		(0.071)
Observations	189	189
R^2	0.985	0.986
Mean of dependent variable	12.63	12.63
Visit same day		
Private share	-0.017	0.002
	(0.020)	(0.029)
Capitation		0.037
		(0.059)
Capitation*Private share		-0.027
		(0.033)
Observations	111	111
R^2	0.808	0.810
Mean of dependent variable	0.380	0.380
Visit in more than 7 days		
Private share	-0.001	0.008
	(0.020)	(0.030)
Capitation		-0.043
		(0.047)
Capitation*Private share		-0.001
		(0.036)
Observations	111	111
R^2	0.830	0.836
Mean of dependent variable	0.238	0.238
High grade on primary health care visit		
Private share	0.016*	-0.005
	(0.009)	(0.024)
Capitation		0.023
		(0.036)
Capitation*Private share		0.019
		(0.029)
Observations	111	111
R^2	0.803	0.821
Mean of dependent variable	0.771	0.771

Table B7 continued.

Avoidable hospitalizations/100 000 inh.		
Private share	-9.467	-18.219
	(14.007)	(14.435)
Capitation		-19.725
		(50.218)
Capitation*Private share		15.720
		(21.487)
Observations	189	189
R^2	0.935	0.935
Mean of dependent variable	898.3	898.3
Granted insurance claims		
Private share	0.005	0.006
	(0.004)	(0.005)
Capitation		0.032**
		(0.014)
Capitation*Private share		-0.009*
		(0.005)
Observations	189	189
R^2	0.767	0.778
Mean of dependent variable	0.0496	0.0496
Net costs for primary health care		
Private share	71.936	110.202
	(79.233)	(105.264)
Capitation		34.320
		(208.423)
Capitation*Private share		-47.057
		(109.664)
Observations	145	145
R^2	0.928	0.928
Mean of dependent variable	2723	2723
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes
County trends	Yes	Yes

Table B8: *The effect of patient choice on observable quality, including county-specific trends.*

	Capitation	Budgetary transfers
	(1)	(2)
Doctor's visits/1000 inh.		
Patient choice	26.133	78.545**
	(28.990)	(24.405)
Observations	138	91
R^2	0.963	0.958
Mean of dependent variable	1360	1381
Antibiotics DDD/100 inh.		
Patient choice	0.148	-0.002
	(0.089)	(0.143)
Observations	138	91
R^2	0.986	0.980
Mean of dependent variable	12.51	12.32
Visit same day		
Patient choice	0.011	0.091*
	(0.021)	(0.042)
Observations	97	59
R^2	0.809	0.879
Mean of dependent variable	0.364	0.401
Visit in more than 7 days		
Patient choice	-0.033*	-0.059
	(0.015)	(0.041)
Observations	97	59
R^2	0.884	0.794
Mean of dependent variable	0.225	0.216
High grade on primary health care visit		
Patient choice	0.005	0.007
	(0.019)	(0.011)
Observations	97	59
R^2	0.836	0.895
Mean of dependent variable	0.784	0.795
Avoidable hospitalizations/100 000 inh.		
Patient choice	-12.649	-22.67
	(18.048)	(24.06)
Observations	138	91
R^2	0.947	0.961
Mean of dependent variable	824.0	866.4
Granted insurance claims		
Patient choice	0.007	0.004
	(0.007)	(0.003)
Observations	138	91
R^2	0.774	0.662
Mean of dependent variable	0.047	0.045
Net costs for primary health care		
Patient choice	180.559	-144.352**
	(115.014)	(41.031)
Observations	126	77
R^2	0.937	0.976
Mean of dependent variable	3093	3023
Time dummies	Yes	Yes
County effects	Yes	Yes
Control variables	Yes	Yes
County trends	Yes	Yes

Essay 4: Sickness absence and parental leave in the welfare service sector: effects of public versus private ownership[87]

[87] Constructive and useful comments from Eva Mörk, Anders Forslund, Erik Lindqvist and seminar participants at Uppsala University and IFAU are gratefully acknowledged.

1 Introduction

Over the past few decades, private provision of public services has become increasingly common in several countries. The introduction of private alternatives in publicly financed welfare services has been advocated as a means for increasing consumer choice, reducing costs, and increasing efficiency. Private firms are often claimed to be more efficient than public firms, the main argument being the stronger incentives affecting private sector management and workers. Two main arguments explaining this difference in incentives can be found in the theoretical literature on public versus private ownership. First, in the property rights literature (see, e.g., Alchian and Demsetz, 1972) the profit-maximizing goal of private firms is considered an advantage in creating incentives to achieve optimal performance. Second, soft budget constraints have been suggested as a potential source of weaker incentives in the public sector (Kornai et al., 2003).

The empirical evidence on the issue largely supports the theoretical prediction on the comparative efficiency of private firms. Several studies have documented that private firms produce goods and services at a lower cost than do public organizations, though the magnitude of the estimated cost savings varies.[88] Privatized state manufacturing enterprises and private firms providing easily contractible services have often been found to outperform publicly owned firms in both cost efficiency and quality. The results concerning efficiency in sectors producing services with contractibility problems, such as prisons and hospitals, often point to cost reductions, but not always with quality maintained. In the industries analyzed here, i.e., Swedish primary schools and preschools, costs have been shown to be lower in privately owned units (Mörk and Hanspers, 2011; Vlachos, 2011), but to my knowledge, no studies examine whether these lower costs are indeed an effect of ownership status per se. However, the large inflow of private providers in the market for welfare services and the comparatively high returns on equity in the private market for education suggest strong profitability for the industry in Sweden (Vlachos, 2011). Since, in Sweden, private providers receive the same compensation as do public providers, there hence seems to be scope for increased efficiency and lower costs. Mechanisms behind these cost reductions

[88] See, for example, Megginson and Netter (2001) and Andersson and Jordahl (2011).

discussed in earlier literature include wage reductions, better management, more flexibility in working practices, more efficient use of capital and labor, and innovation (Domberger and Jensen, 1997). With this in mind, it is interesting to investigate if there are any differences in efficiency in privately and publicly owned establishments in terms of absence behavior among employees. Since absence is sometimes regarded as a form of shirking at the workplace, higher efficiency requirements in private firms could affect worker absence.

This paper investigates the impact of the ownership structure on worker absence among workers in Swedish preschools and primary schools. More specifically, I examine whether the sector of employment matters for absence such as sickness absence, parental leave, and temporary parental leave for care of sick children.

To study whether ownership matters for employee outcomes empirically is a complex matter, as individuals' choice of sector is not random and several factors could give rise to a spurious relationship. For instance, individuals who are more career-motivated may be more likely to work in the private sector and at the same time have less risk of sickness or have less of a preference for having children and long parental leave spells. In this paper, I take advantage of the sharp increase in private welfare service provision that has occurred in Sweden since the 1990s, when reforms aimed at introducing consumer choice and competition were implemented in publicly funded schools, preschools, health care and care for the elderly. The identification strategy relies on using hive-offs, i.e., situations in which a public unit is taken over by a private firm, as a potentially exogenous source of variation in sector of employment. The variation in ownership status within firms allows accounting for time-invariant, unobservable characteristics, through the inclusion of establishment fixed effects in the empirical analysis. However, since not all employees stay on following a hive-off, an instrumental variable (IV) approach is used to account for workers' probability to stay at the establishment.

From a longitudinal, matched employer–employee dataset, I identify firms that have switched ownership status from public to private during the period of 1994-2007. The empirical method is to compare the outcomes for the workers affected by these ownership switches with outcomes for workers unaffected by such changes. The analysis is based on a rich set of annual register data covering the working-age population. The dataset includes a large set of individual characteristics as well as detailed information on sickness spells, parental leave, and temporary parental leave for care of sick children.

As outcome variables, two measures of worker absence are used: parental leave and sickness absence. Since these two outcomes differ in that parental leave (and its length) is a voluntary decision to a greater extent

than is sickness absence[89], it is also possible to separate the mechanisms by which ownership could affect absence.

First, as implied by the property rights literature, profit maximization in private firms demands greater efficiency in production. This involves minimizing absence rates. To achieve a low absence rate, private employers may be more concerned with providing incentives for high job attendance—for instance, through rewards in terms of promotions and wage increases, or through sanctions such as increased risk of job loss. Second, whereas parental leave should be affected mainly by differences in incentives, sickness absence could also be affected by differences in work conditions between public and private employers. The direction of this mechanism is less clear. On the one hand, higher efficiency in private firms may occur at the cost of a poor work environment, characterized by, for instance, larger workloads and stress, which is likely to result in more absence (sickness absence in particular). On the other hand, efficiency could translate into investments in better work practices and terms of employment, resulting in less absence. The introduction of private welfare service provision has often been justified by the argument that private firms are better able to introduce new and innovative work practices. If this is true and the innovations affect the work environment positively, private ownership could be associated with less sickness absence. In addition, a good work environment could also be an important factor helping private firms, which are more concerned with high efficiency, to attract the most productive workers. Finally, if private firms pay their employees more (or less) than do public firms, there might also be an indirect effect on worker absence since higher pay makes absence more costly for employees.

This paper contributes to the literature on the costs and benefits of the private provision of publicly funded services. Since the private provision of publicly funded welfare services is currently the subject of intense debate in Sweden, the issue is highly policy relevant. It is also of particular interest to investigate whether privatization alters the conditions in the labor market for welfare services, which are largely dominated by women. It has been suggested that the public sector is an attractive employer for women as it is perceived as easier to combine public employment with family obligations. For example, Nordli Hansen (1997) shows that women's childcare responsibilities seem to be less punished in terms of lower wages in the public sector. Women in the public sector are also more likely to take longer parental leaves than are women in the private sector (SCB, 2007). It is also well documented that average sickness absence is higher among employees in the public sector than in the private sector in Sweden. Finally, it has also been shown that these differences persist if employees in the privately and publicly owned production of welfare services are compared (Hanspers and Hensvik, 2011).

[89] Only sickness spells lasting long 14 days of longer can be observed in data.

The stylized facts above reflect only simple comparisons of averages and do not control for the potential selection of employees and other confounding variables. To my knowledge, there are no studies that investigate the causal relationship between sector of employment and absence behavior. However, there are a number of studies on the impact of ownership on wages. For instance, La Porta and López-de-Silanes (1999) examine the effects on wages in a large number of privatized firms in Mexico. Their before-and-after estimates indicate an increase in real wages following privatization despite a receding trend in wages in the economy as a whole. Using a more convincing empirical strategy, Monteiro (2010) investigates the effect of privatization of the Portuguese banking industry. She estimates the effect using a difference-in-differences approach in which the control group is constructed by propensity score matching and finds a negative effect on wages immediately after privatization, but a positive effect in the long run. The only study using Swedish data was conducted by Oreland (2010), who finds that wages increased following hive-offs in the Swedish welfare sector. In a related study, Hensvik (2011) finds a positive effect on teacher wages following the introduction of private schools. The wage effect is positive for publicly employed teachers as well, which suggest a competition effect. However, the paper does not address the question of ownership.

The results of the present paper suggest that private ownership has a substantial negative impact on sickness absence in preschools, but no effect on the sickness absence of workers in primary education. The difference does not seem to depend on the fact that non-profit organizations are more prevalent in the provision of preschool services than in primary education. For the other two outcomes—parental leave and temporary parental leave for care of children—no robust relationship with ownership can be found in either industry. As discussed above, parental leave and temporary leave are expected to be mainly affected through the effect of ownership on the incentives for absence, whereas sickness absence also could be affected by the work environment. Hence, the absence of effects on parental leave and temporary leave implies that there is no evidence that private employment has any incentive effects. However, the results for sickness absence indicate that differences in work conditions could matter, at least for workers in the preschool sector.

The paper is organized as follows: Section 2 describes the theoretical arguments to why ownership may affect absence behavior, section 3 gives an overview of the institutional background, and section 4 describes the data used. A description of the empirical strategy is given in section 5 and the results are presented in section 6; finally, section 7 concludes the paper.

2 Theoretical background and related literature

It is often claimed that private firms are more efficient than public firms. The low-powered incentives in public organizations are often suggested as the main explanation for the difference in performance. The general theory of firms describes all types of organizations as suffering from principal–agent problems as owners, managers, and workers act in their own interests, which often collide. The solution to these problems is typically found in designing the optimal monitoring and reward system. The property rights literature (see, e.g., Alchian, 1965; Alchian and Demsetz, 1972) identifies residual rights, such as profits, as the most influential incentive to optimize monitoring and hence performance. In private firms, the owners have exclusive rights to firm assets. They are entitled to profits and also benefit from increasing firm value, since ownership shares can be sold. In contrast, the owners of public firms, i.e., the citizens or the politicians or government officials who act on their behalf, are not entitled to any of the financial gains generated by the firm. Since private owners benefit directly from the value of their shares and the surplus generated by the firm, they are also more concerned with ensuring production that yields high profits and high firm value. In addition, the concentration of ownership in private firms increases the possibilities of monitoring the management of production. Managers can easily be held accountable as the owners are well informed about the production process and the decision process is short. Public sector managers, on the other hand, are less likely to be held responsible for inefficient management since ownership is diffused over the whole collective of citizens, who are unlikely to have knowledge about and express dissatisfaction with inefficiencies in production. Since public managers are less likely to face the costs of inefficient management, their incentives to devote effort to increasing efficiency and reducing costs are weaker than for private managers. Therefore, it could be expected that private managers are also more likely to supply workers with incentives to reduce shirking.

Not only are private firms bound to profit maximization, their incentives to avoid deficits are also greater than within public sector organizations. The concept of soft budget constraints provides an additional explanation as to why public organizations that provide welfare services

may behave less effectively than their private counterparts. The theory of soft budget constraints was initially designed to explain the survival of inefficient industries in socialist economies (Kornai, 1979), but has also been applied to describe the relationship between local governments and public providers (see Duggan, 2000, for the case of hospitals). The theory describes the situation when governments cannot credibly threaten not to cover the budget deficits of a public production unit or local government. Knowing that it will not be allowed to go out of business, but will be "bailed out" by the government, the lower-level unit devotes less effort to not letting costs exceed the budget. There are numerous reasons for why local government may be willing to rescue lower-level administrations. In this context, politicians could be motivated by concern for service users or employees, but also by a desire to protect the presence of public providers in the market.

2.1 Ownership and absence behavior

An important strategy to keep production costs down is to cut personnel costs. This is especially true in the labor-intensive welfare service sector, where personnel costs constitute the largest factor cost. One way of reducing production costs is to ensure a low rate of absence among employees. The employer's cost for employee absence mainly consists of the value of lost production once the value of the production of a stand-in has been netted out. The magnitude of the productivity difference depends on the level of firm-specific human capital possessed by the absent worker compared with the experience of the replacement. The recruitment cost for a stand-in represents another cost to the employer. In some countries, the benefits received by employees due to sickness and childbirth are also by law or contract co-financed by the employer.[90]

The costs mentioned above clearly provide a motive for the employer to minimize absence among its employees. Both the property rights approach and the theory of soft budget constraints suggest that the incentives of private firms cause them to be more concerned with excess costs and high productivity. Due to the differences in incentives, private or public ownership may affect the absence behavior of employees. Below, I outline the two main arguments as to why absence in the private sector can be expected to differ from absence in the public sector.

[90] In Sweden, employers are responsible for sickness payments from the second to 14th day of sickness absence. After the 15th day of sickness absence, sickness pay is covered by sickness insurance, except for 15 percent employer co-financing. Employer co-financing is restricted to a maximum of 4 percent of the company payroll.

First, in the pursuit of lower costs and higher profits, private firms are more likely to require greater effort[91] from their employees, by instituting more efficient monitoring or by more effectively tying rewards and penalties to worker performance. If workers can affect their absence, they have the incentive to minimize absence if they are monitored and if absence is associated with sanctions from the employer. As numerous studies show, employers can "punish" absence, for instance, by lower wage growth, worse career opportunities, or even discharge. For instance, Allen (1981a, 1981b) and Kenyon and Dawkins (1989) suggest that absence may increase the probability of being fired, which results in loss of future earnings. Audas et al. (2004) show that absence can affect the chance of promotion and wage growth. If monitoring is stricter in private firms, the expected cost of absence is higher among private sector employees. However, work effort is sometimes difficult to monitor. According to the efficiency wage model (Shapiro and Stiglitz, 1984), when work effort is difficult to monitor, employers may pay wages that are higher than the market wage to induce workers not to shirk, or not to be absent from work. Arai (1994) finds results that favor the efficiency wage hypothesis in the private sector, but not in the public sector, when studying the role of efficiency wages in explaining inter-industry wage differentials in Sweden. He suggests that the differences in the use of efficiency wages across sectors can be due to lack of product competition in markets dominated by the public sector, but also because of difficulties in identifying the production effect of higher wages in the public sector, due to the problems with performance measurement in the public sector discussed above. It should be noted that these results apply to public and private employees generally, and it is not established whether the observed relationship is valid also for workers in welfare services. Regardless of the method used to influence effort (i.e., monitoring or efficiency wages), private sector firms have stronger incentives to enhance effort. Hence, the expected costs of absence for the employees in terms of career opportunities, wage growth, and risk of discharge can be expected to be higher in the private sector.

Whereas sickness absence is usually unanticipated by the employer, parental leave is more the rule than the exception, at least among women. Being an expected event, it is not necessarily sanctioned ex post by the employer. In a study using Swedish data, Albrecht et al. (1999) find no effect of parental leave on subsequent earnings among women, contrary to career breaks due to other reasons, such as unemployment; there seems to be no difference in this between private and public sectors either. The authors' interpretation of their finding is that since taking parental leave

[91] The case for attendance being a component of worker effort is made, for example, by Flabbi and Ichino (2001) and Audas et al. (2004).

is viewed as the norm for women, it cannot work as a signal of career commitment, resulting in a pooling equilibrium in which parental leave leads to no penalty on women's subsequent earnings. For men, they find a negative impact of parental leave on subsequent earnings, possibly indicating that male parental leave uptake represents an unanticipated outcome to the employer. These results suggest that there might be reasons to expect no or weaker effects of private sector employment on parental leave than other types of absence, such as sickness absence, at least for women. It should be noted that the share of parental leave taken by women has decreased in recent years, which may have caused employers to adjust their expectations regarding parental leave uptake for both men and women. During the period analyzed by Albrecht at al. (1999), women's share of parental leave days was 91 percent, but since then the share has slowly decreased to about 78 percent in 2010. In a more recent study, Johansson (2010) finds that parental leave had negative income effects for both women and men, but this study does not separately estimate the effects for private and public employees.

Second, ownership could matter for the terms and conditions of work, i.e., work income, work environment, and level of job security. The effect of the wage level on absence is ambiguous since there are two conflicting mechanisms that contribute to the overall effect of income on absence. A wage increase causes an income effect that increases the tendency to be absent if leisure is a normal good. A wage increase also produces a substitution effect that tends to reduce absence, since a wage increase makes absence more costly for the worker. The empirical evidence on the direct relationship between income and absence is mixed, and negative as well as positive or insignificant relationships have been found (for a review of relevant studies, see Brown and Sessions, 1996). There might also be differences in the work environment and the level of job security between the private and public sectors. It is well documented that the characteristics of the work environment, such as physical and psychological job demands, job control, and social support at the workplace, are important factors in explaining the health and sickness absence of employees (see, e.g., Vahtera et al., 2000). The level of job security can also affect worker effort or absenteeism (Arai and Thoursie, 2005; Ichino and Riphahn, 2005). In which direction private employment could affect absence through this mechanism is less apparent. On the one hand, in the pursuit to reduce costs, private employers may be more likely to worsen the work environment by increasing the demands on performance, which could be stressful and increase the risk of sickness. On the other hand, private firms may find it worthwhile to invest in improved work practices and work environment. For instance, control and social support are proven important factors counteracting harmful stress at the workplace in the

demand and control literature (Theorell, 2003). If private firms are more willing to employ work routines that emphasize the influence of the employees, private employment could be associated with improvements in worker health. However, it should be noted that in Sweden, employers are required to finance short-term sickness leave, but not long-term sickness absence. Unless the costs of lost firm-specific human capital and the hiring cost of replacements are very high, the incentives to improve the work environment are quite weak, since poor work conditions are mainly associated with long-term leave.

To summarize, the theoretical arguments described above identify the stronger incentives of private sector employees as possible causes of lower absence among privately than publicly employed workers. Due to more efficient rewarding and sanctioning in private firms, the cost of absence is higher in private firms and therefore lower among private employees. Regarding the second mechanism, differences in work conditions, the direction of the relationship is less clear. On the one hand, the focus on efficiency and profits in private firms may be associated with a harmful work environment, causing higher sickness absence rates; on the other hand, private firms may offer their employees a better work environment as a result of an innovative approach in work practices. Following this line of reasoning, the relationship between private sector employment and sickness absence is ambiguous, depending on the direction of the second mechanism and on which effect dominates. Even though the work environment could affect the decision to have children and the uptake of parental leave and temporary leave on the margin, the incentive effect is likely most relevant.[92] Hence, lower uptake of parental leave could be expected in private firms. The findings of Albrecht at al. (1999) discussed above could suggest that no effect on parental leave is to be expected, at least not among women, as parental leave is not necessarily sanctioned ex post being an anticipated outcome.

[92] If parents substitute sickness absence for temporary parental leave to avoid the sickness insurance waiting period, a phenomenon confirmed by Persson (2011), the work environment could also affect temporary parental leave.

3 Institutional framework

3.1 Privatization of welfare services

Welfare services such as healthcare, care of the elderly, education, and childcare have traditionally been provided almost exclusively by the public sector in Sweden. In the 1990s, several of these markets were opened up to competition and to private establishments to increase consumer choice, but also to provide incentives to increase productivity and reduce costs. The reforms implemented in the markets analyzed in this paper, primary schools and pre-schools,[93] are described in more detail below.

Municipalities are legally required to provide full-time preschool for children when parents work or study, and to arrange part-time preschool for children whose parents are unemployed or on parental leave. Parents are allowed to influence the choice of preschool for their children as municipalities should consider the wishes of parents when allocating preschool places. A large majority of Swedish children attend preschools, and 81 percent of children aged 1–5 years were enrolled in municipally funded preschools in 2008. Privately owned preschools have been allowed since the 1980s, but eligibility for public funding was restricted to preschools run by non-profit organizations, such as staff- or parent-owned cooperatives. Preschools are heavily subsidized by municipalities, as the fees paid by parents cover only about 10 percent of the total costs of childcare. Public financing is based on the number of enrolled children, and the amount received by private providers should not unreasonably deviate from the compensation given to public providers. To be eligible for public funds, it is necessary to obtain approval from the municipality where the preschool is situated. The approval is based on commitment to the requirements of the Swedish National Agency for Education. These include running the preschool according to the preschool curriculum and admitting all preschool-eligible children as long as there is room for more children. Since 2006, for-profit organizations can run preschools with public funding according to national law. In many municipalities,

[93] Other sectors that would be interesting to study are care of the elderly and health care. Elder care workplaces are often impossible to identify in the data, and is hence omitted from the analysis. Studying effects on sickness absence in health care may also be afflicted with problems as the privatization of health care is likely to be endogenous in the model.

for-profit preschools could receive public funds even before this, following a law passed in 1992 giving municipalities the right to decide whether to fund for-profit preschools.

The introduction of private alternatives in the primary school sector took place somewhat later, through a voucher reform introduced in 1992. Since then, privately owned schools have been entitled to public funding from municipalities if they comply with the requirements of the Swedish National Agency for Education. Primary education is free of charge and, since the reform, has been financed by vouchers that follow the student's choice of provider. As in preschools, the funding amount per student in private primary schools should be calculated on the same basis as for public primary schools.

Figure 1 illustrates the expansion of private providers in the preschool and primary school sector from 1994 to 2008. The figure shows a large difference in the proportion of children attending private preschools and primary schools, most likely reflecting the fact that private preschools have been allowed for a longer time. At the beginning of the period, 12 percent of preschool children attended a private preschool, whereas less than 2 percent of primary school students went to a private school. Since the introduction of the primary school voucher reform there has been a large increase in the share of pupils who attend a privately owned primary school, and private primary schools educated 10 percent of Swedish children in 2008. In the same year, the share of privately enrolled preschool children had risen to 18 percent.

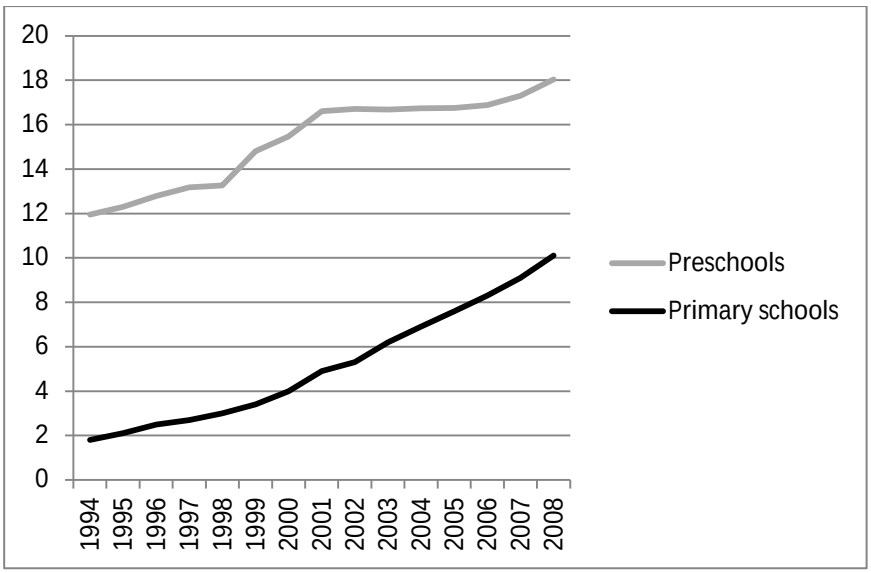

Figure 1. Share of children in private preschools and primary schools, 1994–2008.

Figure 2 shows the share of private employees working in for-profit versus non-profit firms in both industries. As Figure 2 shows, for-profit organizations are much more common in the primary school sector than in the preschool sector, where non-profit organizations represent the majority of private provision. One explanation is that personnel and parent cooperatives are especially common in the preschool sector. It is evident that for-profit firms have become increasingly common in both preschools and primary schools. Following the allowance of for-profit firms in the preschool sector, there was a large jump in the prevalence of for-profit provision, reaching almost the levels found in primary education, where for-profit organizations have been allowed to run schools since 1992. Since the end of the 1990s, the for-profit share of primary schools has increased more rapidly.

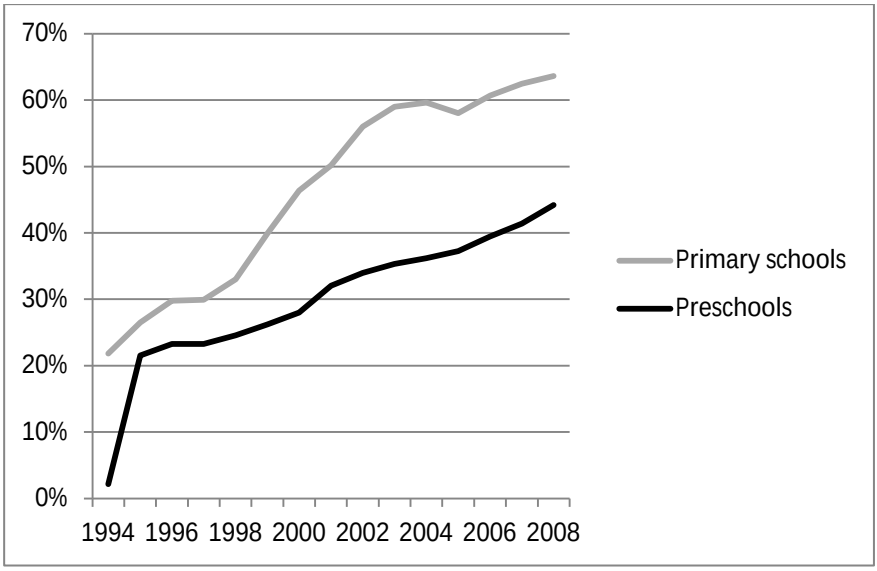

Figure 2. Share of privately employed preschool/primary school personnel working in for-profit firms, 1994–2008.

3.1.1 Privatization through ownership change

The reforms described above have caused an increase in private alternatives in the school and preschool markets. In some cases, private schools or preschools emerge through ownership change. These changes have become known as *hive-offs*. It is not a juridical term, but is recognized by counties and municipalities as a form of privatization in which employees or existing firms take over formerly municipal establishments to run as private firms. For instance, in the municipality of Täby, which has been something of a pioneer in private welfare service provision, preschool hive-offs have occurred both through principals buying preschools and though outsourcing to firms with preschools in other municipalities. Formally, it is not legal for the municipality to offer to sell an establishment. Still, when hive-offs occur, they have often been preceded by municipalities or counties indicating that they favor such a development. The price paid by the buyers in a hive-off is supposed to be the market price, although there has been controversy in the media regarding hive-offs in which municipalities have been accused of asking below-market-value prices.

An advantage with using variation in ownership caused by hive-offs is that when private firms take over a public unit, the conditions remain similar to before. The new owner takes over the facility including premises, basic technology, staff, and students. When a hive-off occurs, employees are entitled to remain in their positions (according to labor legislation, ownership changes are not a valid reason for dismissal, 7 § LAS). Employees can also choose to remain in public sector employment, but at another establishment. However, as described in section 4, most employees stay following a hive-off. Since workers in firms subject to hive-offs are still employed by the same firm, the same rules of job security apply as if their workplace had remained under public management. Also for the workers who remain, the same order of seniority applies even if they move to another public unit, since they can apply the years spent at the hived-off unit to their total tenure with the public employer.

3.2 Social insurance policy

Sweden's social insurance system consists of governmental insurance that compensates individuals who cannot support themselves due to, for instance, illness, disability, childbirth, or illness of one's child. The benefits are usually based on previous earnings, but individuals without employment can receive a smaller guaranteed amount. In addition to the governmental benefits, it is common for employers to provide workers with extra benefits as a result of agreements between unions and employ-

ers' organizations. In case of illness, employers are also responsible for compensating workers during the first days of sickness absence. Below, I describe in more detail the rules concerning sickness absence, parental leave, and temporary parental leave to care for children.

3.2.1 Sickness absence

For employed individuals, the first 14 days of sickness absence are financed by sick pay from the employer, except for the first day of the spell, which is a non-reimbursed waiting period. For spells that last more than seven days, a doctor's certificate is required. The days of the sickness spell that follow the sick pay period are covered by a sickness benefit paid by the Swedish Social Insurance Agency. The sick pay and sickness benefit replace 80 percent of the annual income up to the sickness benefit ceiling.[94]

3.2.2 Parental leave

According to Swedish parental leave legislation, parents are entitled to 480 days of paid parental leave for each child. These days are divided equally between both parents, but can be transferred to the other parent, except for the 60 days reserved for each parent. Most of the days are reimbursed by 80 percent of the wage income up to a ceiling, but 90 days are reimbursed at a flat rate of only SEK 180 per day (SEK 60 per day for children born prior to 1 July 2006). Parents without or with low previous income from employment are only entitled to a minimum level of benefits of SEK 180 per day. There is great flexibility in how the parental leave can be used. Parents can choose to take leave for an extended period, single days, or partial days. It is also possible to be absent from work for a longer period than there are paid days of parental leave by claiming benefits for only parts of the week. The parental leave benefit can be used at any time from 60 days before the expected delivery until the child turns eight years old.

3.2.3 Temporary parental leave for care of children

When children under 12 years old are ill, contagious, or must attend a healthcare facility, parents or another adult[95] who takes care of the child are entitled to leave with 80 percent of the lost income replaced by tem-

[94] In 2007 the sickness insurance ceiling was SEK 302,250.

[95] Only parents, a person who lives with a parent, foster home parents, persons who receive a child with a view to adoption, and other persons with legal custody of a child are entitled to benefits directly, but benefits can be transferred to another person who stays away from work to look after the child.

porary parental benefit. As with parental leave benefits and sickness insurance, only income up to a ceiling is reimbursed. Unlike sickness insurance, no waiting period applies and benefits can be received from the first day. From the eighth day of the care period, a doctor's certificate is required. Temporary parental leave benefit can be granted for a maximum of 120 days per year and child. Benefits can be claimed for whole and partial days. Since temporary parental leave is intended for parents who work, it is only granted to parents on parental leave in exceptional cases, such as when children need hospital treatment.

3.2.4 Benefits from the employer

In addition to the governmental benefits, employers can pay extra benefits to workers absent due to sickness or parental leave. The size of the extra benefits is regulated through collective agreements. Private and public employees in the welfare sector are subject to different agreements between unions and employers' associations. Overall, these agreements for private and public employees are quite similar, but differ somewhat in details such as the number of days of extra benefit and the tenure requirements.

Teachers' collective agreements (for both school and preschool teachers) entitle workers to extra parental leave benefits financed by the employer. Public sector teachers who have been employed for at least one year are entitled to 10 percent extra parental leave benefits for 90 days. The agreements covering most teachers working in the private sector also entitle teachers to 10 percent extra benefits, but the number of days for which the extra benefits can be claimed differs somewhat. Most agreements give extra benefits for 120 days at most, but the requirement is often that one must have worked for more than 2–4 years to be entitled to the maximum period of 120 days. Workers employed for less time receive extra benefits for a 30–90 day period.

The agreement covering public sector childcare nurses (i.e., childcare workers with a secondary school childcare education) entitles them to an extra benefit of 10 percent of lost income for a maximum parental leave period of 150 days for workers employed for at least one year prior to childbirth. The agreement covering most childcare nurses employed in the private sector entitles them to receive 10 percent of their wage for 60–120 days, depending on tenure. To be eligible, at least one year of employment is required. The agreements also entitle both private and public childcare nurses to extra sickness benefits of approximately 10 percent of the lost income for days 15–360 of a sickness absence spell.

4 Data

The data used in this study consist of a panel of individuals working in Swedish preschools or primary schools observed over the 1994–2008 period. A linked employee–employer database (RAMS) is used to identify all individuals[96] working in the relevant industries[97] and to classify them according to institutional sector (i.e., private or public). The data also contain detailed information on the corporate form of a firm, so it is possible to separate, for example, private limited companies from non-profit organizations, foundations, etc. The register contains an identifier for the establishment at which an individual works as well as information on the main industry branch of the establishment. These data are combined with information on sickness absence, parental leave, and temporary parental leave for care of children from the Swedish Social Insurance Agency as well as individual demographic variables (i.e., age, gender, education, country of birth, family status, number of children, and age of youngest child). In addition, a few municipal characteristics (i.e., tax rate, political majority, number of preschools/primary schools in each municipality, and proportion of children in private sector preschools/primary schools per municipality) are included in the analysis. Table 1 in the Appendix presents descriptive statistics for the full sample.

4.1 Sickness absence

Data on sickness absence capture all sickness spells lasting longer than the period financed by the employer. This means that only spells lasting 14 days or longer can be observed. This is unfortunate since most sickness spells are short; a majority of spells lasts less than three days. The period financed by the employer has also varied over years. Between

[96] Individuals are defined as those working in preschools or primary schools if employment there is their main source of work income. To capture long-term absence, individuals who did not work in the industries but are absent due to sickness or parental leave are also defined as belonging to the labor force of these industries if they earned their main income in these industries the year before.

[97] Here, *industry* is used to describe a field of work, i.e., preschools or primary schools, and *sector* refers to the different parts of the economy owned by either private firms or governments.

1997 and 1998, only spells longer than 28 days and between July 2003 and December 2004 only spells longer than 21 days can be observed in the data. To obtain a measure of sickness absence that is as short as possible, the years mentioned above are excluded from the analysis. The analysis uses two measures of sickness absence: a dummy for being absent due to sickness for at least a 14-day spell, and a continuous measure of the number of days absent due to sickness, given that the spell was at least 14 days long.

4.2 Parental leave

The register data contain information on parental leave for each individual and child. The measure used in the analysis is the yearly number of days of parental leave per individual. Since the number of days in a parental leave spell does not necessarily correspond to the number of days of parental benefits, several different measures can be calculated. The paid number of days is the time spent off work that is actually reimbursed by parental leave benefit. The total number of days captures the full length of the parental leave spell, with or without parental leave benefit. (For instance, a parent can be absent from work due to parental leave for four weeks, but only apply for benefits for three days per week. In this case, the paid days of parental leave would be 12 days but the total days of parental leave would be 28 days). To save space, only the results of the analysis of the paid number of parental leave days are reported. To verify that the results are robust to differences in parental leave without benefits across sectors, all regressions were also run using the total number of days as an outcome variable. The results of this analysis are available upon request.

4.3 Temporary parental leave for care of children

The third outcome variable of interest is temporary parental leave for care of children, i.e., absence from work when a child is ill. Data contain annual information on the number of days of absence per individual and child. The measure used in the analysis is the number of days of absence to care of children per individual. As a rule, temporary leave is only granted for children up to a maximum age of 12 years. Hence, in the

analysis of temporary parental leave, only individuals whose children are under 12 years of age are included.[98]

4.4 Descriptive analysis

Figures 3 and 4 depict the difference in worker absence between the public and private sectors. The figures confirm the pattern of higher sickness absence among public sector employees found in previous studies. Sickness absence is more frequent and of longer duration among public than private employees in preschools as well as primary schools. In preschools, the share of publicly employed who were absent from work due to sickness is on average 25 percent while the corresponding number for privately employed is 20 percent. In primary schools, sickness absence is lower and the difference is also smaller.

It is also evident that private employees take more parental leave than do public employees. Table A1 in the Appendix shows that this may be because private employees are more likely to have young children. However, the uptake of temporary leave for care of children is similar for public and private employees in both sectors even though private employees have more and younger children.

[98] In certain cases, it is possible to receive temporary parental benefit for a child who has attained the age of 12 (but is under 16), for example, if the child has an illness or disability that requires special supervision or care.

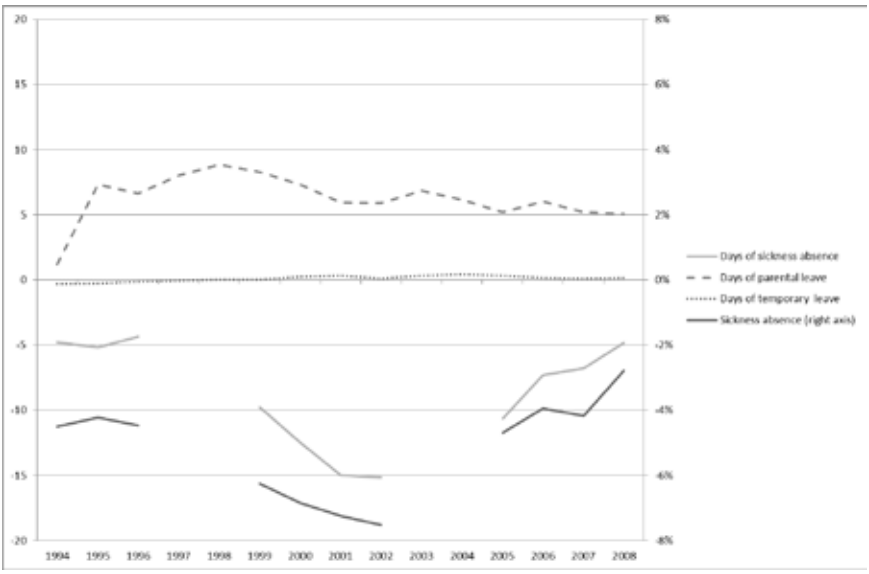

Figure 3. Worker absence in preschools, public–private differences.

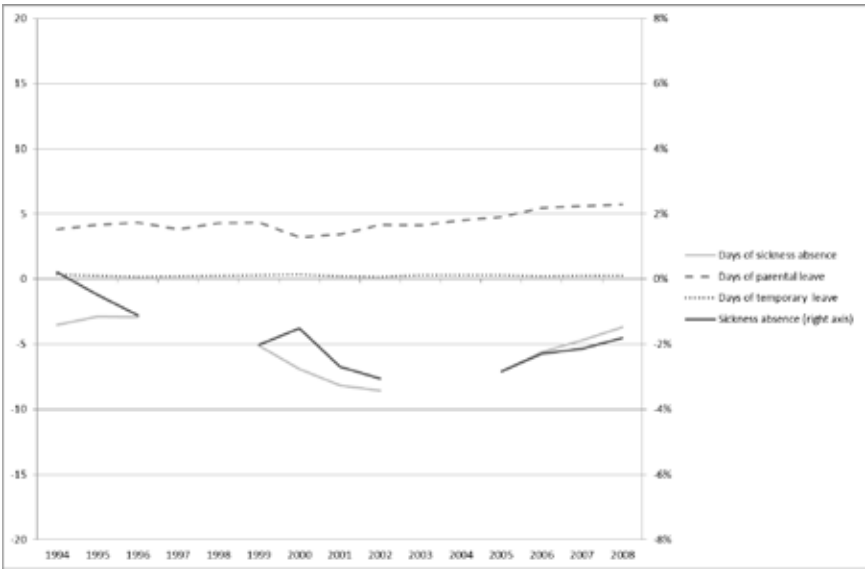

Figure 4. Worker absence in elementary schools, public–private differences.

4.5 Hive-offs

The variation in sector of employment used in the IVs analysis is based on transitions of work places from public to private ownership. These transitions are identified by two complementary methods: either by an establishment changing ownership status from public to private, or by the majority of employees moving from a workplace in the public sector to a workplace in the private sector. The last method is used to ensure that transitions in which the new establishment gets a new identification number become part of the analysis. If more than one company is represented at a workplace, only the one with the most employees is considered. Individuals employed at the workplace in the last year the firm was publicly owned are categorized as subject to a hive-off.

By this definition of hive-offs, 134 out of 14,046 preschools and 32 out of 4,923 primary schools were defined as privatized during the 1994–2007 period. The hived-off units represent 5 percent of the private preschools and 3 percent of the private primary schools in the sample.

The hive-offs were concentrated in the Stockholm area: 85 percent of the preschool hive-offs and 30 percent of the primary school hive-offs took place in the Stockholm region. The rest of the hive-offs were not concentrated in other large cities, but were spread over the country. Preschool hive-offs have occurred in 31 municipalities (16 in the Stockholm region) and primary school hive-offs in 24 municipalities (6 in the Stockholm region). Hive-offs occurred throughout the study period in both sectors, but peaked in 2000–2002. About 50 percent of all hive-offs of both preschools and primary schools occurred during this period.

5 Empirical strategy

This paper analyzes the effect of private versus public ownership on sickness absence and parental leave. The empirical model of interest is given by the following equation:

$$y_{ijt} = \alpha_0 + \beta_1 private_{ijt} + \rho X_{ijt} + \gamma_t + \varepsilon_{ijt} \quad (1)$$

where *i* indexes individuals, *j* establishments, and *t* time; y_{ijt} represents the outcome variables of interest, i.e., sickness absence, days of sickness absence, days of parental leave, or days of temporary parental leave for care of children.[99] $private_{ijt}$ is an indicator equal to one if the establishment where the individual worked belonged to the private sector at time *t*. X_{ijt} is a set of individual and municipal characteristics (including municipal fixed effects for the location of the establishment) and γ_t represents common time effects; X_{ijt} also include the size of the establishment measured as the number of workers to account for the fact that private units may be smaller, which may affect absence rates. The individual characteristics are age, age squared, gender, educational attainment, number of children in the household (six categories), dummies for age of youngest child (i.e., 0–2 years, 3–6 years, and 7–12 years), marital status, and immigrant status. To account for the impact of competition and other political traits in the municipality, which may be correlated with the likelihood of working in a private establishment and also with the risk of absence, a few municipal characteristics are also included: share of children enrolled in private preschools or primary schools, number of establishments in each industry, municipal tax rate, and political majority. The parameter of interest is β, which would give the causal effect of private sector employment if this is based on random assignment given the observable covariates included in the model.

However, one can be concerned that the standard OLS estimates from equation (1) might be biased due to selection based on non-observable worker characteristics. For instance, if individuals with lower risks of sickness absence or less preference for parental leave self-select into the private sector, the effect of employment in the private sector would be

[99] These variables are described in detail in section 4.

overestimated. To address this problem, I use hive-offs, i.e., situations in which work units in the public sector have become private, as an instrument for private sector employment. The ownership switches caused by hive-offs provide a potential source of exogenous variation in private sector employment that can be exploited to estimate a causal effect of employer ownership status on sickness absence and parental leave.

In the first part of the analysis, the variation in sector of employment from hive-offs is used to control for time-invariant unobservable characteristics by the inclusion of establishment fixed effects. This means estimating the equation above including establishment fixed effects. However, not all employees stay with an establishment following a hive-off, so an IV approach is used to account for selection out of the establishment. The model is estimated using two-stage least squares (2SLS). In the first step, the equation for sector of employment, $private_{ijt}$, is modeled as follows:

$$private_{ijt} = \alpha_1 + \lambda hiveoff_{ijt} + \phi X_{ijt} + \gamma_t + \mu_{ijt} \quad (2)$$

where $hiveoff_{ijt}$ is the instrumental variable. It takes the value one in year *t* and all following years if the individual worked in a public firm at time $t - 1$ that switched ownership status (as described in section 4.5) to private sector at time *t*. X_{ijt} represents the control variables in equation (1) above and γ_t is the common time trend. In the second step, the first-step model is plugged into equation 1, together with all control variables. The IV estimate of β_1 then captures the average effect of $private_{ijt}$ on y_{ijt} for those who react to the instrument, that is, those who choose to stay at the establishment after privatization.

The requirements for this approach to work is that the instrument is relevant, i.e., correlated with the explanatory variable[100] (private sector), i.e., $Cov(Z_{jt}, X_{jt}) \neq 0$, but uncorrelated with the error term, i.e., $Cov(Z_{jt}, \varepsilon_{ijt}) = 0$. The crucial assumption of this model is therefore that the privatization decision is not based on unobserved characteristics of the establishment or the employees in it, which are also correlated with the outcome variables.

Is this a credible assumption? The process of public units turning private is poorly documented. It is possible that units with good characteristics, i.e., in which the employees are less sick or less prone to take parental leave, are more likely to be sold. On the other hand, municipalities may be more willing to give up establishments with poor performance or characteristics. Jordahl and Andersson (2011) suggest that efficient and well-managed firms are more likely to be outsourced. Their observation

[100] The results from the first stage regressions are reported together with the regression results in Section 6.2.

is supported by a study of privatizations in Czechoslovakia, where the more profitable companies were the first to be privatized in order to increase state revenues and promote the privatization program (Gupta et al., 2008). If such problems are present, the IV approach will yield biased results. To explore whether the hived-off units were any different from other public units, Table 1 compares the average values of the outcome variables prior to a hive-off in firms that switched ownership and firms that did not. Since the number of employees in not-yet-hived-off establishments decreases over time, the means and standard deviations are weighted by the number of observations each year.

Table 1. *Employee characteristics in public and hived-off firms (prior to hive-off), 1994–2006.*

	Not hived off			Hived off					
Preschools	Obs.	W. mean	W. std. dev	Obs.	W. mean	W. std. dev	Diff.	*t*-stat	Pr(|T|>|*t*|)
Sickness absence	909844	0.48	0,.43	9389	0.23	0.42	0.02	4.8	0.00
Days of sickness absence	909844	23.43	71.24	9389	19.92	63.85	3.51	5.3	0.00
Days of parental leave	903853	16.75	52.21	9365	16.88	53.82	–0.13	–0.2	0.82
Days of temporary parental leave	261995	9.34	9.53	2828	11.40	11.57	–2.06	–9.5	0.00
Primary schools									
Sickness absence	1640433	0.19	0.39	2156	0.18	0.38	0.01	1.4	0.18
Days of sickness absence	1640433	20.14	67.46	2156	19.81	67.03	0.32	0.2	0.82
Days of parental leave	1628664	9.95	38.85	2153	10.71	42.80	–076	–0.8	0.41
Days of temporary parental leave	308646	6.60	8.17	404	6.25	7.57	0.35	0.9	0.35

As Table 1 shows, there are some differences in the outcome variables in firms subject to hive-offs compared with public units that did not switch ownership. *T*-tests show that the differences in the sickness absence measures and in temporary parental leave uptake in preschools are significant. In primary schools, no significant differences are found. I further explore the robustness of the IV estimates by including the lagged values of the dependent variable in section 6.3.

When compliance with the instrument is voluntary, a limitation of IV is that IV will only consistently estimate the treatment effect for those influenced by the instrument (Angrist and Imbens, 1994). If treatment effects are heterogeneous in the employees experiencing a hive-off, the estimate might not be informative regarding the average effect of private

ownership. Table A2 in the Appendix presents the mean values of a selection of the variables used in the analysis *prior to* privatization, distinguishing between the employees who chose to stay and those who did not. The table shows that in both preschools and primary schools, those who did not stay were sick more often than were those who stayed, prior to ownership change. The average length of the sickness spells was also greater among non-stayers. When it comes to parental leave, the pattern differs between the two industries. In preschools, those who stay claimed less parental leave than did those who did not stay, whereas the pattern is the reverse in primary schools. This could naturally reflect that non-stayers in preschools and stayers in primary schools were more likely to have young children. However, the level of temporary leave uptake seems quite similar between those who do and do not stay. Another common trait is that those who stayed were better paid than those who did not. Of course, this difference can reflect that workers with temporary contracts (e.g., replacements) are overrepresented in the latter group. Considering the differences, it should be noted that the analysis might be valid only for the group of compliers, i.e., individuals who chose to stay in the establishment following a hive-off. To investigate whether a potential effect does not merely capture a selection effect, the reduced-form effects are also included in the analysis.

A potential concern when using hive-offs as an instrument for private ownership is that the hive-offs may affect workers through other channels than private ownership per se. If hive-offs cause employees to leave the labor force or their industry of employment, the estimate may be biased. To investigate whether this is a problem, I estimate the probability of leaving the labor force or the industry, in addition to estimating the first-step regression presented in section 6.2.

A related concern is that the privatization process itself might have affected workers. For instance, organizational changes could affect the outcomes. As a sensitivity test, I exclude observations in a three-year window adjacent to the ownership change comprising of the last year of public ownership plus the first and second years of private ownership (see section 6.3). Using this time window also reduces the risk of measurement error of the exact timing of the hive-off due to the fact that ownership status is reported only once a year.

The variable of interest in this paper is ownership and not competition. The population of private employees are, on average, subject to more competition than are public employees, since private firms almost exclusively coexist with a public provider. It may therefore be relevant to control for the degree of competition in the market to avoid confusing the ownership effect with a competition effect. To control for this, I include measures of the amount of competition measured as the share of privately

performed services and the number of establishments in each industry and municipality. Including only the number of establishments in the municipality would not convey the full picture, since they may all be owned by the municipality, meaning that only one firm is operating in the market. Although public units may compete with each other to some extent, the competition from private sector firms measured as the share of services performed by private firms is also included in the analysis.

6 Results

This section presents the results of regressing employment in the private sector on the outcome variables sickness absence, parental leave, and temporary parental leave for care of children.[101] I begin with the fixed-effects results, and thereafter provide the results of using the IV approach. The sample used consists of workers in preschools and primary schools from 1994 to 2007, except for the sample used to estimate the effect on temporary parental leave, which consists only of parents of at least one child aged 12 years or younger.[102]

6.1 Fixed-effects analysis

An interesting starting point for the empirical investigation is to compare the outcomes of privately and publicly employed workers, given a set of control variables. As the descriptive analysis shows, there are some differences in the outcome variables across the two sectors, in both industries. Since the composition of the workforce differs between the two sectors, a first step towards identifying the causal effect of ownership status is to control for these observable characteristics. Table 2 reports the baseline results of equation (1). Columns 1 and 4 report the estimated relationship between employment in private preschools and primary schools when including a number of individual and municipal-level covariates. The regressions in columns 2 and 5 also include individual specific effects, which mean that the relationship is identified by those who change sector status, either through moving to another employer or through a change in owner.

[101] The measure for parental leave and temporary parental leave for care of children used here is the net number of days of leave, i.e., the actual time reimbursed by parental leave benefit or temporary parental leave benefit. The regressions have also been estimated using alternative measures. For parental leave, the alternative measure consists of the total number of days of leave, which also includes days of leave without benefits. For temporary leave, it consists of the gross number of days, which means the number of calendar days for which any leave has been reimbursed. Using the alternative measures yields approximately the same results. These results are available from the author upon request.

[102] All regressions including parental leave have also been estimated using the sample of parents of children 12 years or younger, with approximately the same results. These results are available from the author upon request.

The OLS results suggest that there is a negative relationship between employment in the private sector and sickness absence in both preschools and primary schools. Being employed in a private sector preschool is associated with a 3.5 percentage-point lower risk of being absent due to sickness and with approximately five day shorter sickness spells. These two estimates correspond to 15 and 21 percent of the sample means, respectively. Including individual fixed effects does not markedly change the results for preschools. The OLS estimates for primary schools indicate a 1.6 percentage-point lower risk of sickness absence and 2.3 day shorter sickness spells, representing 9 and 12 percent of the sample means, respectively. The results for days of sickness absence in primary schools are not robust to the inclusion of individual fixed effects. The OLS results also suggest a negative relationship, albeit statistically insignificant and of small magnitude, between private sector employment and parental leave in the preschool sector. In the primary school sector, no statistically significant relationship is found between private sector employment and parental leave.

The last panel shows that private sector employment is associated with less temporary parental leave for care of children: almost one day less per year in preschools and half a day less per year in primary schools. The estimated coefficients amount to about 12 and 13 percent of the sample means, respectively. The results are robust to the inclusion of individual fixed effects, although the point estimates decrease somewhat.

Even when including individual fixed effects, sector of employment is still likely endogenous since the sector is actively chosen by the employee. To avoid the bias arising from workers' actively choosing the sector, I include establishment fixed effects in the following regressions. The variation is hence based on firms switching ownership status from public to private for those who stay with the same firm. Which individuals choose to stay in the firm following a hive-off is naturally not random, but this approach allows the effect of privatization to be estimated for a group of individuals who did not actively seek private employment. Columns 3 and 6 report the results of this analysis.

Table 2. *OLS results of private sector employment on sickness absence, parental leave and temporary leave for care of children.*

	Preschools			Primary schools		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Sickness absence						
Private	–0.035***	–0.033***	–0.064***	–0.016***	–0.016***	0.017*
	(0.002)	(0.003)	(0.009)	(0.003)	(0.003)	(0.010)
Observations	1,153,984	1,101,922	1,153,601	1,878,625	1,805,613	1,878,525
R^2	0.042	0.447	0.070	0.043	0.450	0.052
Mean of dep. var.	0.238	0.245	0.238	0.183	0.186	0.183
Dependent variable: Days of sickness absence						
Private	–4.711***	–4.073***	–5.913***	–2.305***	–0.545	–0.173
	(0.276)	(0.424)	(1.233)	(0.370)	(0.483)	(1.970)
Observations	1,153,984	1,101,922	1,153,601	1,878,625	1,805,613	1,878,525
R^2	0.037	0.419	0.059	0.034	0.437	0.041
Mean of dep. var.	22.29	23.11	22.29	19.63	20.23	19.63
Dependent variable: Parental leave						
Private	–0.108	–0.060	–0.917*	0.038	–0.092	0.185
	(0.093)	(0.235)	(0.530)	(0.127)	(0.245)	(0.886)
Observations	1,590,988	1,585,137	1,590,644	2,619,213	2,611,458	2,619,139
R^2	0.557	0.637	0.561	0.496	0.609	0.497
Mean of dep. var.	16.58	16.62	16.57	10.32	10.33	10.31
Dependent variable: Temporary leave for care of children						
Private	–0.843***	–0.657***	–0.056	–0.502***	–0.140*	–0.646
	(0.058)	(0.094)	(0.305)	(0.077)	(0.083)	(0.398)
Observations	638,599	630,154	638,089	851,183	839,783	851,066
R^2	0.109	0.473	0.150	0.091	0.492	0.108
Mean of dep. var.	6.64	6.70	6.64	3.91	3.97	3.94
Individual fixed effects	No	Yes	No	No	Yes	No
Establishment fixed effects	No	No	Yes	No	No	Yes

Notes: Each cell represents a separate regression. All regressions control for time effects, age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), dummies for number of children, age of youngest child (3 categories), size of establishment, tax rate, political majority, number of establishments within industry, and share of children in private preschools/primary schools. All columns except 2 and 6 include dummies for gender and immigrant status. The regressions in columns 1, 2, 5, and 6 include municipal dummies. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

The results for sickness absence in the preschool sector are in line with the OLS estimates, but of somewhat larger magnitude. They suggest that hive-offs are associated with a 27 percent decrease in both sickness absence and days of sickness absence in relation to the sample mean. The larger magnitude could be due to the sample used in these estimations, which includes only employees with tenures of more than one year. This issue is further investigated in Table 5 in section 6.2. For primary schools, the negative estimate of sickness absence turns positive and significant at the 10 percent significance level when using within-establishment variation in ownership, whereas the coefficient for duration of sickness absence remains negative but is not significant.

The estimate of parental leave, which is statistically significant at the 10 percent level, implies a reduction in days of parental leave of 6 percent following a change in ownership status. In the primary school sector, the negative relationship between private employment and incidence of sickness absence is positive when fixed establishment effects are taken into account, whereas the estimates of duration of sickness absence are insignificant. The estimate of parental leave also remains insignificant. As with the OLS results, private sector employment is associated with fewer days of temporary parental leave, but the coefficient is not statistically significant.

The estimates in columns 3, 4, 7, and 8 are based on variation from employees who remained at the establishment following privatization. One cannot exclude the possibility that only individuals with certain characteristics do so. To determine the effect for the treated but taking into account that the likelihood of remaining may differ along observable characteristics, it is motivated to employ an approach involving instrumental variables. The hiving-off of an establishment is used as an instrument for employment in the private sector. If the assumption that those who did not stay were unaffected by privatization is valid, 2SLS provides a consistent estimate of private sector employment for those who chose to stay in private sector employment following privatization.

6.2 Two-stage least squares estimates

Before proceeding to the results of the IV analysis, I present the first-stage estimates of the instrument for the probability of private employment. In addition, I estimate the effect of hive-offs on the probability of leaving the labor force and on the probability of leaving employment in

preschools or primary schools to work in another industry.[103] As columns 1 and 4 of Table 3 show, most of the employees in firms subject to hive-offs become privately employed.[104] This implies that the first-stage relationship is strong and the *F*-test statistic (presented together with the IV results) on the instrument also shows that a weak instrument is not a concern.

If hive-offs affected the choice of participating in the labor force, the assumption that the instrument only affects the outcomes through the ownership change might be violated. Columns 2, 3, 5, and 6 in Table 3 establish that the probability of leaving the industry or the labor force was not positively affected by the hive-off. However, there is a significant negative relationship between hive-offs and the probability of preschool employees leaving the industry. Also the relationship between hive-offs and the probability of leaving the labor force is negative and significant in both industries. The estimates are however small in comparison with the estimate for private employment, and the negative effect on leaving the preschool sector could also result from a better work environment.

Table 3. *The impact of hive-offs on private employment and the probability of leaving the labor force or the industry of employment.*

	Preschools			Primary schools		
	(1)	(2)	(3)	(4)	(5)	(6)
	Private	Left labor force	Left industry	Private	Left labor force	Left industry
Hive–off	0.775***	–0.001**	–0.071***	0.592***	–0.001**	–0.023
	(0.019)	(0.000)	(0.006)	(0.108)	(0.000)	(0.022)
Observations	1,435,866	1,283,751	1,296,290	2,551,423	2,070,203	2,270,465
R^2	0.070	0.042	0.447	0.106	0.451	0.029

Notes: Each cell represents a separate regression. All regressions control for time effects, age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), gender, immigrant status dummies for number of children, and age of youngest child (3 categories). The regressions in columns 3 and 6 also account for size of establishment, tax rate, political majority, number of establishments within industry, and share of children in private preschools/primary schools. All the regressions include municipal dummies. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

[103] The sample used in these regressions is naturally smaller than the main sample due to the analysis demands two observations for each individual: one to establish that they worked in the relevant industry and one to establish that they left the labor force/industry.
[104] Note that the sample used to estimate the first-stage regressions presented in Table 3 differs from the sample used to estimate effects on sickness absence (in which four years of data are omitted) and from the sample used to estimate effects on temporary parental leave (which contains only parents of children aged 12 or younger).

Table 4 presents the IV estimates of private sector employment, using hive-offs as an instrument for employment in the private sector. The table also includes the first-stage *F*-statistics, which indicate a strong relationship between the instrument and private sector employment, as the *F*-statistics are beyond the commonly suggested critical values (Staiger and Stock, 1997).

Columns 1 and 3 display the baseline IV estimates. The regressions in this part of the analysis do exclude workers in private firms not subject to hive-offs. Since workers in private firms might be systematically different for instance in terms of health status, the instrument might be correlated with the error term. Including them could hence violate the exogeneity assumption.

Starting with the preschool sector, the IV estimate in column 1 indicates a negative effect of private sector employment on both incidence and duration of sickness absence. The point estimate for sickness absence suggests that private employment is associated with a 6.6 percent lower probability of sickness absence, which corresponds to 27 percent of the sample mean. The reduction in days of sickness absence is approximately 10 days, which amounts to 42 percent of the sample mean. Hence, the various OLS specifications seem to underestimate the negative relationship between private employment and sickness absence in preschools.

The OLS estimates suggested a small negative effect of private employment on parental leave in preschools, when including establishment fixed effects. Using the IV approach yields an estimate that is closer to zero and not statistically significant. For temporary parental leave, the results are insignificant, in line with the estimates when establishment fixed effects are included.

The negative relationship found in the OLS analysis of sickness absence in primary schools turns positive, but small and statistically insignificant when the IV approach is applied. This is in line with the results controlling for time-invariant establishment characteristics in column 6 of Table 2, but contrary to the fixed-effects estimates, the IV estimates are not statistically significant. As in the first part of the analysis, no effect on parental leave or temporary parental leave can be found in primary schools.

Table 4. IV results, effects of private sector employment on sickness absence, parental leave, and temporary leave for care of children.

	Preschools		Primary schools	
	IV	Reduced form	IV	Reduced form
	(1)	(2)	(3)	(4)
Dependent variable: Sickness absence				
Private	–0.066***	–0.051***	0.004	0.002
	(0.011)	(0.009)	(0.020)	(0.012)
Observations	1,015,192	1,015,192	1,796,654	1,796,654
R^2	0.040	0.040	0.043	0.043
Mean of dep. var.	0.245	0.245	0.184	0.184
1st-stage *F*-statistics	1512.49		26.69	
Dependent variable: Days of sickness absence				
Private	–10.39***	–7.795***	–1.980	–1.168
	(1.804)	(1.445)	(4.453)	(2.671)
Observations	1,015,192	1,015,192	1,796,654	1,796,654
R^2	0.038	0.038	0.034	0.034
Mean of dep. var.	23.28	23.28	19.85	19.85
1st-stage *F*-statistics	1512.49		26.69	
Dependent variable: Net days of parental leave				
Private	–0.518	–0.401	–0.592	–0.350
	(0.431)	(0.333)	(0.888)	(0.514)
Observations	1,435,866	1,435,866	2,551,423	2,551,423
R^2	0.556	0.556	0.494	0.494
Mean of dep. var.	15.54	15.54	9.941	9.941
1st-stage *F*-statistics	1602.92		30.21	
Dependent variable: Temporary leave for care of children				
Private	–0.140	–0.107	–0.079	–0.057
	(0.357)	(0.274)	(0.522)	(0.373)
Observations	555,902	555,902	806,460	806,460
R^2	0.111	0.111	0.093	0.093
Mean of dep. var.	6.704	6.704	3.943	3.943
1st-stage *F*-statistics	969.69		65.46	

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), dummies for number of children, age of youngest child (3 categories), size of establishment, tax rate, political majority, number of establishments within industry, and share of children in private preschools/primary schools. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

In comparison with the 2SLS results, columns 2 and 4 display the results of the reduced-form regression. As Table A2 in the Appendix suggests, not all individuals complied when hive-offs took place, and these estimates represent the intention-to-treat effect on individuals working in public establishments subject to hive-offs. As previously shown, leavers differ significantly in characteristics from those who stayed in the firms following hive-offs. Assuming that their behavior would have continued in the same manner with a private employer as in their present occupation, it could be informative to estimate the reduced-form effect for all

employees affected by the hive-off. This approach is only useful given that the non-stayers did not leave the industry of employment or the labor force altogether following a hive-off. As Table 3 shows, the estimated impact on leaving the labor force and the industry (preschools or elementary schools) following a hive-off was very small, suggesting that individuals leaving the establishment continued to work in the public sector.

The reduced-form estimates correspond well to the 2SLS results, although the magnitude is somewhat lower. This is expected, as some of the individuals included in the treatment group did not switch to a private employer following hive-off. Instead, this estimate represents an intention-to-treat effect. However, it is not possible to know how these individuals would have responded to a hive-off. Assuming that there are two types of individuals, where the first type is less absent with a private employer and the second type is more absent with a private employer and also more likely to leave when the firm is hived off. Then, the reduced-form estimate would not provide any good indication of the direction of the average treatment effect. Since the vast majority of the workers stay at the establishment following hive-offs, a large positive effect on absence would be required among the second type individuals to net out the effect on sickness absence in preschools. Nevertheless, since it is difficult to exclude this possibility completely, it should be kept in mind that the found effects may only be valid for the group of compliers.

To summarize, the OLS analysis suggests a negative relationship between private sector employment and sickness absence and temporary parental leave in both preschools and primary schools. Using variation in private sector employment attributable to hive-offs of public sector units indicates no significant causal effect of private sector employment on sickness absence, parental leave, or temporary parental leave among primary school employees. In preschools, the sickness absence behavior among the employees seems to have changed as a result of a firm switching from public to private ownership. Neither the results on parental leave or temporary parental leave in preschools are not statistically significant in the IV analysis. The result on temporary parental leave is in line with the OLS analysis, but whereas the results from the OLS model with establishment fixed effects suggested a small negative relationship between private employment and parental leave in preschools, the IV results cannot exclude a zero effect.

It is somewhat surprising that the point estimates of sickness absence in preschools from the IV analysis are of larger magnitude than those from the OLS analysis. If there is sorting of individuals with lower risk of sickness absence to private firms, the OLS analysis would overestimate rather than underestimate the relationship between private sector employment and sickness absence. One possibility is that this pattern reflects

differences in tenure across the treatment groups in the OLS and IV analysis. When using hive-offs as an instrument, only individuals who worked for at least two years become part of the treatment group, whereas the control group, also consist of workers employed for less than two years. The difference could hence reflect that tenure is longer for the sample of workers defined as subject to hive-offs. To account for the discrepancy, I rerun the OLS regressions from columns 1 and 4 of Table 3 and the IV regressions, using only employees who worked at an establishment for at least two years.[105] Table 5 presents the results.

[105] Including a three-year window yields approximately the same results. These are omitted from the table.

Table 5. *Effects of private sector employment on sickness absence, parental leave, and temporary leave for care of children, tenure >1 year.*

	Preschools		Primary schools	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
Dependent variable: Sickness absence				
Private	–0.095***	–0.073***	–0.012	0.0002
	(0.009)	(0.011)	(0.015)	(0.021)
Observations	903,118	903,118	1,634,387	1,634,387
	0.037	0.037	0.042	0.042
Mean of dep. var.	0.254	0.254	0.189	0.189
1st-stage *F*-statistics	-	1464.48	-	75.99
Dependent variable: Days of sickness absence				
Private	–11.66***	–11.38***	–1.906	–2.507
	(1.526)	(1.860)	(3.322)	(4.552)
Observations	903,118	903,118	1,634,387	1,634,387
	0.035	0.035	0.033	0.033
Mean of dep. var.	24.95	24.95	20.91	20.91
1st-stage *F*-statistics	-	1464.48	-	75.99
Dependent variable: Net days of parental leave				
Private	–0.371	–0.759*	–0.540	–0.505
	(0.361)	(0.424)	(0.730)	(0.890)
Observations	1,289,828	1,289,828	2,338,133	2,338,133
	0.562	0.562	0.500	0.500
Mean of dep. var.	15.85	15.85	10.18	10.18
1st-stage *F*-statistics	-	1473.37	-	72.09
Dependent variable: Temporary leave for care of children				
Private	–0.216	–0.201	–0.130	–0.160
	(0.332)	(0.361)	(0.405)	(0.516)
Observations	508,377	508,377	744,907	744,907
	0.113	0.113	0.096	0.096
Mean of dep. var.	6.781	6.781	3.979	3.979
1st-stage *F*-statistics	-	1053.47	-	110.96

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), dummies for number of children, age of youngest child (three categories), size of establishment, tax rate, political majority, number of establishments within industry, and in private preschools/primary schools. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Imposing the tenure restriction leads to convergence of the OLS and 2SLS estimates for both sickness absence outcomes in preschools. It seems that, on average, longer tenure among switchers caused an upward bias of the 2SLS estimates. The corresponding OLS estimates using this sample are also slightly higher than the one using the full sample, suggesting that part of the estimated relationship between private employment and sickness absence in preschools picked up differences in tenure, affecting the rate and duration of sickness absence. The OLS estimates of sickness absence in primary schools also rise when the difference in tenure across sectors is taken into account. When limiting the sample to em-

ployees with tenure of at least two years also yields a negative effect on parental leave in preschools which is statistically significant on the 10 percent level of statistical significance. The estimate corresponds to 5 percent of the sample mean, which is not a negligible effect.

6.3 Robustness checks

Even though the results of the IV and the establishment fixed-effects analysis are coherent, a number of concerns can be raised concerning the IV approach. To test the validity of the results, I perform a number of robustness tests. The results are presented in Tables 6 and 7, where column 1 contains the baseline IV results for reference.

As previously discussed, a central assumption when using an IV approach is that the instrument only affects the outcome through the endogenous variable. If hive-offs themselves affect the outcome variables, the analysis may suffer from bias. For instance, stress from organizational change could affect worker health or insecurity about one's future in the new firm could affect the decision to have children. In column 2 of Tables 6 and 7, observations within a three-year window around the occurrence of privatization are excluded to account for changes in the outcomes related to the hive-off itself. The inclusion of the three-year window leads to a minor increase in the magnitude of the estimates of sickness absence in preschools compared with the baseline IV estimate in column 1, but does not otherwise alter the main results.

As Table 1 shows, sickness absence in hived-off preschools differs from the levels in preschools that remained publicly owned. It is reassuring that the results of the corresponding models incorporating establishment fixed effects yield results similar to the IV results. Nevertheless, I also estimate the IV model with controls for the lagged values of the outcome variable.[106] The estimates, presented in column 3, are somewhat smaller in magnitude than the baseline results. The magnitude of the effect on sickness absence in pre-schools is somewhat lower, i.e., 6.1 versus 6.6 percent, and for days of sickness absence, 9 instead of 10 days, but the effects are still statistically significant. In column 4, the two former specifications, with a three-year window and lags, are combined; also these results are comparable in size to the baseline results for preschools. However, the estimated effect on sickness absence in primary schools using this specification yields a positive effect on sickness absence in primary schools, which could be interpreted as a long run effect

[106] The lagged outcome variables consist of the average number of years with a sickness absence spell, or the average number of days of sickness absence, parental leave, and temporary leave for care of children in the years prior to the current year.

for employees in primary schools. The result is also consistent with the results from the OLS analysis including establishment fixed effects. Both estimates are however only significant on the 10 percent level of statistical significance.

Controlling for previous parental leave uptake yields a smaller and statistically insignificant effect for preschool workers, in comparison to the significant estimates from the fixed effects analysis and the IV analysis with a tenure restriction.

Another concern of the empirical approach is that the comparison group consisting of public employees is subject to an outflow of individuals to the private sector. If the workers self-select out of the public sector labor force and if the self-selection is based on unobservable characteristics, the estimated effect could be over- or underestimated.

To investigate this issue, I divide the sample into high- and low-competition municipalities. In municipalities with few private providers, selection out of the public labor force can be expected to be smaller than in municipalities with more competition, especially since teachers and preschool personnel belong to groups with low geographic mobility (Hedberg, 2005). The sample is divided into workers in high-competition municipalities, where 15 percent or more of the children are enrolled in private preschools or primary schools, and workers in low-competition municipalities with less than 15 percent of the children attending a private unit. For preschools, the results for sickness absence in columns 5 and 6 of Table 6 yield point estimates of approximately the same magnitude. The estimate for workers in low-competition municipalities is insignificant, but when including a three-year window in columns 7 and 8, the estimate for workers in low-competition municipalities is larger. The fact that the magnitude of the point estimate in low-competition municipalities is larger than the corresponding estimate in-high competition municipalities is reassuring.

Table 6. *Robustness checks, preschools.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Window	Lag	Lag+window	High	Low	High+window	Low+window
Dependent variable: Sickness absence								
Private	–0.066***	–0.080***	–0.061***	–0.072***	–0.071***	–0.070	–0.080***	–0.120***
	(0.011)	(0.013)	(0.010)	(0.011)	(0.011)	(0.043)	(0.013)	(0.043)
Observations	1,015,192	1,011,621	856,622	853,193	353,243	661,949	349,909	661,712
R^2	0.040	0.040	0.140	0.140	0.042	0.040	0.042	0.040
Mean	0.245	0.245	0.251	0.250	0.245	0.245	0.245	0.245
1st-stage *F*-statistics	1512.49	995.79	1435.9	947.03	1759.8	29.65	1203.47	16.46
Dependent variable: Days of sickness absence								
Private	–10.39***	–10.77***	–8.610***	–8.377***	–11.40***	–8.772	–11.46***	–13.43*
	(1.804)	(2.251)	(1.539)	(1.968)	(1.900)	(6.870)	(2.391)	(7.245)
Observations	1,015,192	1,011,621	856,622	853,193	353,243	661,949	349,909	661,712
R^2	0.038	0.038	0.160	0.160	0.035	0.040	0.035	0.040
Mean	23.28	23.27	24.88	24.88	23.56	23.13	23.54	23.12
1st-stage *F*-statistics	1512.49	995.79	1435.9	947.03	1115.79	958.511	605.519	1024.63
Dependent variable: Net days of parental leave								
Private	–0.518	–0.452	–0.269	–0.104	–0.450	–1.607	–0.462	–0.673
	(0.431)	(0.489)	(0.427)	(0.491)	(0.447)	(1.523)	(0.490)	(1.767)
Observations	1,435,866	1,431,281	1,269,085	1,264,628	491,640	944,226	487,497	943,784
R^2	0.556	0.556	0.561	0.561	0.551	0.559	0.550	0.559
Mean	15.54	15.54	14.90	14.90	14.44	16.11	14.43	16.11
1st-stage *F*-statistics	1602.92	1137.5	1546.42	1095.83	2011.74	29.84	1389.31	23.05
Dependent variable: Temporary leave for care of children								
Private	–0.140	–0.065	–0.325	–0.276	0.063	–1.379*	0.0457	–0.487
	(0.357)	(0.482)	(0.310)	(0.417)	(0.344)	(0.813)	(0.460)	(1.320)
Observations	555,902	554,146	487,400	485,691	181,831	374,093	180,251	373,917
R^2	0.111	0.111	0.206	0.206	0.111	0.112	0.111	0.112
Mean	6.704	6.701	6.735	6.733	6.961	6.579	6.955	6.580
1st-stage *F*-statistics	969.69	623.04	940.74	604.78	1175.93	27.51	774.15	19.53

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), dummies for number of children, age of youngest child (3 categories), tax rate, political majority, number of establishments within industry, and share of children in private pre-schools/primary schools. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 7. *Robustness checks, primary schools.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Window	Lag	Lag+window	High	Low	High+window	Low+window
Dependent variable: Sickness absence								
Private	0.004	0.048	0.015	0.063*	0.011	–0.0143	0.0422	0.0560
	(0.020)	(0.036)	(0.017)	(0.034)	(0.031)	(0.0406)	(0.0401)	(0.076)
Observations	1,796,654	1,795,672	1,599,407	1,598,443	96,170	1,700,484	95,887	1,699,785
R2	0.043	0.043	0.141	0.141	0.039	0.044	0.039	0.044
Mean	0.184	0.184	0.188	0.188	0.178	0.184	0.178	0.184
1st-stage F-statistics	26.69	19.53	26.73	19.54	43.95	12.45	27.33	13.11
Dependent variable: Days of sickness absence								
Private	–1.980	1.533	–0.219	5.494	–0.442	–4.750	2.181	3.156
	(4.453)	(6.537)	(3.893)	(6.210)	(4.046)	(8.138)	(5.511)	(14.321)
Observations	1,796,654	1,795,672	1,599,407	1,598,443	96,170	1,700,484	95,887	1,699,785
R2	0.034	0.034	0.162	0.162	0.027	0.035	0.027	0.035
Mean	19.85	19.85	20.91	20.91	19.59	19.87	19.60	19.87
1st-stage F-statistics	26.69	19.53	26.73	19.54	43.95	12.45	27.33	13.11
Dependent variable: Net days of parental leave								
Private	–0.592	0.311	–0.539	0.409	1.233	–2.664*	1.633	–1.590
	(0.888)	(1.243)	(0.900)	(1.283)	(1.232)	(1.597)	(1.507)	(1.745)
Observations	2,551,423	2,550,178	2,346,431	2,345,199	105,219	2,446,204	104,914	2,445,264
R2	0.494	0.494	0.500	0.500	0.512	0.494	0.512	0.494
Mean	9.941	9.940	9.989	9.988	11.22	9.886	11.21	9.885
1st-stage F-statistics	30.21	19.43	29.95	19.07	38.16	16.19	24.10	11.51
Dependent variable: Temporary leave for care of children								
Private	–0.079	0.060	–0.168	–0.053	–0.231	0.169	0.038	0.043
	(0.522)	(0.790)	(0.514)	(0.749)	(0.653)	(0.639)	(0.838)	(1.031)
Observations	806,460	806,081	740,599	740,224	31,532	774,946	31,424	774,675
R2	0.093	0.093	0.181	0.181	0.085	0.093	0.085	0.093
Mean	3.943	3.943	3.996	3.996	4.564	3.918	4.567	3.917
1st-stage F-statistics	65.46	36.83	65.49	36.29	168.22	29.09	93.86	18.00

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and school teacher in primary schools), dummies for number of children, age of youngest child (3 categories), tax rate, political majority, number of establishments within industry, and share of children in private pre-schools/primary schools. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

6.4 Heterogeneous effects

The difference in the effects on sickness absence between the preschool and primary school sectors found in the estimates shown above is somewhat puzzling. One possible explanation is the difference in prevalence of non-profit firms running preschools as contrasted with primary schools, as illustrated in Figure 2. The mechanisms governing employee absence behavior may differ between non-profit and for-profit firms. In particular, some of the non-profit firms acting in these markets are cooperatives owned by their personnel. It is possible that the behavior of firms with an organizational structure in which workers are in control differs from that of firms with an outside owner in a way that affects absence behavior. For instance, personnel cooperatives might give a good work environment a high priority. The first two columns of Tables 8 and 9 explore this issue. In column 1, the sample is limited to workers in the public sector and for-profit firms, and column 2 reports the results when employees in non-profit firms are compared with public sector employees. As the first panel show, the effect of hive-offs on the incidence of sickness absence is driven by for-profit preschools. However, the effect on the length of sickness absence is similar when a non-profit firm takes over a preschool and when the new owner is a for-profit firm. Hence, the effect on sickness absence in preschools seems not to be entirely driven by non-profit ownership. In primary schools, no effect can be found in either for-profit or non-profit firms. In fact, the instrument does not meet the validity requirement when limiting the sample to individuals working in either public or non-profit schools, which could be due to the small number of hive-offs to non-profit firms. An alternative explanation for the difference in the effect of private employment on sickness absence between preschools and primary schools is that the initial work environment in public preschools is worse than in public primary schools, creating more potential for improvements. Interestingly, the estimates on parental leave and temporary parental leave are negative and statistically significant for non-profit preschools. This finding does not support the argument of an incentive effect of private employment, since it is expected to be greater in for-profit firms.[107]

I also examine whether the results differ for employees with a university education in their profession and those without. For preschools, the sample is divided into individuals with a university degree in preschool

[107] I have also estimated heterogeneous effects including the lagged variables and a 3-year window. In these regressions, the significant results on parental leave and temporary parental leave in non-profit preschools turn statistically insignificant. Generally, the results are similar to the baseline results with the exception of a imprecisely estimated but negative impact on temporary parental leave for male workers. The results from these regressions are available upon request.

education and individuals without this type of education. The results shown in columns 3 and 4 of Table 8 indicate no large differences between preschool personnel with a university degree and those without a degree. The analysis of educated teachers and personnel without a teaching degree in primary schools yields no significant results. These results are shown in columns 3 and 4 of Table 9.

Since the patterns of sickness absence and parental leave differ between genders, it is interesting to see whether there are any differential effects between male and female employees. The results in columns 5 and 6 suggest that private employment has a larger effect on sickness absence among female than male preschool employees. The probability of sickness absence is 6.8 percentage points lower for privately employed women, representing 27 percent of the sample mean. For men, no statistically significant effect on sickness absence is found. The point estimate is negative, but the standard errors large, most likely due to the small number of men working in preschools.

Since women take the greater share of parental leave in Sweden, it has been suggested that parental leave is an anticipated outcome and therefore not sanctioned in terms of drops in subsequent wages for women (Albrecht et al., 1999). The same study found that the wage loss is higher for men than for women who take parental leave. If avoiding absence is regarded as more important when employers are private firms, a negative effect of private sector employment on men's uptake of parental leave is expected. The direction of the point estimates for male and female preschool workers are in line with this hypothesis in primary schools but not in preschools. None of the estimated effects for are statistically significant though.

To find out whether the effects vary between employees depending on marital status, the regressions are run separately for single and married (or cohabiting with common children) workers.[108] This is particularly interesting for the outcome temporary parental leave, since those who have a partner may have greater opportunities to shift leave obligations to the other parent. However, the results in columns 7 and 8 of Tables 8 and 9 do not support this hypothesis.

[108] It is only possible to distinguish cohabiting couples in data if they have common children. Individuals without common children are categorized as singles.

Table 8. IV results, effects of private sector employment on sickness absence, parental leave, and temporary leave for care of children in preschools, heterogeneous effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	For-profit	Non-profit	Preschool teacher education	No preschool teacher education	Female	Male	Partner	Single
Dependent variable: Sickness absence								
Private	–0.057***	0.007	–0.066***	–0.067***	–0.068***	–0.013	–0.062***	–0.080***
	(0.013)	(0.042)	(0.016)	(0.014)	(0.011)	(0.043)	(0.013)	(0.018)
Observations	1,013,508	1,010,690	378,246	636,946	961,035	54,157	664,311	350,881
R^2	0.040	0.040	0.035	0.043	0.038	0.079	0.031	0.071
Mean	0.245	0.245	0.224	0.257	0.249	0.165	0.252	0.232
1st-stage *F*-statistics	150.14	24.17	132.17	187.81	181.31	46.45	170.16	120.96
Dependent variable: Days of sickness absence								
Private	–10.60***	–9.591*	–10.42***	–10.04***	–10.65***	–6.475	–9.719***	–12.53***
	(2.219)	(5.057)	(2.829)	(2.264)	(1.857)	(6.016)	(2.119)	(3.133)
Observations	1,013,508	1,010,690	378,246	636,946	961,035	54,157	664,311	350,881
R^2	0.038	0.038	0.038	0.039	0.037	0.042	0.032	0.053
Mean	23.29	23.29	22.97	23.46	23.94	11.56	24.03	21.86
1st-stage *F*-statistics	150.14	24.17	132.17	187.81	181.31	46.45	170.16	120.96
Dependent variable: Net days of parental leave								
Private	–0.529	–2.037*	–0.603	–0.320	–0.476	0.708	–0.543	–0.271
	(0.530)	(1.158)	(0.715)	(0.526)	(0.431)	(1.165)	(0.607)	(0.330)
Observations	1,433,460	1,429,366	537,271	898,595	1,360,820	75,046	949,901	485,965
R^2	0.557	0.556	0.568	0.548	0.576	0.230	0.554	0.455
Mean	15.54	15.56	17.98	14.08	16.17	4.133	21.99	2.926
1st-stage *F*-statistics	154.60	26.63	147.54	196.47	194.45	47.42	186.38	129.22
Dependent variable: Temporary leave for care of children								
Private	0.278	–1.299*	–0.336	0.024	-0.146	0.263	–0.040	–0.615
	(0.406)	(0.762)	(0.639)	(0.384)	(0.367)	(0.925)	(0.390)	(0.675)
Observations	554,965	553,712	242,089	313,835	530,819	25,105	471,898	84,026
R^2	0.111	0.112	0.117	0.112	0.109	0.103	0.112	0.119
Mean	6.706	6.701	6.986	6.487	6.852	3.569	6.645	7.034
1st-stage *F*-statistics	108.95	15.74	62.00	122.08	100.41	70.16	92.86	52.30

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and teacher in primary schools), dummies for number of children, age of youngest child (3 categories), tax rate, political majority, number of establishments within industry, and share of children in private preschools/primary schools. Standard errors robust for clustering at the establishment are level shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 9. IV results, effects of private sector employment on sickness absence, parental leave, and temporary leave for care of children in primary schools, heterogeneous effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	For-profit	Non-profit	Teacher education	No teacher education	Female	Male	Partner	Single
Dependent variable: Sickness absence								
Private	0.008	0.074	0.000	0.004	–0.006	0.040	–0.008	0.018
	(0.025)	(0.11)	(0.032)	(0.031)	(0.024)	(0.042)	(0.026)	(0.037)
Observations	1,785,279	1,784,865	845,538	951,116	1,387,598	409,056	1,190,093	606,561
R^2	0.043	0.043	0.035	0.040	0.038	0.039	0.037	0.066
Mean	0.184	0.184	0.148	0.215	0.202	0.120	0.186	0.178
1st-stage *F*-statistics	16.42	6.34	33.93	33.66	35.61	39.97	37.75	29.80
Dependent variable: Days of sickness absence								
Private	–4.713	10.50	–6.163	1.956	–3.290	2.532	0.803	–7.139
	(5.056)	(22.62)	(5.253)	(6.925)	(5.272)	(7.292)	(5.237)	(6.003)
Observations	1,785,279	1,784,865	845,538	951,116	1,387,598	409,056	1,190,093	606,561
R^2	0.034	0.034	0.038	0.033	0.032	0.033	0.029	0.048
Mean	19.86	19.86	18.26	21.27	22.03	12.48	20.21	19.15
1st-stage *F*-statistics	16.42	6.34	33.93	33.66	35.61	39.97	37.75	29.80
Dependent variable: Net days of parental leave								
Private	–0.745	–4.674	–1.444	0.302	–0.362	–0.796	–1.269	1.511
	(0.942)	(3.381)	(1.053)	(1.347)	(0.972)	(1.263)	(1.546)	(1.206)
Observations	2,535,372	2,534,766	1,187,965	1,363,458	1,974,716	576,707	1,694,889	856,534
R^2	0.494	0.494	0.515	0.477	0.583	0.221	0.497	0.368
Mean	9.943	9.943	10.53	9.429	11.86	3.372	14.16	1.599
1st-stage *F*-statistics	15.55	5.70	31.97	29.21	32.02	36.20	33.97	28.18
Dependent variable: Temporary leave for care of children								
Private	0.085	0.384	–0.686	0.278	–0.346	1.074	–0.108	–0.187
	(0.620)	(1.737)	(0.764)	(0.932)	(0.573)	(0.833)	(0.601)	(1.357)
Observations	815,889	814,137	371,561	434,917	623,007	183,471	687,564	118,914
R^2	0.093	0.092	0.068	0.095	0.090	0.067	0.091	0.105
Mean	3.940	3.937	3.056	4.700	4.351	2.556	3.886	4.272
1st-stage *F*-statistics	18.23	8.70	23.97	50.78	41.26	23.84	45.92	14.54

Notes: Each cell represents a separate regression. All regressions control for time effects, municipality of employment, gender, immigrant status age, age^2, marital status, education dummies (7 categories), education within profession (i.e., preschool teacher and childcare nurse in preschools and school teacher in primary schools), dummies for number of children, age of youngest child (3 categories), tax rate, political majority, number of establishments within industry, and share of children in private preschools/primary schools. Standard errors robust for clustering at the establishment level are shown in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively

7 Concluding remarks

This paper investigates the effect of private employment in welfare services on sickness absence, parental leave, and temporary parental leave for care of children within preschool and primary education. The analysis shows a significant negative impact of private employment on sickness absence in preschools but no corresponding effect in primary schools. For preschool employees who stayed at the establishment following a hive-off, private employment is on average associated with an approximately 6.6 percentage point's lower risk of sickness absence and about 10 days shorter sickness absence spells. No corresponding effect was found in primary schools. In fact, some of the results indicate a positive effect on sickness absence in primary schools, but the results are imprecisely estimated. In neither industry, private employment seems to be associated with temporary parental leave for care of children, but in preschools some of the results suggest a small negative impact on parental leave. Also these results are imprecisely estimated. The results for different groups suggested that the impact on parental leave was largest for employees in non-profit pre-schools, but the result is not robust in the long run and when accounting for the length of previous spells.

Two explanations have been proposed for the potential difference in absence behavior across sectors. First, the property-rights approach proposes that the incentives to attain optimal performance are stronger in private sector than public sector employment. Due to differences in rewards, private employees would be less prone to absence since a private employer is more concerned with optimal performance. Second, differences in work conditions could affect employee absence, particularly sickness absence. A compelling argument for the finding of significant effects on sickness absence but not on parental or temporary leave is that the incentive effect is marginal or nonexistent, whereas work conditions differ between sectors. However, the results are not coherent across the analyzed industries. A potential explanation for this finding is that there is greater scope for improvements in the work environment in preschools, where the sickness absence rate is higher than in primary schools.

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Appendix A: Descriptive statistics

Table A1. *Descriptive statistics, 1994–2007.*

	Preschools			Primary schools		
Outcome variables:	Public	Private	Hived-off	Public	Private	Hived-off
Sickness absence (dummy)	0.25	0.19	0.22	0.18	0.16	0.17
Days of sickness absence	23.29	15.25	20.99	19.85	14.77	18.79
Paid days of parental leave	16.02	20.40	14.95	10.13	14.37	10.01
Days of leave for care of children*	9.35	9.32	20.42	3.94	3.84	3.89
Background characteristics:						
Age	40.89	36.39	42.66	43.95	39.10	44.51
Immigrant	0.10	0.11	0.11	0.07	0.19	0.13
Married/cohabiting	0.66	0.58	0.66	0.66	0.57	0.65
Female	0.95	0.93	0.97	0.77	0.73	0.82
Primary school<9	0.03	0.01	0.01	0.04	0.01	0.03
Compulsory school=9	0.05	0.07	0.04	0.05	0.07	0.05
Upper secondary school <3	0.34	0.28	0.38	0.15	0.13	0.13
Upper secondary school =3	0.10	0.19	0.08	0.08	0.14	0.04
University undergraduate<3	0.37	0.31	0.42	0.20	0.20	0.21
University undergraduate=>3	0.10	0.13	0.07	0.49	0.45	0.53
University graduate	0.00	0.00	0.00	0.00	0.00	0.00
Childcare education (secondary school level)	0.37	0.31	0.43			
Preschool teacher education	0.24	0.20	0.26			
School teacher education				0.47	0.36	0.51
Income from work	1515.84	1461.94	1729.12	1938.60	1837.97	2197.87
Children=1	0.20	0.21	0.20	0.18	0.20	0.19
Children=2	0.24	0.24	0.23	0.19	0.22	0.20
Children=3	0.08	0.08	0.07	0.07	0.08	0.06
Children=4	0.01	0.01	0.01	0.01	0.02	0.01
Children=5	0.00	0.00	0.00	0.00	0.00	0.00
Children=6	0.00	0.00	0.00	0.00	0.00	0.00
Children>6	0.00	0.00	0.00	0.00	0.00	0.00
Youngest child= 0–2	0.12	0.17	0.12	0.10	0.15	0.10
Youngest child= 3–6	0.13	0.13	0.13	0.09	0.11	0.09
Youngest child= 7–12	0.16	0.14	0.16	0.14	0.15	0.13
Size of establishment (# employees)	30.83	11.62	16.69	64.61	40.98	46.27

Table A1 continued.

Municipal characteristics:						
Right-wing majority	0.23	0.35	0.60	0.27	0.34	0.58
Tax rate	31.43	30.78	29.54	31.46	30.87	29.80
Share of children in private preschools/primary schools	0.13	0.25	0.36	4.66	10.35	10.26
Number of pre-schools/primary schools	135.67	214.44	242.59	45.67	99.91	71.37
Observations:	1429432	207098	17067	2550348	118439	3812

* The descriptive statistics are only for individuals with children aged 12 or younger.

Table A2. *Pre-privatization characteristics of individuals in firms subject to hive-off, 1994–2006.*

	Preschools		Primary schools	
	Did not stay	Did stay	Did not stay	Did stay
Outcome variables:				
Sickness absence	0.30	0.18	0.17	0.12
Days of sickness absence	35.59	13.95	20.90	11.70
Paid days of parental leave	23.71	17.15	8.36	10.68
Days of leave for care of children	8.47	8.69	3.07	3.72
Background characteristics:				
Age	40.20	40.79	44.19	42.48
Immigrant	0.16	0.10	0.08	0.14
Married/cohabiting	0.63	0.66	0.64	0.66
Female	0.97	0.98	0.86	0.80
Childcare education (secondary school level)	0.42	0.43		
Preschool teacher education	0.26	0.29		
School teacher education			0.55	0.50
Income from work	1394.84	1655.14	2019.03	2060.98
No of children in the household	0.98	0.99	0.73	0.91
Youngest child 0–2	0.18	0.13	0.07	0.11
Youngest child 3–6	0.14	0.15	0.07	0.11
Youngest child 7–12	0.13	0.15	0.12	0.16
Municipal characteristics:				
Size of establishment (# employ-ees)	19.47	15.666	31.20	43.06
Right-wing majority	0.46	0.62	0.64	0.53
Tax rate	29.56	28.92	29.98	29.38
Share of children in private preschools/primary schools	0.24	0.27	4.88	7.17
Number of preschools/primary schools	267.62	248.09	32.76	73.42
Observations	1472	6795	790	1250