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*Childcare - A safety net for children?*

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## CHILDCARE - A SAFETY NET FOR CHILDREN?

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# Childcare - A safety net for children? <sup>1</sup>

by

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## Abstract

We analyze how access to childcare affects health outcomes of children with unemployed parents using a reform that increased childcare access in some Swedish municipalities. For 4–5 year olds, we find an immediate increase in infection-related hospitalization, when these children first get access to childcare. We find no effect on younger children. When children are 10–11 years of age, children who did not have access to childcare when parents were unemployed are more likely to take medication for respiratory conditions. Taken together, our results thus suggest that access to childcare exposes children to risks for infections, but that need for medication in school age is lower for children who had access.

Keywords: Childcare; Child health; Unemployment; Quasi-experiment

JEL-codes: I14, J13

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## 1 Introduction

Children of unemployed parents have worse health than children whose parents are working.<sup>2</sup> Studying Swedish children, Mörk et al. (2014b) find that children with at least one unemployed parent are 17 percent more likely to be hospitalized in a year than children whose parents are employed. Since poor childhood health has been shown to have persistent effects on child development and adult outcomes, understanding how to improve the health outcomes of disadvantaged children is highly relevant.<sup>3</sup> In this paper, we ask whether access to high quality childcare at age 2–5 affects health outcomes among children with unemployed parents. We study the immediate effects on physical health as well as the effects at age 10–11 on physical and mental health. To this end, we use rich register data on hospitalizations and drug prescriptions and exploit a Swedish reform that improved access to formal childcare for children with unemployed parents. Before the reform, municipalities varied with respect to whether they offered childcare to children with unemployed parents. After the reform, offering childcare to those children became mandatory. Comparing the change in health of children of unemployed parents residing in municipalities that had to change policy with the corresponding change for children of unemployed parents living in municipalities that already before the reform offered childcare to these children, we estimate the causal effect of having access to childcare in a difference-in-differences framework.

There is vast evidence that childcare improves cognitive outcomes especially among disadvantaged children.<sup>4</sup> Less is known about the causal effects of childcare on children's health outcomes. There is however a large literature studying the associations between attending childcare and short run health outcomes such as the prevalence of respiratory infections, diarrheal illness and the use of antibiotics. These studies typically

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<sup>2</sup> This has been shown using U.S. data for birth weight (Lindo, 2011) and parental reported health and mental health (Schaller and Zerpa, 2015), in Scandinavia for hospitalization (Mörk et al., 2014b; Christoffersen, 2000), and physiological problems (Sund et al., 2003; Katliala-Heino et al., 2001; Christoffersen, 1994), in Slovakia for self-rated health and long-term well-being (Sleskova et al. 2006) and in Holland for behavioral problems (Harland et al., 2002).

<sup>3</sup> In her survey, Currie (2009) present evidence that low birth weight has been found to reduce test scores, the likelihood of high school graduation and earnings, and that individuals with better self-rated health during childhood have higher incomes as adults. Mörk et al. (2014a) find similar results for Sweden.

<sup>4</sup> Most of these studies find that access to high quality childcare improves cognitive outcomes for disadvantaged children (e.g. Felfe and Lalive, 2018; Fitzpartrick, 2008; Gathman and Sass, 2018; Felfe et al., 2015; Drange and Havnes, 2018; Cornelissen et al. forthcoming). The quality of the home environment, and thus the alternative mode of care, as well as child age when attending childcare, seem to matter for the effects of childcare on cognitive development (see Cascio, 2015 for an overview).

find that attending childcare is associated with a temporary higher prevalence of diseases and antibiotics prescriptions), followed by a period with a reduced likelihood of illness, and no changes in the long run (see, e.g., Lu et al., 2004; de Hoog et al., 2014; Ball et al., 2002). These studies point to an acquired immunity, in line with the so-called hygiene-hypothesis suggested by Strachan (1989), but that the immunity effect seems to fade out over time.

Papers aiming at estimating causal effects of childcare using experimental and quasi-experimental methods are less common. The earlier evidence of immediate effects of attending childcare on physical illnesses such as infections and colds is supported by findings in Baker et al. (2008) and van den Berg and Siflinger (2018). Baker et al. (2008) find negative effects of childcare on a number of child health related outcomes (reported by parents), such as whether the child is in excellent health or experienced throat or ear infections, when universal childcare was introduced in Quebec. The negative effects seem to persist later in life (Baker et al. 2015).<sup>5</sup> In a study of Southern Sweden using register data, van den Berg and Siflinger (2018) find that cohorts with longer exposure to a regime of low childcare fees, and potentially higher childcare enrollment, tends to have more infections at a younger age but fewer infections at ages 6–7.<sup>6</sup> They term this a *substitution effect*. Liu and Skans (2010) instead, do not find any effects on hospitalizations of 1–16 year olds of a parental leave reform which likely lead families to substitute formal childcare for parental care during the child’s second year of life.<sup>7</sup>

Earlier evidence on mental health is more mixed. After the reform in Quebec, parents reported that their children showed more aggression, and had worse motor and social skills, once being enrolled in childcare and in the long-run there is evidence of increased criminal activity for boys (Baker et al., 2008; 2015). In contrast, the study by van den Berg and Siflinger (2018) finds that cohorts with potentially higher childcare enrollment were less likely to experience behavioral problems, such as developmental and behavioral disorders. Similarly, Yamaguchi et al. (2017), in a study on Japanese data, find that childcare reduced inattention and hyperactive behavior among children aged

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<sup>5</sup> In particular, Baker et al. (2015) find negative effects on self-reported health and life satisfaction also at ages 12–20. They also find lasting negative effects on non-cognitive skills and as well as higher rates of youth crime, especially for boys.

<sup>6</sup> The analyzed fee reductions were the result of a reform in 2001 that harmonized the fees across municipalities and substantially lowered them.

<sup>7</sup> Parental leave was extended from 12 to 15 months in 1989.

2.5 of low-educated mothers. There is also evidence that enrollment age may matter for the health effects of attending childcare. Kottenlenberg and Lehrer (2014) study the Quebec reform and show that the negative effects of childcare are mostly driven by children who started childcare at early ages.<sup>8</sup> For children aged three there are instead benefits, in terms of better development scores, of attending childcare.

We contribute to the existing literature in several ways. First, we focus on the effects of childcare access on children of unemployed parents. This group of children is arguably a group of particular policy relevance, because of their vulnerability. Second, compared to much of the earlier literature on childcare, we focus on child health and also provide estimates relating to childcare exposure at toddler and preschool age. In addition, we are able to follow the children and explore effects at 10–11 years of age. Third, our study has some data related and methodological advantages. We use register data on in-patient care and prescription drugs to measure child health outcomes. These are arguably more objective than the parent reported outcomes used in the Canadian studies. In particular, there is a risk that the way parents evaluate and report their children's health status may be affected by the fact that children are in childcare. An advantage, in relation to the study by van den Berg and Siflinger (2018), is that we are able to control for health trends since we rely on regional variation in reform exposure to identify causal effects.<sup>9</sup>

As mentioned above, we rely on register data to measure health outcomes. More specifically, we use data from the National Patient Register, which contain information of all hospital stays in Sweden, including detailed information about diagnoses. Incidences that lead to hospital stays are of course rather serious and we will not be able to pick up less severe health problems with these data.<sup>10</sup> As a complement, we therefore also analyze prescriptions for medical drugs. Unfortunately, prescription drug data are

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<sup>8</sup> Kottenlenberg and Lehrer (2013) also show that the findings in Baker et al. (2008) are robust to the inclusion of additional years of data, implying that the negative effects originally found are not due to initial implementation problems.

<sup>9</sup> van den Berg and Siflinger use the fact that different cohorts were exposed to lower childcare fees for different number of years, depending on their age when the maximum fee reform was introduced. Hence, even though they control for time trends in a flexible way, they are unable to control for cohort-specific time shocks. In a sensitivity analysis, they investigate whether the effects on health are heterogeneous with respect to the reform-induced reduction in childcare fees, which differed between municipalities, but do not find any statistically significant heterogeneity.

<sup>10</sup> On the other hand, one might argue that it is the more severe health problems that are likely to result in negative long run outcomes and therefore are mostly interesting.

only available from 2005, which implies that we will not be able to use these outcomes in the short run analysis.

Since Swedish registers do not include any information on childcare attendance we do not know which children attend childcare. Hence, our estimates should be interpreted as intention-to-treat effects.<sup>11</sup> Using survey data, we show that enrollment increased substantially, by 20-percentage points, among children with unemployed parents in treated municipalities compared to enrollment in control municipalities, implying the existence of a first stage.

Our results show that access to childcare did not affect hospitalization rates for children aged 2–3, for any of the diagnoses that we investigate. This result is in line with Liu and Skans (2010) who find no effect on hospitalization of the increase in parental care during children’s second year of life. For preschool children, 4–5 years old, we find that access to childcare caused an increase in hospitalization for infections the first year after the reform. This result supports findings in a number of correlation studies of a temporary increase in the risk of infections when children first enroll in childcare.

In the medium-run, we find no evidence of an effect of earlier childcare access on hospitalizations at age 10–11. Neither do we find effects on prescriptions of antibiotics. As for ADHD-medication and psycholeptics (prescribed to treat anxiety or sleeping problems), estimates point to that gaining access to childcare may have increased mental health problems, but standard errors are large and we cannot rule out zero or positive effects. Prescriptions for respiratory conditions at age 10-11, however, declined by some five percent for children who had access to childcare when parents were unemployed. This result supports either the hygiene-hypothesis or the presence of a substitution effect as found in van den Berg and Siflinger.

As earlier evidence shows that family characteristics, such as the education level of parents, matter for the impact of childcare on cognitive skills, we study whether there are heterogeneous effects with respect to the education level of the mother. We find that the immediate increase in infections among preschool children is entirely driven by children whose mothers have only compulsory education. Because we find that the parents with a low education were equally likely to have been employed when the child

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<sup>11</sup> Lack of information on who is actually treated by the reform may introduce measurement errors in our treatment variable. Estimates may hence suffer from attenuation bias.



was younger, we can rule out that the increase in hospitalization rates is a result of these children's lesser exposure to childcare at an early age. A potential explanation could instead be that being exposed to the childcare environment has larger effects on children with unemployed parents because the parents do, to lesser extent, provide appropriate preventive and primary care.

The rest of the paper is organized as follows. In the next section, we discuss potential effects on child health in general and on hospitalization in particular, when a child is cared for at home by an unemployed parent instead of attending center-based childcare. Thereafter, in section 3, we describe the institutional setting as well as the reforms that allow us to identify the causal effects. Section 4 presents the quasi-experimental strategy and section 5 our data. We then turn to the results in section 6. Finally, section 7 concludes.

## **2 How can the mode of care be expected to affect child health?**

This paper focuses on the short and medium-run health consequences for children with unemployed parents who are either cared for at home by their unemployed parent or attending center-based childcare. Is a child more likely to suffer injuries at home or at the childcare center? Does the mode of care matter for whether respiratory conditions lead to hospitalizations and need for medication later in life? How is the likelihood of catching serious infections affected by attending center based childcare? To what extent does having access to childcare affect behavioral problems and psychological well-being in school ages? In this section, we discuss why being at home with an unemployed parent or attending center-based childcare might have different health consequences for a child.

In childcare the child is attended by professional staff, trained in early childhood education and development, in a facility especially designed for children. This may increase the likelihood of early detection of health problems, and hence exposure to preventive health measures, reducing the need for hospitalization. Furthermore, this may also reduce the risk of injuries and poisoning and stimulate the child's psychosocial development. However, a group of children is also a fertile environment for spreading child related viruses and infections (Lu et al., 2004; de Hoog et al., 2014; Ball et al., 2002). While serious illnesses may have negative long run effects, it has been

argued that contacting minor infections early in life can build a child's immune system and lead to fewer infections later, the so called *Hygiene-hypothesis* (Strachan, 1989). Almond and Currie (2011) however also discuss the possibility that poor health in early childhood can make the child more sensitive later on. Results from observational studies tend to show that entering childcare only gives rise to a timing effect on *when* the child get infections and respiratory conditions.<sup>12</sup> This effect is what van den Berg and Stifflinger (2018) call a substitution effect. Being in a large group of children might also be stressful for sensitive children, and may thus lead to more anxiety and aggression; a hypothesis that is supported by empirical evidence in e.g. Baker et al. (2008).<sup>13</sup>

Compared to a facility designed for the care of children, the home environment of most children is full of potentially dangerous products, such as detergents and kitchen knives. Yet, parents are experts on their own children, and can focus on the individual child to a larger extent than childcare personnel who have many children to attend and care for. Being away from parental care for many hours per day might also be detrimental for children's attachment to their parents, and thereby their psychological well-being later on (see, e.g., NICHD-ECCRN, 2003). However, experiencing unemployment may be stressful and thereby negatively affect the quality of parenting.<sup>14</sup> Parenting quality may also be affected by having children at childcare centers if parents learn parenting skills from teachers or other parents, or if parents experience less stress when they do not have to activate and care for their children full-time. Yamaguchi et al. (2017) show that enrollment at childcare centers improved parenting quality among low-educated mothers. Other indirect effects of childcare access may be that unemployed parents could find a new employment sooner when they can spend more time on job search, which will increase family income.<sup>15</sup>

Whether a child's health will benefit or be harmed by spending time at home due to parental unemployment instead of at a childcare facility is thus an open question. In

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<sup>12</sup> Ball et al. (2002), for example, finds that attendance at large daycare centers was associated with more common colds during the preschool years but less during early school years. This acquired immunity was however waned by age 13.

<sup>13</sup> That children in childcare may also suffer from fewer one-to-one interactions with adults is also supported by evidence in Fort et al. (2016) who find attending childcare at ages 0–2 reduces IQ at ages 8–14 for children in advantaged families.

<sup>14</sup> E.g. Eliason and Storrie (2009) and Browning and Heinesen (2012) show that individuals that experience job loss due to a plant closure experience negative health consequences. Furthermore, Eliason (2011) and Huttunen and Kellokumpu (2016) find an increased risk of divorce following a job loss.

<sup>15</sup> However, if the value of the parent's time (leisure) at home is higher when there is no need to care for the child, unemployment duration may instead increase. Vikman (2010) finds a 17 percent increase in the likelihood that mothers find employment when childcare is available. She finds no similar effect for unemployed fathers.

particular, effects are likely to differ depending on the health outcome considered. Also, the effects can be expected to differ depending on the quality of the care provided by parents and childcare facilities. This is relevant for diagnoses related to injuries and respiratory conditions, where home conditions, such as child safety awareness and indoor environment, as well as caregivers' awareness of early signals of illnesses, are likely to matter. We would expect an increased likelihood of infections when children first attend childcare (or start school if they have not attended childcare during early ages). However, whether infections require hospital care is likely to depend on their severity, but also on the quality of preventive or primary care available to the child. Finally, when it comes to mental health and behavioral problems in the medium-run, Canadian evidence points to increased anxiety and aggression as a result of increased childcare enrollment, while evidence from Japan shows that childcare enrollment among children of low-educated mothers reduces inattention and hyperactivity symptoms. In this study, we attempt to capture effects on physical and mental health discussed in this section by investigating hospitalizations related to injuries, infections, respiratory conditions, as well as prescription of medical drugs related to infections (anti-infectives), respiratory conditions and mental and behavioral problems (ADHD-medication and psycholeptives).

### **3 Childcare and health care in Sweden**

#### **3.1 Childcare and the reform**

In the year 2000 as many as 66 percent of Swedish children aged 1–5 attended publicly financed childcare. Swedish childcare is heavily subsidized and of high quality and it is the 290 municipalities which are responsible for providing childcare to all children living in the municipality. Before July 2001, municipalities were obliged to provide childcare for children whose parents were either working or full time students, from when the child turned one until school start (i.e. in the fall of the year the child turns six).<sup>16</sup> The average enrollment age for children born 1999 was 18 months (Duvander, 2006). Whether to also provide childcare for children whose parents were unemployed or on parental leave with a younger sibling was determined locally by each

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<sup>16</sup> Compulsory school formally starts at age seven, but most children enroll in a voluntary preschool year from age six organized by schools. Parents in Sweden are entitled to 16 months of paid parental leave.

municipality.<sup>17</sup> In July 2001, a new law came into place requiring municipalities to offer preschool of at least 3 hours per day or 15 hours per week to children whose parents were unemployed. This paper exploits this policy change to isolate a causal effect on child health of access to childcare for children with unemployed parents.

The aim of the policy was to increase childcare enrollment among disadvantaged children and to facilitate job finding for unemployed parents (primary mothers). There were other policy changes in 2002 and 2003 that also potentially increased enrollment in childcare among children with employed parents or with parents who did not participate in the labor force. In 2002 there was a reduction in childcare fees, in 2003 children whose parents were on parental leave with a younger sibling were granted access and 15 hour per week of childcare, free of charge. Additionally, 4 and 5 year old children of 4 and 5 year were offered free childcare for 525 hours per year.<sup>18</sup>

Swedish childcare is of high quality. In their family database, OECD uses two main types of information to capture childcare quality: child-to-staff ratios and the minimum qualifications required for childcare staff. Sweden rates high in both dimensions.<sup>19</sup> Both before and after the reform, child groups were relatively small (around 17 children per group) and the child-to-staff ratio low (around 5.3–5.5 children per staff). About 50 percent of the childcare employees have a university degree from a preschool teacher-training program and 40 percent of childcare employees have an appropriate vocational high school degree specializing in the care of young children in day care.<sup>20</sup> Important for the results in the present study is that there is no indication that the quality of childcare changed, as a consequence of the studied reform; staff ratio and child groups remained stable compared to before the reform (Mörk et al., 2013). One reason why the staff ratios did not decrease is that central government introduced additional intergovernmental grants to compensate for cost increases caused by the reform.

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<sup>17</sup> Municipalities were however obliged to provide a childcare slot for children who were judged to be in special need of childcare, regardless of parental employment status.

<sup>18</sup> In 2002, childcare fees were harmonized across municipalities and average fees were also reduced, implying that the share of childcare costs paid by parents was reduced from 16 to 10 percent. After the reform, parents paid three percent of household income for the first child, up to a maximum to 145 euro per month. The fees for the second and third enrolled child were lower and the fourth child was free of charge. Lundin et al. (2008) and Mörk et al. (2013) analyze the introduction of a maximum fee and find no effects on parental employment but some positive effects on fertility. van den Berg and Siflinger (2018) study the effect on child health by comparing the health of children in cohorts which paid higher fees with the health of children with cohorts which paid lower fees. In January 2003, municipalities were obliged to offer 525 hours of free-of-charge preschool per year for all children of ages 4–5.

<sup>19</sup> <https://www.oecd.org/els/soc/PF4-2-Quality-childcare-early-education-services.pdf>

<sup>20</sup> The information about childcare quality is taken from The Swedish National Agency for Education's yearly reports "Beskrivande data om barnomsorg, skola och vuxenutbildning".

### **3.2 Health care for children**

When studying health outcomes of children based on hospitalizations and drug prescriptions, a potential concern is that factors such as family income or other characteristics affect access to care. We argue that this is a limited problem in our setting. There is universal health insurance coverage in Sweden. The Child Health Program provides vaccinations and preventive care with regular checkups from birth to school start after which the School health care program takes over the responsibility. These programs are free of charge and have almost 100 percent enrollment.<sup>21</sup> Also dental care is free of charge until age 20. Patient fees, for both primary and hospital care are heavily subsidized. There is also a high-cost protection policy in place, implying that there is a low maximum amount that families have to pay during a year. During the period studied in this paper, the cap on health care expenses was SEK 900 (equivalent to EUR 113) during a twelve months period. In most counties, persons below 20 pay did not pay any patient fees.

For hospital care, counties were not allowed to charge more than 80 SEK per day and night. For prescription drugs, individuals payed the full cost up to 900 SEK, after which costs were reduced gradually implying that nobody had to pay more than 1,800 SEK during a twelve months period.<sup>22</sup> The cap on health care expenditures also applies to children. Families add up the expenditure on patient fees and prescriptions respectively for all their children (0–18 year olds). The caps above applied to the total costs for all children. (Ds 2011:23). Thus, health care costs should not make up an obstacle for receiving care, not even for low-income families.

## **4 Empirical strategy**

Our aim is to investigate how access to childcare affects the health of children with unemployed parents. We investigate both short-run effects, i.e. health outcomes in the same year that children have or do not have access to childcare, and effects in the medium run, i.e. health outcomes measured when the children are 10–11 years old. Because we do not have access to data on actual attendance, we estimate reduced form effects. Section 5.1 provides evidence using survey data that the reform implied a

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<sup>21</sup> See Socialstyrelsen (2014)

<sup>22</sup> Costs were reduced with 50 percent for the amount between 900 and 1,700, with 75 percent for the amount between 1,700 and 3,300 and with 90 percent for amounts between 3,300 and 4,300, and with 100 percent for amounts above 4,300.

substantial increase in enrollment among children of unemployed parents. Below we present the identification strategy for the short-run and medium-run effects respectively.

#### 4.1 Identifying the short-run effects

When analyzing how access to childcare affects short-run health outcomes of children exposed to parental unemployment we estimate the following event type difference-in-differences specification:

$$y_{imt} = \sum_{t=1998}^{2004} \mu_t TREATED_{imt} + \delta \mathbf{X}_{it} + \pi UE_{mt} + \tau_t + \varphi_m + \varepsilon_{itm} \quad (1)$$

where  $Y_{imt}$  is the health outcome for child  $i$  exposed to parental unemployment in municipality  $m$  during year  $t$ , and  $TREATED_{imt}$  is a dummy variable taking the value one for children who at time  $t$  live in municipalities that changed their policy due to the reform, and where  $\tau_t$  and  $\varphi_m$  are year and municipal fixed effects, respectively. We also control for the municipal unemployment rate for individuals aged 25–34 years ( $UE_{mt}$ ) as well as child-specific characteristics including dummy variables for child age (in months at the end of the year) and sex, number of siblings age 0–10 years and birth order, maternal age at first birth as well as maternal and paternal level of education (compulsory schooling, upper-secondary schooling, higher education or unknown), and region of birth (non-Nordic), captured by the vector  $\mathbf{X}_{it}$ . The parameters of interest are the  $\mu_t$  for  $t \in [1998, 2004]$ .<sup>23</sup> We have chosen year 2000 (the year before the reform was introduced) as the reference year to which the other years are compared. If being at home with an unemployed parent or being cared for in center-based care matters for child health we would expect  $\mu_t \neq 0$  for  $t \in [2002, 2004]$ , where a negative (positive) sign would indicate better (worse) health outcomes for children with unemployed parents when having access to childcare than when being home. Looking at the estimated coefficients for the years before the reform (i.e.  $\mu_{1998,1999}$ ) we can observe

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<sup>23</sup> We limit the study to the years 1998–2004. One reason is that information on municipality policy regarding access to childcare for unemployed is based on responses to a survey conducted in 1998 and 2001. Estimating the model including years before 1998, would rest on the assumption that municipalities had the same childcare access policy for earlier years. Another potential problem with extending the sample period is that other policies may have affected the studied groups. Since the new regulations was introduced in the summer of 2001, and it is unclear to what extent the municipalities that previously had not offered childcare for children with unemployed parents were able to offer the slots already in the second half of 2001, it is unclear whether we can expect to see any effects in 2001.

whether the trends in health among children exposed to parental unemployment in treated and control municipalities were the same. If  $\mu_t \neq 0$  for  $t = [1998, 1999]$ , we would worry that the assumption of parallel trends is violated, and we would have reasons to doubt that the estimated effects capture causal effects.

#### 4.2 Identifying the medium-run effects

To identify the medium run effects i.e. effects on health outcome when the children are 10–11 years old we exploit the fact that children have been denied access to childcare during different number of years, depending, on the one hand, on exposure to parental unemployment, and on the other hand on the municipal policy for offering childcare to children with unemployed parents. More specifically, we estimate the following triple difference (DDD)-specification:

$$y_{imc} = \alpha NOACCESS_{icm} + \beta UNEMP_i + \delta UNEMP_i * cohort_c + \gamma UNEMP_i * municipalilty_m + \delta \mathbf{X}_i + \theta_{cm} + \varepsilon_{icm} \quad (2)$$

where  $y_{imc}$  is the health outcome at age 10–11 for child  $i$  in municipality  $m$  and birth cohort  $c$ ,  $UNEMP_i$  counts the number of years during which the child was exposed to parental unemployment between ages 2–5, and  $NOACCESS_{icm}$  counts the years during which unemployment exposure coincided with not having access to childcare (because of having an unemployed parent and living in a treatment municipality before the reform), which differ between cohorts. Since the reform was implemented in July of 2001, 2001 is counted as a half year.  $\theta_{cm}$  are municipality-specific cohort-effects and  $\mathbf{X}_i$  is a vector of child specific control variables (the same as for the short-run analysis, but measured when the child is two) The parameter of interest is  $\alpha$  that shows how exposure to parental unemployment without access to childcare affects health 5–6 years after the child was in childcare age (at age 10–11).

#### 4.3 Threats to identification

The identifying assumption that must hold for the difference-in-differences specification to estimate causal effects is that the health of unemployed children in the treated municipalities would have been similar to that in the control municipalities if they had had access to childcare already before the reform. This assumption will not hold if there

are other changes in society that affected hospitalization rates in treated and non-treated municipalities differently. Although this cannot be formally tested, we can study whether the assumption is plausible. We do this by studying the development of health of children exposed to parental unemployment in treatment and control municipalities before the reform.

In addition, it might also be the case that the reform affects both selection into unemployment and unemployment duration.<sup>24</sup> In order to investigate whether selection on the extensive margin is important, we investigate how sensitive the estimated effects are to the inclusion of a number of parental controls, such as parental education, country of origin and maternal age at first birth. If the point estimates remain relatively unchanged when controlling for these parental controls, we will conclude that selection into unemployment is not a serious issue. We will deal with the potential selection on the intensive margin by not conditioning on the length of parental unemployment, but only consider being exposed to any parental unemployment during a year (see section 5.2. for details and a longer discussion).

The identifying assumption that must hold for the DDD-specification to estimate causal effects is that there are no differences in how health develops for children depending on parental employment status across cohorts in treated and untreated municipalities that is unrelated to childcare enrollment. Ideally, we would like to test this identifying assumption using a placebo-specification, but due to the limited time-period for which we observe prescription data, this is not possible.

As discussed earlier, the analyzed reform was followed by other childcare reforms that may also have increased enrolment in childcare. Most importantly, childcare became cheaper, both through the implementation of a maximum fee in 2002, and through the introduction of 525 hours of free-of charge preschool per year for all children aged 4–5. For the short-run analysis, these additional reforms would be problematic only if they increased enrollments among children with unemployed parents to a larger extent in the control municipalities than in the treated municipalities (for example if childcare was more expensive in these municipalities before the reform). In this case, there would be no detectible first stage effect on the enrollment rates of the

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<sup>24</sup> In a study of the same reform as the one studied in this paper, Vikman (2010) finds a 17 percent increase in the likelihood that mothers find employment when childcare is available. She finds no similar effect for unemployed fathers.



studied group in the treated municipalities and we would likely underestimate the effect of having access to childcare with the strategy outlined in section 4.1. The same is true for the identification of the medium-run effects, except that we here also need the reforms not to increase enrollment among children with employed parents more than among children with unemployed parents. Below, we use survey data on childcare enrollment in order to investigate how the reform package affected childcare enrollment in the different groups and whether there is a “first stage”. If such a first-stage exists, we are assured that the studied reform had an effect on enrollment rates over and above possible enrolment effect of other reforms that were implemented during the same period.

## **5 Data and measurement issues**

In this section, we first discuss how we identify treated and control municipalities, thereafter we present the individual level data and discuss how we measure health outcomes and parental unemployment. Finally, we present some descriptive statistics.

### **5.1 Treatment and control municipalities**

As mentioned earlier, prior to the reform in 2001, municipalities could choose to provide childcare access for children whose parents were unemployed. After the reform, all municipalities were mandated to offer at least 15 hours of childcare to this group of children. In this paper, we exploit the resulting change in municipal policies to estimate causal effects of the availability of childcare for children with unemployed parents. More specifically, we will compare municipalities that changed policy because of the reform to municipalities that already before the reform gave children with at least one unemployed parent access to childcare.

Information about which policies that were in place in different municipalities before the reform is taken from surveys conducted by the Swedish National Agency for Education in 1998 and 2001. In the surveys, municipalities were asked about their childcare fees childcare policies in general. More specifically they were asked what happens if i) a child already had a slot and a parent became unemployed and ii) a child did not have a slot and at least one parent was unemployed. We consider a municipality as treated if children with unemployed parents did not get a slot or they lost their slot if

a parent became unemployed according to both of the survey rounds. Applying these criteria, we identify 75 treatment municipalities.

Based on the responses to the survey, only seven municipalities can be defined as untreated municipalities, in the sense that they did not have restrictions on the access to childcare for children with unemployed parents. However, these municipalities are very different from the reform municipalities, with lower unemployment rates and different trends in hospitalization rates before the reform. For the remaining 207 municipalities, the policies with respect to offering childcare to children with unemployed parents are not quite clear, making it difficult to identify suitable control municipalities based on survey responses. An alternative strategy to identify a suitable set of control municipalities is to select them based on actual enrollment rates of children of unemployed and employed parents prior to reform. For this purpose, we use another survey conducted by the National School board in 1998 and 2002, the Parent survey, which asked parents about childcare enrollment.<sup>25</sup> Our aim is to identify 75 suitable control municipalities where children to unemployed parents had access to childcare already before the reform. We therefore select municipalities where i) the enrollment rates of children in prior to the reform were similar regardless of parental unemployment status and ii) the differences in enrollment between children with employed and unemployed parents did not change as the reform was introduced.<sup>26</sup> Using this procedure, we identify 75 municipalities that are not likely to have been affected by the reform.

Figure A 2 in Appendix shows that treated and control municipalities are scattered across Sweden. Table A 1 shows descriptive statistics for the treated and control municipalities and for the country as a whole. The treatment municipalities are in general less populated, have fewer children in childcare age, are to a larger extent run by a left-wing majority and have somewhat higher unemployment rates compared to control municipalities. As expected, childcare enrollments rates are lower, and so is municipal spending on childcare. In the empirical analysis, we control for these differences through the inclusion of municipality fixed-effects and by controlling for the municipal employment rate. Importantly, the child-to-staff ratio, which is a proxy for childcare quality, is similar in both treatment and control municipalities, and does not

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<sup>25</sup> The survey was stratified to make the responses representative at the municipal, as well as the national, level.

<sup>26</sup> The selection of control municipalities is further described in Appendix..

change in connection with the reform, see Figure A 3 in the Appendix. Overall, the municipalities included in the study do not deviate greatly from the rest of the country.

An important question is if the reform had any impact on childcare enrollment, i.e. if there is a first stage. We cannot measure this at the individual level, because of lack of register data. Instead we use the Parent surveys, to investigate how the enrollment rates for children with unemployed parents changed due to the reform, see Table 1 for the results.<sup>27</sup> Columns 1–2, provide the difference-in-differences estimates of the change in enrollment for children with unemployed parents, comparing treatment to control municipalities before and after the reform (column 1 without controls and column 2 including controls for parental education and child age and sex). The estimated effect is very stable and suggests a 20–21 percentage point increase in enrolment as a result of the reform. Because there were other reforms during the same period that may have affected enrollment of other groups, we also provide DDD estimates where we include children of employed parents as a further control groups. The results are presented in columns 3–4. The estimated effect is now slightly smaller, 19.2–19.8 percentage points, but still very similar to the DD estimates.<sup>28</sup> Comparing this 19–20 percentage points increase in enrollment to the pre-reform enrollment rate of 57 percent for children with unemployed parents implies an increase by 34–37 percent increase in enrollment.<sup>29</sup>

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<sup>27</sup> Information on enrollment and average hours in childcare in control and reform municipalities for children of employed and unemployed parents is displayed in Table A 2.

<sup>28</sup> We have also estimated the first stage for different educational background. The increase in enrollment is similar across maternal education groups.

<sup>29</sup> Pre reform enrollment is not zero for children with unemployed parents. Before the reform, these children could be granted access to childcare if the family was considered in extra need by social services in the municipality. Another reason could be that the parent had only been unemployed for a short time and the child had not yet lost the childcare slot.

Table 1: First stage: the effect of the reform on enrollment in childcare among children of unemployed parents

	(1) DD Unemployed, treated vs control	(2) DD Unemployed, treated vs control	(3) DDD Unemp-emp, treated vs control	(4) DDD Unemp-emp, treated vs control
Reform- Effect	0.203*** (0.036)	0.211*** (0.032)	0.192*** (0.035)	0.194*** (0.033)
Observations	5,306	5,306	45,533	45,533
R-squared	0.078	0.144	0.073	0.093
Time and muni fe	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Note: Robust standard errors in parentheses, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Controls include parental education indicators (compulsory, high school, tertiary), child age dummies and child sex.

## 5.2 Individual level data

We base the analysis on population wide Swedish register data from Statistics Sweden, the National Board of Health and Welfare, and the Public Employment Service. Population registers allow us to link parents to children and contain information on sex of the child, child age in months, number and age of siblings and parental age. Matched to these data are taxation- and education registers with information on parental earnings, and education, as well as information about residential municipality. Information about parental unemployment is available in the data from the Public Employment Service. Health outcomes are taken from The National Patient Register (Hospital discharge register (NPR)) and from the Prescription Drug Register (PDR), both from the National Board of Health and Welfare. NPR contains information on all patients who are discharged from in-patient care in Swedish hospitals and include detailed diagnoses, whereas PDR, which exists since 2005, contains records of all over-the-counter sales of prescription drugs, with information on the patient and on the active substance. During the years around the childcare reform, high quality data on out-patient care did not exist for the whole of Sweden, but only for a few counties, hence our focus on in-patient care.

We sample all children born 1993–2002 and their parents. When analyzing short-run effects we keep, for each year 1998–2004, the children who are 2–5 years old at the end of the year and who were exposed to parental unemployment at some point during that year. We define child exposure to parental unemployment as having at least one parent who is registered as unemployed at the Public Employment Service at least one day

during a specific year. A reason for including children with very little exposure in the group is that previous research has shown that the length of unemployment was affected by the reform (Vikman, 2010) and that unemployment duration is hence endogenous to the reform. However, as is clear from Figure A 4 in the Appendix, the majority of children experience considerably longer parental unemployment spells, and as much as 18 percent of the children with unemployed parents experience parental unemployment during the whole year. The length of spells changes over time, but the pattern of change is similar in treatment and control municipalities.

When analyzing medium-run effects, our sample includes children exposed to parental unemployment and children whose parents are employed, where we define a child as having employed parents if neither parent is register as unemployed during the year and both parents have annual earnings that exceed a threshold defined as two times the *price base amount*.<sup>30</sup> Due to data limitations, we restrict this sample to children born 1995–2000.<sup>31</sup> Since the new policy was introduced in July 2001 access to childcare differs across cohorts. Figure 1 shows in what ages a child with unemployed parents did not have access to childcare depending on the birth year of the child. For example a child born in 1995 with unemployed parents did not have access to childcare at any age, whereas a child born 1997 had access to childcare at age 5 if the parent was unemployed. Cohorts born between 1996 and 1999 are partially treated and cohorts born in 2000 had access to childcare their whole childhood.

As mentioned above, we do not have information about which children actually attended childcare, and the parameter that we estimate will therefore be the intention to treat effect. In addition, the way we define exposure to unemployment implies that we may have measurement error in our treatment variable (having access to/not having access to childcare) and the estimates may therefore suffer from attenuation bias.

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<sup>30</sup> The Price base amount (*prisbasbelopp*) is based on the consumer price index and adjusted annually by the government. Between years 1998 and 2004 the amount has been 36,400–39,300 SEK in nominal value (roughly 4,000 Euro).

<sup>31</sup> Data on prescriptions is only available 2005-2011.

Figure 1 Treatment status by cohort and age of child

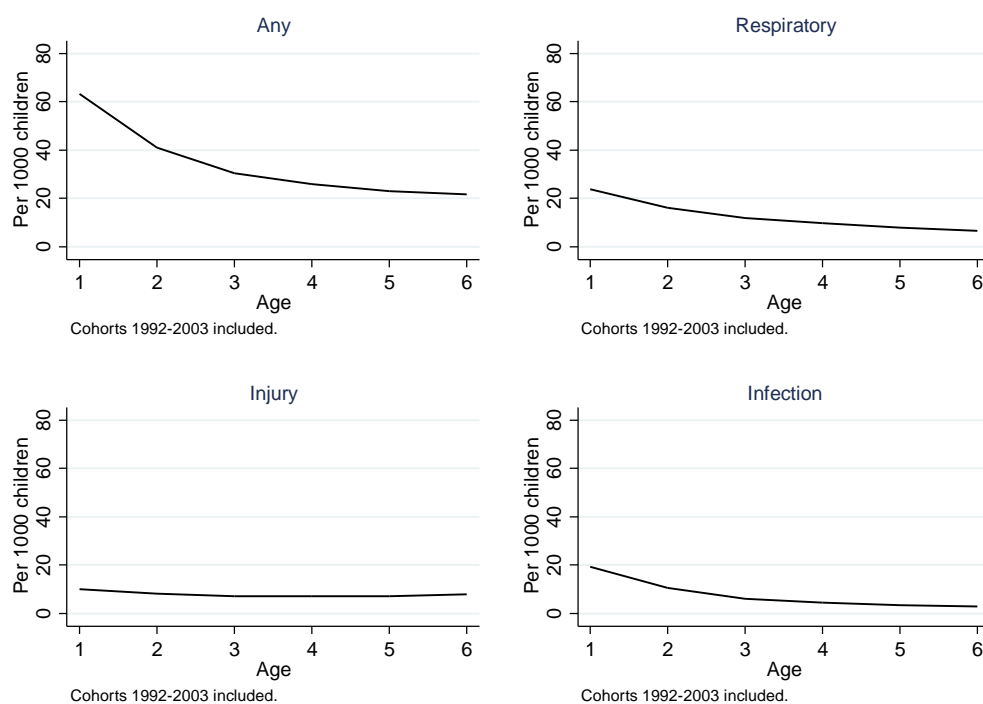
		Calendar time																	
		1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	
Year of birth	Not	1993	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
		1994	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
		1995	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
	Partly	1996		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
		1997			0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
		1998				0	1	2	3	4	5	6	7	8	9	10	11	12	13
		1999					0	1	2	3	4	5	6	7	8	9	10	11	12
	Fylly	2000						0	1	2	3	4	5	6	7	8	9	10	11
		2001							0	1	2	3	4	5	6	7	8	9	10
2002									0	1	2	3	4	5	6	7	8	9	

# Childcare attending age      # Age when medium-run outcome is measured

In the short-run analysis, we measure child health using the in-patient register. We consider a child as hospitalized during a year if (s)he is observed in the in-patient care register at least once in a year. In addition to investigating effects on hospitalization for any diagnosis, we also investigate the effect of access to childcare on diagnoses related to injuries, respiratory diseases and infections.<sup>32</sup> These diagnoses groups make up some of the most common reasons for why children are hospitalized. They are also cover conditions that, as argued in Section 2, may plausibly be affected if a child attends childcare rather than stay home with parents. Figure 2 shows hospitalization rates, by age, for the different diagnoses. Hospitalizations clearly vary with the age of the child. The risk of being hospitalized is highest among the youngest children and decreases as the child gets older, especially the first three years of a child’s life. Since the health of 2–3 year old children is different from the health of 4–5 year old children and since, as discussed in the introduction, earlier studies have found that enrollment age may matter for the effects of childcare attendance on health as well as on cognitive and non-cognitive development we will study the two age groups separately.

<sup>32</sup> We have also considered effects on total number of hospitalizations, but this turns out to be a very noisy measure. Hence, these results are not presented in the paper. The ICD10 codes of the diagnosis considered in this study are listed in Table A 3 in Appendix. Hospitalization for a specific condition is based on the diagnosis codes for the main diagnosis and the first five auxiliary diagnoses in the register. Both hospitalization and drug prescriptions are measured as in 1,000 children.

Figure 2 Hospitalization per 1,000 children different diagnoses, by age.



The benefits of using hospitalization records when measuring health are that hospitalization can be regarded as a relatively objective measure (as opposed to self-reported health measures). Moreover, hospitalizations capture rather serious health events, which are likely to have long run effects. However, although in some sense objective, hospitalization is still the result of a professional judgment by a physician, based on the child’s health status and on the fact that the child has been taken to the hospital, i.e. the care seeking behavior of the child’s parents. The seriousness of health conditions that require hospital care is however such that one should expect children who need care to eventually get to the hospital. Also, neglect to seek primary care, may result in a need for hospital care. As described, earlier hospital care for children is not expensive in Sweden. Thus, the cost should not be an obstacle for seeking care, even for poor families.

In the medium-run analysis, we measure child health when children are 10–11 years of age, using data from both the inpatient- and the drug prescription registers. From the inpatient-register, we create a dummy variable indicating whether the child was hospitalized any time during the considered age-span. From the drug prescription

register, we first construct indicators for if the child has been prescribed any 1) anti-infectives and 2) medication for respiratory conditions in the calendar years the child is 10 and 11 years old. These medication groups match the hospitalization diagnoses studied, but capture also less severe conditions. Second, in order to capture effects on the child psychiatric health and behavioral problems, we create indicator for being prescribed ADHD-medication or psycholeptics, i.e. medications for sleeping problems and anxiety. The ICD10 codes of the diagnoses and the ATC codes of the drugs considered in this study are listed in Table A 2 in Appendix.<sup>33</sup> Hospitalizations of young children for psychiatric conditions are very rare, which is why drug prescriptions are of special interest.

Using prescriptions of drugs, we pick up less severe health problems and these data are hence a valuable complement to the hospitalization data. Drug prescriptions require a diagnosis by a health professional and are thus an indication of an objective evaluation of the child's health condition. However, care-seeking behavior is likely to play an important role in the likelihood of getting a prescription. Moreover, only actual purchases are registered which may introduce a further social element if economic conditions influence the parents' likelihood of collecting the medication. Since, as described in section 3.2, a high-cost protection in place for prescription drugs, also low-income household should be able to afford to collect prescribed medications.

For natural reasons, register data does not contain any information about the quality of parental care. Given earlier evidence of larger positive effects of childcare on cognitive outcomes for children of lower socio-economic status (see, e.g. Liu and Skans, 2010; Felfe et al., 2015; Havnes and Mogstad, 2011), we use maternal education as a proxy for parental care when looking for heterogeneous effects.

### **5.3 Descriptive statistics**

Table 2 shows descriptive statistics for children with at least one unemployed parent for the pre-reform period 1998–2000 and the post-reform period 2002–2004 by treatment status of the municipality. Mothers in the treated municipalities are somewhat younger at first birth, and both parents are less likely to be born outside the Nordic countries and less likely to have higher education. These differences in background characteristics

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<sup>33</sup> Hospitalization for a specific condition is based on the diagnosis codes for the main diagnosis and the first five auxiliary diagnoses in the register.



motivate the inclusion of control variables in the estimations. Children in the treatment municipalities are more likely to be hospitalized, and over time, hospitalization rates decline in both treatment and control municipalities.

Table 2 Descriptive statistics for 2–5 years old children who experience parental unemployment in pre-reform years 1998–2000 and post-reform years 2002–2004 by treatment status of the municipality.

	Pre (1998–2000)		Post (2002–2004)	
	Control	Treatment	Control	Treatment
<i>Mother</i>				
Age at first birth	25.40 (5.56)	24.60 (5.12)	26.14 (6.12)	25.12 (5.70)
Compulsory education	0.21 (0.41)	0.20 (0.40)	0.19 (0.39)	0.17 (0.38)
Upper secondary education	0.54 (0.50)	0.62 (0.48)	0.51 (0.50)	0.61 (0.49)
Higher education	0.23 (0.42)	0.17 (0.38)	0.29 (0.45)	0.21 (0.40)
Unknown education	0.02 (0.15)	0.01 (0.10)	0.02 (0.14)	0.01 (0.11)
Non-Nordic born	0.27 (0.44)	0.13 (0.33)	0.30 (0.46)	0.16 (0.37)
<i>Father</i>				
Compulsory education	0.21 (0.41)	0.20 (0.40)	0.18 (0.38)	0.17 (0.38)
Upper secondary education	0.52 (0.50)	0.61 (0.49)	0.52 (0.50)	0.63 (0.48)
Higher education	0.24 (0.42)	0.16 (0.37)	0.27 (0.45)	0.18 (0.38)
Unknown	0.03 (0.17)	0.02 (0.13)	0.03 (0.16)	0.02 (0.14)
Non-Nordic born	0.29 (0.46)	0.14 (0.34)	0.33 (0.47)	0.17 (0.38)
<i>Hospitalizations per 1000 children annually</i>				
Any hospitalization	47.74 (213.21)	49.76 (217.45)	42.31 (201.29)	45.38 (208.13)
Respiratory	18.38 (134.31)	19.35 (137.77)	16.24 (126.39)	16.98 (129.20)
Injury	10.27 (100.81)	11.51 (106.64)	9.88 (98.90)	11.01 (104.35)
Infection	11.78 (107.91)	11.70 (107.52)	10.08 (99.90)	10.49 (101.87)
Observations	208,417	109,950	158,017	78,387

Table 3 shows descriptive statistics for children aged 10-11, for the first and last cohorts in our sample. Whereas the first cohort (born in 1995) only had access to childcare in case of parental unemployment if they lived in a control municipality, the last cohort (born in 2000) had access to childcare regardless of where they lived. It turns out that children in the treatment municipalities are somewhat more exposed to parental

unemployment than those in the control municipalities, which is as expected given the differences in parental education level that was observed in Table 2.

Furthermore, children in treatment municipalities have somewhat worse health outcomes than those in the control municipalities, as is evident for both hospitalization and drug prescriptions. What is most striking from the table is the sharp increase in prescriptions of ADHD medication and psycholeptics (*mental*) between the two cohorts. However, this pattern exists both in the treatment and in the control municipalities.

Table 3 Descriptive statistics for children 10–11 years old children

	Cohort 1995		Cohort 2000	
	Control	Treatment	Control	Treatment
Years of UE exposure	1.35 (1.31)	1.58 (1.33)	1.47 (1.59)	1.71 (1.65)
Share with some UE exposure	0.58 (0.49)	0.64 (0.48)	0.55 (0.50)	0.60 (0.49)
<i>Hospitalizations per 1000 children</i>				
Any hospitalization	30.84 (172.89)	32.93 (178.46)	27.05 (162.22)	33.76 (180.62)
Respiratory	5.73 (75.46)	5.24 (72.21)	4.45 (66.54)	5.25 (72.30)
Injury	15.74 (124.48)	16.58 (127.69)	14.96 (121.41)	17.08 (129.57)
Infection	3.57 (59.64)	4.79 (69.02)	3.38 (58.08)	4.14 (64.20)
Mental	1.10 (33.21)	1.20 (34.57)	1.51 (38.84)	1.64 (40.49)
Hospital visits	39.65 (341.26)	43.64 (320.65)	39.52 (427.29)	45.65 (309.82)
<i>Prescriptions per 1000 children</i>				
Mental prescription	6.68 (81.44)	8.38 (91.13)	17.85 (132.39)	21.02 (143.45)
Antibiotics prescription	96.84 (295.75)	101.13 (301.51)	106.12 (308.00)	122.11 (327.42)
Respiratory prescription	88.85 (284.54)	99.08 (298.77)	116.22 (320.49)	130.58 (336.96)
N	38,940	17,552	35,752	15,224

## 6 Results

We first present how hospitalization rates related to various diagnoses among 2–5 year old children of unemployed parents are affected by having access to childcare. Then, we turn to the analysis of the medium-run effects (at ages 10–11) on hospitalization and

drug prescriptions, including behavioral disorders, of not having access to childcare at ages 2–5.<sup>34</sup>

### 6.1 Effects on child health in the short run

In order to get at short-run effects on child health of having access to childcare when parents are unemployed we estimate equation 1. As motivated above, we estimate the model separately for children aged 2–3 and 4–5 years old.

The descriptive statistics in Table 2 show that parents differ somewhat in characteristics in the treatment and control municipalities. We are therefore interested in whether the results are sensitive with respect to the inclusion of control variables. This is especially important since selection into unemployment could differ depending on whether children with unemployed parents have access to childcare or not. We hence estimate the model using three different sets of controls. First, we only control for a number of child-specific characteristics (sex, age in months and birth order). Second, we add parental controls for maternal and paternal education, age of mother at first birth, whether the mother or father is of non-Nordic origin and number of children of the mother. Third, we add the municipal unemployment rate (among 25–34 year olds). Results, available in Table A 4 in the Appendix show that the point-estimates of our main interest (the estimates of  $\mu_t$  in Equation 1) are very similar across specifications. We take this as evidence of similar sorting into unemployment in treatment and control municipalities before and after the reform. In order to increase precision, we focus on the full specification in the rest of the paper.

Figure 3 shows the differences in hospitalization rates between children of unemployed parents in control and treatment municipalities for children aged 2–3 years, compared to differences in hospitalization rates in the year before the reform, 2000. Looking at the coefficients for the years 1998 and 1999, we can assess whether there are indications of different trends in hospitalization rates for children in treated and control municipalities already before the analyzed reform of 2001, in which case we would be worried that the identifying assumption of parallel trends is violated. The coefficients for the years 2002, 2003 and 2004 measure the effect of having access to childcare for

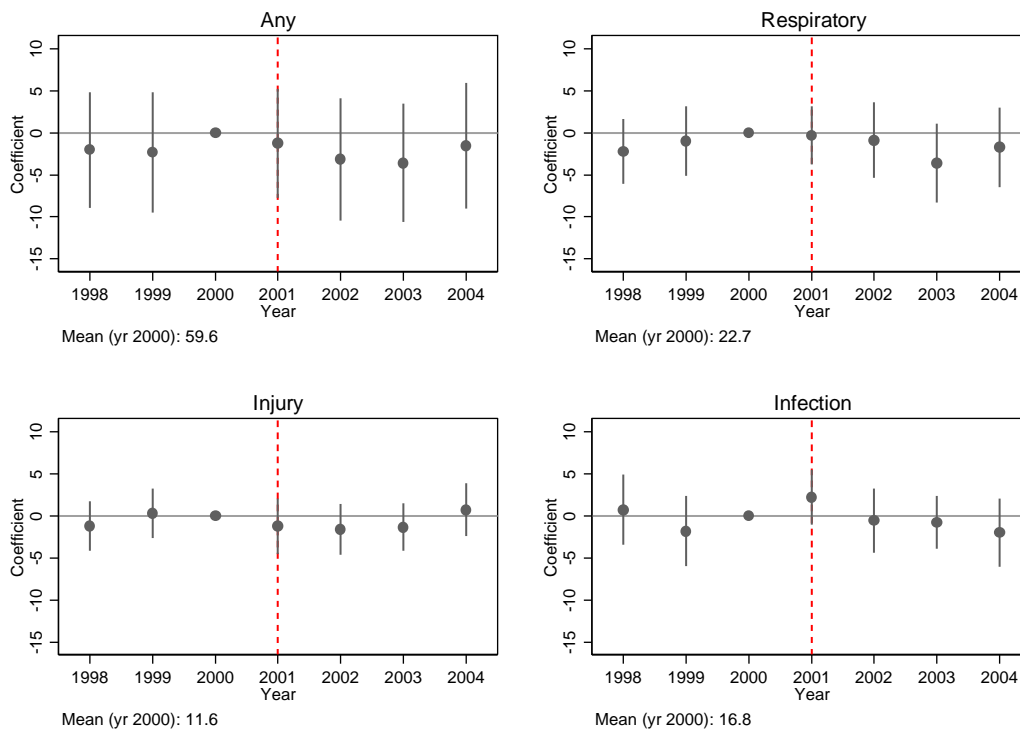
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<sup>34</sup> We have also conducted all estimations excluding Stockholm from the control municipalities, given that Stockholm is much bigger than the other control municipalities and might behave differently, since it is the capital of Sweden. The results excluding Stockholm are very similar to those presented in the paper, including those for the first stage, and are available upon request.

children with unemployed parents. Since the reform was introduced in the middle of 2001, and it is uncertain how quickly the municipalities implemented the reform, we are not certain to what extent children with unemployed parents actually had access to childcare in the treated municipalities this year. We present the coefficient for that year for completeness.

Looking at the pre-reform coefficients, the point estimates are both statistically and economically insignificant which ensures us that before the reform hospitalization rates developed similarly in control and treatment municipalities. This is however also true for the post-reform coefficients, indicating that having access to childcare or not did not affect hospitalization. For any hospitalization and for hospitalization due to respiratory diagnoses, there are some indication of a decrease in hospitalization with around 3–4 less children per 1,000 in the two/one years following the reform, but these effects are not statistically significant and, at least for any hospitalization, small in size.

Figure 3 Results: Difference-in-differences specification. Children aged 2–3 years old.



Note: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother’s age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

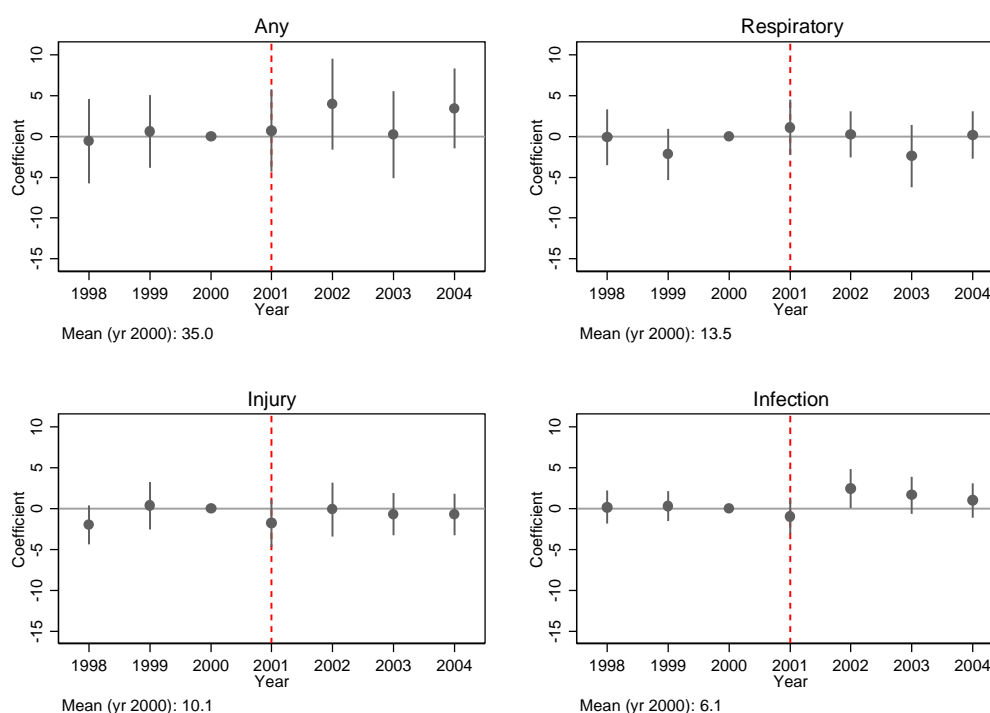
Next, we turn to the preschool aged children (aged 4–5 years). The corresponding results are presented in Figure 4. Also for this age group, there are no indications of differential health trends before the reform was implemented in 2001. Once the reform is introduced however, there is a, statistically significant, positive effect for the year 2002 on hospitalizations for infections, which indicates that children with unemployed parents were more likely to be hospitalized due to infections once they had access to childcare. The increase in hospitalization when having access to childcare is 2.42 more children per 1,000 children hospitalized annually, which corresponds to an increase of more than 40 percent.<sup>35</sup> The effect lasts only for the first year the children had access; the estimates for the years 2003 and 2004 are not statistically different from zero.<sup>36</sup>

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<sup>35</sup> In Section 5.1 we studied the change in enrollment using survey information on enrollment reported by parents at a particular point in time and found that enrollment among children with unemployed parents increased with about 20 percentage points. Is it appropriate to use this estimate to calculate an IV estimate? Since our analysis regard as treated children whose parents have been unemployed at least one day during the year, it is not straightforward to infer the reform induced increase in childcare attendance from the estimations using survey data, where unemployment is measured at the time of the survey. Yet, if we do, the first stage estimate from the survey implies that we ought to multiply our estimates with 5.

<sup>36</sup> Estimating the model, excluding Stockholm, the effect persists also in 2003, at least at the 10-percent level.

Figure 4 Results: Difference-in-differences specification. Children aged 4–6 years old.



Note: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother’s age at first birth, father and mother region of birth (Nordic, non-Nordic), number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

### 6.1.1 Heterogeneous effects

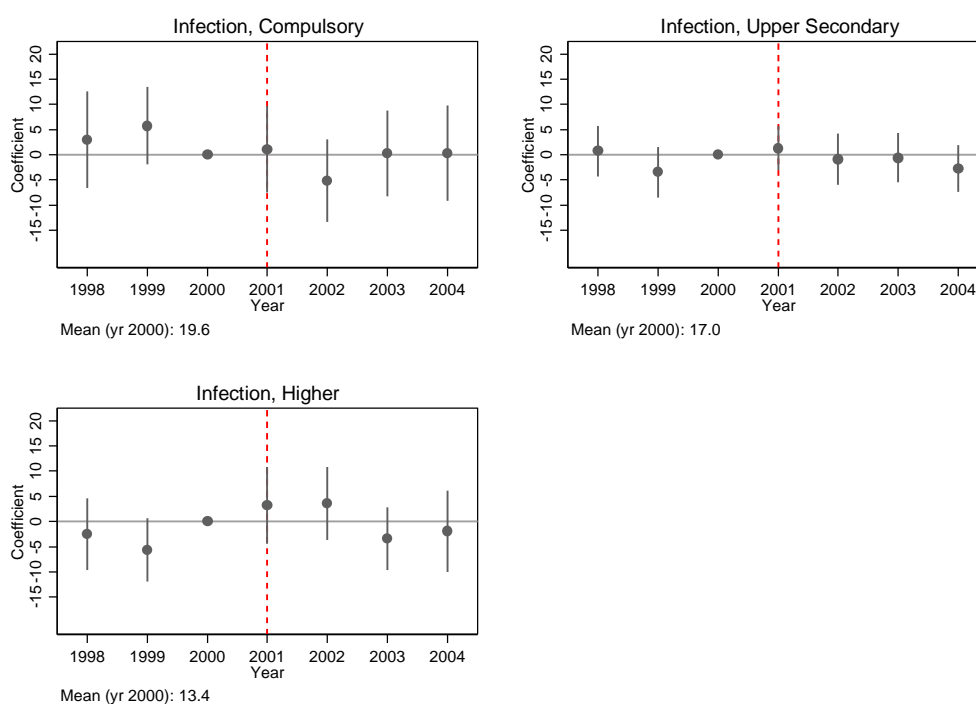
To gain understanding about the effect on infections we study whether certain groups of children are more affected by access to childcare than others. As earlier literature shows that family background can be important for the impact of attending childcare we study heterogeneous effects depending mother’s education level.<sup>37</sup> For completeness, we do this for both age groups.

Figure 5 shows the resulting estimates for children aged 2–3 and Figure 6 shows the same for children aged 4–5 (see Table A 5 and Table A 6 in the Appendix for point estimates and standard errors). From the first figure it is clear that the zero-effect found when looking at all children remains. There are no statistically significant effects for any education level for the younger children. Turning to the older children, results

<sup>37</sup> We have also analyzed whether the effects differ depending on child sex or by maternal or paternal unemployment. Our results, available upon request, do not show any such patterns, although the effects for paternal unemployment are somewhat noisy.

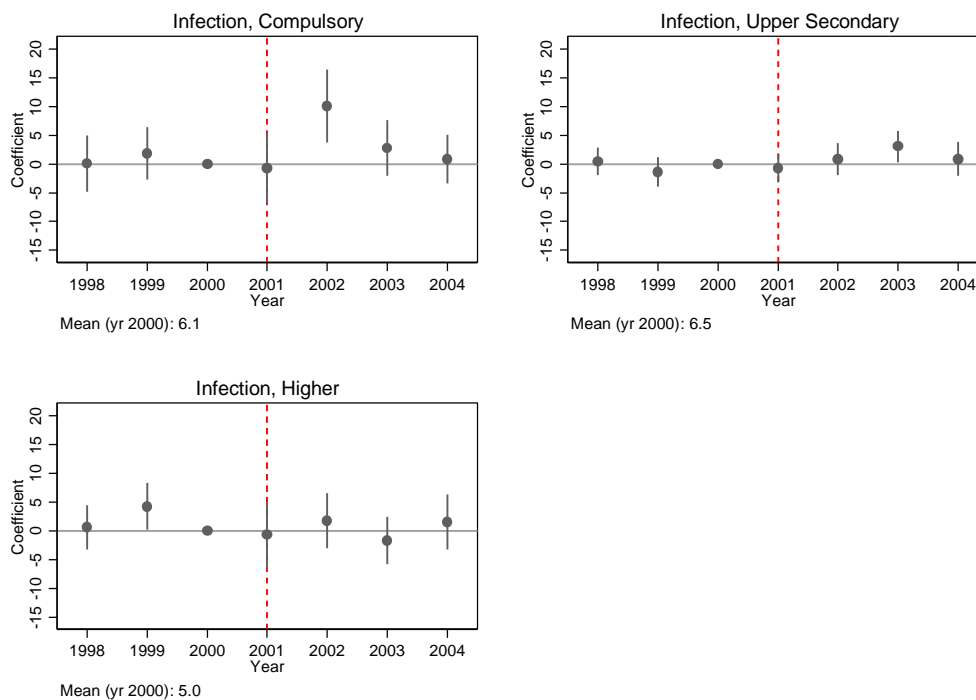
clearly show that the effect found for the whole group is driven by children of mothers with only compulsory education. For these children, there is a large increase in the risk of hospitalization due to infections in 2002, the first year when children with unemployed parents had access to childcare; 10 more children per 1,000. There is also a statistically significant effect on hospitalization for infections in 2003 for the children with upper secondary schooling and among children of highly educated mothers there is statistically significant effect before the reform, suggesting pre-reform trends for this group.

Figure 5 Infection related hospitalization by education level of mother. Children aged 2–3 years old.



Note: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic), number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

Figure 6 Infection related hospitalization by education level of mother. Children aged 4–5 years old.



Note: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic), number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

There are at least two potential explanations for why children with low educated mothers have more infections. First, it could be that these children in general are more vulnerable to health shocks. The same exposure to germs, viruses and bacteria may have worse consequences for children with parents with low education because the children are less resilient and/or because they do not receive as appropriate preventive and primary care. Second, it could be that children of mothers with low education were less likely to attend childcare when younger and therefore are more vulnerable once they enroll. Remember that during the pre-period, when these children were younger age, childcare was only available for children whose parents were working or being full-time students.

To investigate to what extent children, whose parents were unemployed when they were 4–5 year olds, were likely to have attended childcare when younger, we look at the



share of mothers who either were unemployed or received student benefits when their children were 2–3 year olds. Table A 7 in the Appendix shows these shares by maternal education. There are no indications that mothers with only compulsory education were previously unemployed to a larger extent than mothers with upper secondary education. Hence, it is fair to say that among 4-5 year olds whose parents are unemployed, those with compulsory educated mothers are as likely to have been exposed to childcare at younger ages as those with more educated mothers. A more probable explanation for their higher risk for hospitalization is hence that children with compulsory educated mothers are more likely to need hospital care either because they are more vulnerable at the outset or because they do not receive as appropriate preventive care as other children.

## 6.2 Effects on child health in the medium run

Next, we turn to medium-run effects and estimate equation 2, where we measure health outcomes when children are aged 10–11. Results are presented in Table 4, where the top panel shows the effects on hospitalizations and the bottom panel shows effects on drug prescriptions.<sup>38</sup> The parameter estimate shows how many more children per 1,000 who are hospitalized/prescribed a drug at ages 10–11 if they experienced any parental unemployment during a year when they were aged 2–5, when there was no access to childcare as opposed to experiencing unemployment when there was access to childcare.

Starting with the results for hospitalization, there is no indication that childcare access when parents are unemployed has any effect on hospitalization at ages 10–11, the estimates are both statistically and economically insignificant. Turning to results for prescriptions of ADHD-medication or psycholeptics (*mental*), our point estimate is negative, suggesting a reduction of about 10 percent when compared to the population mean, implying that *not* having access to childcare is beneficial. However, the standard errors are of the same magnitude as the estimate, implying that a confidence interval of 95-percent spans effects of [-3.39, 0.97], corresponding to a change of -24 percent to 7 percent. Also for antibiotics, we do not find evidence of any statistically significant effect and the estimate is small relative to the population mean. However, for

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<sup>38</sup> We have also investigated whether the effects differ with respect to maternal education. The point estimates are similar for all education levels but the standard errors are large, so it is difficult to draw any firm conclusions. Therefore we do not present these results in the paper, but they are available upon request.

respiratory conditions, not having access to childcare, increases the risk of needing prescriptions for respiratory problems at ages 10–11; a year of no access increases the likelihood of drug prescription by 6 percent, compared to the population mean. This effect is in line with the hygiene hypothesis, or the substitution effect suggested in van den Berg and Siflinger, which states that little exposure to infections and respiratory illness in early childhood, may shift problems to a higher age.

Table 4. Hospitalizations and prescriptions at age 10–11 years.

	Hospitalizations				
	(1) Any	(2) Respiratory	(3) Injury	(4) Infection	(5) Psychiatric
No childcare access	-0.233 (1.174)	0.060 (0.489)	0.220 (0.782)	0.123 (0.465)	-0.203 (0.247)
N	308,623	308,623	308,623	308,623	308,623
Mean outcome	29.77	5.16	15.81	3.24	1.29
	Prescriptions				
	Respiratory		Antibiotics		Mental
No childcare access	7.178*** (2.563)		0.787 (2.179)		-1.210 (1.111)
N	308,623		308,623		308,623
Mean outcome	125.74		133.07		14.23

Note: Standard errors clustered at municipality level. Control variables at age 2: Sex of child, age of child in months at the end of the year, sibling order number, education level of parents, mother's age at first birth, non-Nordic parents, number of children 0-10 years in the family, municipal UE rate (among 25-34 years old), municipality, year of birth and interaction of municipality and year of birth and interaction of years of unemployment and year of birth as well as year of birth and municipality.

\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

## 7 Concluding comments

In this paper, we have evaluated whether access to childcare for children with unemployed parents affects child health by investigating short run effects on hospitalization and medium run effects on hospitalizations and medical drug prescriptions. Using a nationwide reform, which made it mandatory instead of voluntary for municipalities to provide childcare for children with unemployed parents, we investigate the exogenous change in childcare provision for this disadvantage group. The reform potentially changed the mode of care from care at home by an unemployed parent to at least 15 hours per week in a high quality center based childcare. The expected effects of hospitalization are ambiguous. At centered based childcare the child is attended by professional staff, trained to detect early health problems and reduce the

risk of accidents and poisoning. However, the child is part of a larger group, which increases exposure for infections and the child may receive less attention and adult and parental time.

We find that among preschool children (4–5 year old) access to childcare led to an increase in hospitalization for infection. However, within one year after the reform the hospitalization rate due to infections among children with unemployed parents falls back again. A potential explanation for this pattern is that when children in age 4–5, who have not previously had access to childcare, are more sensitive to infections when they first start to attend childcare. Once they have attended childcare for some time they have built up resistance and the hospitalization rate falls back to its original level. The effect on hospitalization due to infections is entirely driven by children with low-educated mother. This may suggest that more vulnerable children are more likely to be hospitalized when exposed to infections because they may not get the same preventive and primary care. Among toddlers (children age 2–3) there is no evidence that the mode of care matters for hospitalization in the short run. In the longer run, at age 10–11, we find no effects of childcare access on hospitalizations, but a possible increase in the prescriptions of drugs related to respiratory conditions for children who did not have access to childcare when the parents were unemployed. This result lends supports to the hygiene-hypothesis or the presence of a substitution effects shifting the risk of contraction illness in time.

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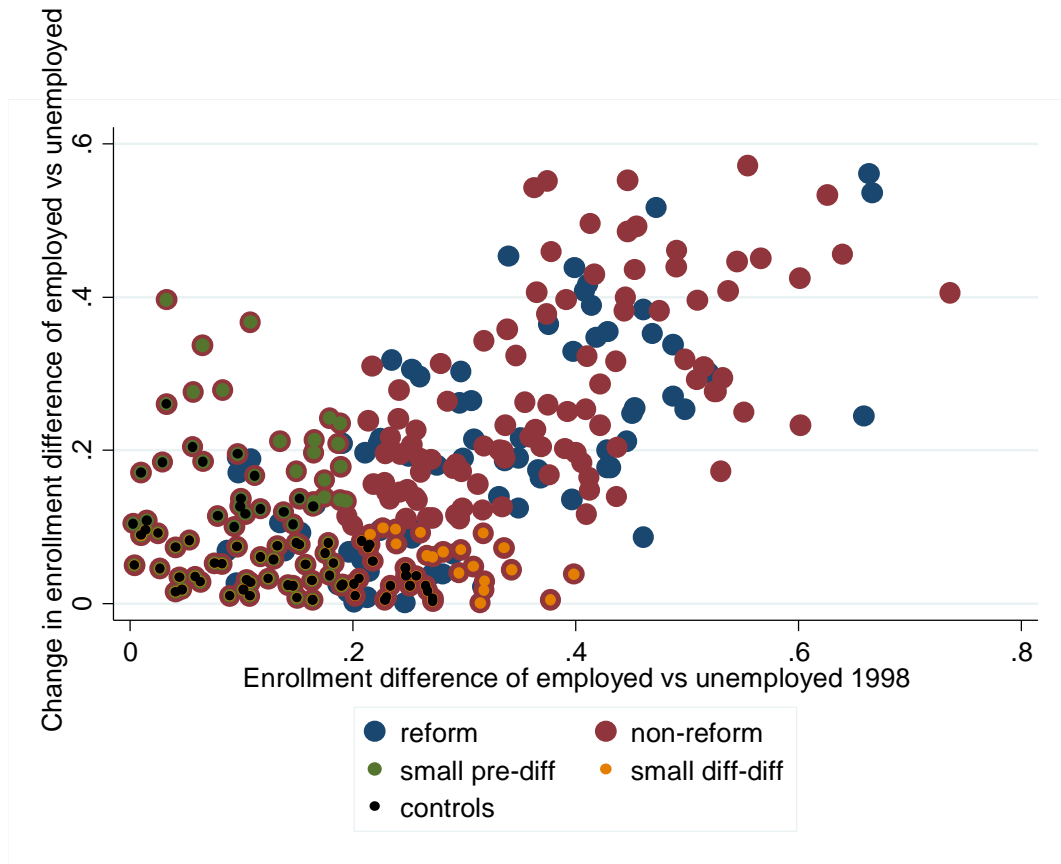
## Appendix

### Selection of control municipalities

In order to estimate effects of the reform we need to identify a set of control municipalities in which the reform did not significantly change the access to childcare for children with unemployed parents. To this end we study enrollment rates before and after the reform. Information on enrollment among children with unemployed and employed parents is collected from the Parent survey conducted by the National School board in 1998 and 2002. The survey is answered by parents and pertains to the status of the parent and child at a specific point in time. This is not an exact measure of the enrollment rates in different groups since, for example, the status of the parent may change over the year and there is no information on how long the parent have been unemployed. We define *pre-diff* as the difference in enrollment rate between children of employed parents and unemployed parents in 1998 in a given municipality, and the *diff-diff* as the change in the enrollment difference between children of employed and unemployed parents between the two surveys.

Figure A 1 plots the municipal *diff-diff* against the *pre-diff*. Blue dots represent the 75 reform municipalities that, according to the municipal survey had restrictions for unemployed parents prior to the reform. The red dots are all other municipalities. A small green dot has been placed in the middle of the red dot for the 75 non-reform municipalities with the smallest pre-reform enrollment difference (*pre-diff*). A slightly smaller yellow dot is marking the 75 municipalities with the smallest change in enrollment difference (*diff-diff*). We choose as our control municipalities the 75 municipalities that have the smallest sum of *pre-diff* and *diff-diff*. These are marked by a tiny black dot.

Figure A 1 Difference in difference 1998 to 2002 and difference in childcare enrollment in 1998 by parental employment status in Swedish municipalities.



## Figures

Figure A 2 Map over treated and control municipalities

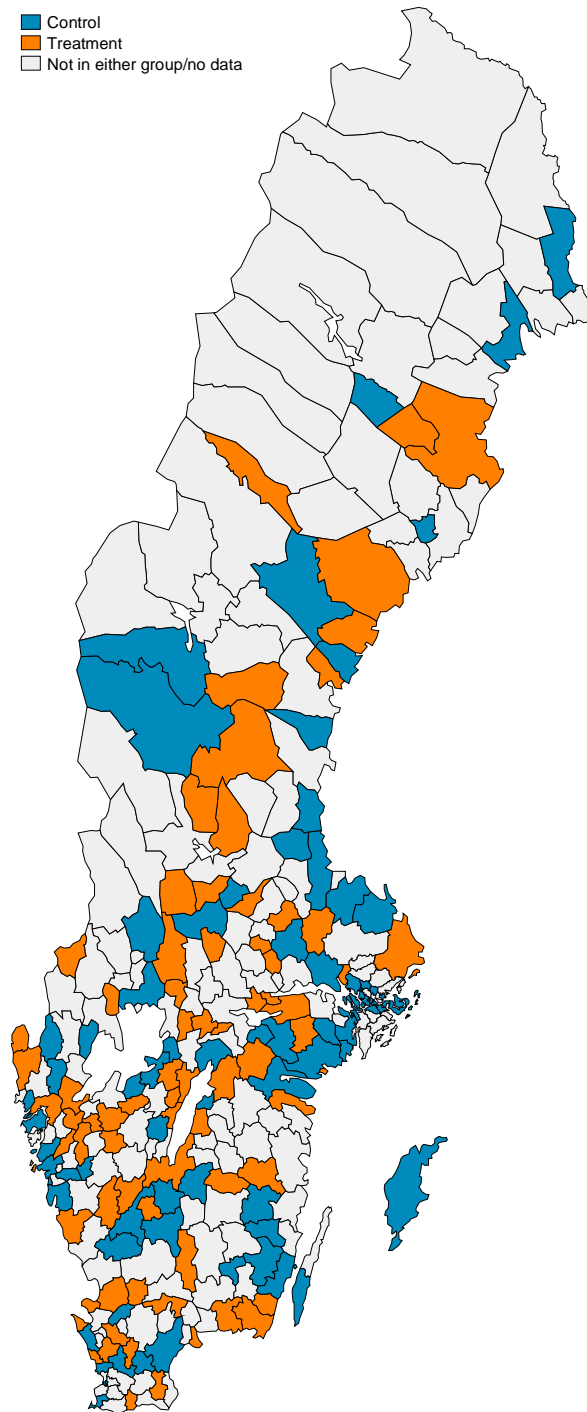


Figure A 3 Child-staff ratio in municipal childcare in treatment and control municipalities for 1998–2005

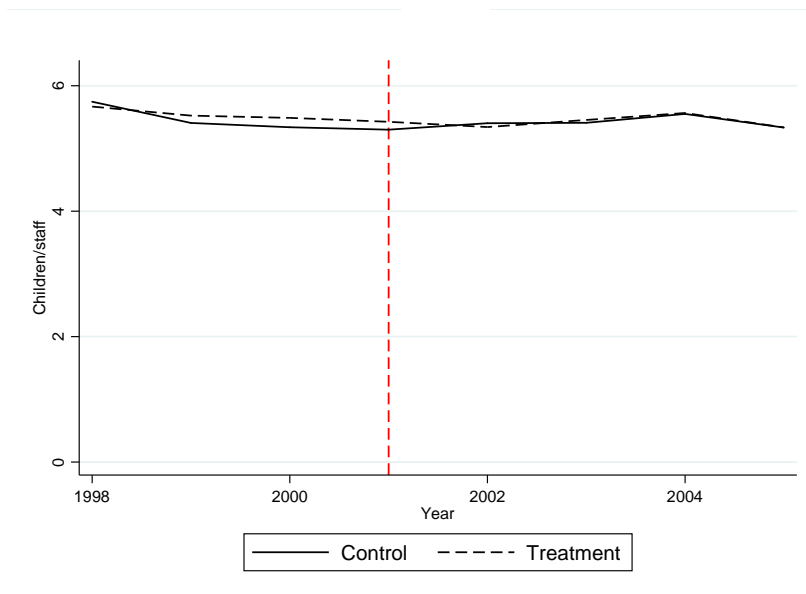
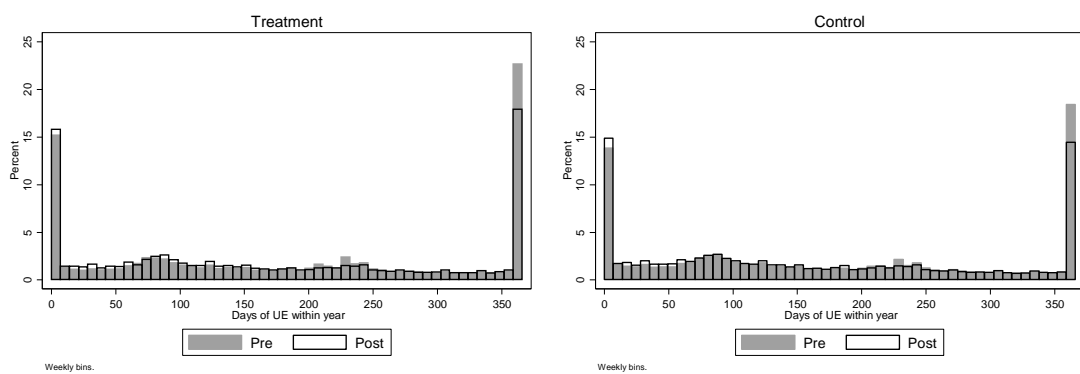


Figure A 4 Number of days in a year for which children with unemployed parents experience parental unemployment, in treatment and control municipalities pre (1998–2000) and post (2002–2004) reform



## Tables

Table A 1 Municipal level characteristics in 2001

	Control	Treatment	All
<i>Municipal budget, spending/capita</i>			
Sports and recreation	951.45 (321.29)	963.23 (318.78)	1,007.52 (335.83)
Childcare	4,598.17 (796.13)	4,034.55 (547.01)	4,324.60 (779.36)
Family support	2,074.75 (798.93)	1,940.51 (566.11)	1,957.50 (723.00)
Education	12,541.64 (1,394.10)	12,650.48 (1,184.22)	12,728.06 (1,392.85)
<i>Other municipal characteristics</i>			
Left wing majority	0.33 (0.47)	0.37 (0.49)	0.39 (0.49)
Population share 1-5 year olds (%)	5.11 (0.80)	4.86 (0.55)	4.99 (0.70)
Childcare participation	65.73 (8.62)	59.56 (8.74)	62.35 (9.78)
Staff at childcare / child (municipal)	5.44 (0.15)	5.47 (0.11)	5.32 (0.62)
Staff at childcare / child (private)	5.54 (0.16)	5.70 (0.22)	5.50 (0.81)
Unemployment rate, 25-34 year olds	10.39 (3.61)	11.11 (3.24)	11.15 (3.63)
Population	45,389 (100,929)	20,738 (202,50)	30,827 (58,499)
N	75	75	289

Table A 2 Enrollment in reform and control municipalities from Parent Survey

		Pre reform 1999		Post reform 2002	
		Reform municipality	Control municipality	Reform municipality	Control municipality
Unemployed	Enrollment	0.57	0.78	0.81	0.82
	Hours/week	20.85	24.83	20.96	24.39
Employed	Enrollment	0.90	0.93	0.95	0.97
	Hours/week	30.41	33.21	30.68	32.94
Total	Enrollment	0.85	0.91	0.94	0.95
	Hours/week	29.55	32.15	29.80	32.05

Table A 3 ICD10 diagnosis codes of the study

Variable	Definition	Example
<b>Hospitalizations</b>		
Hospitalization	=1 if admitted to hospital for any reason	
Infection	=1 if admitted to hospital with diagnoses code ICD10 A00-A99, B10-B84, B90-B99, B00-B09, B85-B89	Infectious diarrhoea, mononucleosis, chickenpox.
Respiratory	=1 if admitted to hospital with diagnoses code ICD10 J07-J08; J19-J39; J48-J99	Upper and lower respiratory infections.
Injury and poisoning	=1 if admitted to hospital with diagnoses code ICD10 S00-S99, T00-T35, T36-T65, T66-T98	Broken arm or ankle, overdose of a medication.
Psychiatric	=1 if admitted to hospital with diagnosis codes F00–F99	Insomnia, behavior disorder, anxiety, depression
<b>Prescriptions</b>		
Antibiotics	=1 if receives prescription to medication with ATC code J01	Ear infection, urinary infection.
Respiratory	=1 if receives prescription to medication with ATC codes R01-R06	Asthma-related, cough.
Mental	=1 if receives prescription to medication with ATC codes N06B, N06A, N05	ADHD, depression, insomnia

Table A 4 Difference-in-differences estimates, including different sets of covariates.

	Children age 2–3 years			Children age 4–5 years		
<b>Any diagnosis</b>						
1998	-2.328 (3.536)	-2.285 (3.526)	-2.043 (3.479)	-0.569 (2.635)	-0.453 (2.645)	-0.586 (2.628)
1999	-2.129 (3.603)	-2.116 (3.598)	-2.333 (3.614)	0.717 (2.272)	0.768 (2.271)	0.622 (2.259)
2001	-1.106 (3.308)	-1.062 (3.311)	-1.249 (3.282)	0.721 (2.576)	0.692 (2.562)	0.713 (2.553)
2002	-3.081 (3.651)	-3.300 (3.660)	-3.139 (3.677)	3.988 (2.777)	3.908 (2.810)	3.913 (2.805)
2003	-3.171 (3.571)	-3.582 (3.551)	-3.572 (3.572)	0.618 (2.667)	0.490 (2.676)	0.196 (2.683)
2004	-0.882 (3.702)	-1.375 (3.722)	-1.523 (3.787)	3.918 (2.461)	3.729 (2.475)	3.395 (2.467)
<b>Respiratory</b>						
1998	-2.378 (1.976)	-2.316 (1.974)	-2.251 (1.949)	-0.098 (1.727)	-0.057 (1.731)	-0.111 (1.729)
1999	-0.990 (2.087)	-0.972 (2.083)	-0.998 (2.075)	-2.149 (1.564)	-2.120 (1.564)	-2.199 (1.585)
2001	-0.201 (1.763)	-0.169 (1.763)	-0.322 (1.753)	1.077 (1.693)	1.036 (1.683)	1.097 (1.699)
2002	-0.701 (2.232)	-0.815 (2.253)	-0.881 (2.283)	0.277 (1.442)	0.202 (1.443)	0.223 (1.424)
2003	-3.417 (2.348)	-3.619 (2.343)	-3.616 (2.358)	-2.174 (1.929)	-2.286 (1.918)	-2.438 (1.928)
2004	-1.466 (2.365)	-1.684 (2.362)	-1.725 (2.382)	0.397 (1.503)	0.267 (1.505)	0.162 (1.471)
<b>Injury</b>						
1998	-1.308 (1.472)	-1.292 (1.474)	-1.199 (1.476)	-1.965* (1.187)	-1.903 (1.190)	-1.992* (1.202)
1999	0.379 (1.476)	0.380 (1.479)	0.320 (1.489)	0.275 (1.468)	0.295 (1.464)	0.355 (1.468)
2001	-1.337 (1.655)	-1.307 (1.656)	-1.245 (1.657)	-1.809 (1.459)	-1.839 (1.461)	-1.807 (1.458)
2002	-1.712 (1.512)	-1.749 (1.519)	-1.615 (1.515)	-0.081 (1.638)	-0.106 (1.640)	-0.137 (1.661)
2003	-1.360 (1.450)	-1.424 (1.451)	-1.345 (1.441)	-0.737 (1.268)	-0.741 (1.266)	-0.707 (1.292)
2004	0.803 (1.598)	0.749 (1.599)	0.731 (1.602)	-0.646 (1.275)	-0.683 (1.278)	-0.760 (1.288)
<b>Infection</b>						
1998	0.611 (2.141)	0.553 (2.139)	0.729 (2.106)	0.180 (1.022)	0.150 (1.025)	0.163 (1.027)
1999	-1.658 (2.090)	-1.679 (2.085)	-1.837 (2.112)	0.358 (0.934)	0.334 (0.931)	0.296 (0.927)
2001	2.323 (1.647)	2.328 (1.645)	2.196 (1.657)	-0.949 (1.081)	-0.947 (1.080)	-1.002 (1.070)
2002	-0.700 (1.965)	-0.798 (1.967)	-0.587 (1.945)	2.477** (1.211)	2.452** (1.214)	2.421** (1.212)
2003	-0.685 (1.588)	-0.843 (1.611)	-0.805 (1.593)	1.743 (1.154)	1.716 (1.149)	1.611 (1.138)
2004	-1.834 (2.018)	-2.077 (2.035)	-2.011 (2.055)	1.153 (1.076)	1.100 (1.072)	0.994 (1.069)
N	315,562	315,562	315,562	321,576	321,576	321,576
Child controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental controls	No	Yes	Yes	No	Yes	Yes
Municipal controls	No	No	Yes	No	No	Yes

Note: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01 All regressions control for municipal fixed-effects. Standard errors are clustered at municipality level.

Table A 5 Difference-in-differences estimates, heterogeneous effects with respect to maternal education: Children age 2–3 years old.

	Compulsory (1) Infection	Upper secondary (2) Infection	Higher (3) Infection
1998	2.981 (4.850)	0.716 (2.558)	-2.527 (3.627)
1999	5.786 (3.895)	-3.485 (2.536)	-5.647* (3.173)
2001	1.051 (4.326)	1.319 (2.237)	3.150 (3.862)
2002	-5.153 (4.140)	-0.839 (2.569)	3.543 (3.678)
2003	0.294 (4.299)	-0.608 (2.468)	-3.400 (3.171)
2004	0.286 (4.780)	-2.742 (2.354)	-1.934 (4.097)
N	60,455	175,186	73,345
Mean outcome	19.6	17.0	13.4

Note: Standard errors clustered at municipality level. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

Table A 6 Difference-in-differences estimates, heterogeneous effects with respect to maternal education: Children age 4–5 years old.

	Compulsory (1) Infection	Upper Secondary (2) Infection	Higher (3) Infection
1998	0.098 (2.485)	0.438 (1.208)	0.608 (1.934)
1999	1.866 (2.317)	-1.417 (1.302)	4.194** (2.051)
2001	-0.693 (3.094)	-0.701 (1.225)	-0.709 (2.897)
2002	10.106*** (3.208)	0.862 (1.394)	1.787 (2.423)
2003	2.776 (2.422)	3.016** (1.383)	-1.698 (2.087)
2004	0.820 (2.146)	0.846 (1.490)	1.517 (2.402)
N	62,207	181,747	72,205
Mean outcome	6.1	6.5	5.0

Note: Standard errors clustered at municipality level. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

Table A 7 Past experiences of children aged 4–6 with unemployed parents during the post-period

	Compulsory	Upper	Higher
Any parental UE while 2-3 years	0.90 (0.29)	0.90 (0.30)	0.87 (0.34)
Months parental UE while 2-3 years	13.72 (8.01)	13.41 (8.06)	11.44 (8.04)
Parents received student benefit while 2-3 years	0.18 (0.39)	0.13 (0.34)	0.27 (0.45)