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Evidence from Swedish childcare access reform

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Does childcare improve the health of children with unemployed parents? [a]

Evidence from Swedish childcare access reform

by

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Abstract

We analyze how access to childcare affects the health outcomes of children with unemployed parents using a reform that increased childcare access in some Swedish municipalities. While we find no effects of childcare access on hospitalization for 2- to 3-year-olds, our results suggest that 4- to 5-year-olds were more likely to be hospitalized due to infections when they first gained access to childcare. Children aged 10 to 11 years who had access to childcare earlier in their childhood, while their parents were unemployed, were less likely to be prescribed medication for respiratory conditions and allergies. Taken together, our results suggest that the immediate health consequences of childcare access for children of unemployed parents are limited. Our findings support previous evidence that the greater exposure to microorganisms induced by childcare attendance may reduce the risk of developing allergies and asthma.

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.[1] Understanding how to improve the health outcomes of disadvantaged children is highly relevant because poor childhood health has been shown to have persistent effects on child development and adult outcomes.[2] One policy that could help equalize opportunities is the provision of high quality childcare. Given past evidence that childcare improves cognitive outcomes, especially among disadvantaged children[3], this paper asks whether access to high-quality childcare can improve the health outcomes of children with unemployed parents. More specifically, we estimate how childcare access at ages 2–5 years affects immediate physical health as well as physical and mental health at ages 10–11 years. To this end, we use rich registry 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 in 2001, offering childcare to these children became mandatory. Comparing the change in the health of children of unemployed parents who resided in municipalities that had to change policy with the corresponding change for children of unemployed parents living in municipalities that already offered childcare to these children before the reform, we estimate the causal effect of access to childcare in a difference-in-differences framework.

A large body of literature has explored the associations between attending childcare and short-term health outcomes, such as the prevalence of respiratory infections, diarrheal illness and the use of antibiotics. These studies typically find that attending childcare is associated with a temporary increase in the prevalence of diseases and antibiotics prescriptions, followed by a period with a reduced likelihood of illness and no differences in the long run (see, e.g., Lu et al., 2004; de Hoog et al, 2014; Ball et al., 2002). These

[1] This has been shown in the U.S. for birth weight (Lindo, 2011) and parent-reported health and mental health information (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).

[2] In her survey, Currie (2009) presents evidence that low birth weight has been associated with reduced test scores, 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.

[3] 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 the child's age when attending childcare, seem to influence the effects of childcare on cognitive development (see Cascio, 2015 for an overview).

studies point to an acquired immunity that seems to fade over time. Childcare attendance at young age has been connected to a lower likelihood of developing asthma and allergies later in childhood, which has been explained by the so-called *hygiene hypothesis* (Strachan, 1989; Ball et al., 2000).

Papers aiming to estimate the *causal effects* of childcare using experimental and quasi-experimental methods are less common. Baker et al. (2008) report 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 has 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).[4] In a study of southern Sweden using registry 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 tend to have more infections at a younger age but fewer infections at ages 6–7 years.[5] They refer to this as a *substitution effect*. These two papers thus support the earlier evidence that attending childcare has immediate effects on physical illnesses, such as infections and colds. Liu and Skans (2010), on the other hand, do not find any effects on hospitalizations among 1- to 16-year-olds as a result of a parental leave reform that likely led families to substitute formal childcare for parental care during the child's second year of life.[6]

Earlier evidence of the effects 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 when being enrolled in childcare. 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 are less likely to experience behavioral problems, such as developmental and behavioral disorders. Similarly, Yamaguchi et al. (2017), in a study based on Japanese data, find that childcare reduced inattention and hyperactive behavior among 2.5-year-old children of mothers with low education levels. There is also evidence that enrollment age may be a factor in the health effects of attending childcare. Kottenlenberg and Lehrer (2014) study the Quebec reform and show that the negative effects of childcare are mostly

[4] In particular, Baker et al. (2015) also find negative effects on self-reported health and life satisfaction at ages 12–20 years. They also find lasting negative effects on non-cognitive skills.
[5] The analyzed fee reductions were the result of a reform in 2001 that harmonized the fees across municipalities and substantially lowered them.
[6] Parental leave was extended from 12 to 15 months in 1989.

driven by children who started childcare at early ages.[7] For three-year-olds, however, attending childcare confers benefits in terms of better development scores.

We contribute to the existing literature in several ways. First, we focus on the effects of childcare access for children of unemployed parents. These children arguably have particular policy relevance because of their vulnerability. Second, compared to much of the earlier literature on childcare, we focus on child health and provide estimates related to childcare exposure at toddler and preschool ages. In addition, we follow the children and explore the effects of childcare at 10–11 years of age. Third, our study has some data-related and methodological advantages. We use registry data on in-patient care and prescription drugs to measure child health outcomes. These data 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 is affected by the fact that the children are in childcare. A further advantage in relation to the study by van den Berg and Siflinger (2018), which otherwise uses similar high quality data, is that we are able to control for health trends because we rely on regional variation in reform exposure to identify causal effects.[8]

We rely on registry data to measure health outcomes. Specifically, we use data from the National Patient Register, which contains information on all registered hospitalizations in Sweden, including detailed information about diagnoses. Incidents that lead to inpatient hospital care are, of course, rather serious, and we are not able to consider less severe health problems using these data.[9] Therefore, as a complementary method, we also analyze prescriptions for medication. Unfortunately, prescription drug data are only available since 2005, so this outcome cannot be considered in the short-term analysis.

Because Swedish registries 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. Using survey data, we show that enrollment

[7] 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 were not due to initial implementation problems.

[8] van den Berg and Siflinger use that different cohorts were exposed to lower childcare fees for different numbers of years, depending on their age when the maximum fee reform was introduced. Hence, although 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 among municipalities, but do not find any statistically significant heterogeneity.

[9] On the other hand, one might argue that more severe health problems are likely to result in negative long-term outcomes and are therefore more interesting.

increased substantially, by 20 percentage points, among children with unemployed parents in treated municipalities compared to control municipalities, implying the existence of a "first stage".

Our results suggest that access to childcare had no effect on hospitalization rates for children aged 2–3 years for any of the diagnoses that we investigate. This result is in line with Liu and Skans (2010), who find no effect of the increase in parental care on hospitalization during children's second year of life. For preschool children aged 4–5, we find that hospitalizations due to infections increase by 2.4 additional children per 1,000 the first year after the reform, but we find no effects on overall hospitalizations, nor any effects on the other diagnoses that we investigate. 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-term analysis, we find no evidence that earlier access to childcare affects hospitalizations at ages 10–11 years. Our results, however, give support to the so-called hygiene hypothesis. Access to childcare reduced prescriptions for allergies and asthma at ages 10-11, in particular, for first born children who are less likely to be exposed to microorganisms in early childhood in their home environment. One additional year of childcare access for this group, decreases the likelihood of being prescribed medicine for asthma and allergies by 11.4 percent. For children with older siblings we find no effect. For ADHD medication and psycholeptics (prescribed to treat anxiety or sleeping problems), estimates suggest that gaining access to childcare may increase mental health problems, but standard errors are large, and we cannot rule out zero or positive effects.

As earlier evidence shows that family characteristics, such as the parents' education level, influence the impact of childcare on cognitive skills, we study whether there are heterogeneous effects related to the mothers' education level. We find that the immediate increase in infections among preschool children is driven by children whose mothers have only compulsory education. Because we find that the parents with low education are as likely as parents with other education levels to have been employed when the child was in childcare age, we can rule out the possibility that the increase in hospitalization rates is a result of these children's lower exposure to childcare at an early age. Potential alternative explanations are that exposure to the childcare environment has larger effects on children whose parents have low education levels because they are more vulnerable,

the parents are less likely provide appropriate preventive and primary care or they attend childcare centers where infections are more common. In the medium-term analysis, we find no differences by maternal education.

The rest of the paper is organized as follows. In the next section, we discuss the 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. In section 3, we describe the institutional setting and the reforms that allow us to identify the causal effects. Section 4 presents the quasi-experimental strategy, and section 5 presents our data. We then present the results in section 6. Finally, section 7 presents our conclusions.

2 How is the mode of care likely to affect child health?

Is a child more likely to suffer injuries at home or at a childcare center? Does the mode of care affect whether respiratory conditions lead to hospitalizations and a need for medication later in life? How is the likelihood of catching serious infections affected by attending center-based childcare? To what extent does access to childcare affect behavioral problems and psychological well-being during the 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 a professional staff trained in early childhood education and development in a facility especially designed for children. This may increase the likelihood that health problems will be detected early and hence exposure to preventive health measures that reduce the need for hospitalization. Furthermore, these factors may also reduce the risk of injuries and poisoning and stimulate the child's psycho-social development. However, a group of children is also a fertile environment for the spread of 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-term effects, it has been argued that exposure to infectious and noninfectious microorganisms in early childhood may reduce the risk of developing asthma and allergies later in childhood, the so-called *hygiene hypothesis* (Strachan, 1989). Almond and Currie (2011), however, also discuss the possibility that poor health in early childhood can make a child more sensitive to illness later in life. The results of observational studies tend to show that entering

childcare only triggers a timing effect on *when* the child contracts infections and respiratory conditions.[10] 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).[11]

Compared to a facility designed for the care of children, the home environments of most children are full of potentially dangerous products, such as detergents and kitchen knives. However, parents are experts on their own children and can focus more on the individual child than is possible for 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 to children's attachment to their parents and therefore their psychological well-being later in childhood (see, e.g., NICHD-ECCRN, 2003). However, unemployment can be stressful and may negatively affect the quality of parenting.[12] Parenting quality may also be affected when children attend childcare centers if parents learn parenting skills from teachers or other parents or if parents experience less stress as a result of a respite from having to attend to and care for their children full-time. Yamaguchi et al. (2017) show that children's enrollment at childcare centers improved parenting quality among mothers with low levels of education. Other indirect effects of childcare access may be that unemployed parents can find employment sooner when they can spend more time on their job search, which will increase the family income. [13]

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. The effects are likely to differ depending on the health outcome considered. The effects can also be expected to differ depending on the quality of the care provided by the parents and the childcare facility. This is relevant for diagnoses related to injuries and respiratory conditions, for which home conditions, such as child safety awareness and the indoor

[10] Ball et al. (2002), for example, finds that attendance at large daycare centers is associated with more common colds during the preschool years but fewer during the early school years. At age 13 years, no differences remained between children who had and had not attended childcare. This effect is what van den Berg and Siflinger (2018) call a substitution effect.

[11] The possibility that children in childcare may also suffer from fewer one-to-one interactions with adults is also supported by evidence from Fort et al. (2016), who find that attending childcare at ages 0–2 years reduces IQ at ages 8–14 for children in advantaged families.

[12] For example, Eliason and Storrie (2009) and Browning and Heinesen (2012) show that individuals who experienced job loss due to a plant closure experienced negative health consequences. Furthermore, Eliason (2011) and Huttunen and Kellokumpu (2016) find an increased risk of divorce following a job loss.

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

environment, as well as caregivers' awareness of early signs of illnesses, are likely to matter. We would expect an increased likelihood of infections when children first attend childcare (or start school, if they did not attend childcare at earlier ages). However, whether infections require hospital care is likely to depend on both their severity and the quality of preventive or primary care available to the child. Although there are no direct benefits of exposure to infections during childhood in terms of long-term gained immunity, the hygiene hypothesis suggests that more exposure to infectious and noninfectious microorganisms during early childhood may impact the likelihood of developing allergies and asthma (Strachan, 1989). Ball et al (2000) show that children with older siblings and children who attend childcare the first 6 months of life are protected from developing asthma and frequent wheezing later in childhood. Hence, one might expect less need for medication for asthma or other allergy-related respiratory conditions when more children are exposed to childcare. Finally, regarding mental health and behavioral problems in the medium term, 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 mothers with low levels of education reduces inattention and hyperactivity symptoms. In this study, we attempt to capture the effects of childcare on physical and mental health discussed in this section by investigating hospitalizations related to injuries, infections, respiratory conditions, and prescriptions for medicines to treat infections (anti-infectives), respiratory conditions, asthma and allergy-related conditions and mental and behavioral problems (ADHD medications and psycholeptics).

3 Childcare and health care in Sweden

3.1 Childcare and the reform

In 2000, as much as 66 percent of Swedish children aged 1–5 years attended publicly funded childcare. Swedish childcare is heavily subsidized and of high quality, and the 290 municipalities are responsible for providing childcare for 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, starting when the child turned one and continuing until the child started school (i.e., in the fall of the year the

child turns six).[14] The average enrollment age for children born in 1999 was 18 months (Duvander, 2006).[15] Whether to provide childcare for children whose parents were unemployed or on parental leave with a younger sibling was left to the discretion of each municipality.[16] In July 2001, a new law came into place requiring municipalities to offer at least 15 hours per week of childcare for children whose parents were unemployed. This paper exploits this policy change to isolate a causal effect of access to childcare for children with unemployed parents on child health.

The aim of the policy was to increase childcare enrollment among disadvantaged children and to facilitate job finding for unemployed parents. Other policy changes in 2002 and 2003 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 of childcare fees; the following year, children whose parents were on parental leave with a younger sibling were granted access to 15 hours per week of childcare. In addition, 4- and 5-year-old children were offered free childcare for 525 hours per year beginning in 2003.[17]

Swedish childcare is of high quality. The OECD family database, uses two indicators to capture childcare quality: child-to-staff ratios and the minimum qualifications required for childcare staff. Sweden rates high in both dimensions.[18] Both before and after the reform, the child groups were relatively small (approximately 17 children per group), and the child-to-staff ratio was low (5.3–5.5 children per staff member). Approximately 50 percent of the childcare employees had a university degree from a preschool teacher-training program, and 40 percent of the childcare employees had an appropriate vocational high school degree with specialization in caring for young children in day

[14] Compulsory school formally starts at age seven, but most children participate in a voluntary preschool year organized by schools for six-year-olds. Parents in Sweden are entitled to 16 months of paid parental leave.
[15] A small fraction of children are cared for in "family care" units as opposed to center based care. In the year 2000, 3-4 percent of all 1- to 12-year olds were enrolled in family-based childcare or afterschool care (Statistics Sweden, 2001).
[16] 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.
[17] In 2002, childcare fees were harmonized across municipalities, and average fees were 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 children were lower, and the fourth child attended 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 of childcare cost on child health by comparing the health of children in cohorts that paid higher fees with the health of children in cohorts that paid lower fees. In January 2003, municipalities were obliged to offer 525 hours of free-of charge preschool per year for all children ages 4–5 years.
[18] https://www.oecd.org/els/soc/PF4-2-Quality-childcare-early-education-services.pdf

care.[19] Important for the results of the present study, there is no indication that the quality of childcare changed because of the studied reform; staff ratio and child groups remained stable compared to before the reform (Mörk et al., 2013). One reason the staff ratios did not decrease is that the central government introduced additional intergovernmental grants to compensate for cost increases caused by the reform.

3.2 Health care for children

When studying the health outcomes of children in terms of hospitalizations and 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 until a child starts school, after which the School Health Care Program takes over the responsibility. These programs are free of charge and have almost 100 percent enrollment.[20] Additionally, 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 must pay per year. During the period studied in this paper, the cap on health care expenses was SEK 900 (equivalent to EUR 113) during a twelve-month period. In most counties, people younger than 20 years of age do not pay any patient fees.

For hospital care, counties were not allowed to charge more than 80 SEK per day and night during the studied period. For prescription drugs, individuals payed the full cost up to 900 SEK, after which costs decreased gradually, and nobody had to pay more than 1,800 SEK during a twelve-month period.[21] Families add up the expenditure on patient fees and prescriptions for all their children (0- to18-year-olds), and the caps above are applied to the total costs for all children (Ds 2011:23). Thus, health care costs should not be an obstacle to receiving care, even for low-income families.

[19] The information about childcare quality is taken from The Swedish National Agency for Education's yearly reports "Beskrivande data om barnomsorg, skola och vuxenutbildning".
[20] See Socialstyrelsen (2014).
[21] Costs were reduced by 50 percent for amounts between 900 and 1,700, by 75 percent for amounts between 1,700 and 3,300, by 90 percent for amounts between 3,300 and 4,300, and by 100 percent for amounts above 4,300.

4 Empirical strategy

Our aim is to investigate how access to childcare affects the health of children with unemployed parents. We investigate both short-term effects, i.e., health outcomes in the year that children had or did not have access to childcare, and effects in the medium term, 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 survey data evidence that the reform led to a substantial increase in enrollment among children with unemployed parents. Below, we present the identification strategy for the short-term and medium-term effects.

4.1 Identifying the short-term effects

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

$$y_{imt} = \sum_{t=1998}^{2004} \mu_t TREATED_{imt} + \delta \boldsymbol{X_{it}} + \pi UE_{mt} + \tau_t + \varphi_m + \varepsilon_{itm} \qquad (1)$$

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 a value of one for children who at time t live in municipalities that changed their policy due to the reform, and where τ_t and φ_m are year and municipal fixed effects, respectively. We also control for the municipal unemployment rate for individuals aged 25–34 years (UE_{mt}) and for 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, birth order, maternal age at first birth, maternal and paternal level of education (compulsory schooling, upper-secondary schooling, higher education or unknown), and region of birth (Nordic or non-Nordic), captured by the vector $\boldsymbol{X_{it}}$. The parameters of interest are the μ_t for $t \in [1998,2004]$.[22] We chose the year 2000 (the year before the reform was introduced) as

[22] We limit the study to the years 1998–2004. One reason is that information on municipality policy regarding access to childcare for unemployed parents is based on responses to a survey conducted in 1998 and 2001. Estimations of the model that include years before 1998 would rest on the assumption that all 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 were introduced in the summer of 2001, and it is unclear to what

the reference year to which the other years are compared. If being at home with an unemployed parent or receiving 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 who have access to childcare than for those who are cared for at home. Looking at the estimated coefficients for the years before the reform (i.e., $\mu_{1998,1999}$), we can observe whether the pre-reform trends in health among children exposed to parental unemployment in the treatment 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 estimations capture causal effects.

4.2 Identifying the medium-term effects

To identify the medium-term effects, i.e., the effects on health outcomes when the children are 10–11 years old, we exploit the fact that the children were denied access to childcare for different numbers of years, depending on their exposure to parental unemployment and the municipal policy for offering childcare to children with unemployed parents. More specifically, we estimate the following triple-difference (DDD) specification:

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

where y_{imc} is the health outcome at age 10–11 years 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 years, 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 among cohorts. Since the reform was implemented in July 2001, 2001 is counted as a half year. θ_{cm} are municipality-specific cohort effects, and $\boldsymbol{X_i}$ is a vector of child-specific control variables (the same that was used for the short-term analysis but measured when the child is two years old). The parameter of interest is α,

extent the municipalities that previously had not offered childcare for children with unemployed parents were able to offer slots to these children in the second half of 2001, it is unclear whether we can expect to see any effects in 2001.

which shows how exposure to parental unemployment without access to childcare affects health 5–6 years after the child was of childcare age (at ages 10–11 years).

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 children of unemployed parents in treated municipalities would have evolved in the same way as the health of similar children in control municipalities, had they had access to childcare before the reform. This assumption will not hold if there are other societal changes that affect hospitalization rates in treated and control municipalities differently. Although this assumption cannot be formally tested, we can study whether it is plausible. We do this by studying the 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.[23] 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 conclude that selection into unemployment is not a serious issue. We address the potential selection on the intensive margin by not conditioning on the length of parental unemployment but instead considering any exposure to 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 based on parental employment status across cohorts in treated and untreated municipalities that are unrelated to childcare enrollment. Ideally, we would like to test this identifying assumption using a placebo specification using older cohorts, but due to the limited period for which prescription data are available, this is not possible.

As discussed earlier, the analyzed reform was followed by other childcare reforms that may also have increased enrollment in childcare. Most importantly, childcare became

[23] In a study of the same reform examined in this paper, Vikman (2010) finds a 17 percent increase in the likelihood of mothers finding employment when childcare is available. She finds no similar effect for unemployed fathers.

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 years. For the short-run analysis, these additional reforms would be problematic only if they increased enrollment among children with unemployed parents to a larger extent in the control municipalities than in the treated municipalities. In this case, we would likely underestimate the effect of access to childcare with the strategy outlined in section 4.1. The same is true for the identification of the medium-term effects, except that in this case, we also need to find that the reforms do not increase enrollment among children with employed parents more than among children with unemployed parents. Below, we use survey data on childcare enrollment 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 enrollment rates did in fact increase in the treated municipalities compared to those in the control municipalities.

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 the causal effects of the availability of childcare for children with unemployed parents. More specifically, we compare municipalities that changed their policy because of the reform to municipalities gave children with at least one unemployed parent access to childcare before the reform.

Information about which policies were in place in different municipalities before the reform is taken from two surveys conducted by the Swedish National Agency for Education in 1998 and early 2001. In the surveys, municipalities were asked about their childcare fees and childcare access 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 treated if children with unemployed parents did not get a slot or they lost their slot if a parent became unemployed according to both 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 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 for 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 for identifying a suitable set of control municipalities is to select them based on the 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.[24] Our aim is to identify 75 suitable control municipalities where children of unemployed parents had access to childcare before the reform. We select municipalities where i) the childcare enrollment rates 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 when the reform was introduced.[25] Using this procedure, we identify 75 municipalities that are unlikely to have been affected by the reform.

Figure A 2 in the 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 generally less populated, have fewer children of childcare age, are more likely to be governed by a left-wing majority and have somewhat higher unemployment rates compared to control municipalities. As expected, childcare enrollment rates are lower, and so is municipal spending on childcare. In the empirical analysis, we control for these differences through

[24] The survey was stratified to make the responses representative at both the municipal and national levels.
[25] The selection of control municipalities is further described in Appendix.

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 change in relation to 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 and the results should be representative for children with unemployed parents in the whole of Sweden.

5.2 Individual-level data

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

We sample all children born from 1993–2002 and their parents. When analyzing short-term effects, we keep, for each year 1998–2004, the children who were 2–5 years old at the end of the year and 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 registered as unemployed with the Public Employment Service for at least one day during a specific year. A reason for including children with very little exposure in this group is that previous research has shown that length of unemployment was affected by the reform (Vikman, 2010) and that unemployment duration is therefore endogenous to the reform. However, as is clear from Figure A 4 in the Appendix, the majority of children

experienced considerably longer periods of parental unemployment, and as much as 18 percent of the children with unemployed parents experienced parental unemployment for the whole year. The length of the unemployment periods changes over time, but the pattern of change is similar in the treatment and control municipalities.

When analyzing medium-term effects, our sample includes children exposed to parental unemployment and children whose parents are employed. We define a child as having employed parents if neither parent is registered as unemployed during the year and both parents have annual earnings that exceed a threshold defined as two times the *price base amount*.[26] Due to data limitations, we restrict this sample to children born from 1995–2000.[27] Since the new policy was introduced in July 2001, access to childcare differs across the cohorts. Figure 1 shows the ages at which 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 in 1997 had access to childcare at age 5 if the parent was unemployed. Cohorts born between 1996 and 1999 were partially treated, and cohorts born in 2000 had access to childcare their whole childhood.

Figure 1 Treatment status by cohort and age of child

Year of birth		Calendar time																
		1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
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

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 the possibility

[26] The price base amount (*prisbasbelopp)* is based on the consumer price index and adjusted annually by the government. Between the years 1998 and 2004, the amount was 36,400–39,300 SEK in nominal value (roughly 4,000 Euro).

[27] Data on prescriptions are only available for 2005-2011.

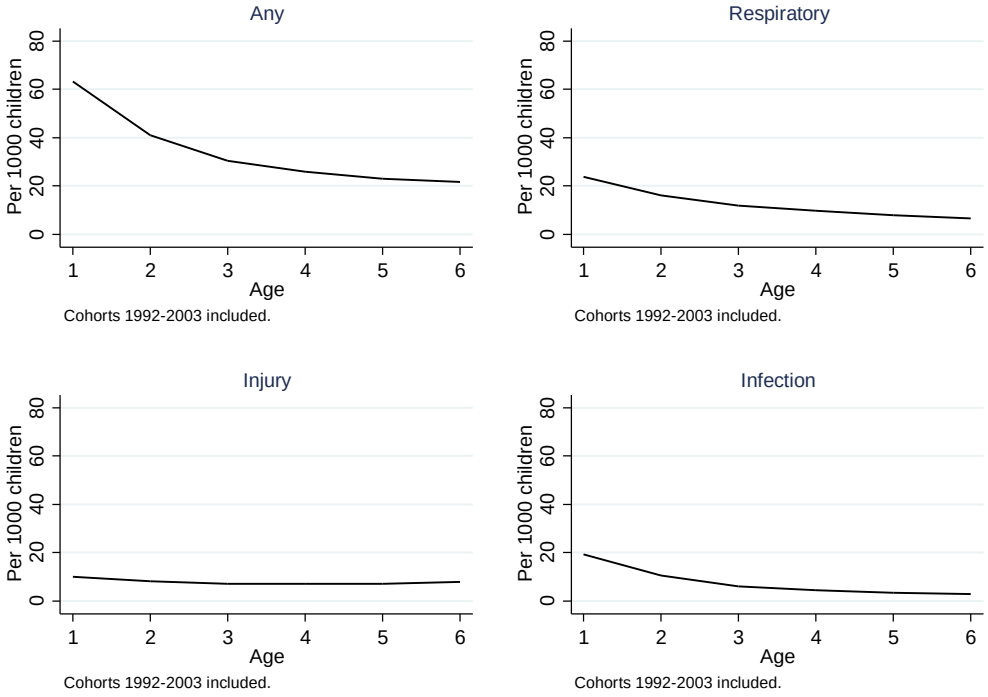
of measurement error in our treatment variable (having access to/not having access to childcare), and the estimates may therefore suffer from attenuation bias.

In the short-term analysis, we measure child health using the inpatient registry. We consider a child hospitalized during a year if (s)he is listed in the inpatient care registry at least once for that year. In addition to investigating the effects of access to childcare on hospitalization for any diagnosis, we also investigate its effect on diagnoses related to injuries, respiratory diseases and infections.[28] These diagnostic groups represent some of the most common reasons for hospitalization among children. They also cover conditions that, as argued in Section 2, may plausibly be affected if a child attends childcare rather than stays 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. Because the health of 2- to 3-year-old children is different from the health of 4- to 5-year-old children and because, as discussed in the introduction, earlier studies have found that enrollment age may be relevant to the effects of childcare attendance on health as well as on cognitive and noncognitive development, we study the two age groups separately.

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-term effects. However, although hospitalization is an objective measure in some senses, it is still the result of a professional judgment made 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, however, is such that one should expect that children who need care will eventually go to the hospital. Additionally, failure to seek primary care may result in a need for hospital care. As previously described, hospital care for children is not expensive in Sweden. Thus, cost should not be an obstacle to seeking care, even for poor families.

[28] The ICD10 codes of the diagnoses 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 registry. Both hospitalization and prescriptions are measured per 1,000 children.

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



In the medium-term analysis, we measure child health when the children are 10–11 years old using data from both the inpatient and the drug prescription registries. From the inpatient register, we create a dummy variable indicating whether the child was hospitalized at any time during the considered age span. From the drug prescription registry, we first construct indicators to describe whether the child has been prescribed any 1) anti-infectives and 2) medication for respiratory conditions in the calendar years in which the child is 10 and 11 years old. These medication groups match the hospitalization diagnoses studied, but they also capture less severe conditions. Moreover, according to the hygiene hypothesis, exposure to infectious and noninfectious microorganisms during early childhood may impact the likelihood of developing allergies and asthma. Although allergies and asthma cause respiratory conditions and are thus captured by 2) above, we also focus on a group of medications used specifically for these diagnoses. Second, to capture effects on the child's psychiatric health and behavioral problems, we create an indicator for being prescribed ADHD medication or psycholeptics, i.e., medications for sleeping problems and anxiety.[29] Hospitalizations of

[29] See Table A3 for details about diagnoses.

young children for psychiatric conditions are very rare, which is why drug prescriptions are of special interest.

Using prescription data, we pick up less severe health problems; hence, these data are a valuable complement to the hospitalization data. 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 receiving 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. Because, as described in section 3.2, high-cost protections are in place for prescription drugs, low-income households should be able to afford to collect prescribed medications. Medications which do not require prescriptions are not included in the data.

For natural reasons, registry data do not contain any information regarding the quality of parental care. Given earlier evidence of larger positive effects of childcare on cognitive outcomes for children with lower socioeconomic 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 1 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, according to the treatment status of the municipality. The 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 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 the treatment and control municipalities.

Table 1. Descriptive statistics for 2-5-year-olds, experiencing parental unemployment in the pre-reform years, 1998–2000, and the post-reform years, 2002–2004, by the treatment status of the municipality.

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

Table 2 shows the descriptive statistics for children aged 10-11 years in the first and last cohorts of our sample. Whereas the first cohort (born in 1995) only had access to childcare in cases 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. The 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 observed in Table 1.

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 for ADHD medications and psycholeptics (*psychiatric*) between the two cohorts. However, this pattern exists both in the treatment and control municipalities.

Table 2 Descriptive statistics for children ages 10–11 years old

	Cohort 1995		Cohort 2000	
	Control	Treatment	Control	Treatment
Years of UE exposure	1.35	1.58	1.47	1.71
	(1.31)	(1.33)	(1.59)	(1.65)
Proportion with some UE exposure	0.58	0.64	0.55	0.60
	(0.49)	(0.48)	(0.50)	(0.49)
Hospitalizations per 1000 children				
Any hospitalization	30.84	32.93	27.05	33.76
	(172.89)	(178.46)	(162.22)	(180.62)
Respiratory	5.73	5.24	4.45	5.25
	(75.46)	(72.21)	(66.54)	(72.30)
Injury	15.74	16.58	14.96	17.08
	(124.48)	(127.69)	(121.41)	(129.57)
Infection	3.57	4.79	3.38	4.14
	(59.64)	(69.02)	(58.08)	(64.20)
Mental	1.10	1.20	1.51	1.64
	(33.21)	(34.57)	(38.84)	(40.49)
Hospital visits	39.65	43.64	39.52	45.65
	(341.26)	(320.65)	(427.29)	(309.82)
Prescriptions per 1000 children				
Psychiatric	6.68	8.38	17.85	21.02
	(81.44)	(91.13)	(132.39)	(143.45)
Antibiotics	96.84	101.13	106.12	122.11
	(295.75)	(301.51)	(308.00)	(327.42)
Respiratory	88.85	99.08	116.22	130.58
	(284.54)	(298.77)	(320.49)	(336.96)
Asthma and allergy	61.63	69.85	80.42	90.32
	(240.49)	(254.90)	(271.94)	(286.65)
Number of observations	38,940	17,552	35,752	15,224

6 Results

In this section, we investigate how having access to childcare at ages 2–5 years affects contemporaneous hospitalization rates for children of unemployed parents as well as how it affects hospitalization rates and drug prescriptions in the medium term (at ages 10–11 years). Before turning to the reduced form estimates, we present indicative evidence of a "first-stage", i.e., that the reform did indeed increase enrollment rates for children with unemployed parents.

6.1 Did the reform increase enrollment rates?

An important question is whether the reform had any impact on childcare enrollment, i.e., whether there is a first stage. We cannot measure this at the individual level because of a lack of registry data. Instead, we rely on the 1998 and 2002 waves of the Parent Survey

discussed in section 5.1 to investigate how the enrollment rates for children with unemployed parents changed due to the reform.

Table 3: 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
		All		
Reform Effect	0.203***	0.211***	0.192***	0.194***
	(0.036)	(0.032)	(0.035)	(0.033)
Observations	5,306	5,306	45,533	45,533
R-squared	0.078	0.144	0.073	0.093
		2-3-year-olds		
Reform effect for 2- to 3-year-olds	0.265***	0.272***	0.255***	0.256***
	(0.051)	(0.049)	(0.048)	(0.049)
Observations	2,147	2,147	18,502	18,502
R-squared	0.146	0.164	0.077	0.084
		4-5-year-olds		
Reform effect for 4- to 5-year-olds	0.148***	0.159***	0.137***	0.141***
	(0.048)	(0.047)	(0.046)	(0.047)
Observations	2,259	2,259	22,482	22,482
R-squared	0.106	0.113	0.059	0.062
Time and municipality 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.

Table 3 presents the first stage results for all children, for 2- to 3-year-olds and for 4- to 5-year-olds.[30] 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 shows estimates without controls, and column 2 includes controls for parental education and child age and sex). The estimated effect is very stable and suggests a 20–21 percentage-point increase in enrollment as a result of the reform for all children. The panels below show that the increase was larger for the younger children. Because there were other reforms during the same period that may have affected the enrollment of other groups, we also provide DDD estimates where we include children of employed parents as an additional control group. 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. This 19–20 percentage point increase in enrollment compared to the pre-reform enrollment rate of 57

[30] Information on enrollment and average hours in childcare in the control and reform municipalities for children of employed and employed parents is displayed in Appendix Table A 2.

percent for children with unemployed parents implies an increase of 34–37 percent in overall childcare enrollment. [31] We also estimated the first stage for different parental educational backgrounds. The increase in enrollment is similar across maternal education groups.[32]

6.2 Effects on child health in the short term

To obtain the short-term effects of access to childcare when parents are unemployed on child health, we estimate equation 1. For the reasons described above, we estimate the model separately for children ages 2–3 and 4–5 years.

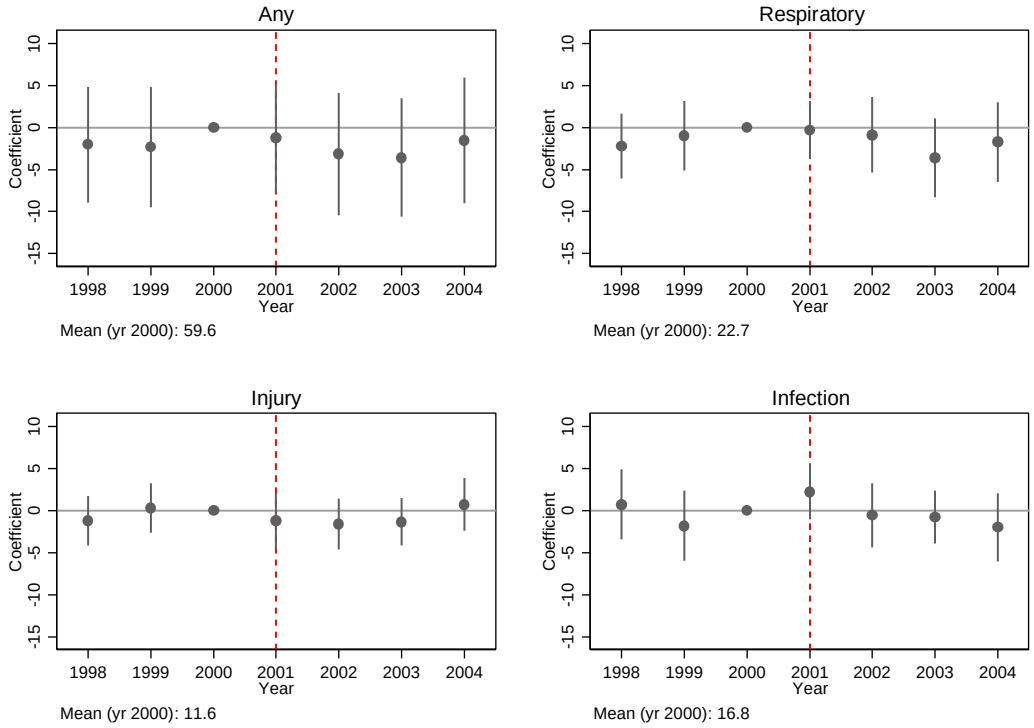
The descriptive statistics in Table 1 shows that the parents in the treatment and control municipalities differ somewhat in characteristics. We are therefore interested in whether the results are sensitive to the inclusion of control variables. This is especially important because selection into unemployment could differ depending on whether children with unemployed parents have access to childcare. 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, mother's age at first birth, whether the mother or father is of non-Nordic origin and the mother's number of children. Third, we add the municipal unemployment rate (among 25- to 34-year-olds). The results, available in Table A 4 in the Appendix, show that the point estimates of our main interest (the estimates of μ_t in equation 1) are very similar across specifications. We take this as evidence of similar sorting into unemployment in the treatment and control municipalities before and after the reform. To increase precision, we focus on the full specification in the rest of the paper.

Figure 3 shows the differences in hospitalization rates between the children of unemployed parents in the control and treatment municipalities for children aged 2–3 years for the years around the year of the reform, 2000. Vertical lines show a 95-percent confidence interval. 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

[31] 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. Additionally, it was possible for a parent to only be unemployed for a short time and for the child to have not yet lost the childcare slot.
[32] Results available upon request.

in treated and control municipalities before the analyzed reform of 2001, in which case we would be concerned that the identifying assumption of parallel trends is violated. The coefficients for the years 2002, 2003 and 2004 measure the effect of access to childcare for children with unemployed parents. Since the reform was introduced in the middle of 2001 and it is unclear how quickly the municipalities implemented it, we are not certain to what extent children with unemployed parents actually had access to childcare in the treated municipalities that year. We present the coefficient for that year for completeness.

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



Note: All regressions control for child's sex, child's age (in months at the end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother's region of birth (Nordic, non-Nordic), number of children aged 0–10 years in the family, municipal unemployment, and municipal and year fixed effects. Vertical lines show a 95-percent confidence interval. Standard errors are clustered at the municipal level.

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 access to childcare did not affect hospitalization. For any hospitalization and for hospitalization due to respiratory diagnoses, there is some indication of a decrease, with approximately 3–4 fewer children per 1,000 in the two/one years following the reform, respectively. However, these effects are not statistically significant and, at least for any hospitalization, are small in size.

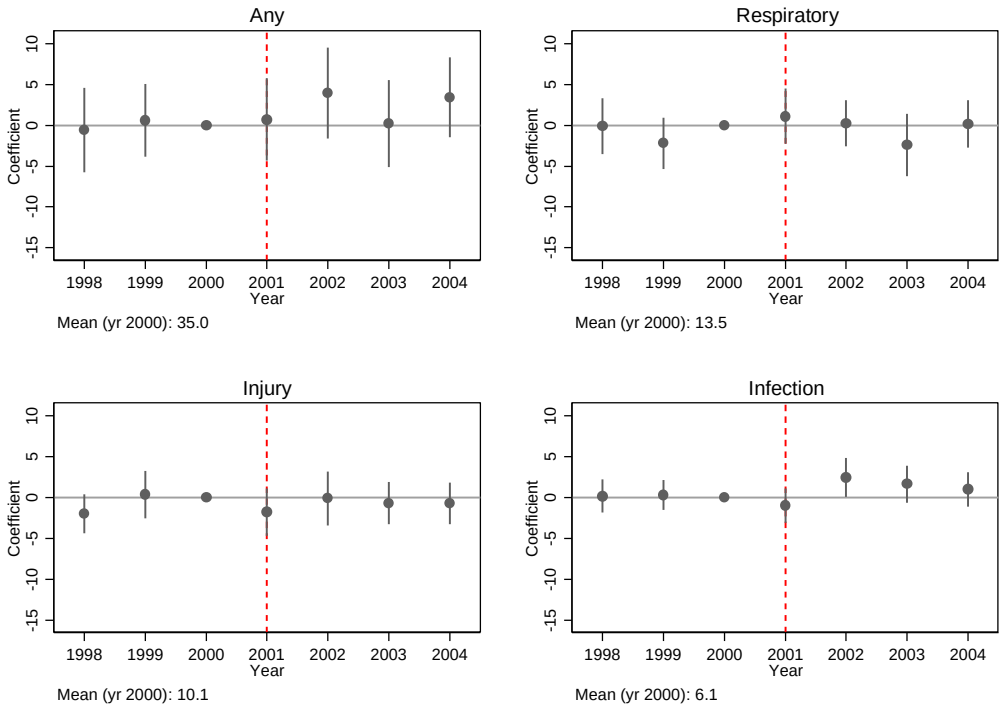
Next, we turn to the preschool-aged children (ages 4–5 years). The corresponding results are presented in Figure 4. For this age group, there are also no indications of differential health trends before the reform was implemented in 2001. Once the reform is introduced, however, there is a statistically significant positive effect for the year 2002 on hospitalizations for infections, which indicates that children with unemployed parents are more likely to be hospitalized due to infections once they have access to childcare. The increase in hospitalization with access to childcare is 2.42 more children hospitalized annually per 1,000 children, which corresponds to an increase of more than 40 percent. If we take the first-stage estimates in Table 3 seriously, our reduced form estimate can be translated into an IV estimate of 12 more children per 1,000, which corresponds to a 200-percent increase. This is a large effect but comparable to Baker et al. (2008) who find that childcare attendance increased parent-reported nose-and throat infections by 237–451 percent. However, the results should be interpreted with caution since unemployment is measured differently in the survey than in our study.[33]

The effect is significant only for the first year the children had access; the estimates for the years 2003 and 2004 are not statistically different from zero at any standard significance level.[34] A possible explanation for this time pattern is that the children who are exposed to childcare for the first time as a result of the reform are affected, possibly because the parents have been unemployed for a long time and the children were not previously enrolled in childcare. One year after the reform, most children would have had access to childcare at some point. Hence, this initial effect on hospitalization rate reverts to its original level. It is less clear why this increase in infections is present for 4- to 5-

[33] In the survey data, unemployment is measured at the time of the survey, whereas we consider a child as having an unemployed parent if at least one of the parents was unemployed at least one day during the year.
[34] Estimating the model, excluding Stockholm, the effect persists in 2003, at least at the 10-percent level.

year-olds but not for 2- to 3-year-olds, although it is possible that the higher overall risk of hospitalization for infections in younger ages implies that exposure to childcare makes less of a difference.[35] Furthermore, the first-stage regressions suggest that the reform has a greater impact at younger ages because older children are more likely to be enrolled at baseline. It is hence possible that the affected 4- to 5-year-olds represent a more vulnerable group.

Figure 4 Results: Difference-in-differences specification. Children ages 4- to 5 years.



Note: All regressions control for child's sex, child's age (months at the end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother's region of birth (Nordic, non-Nordic), number of children aged 0–10 years in the family, municipal unemployment, and municipal and year fixed effects. Standard errors are clustered at the municipal level. Vertical lines show a 95-percent confidence interval.

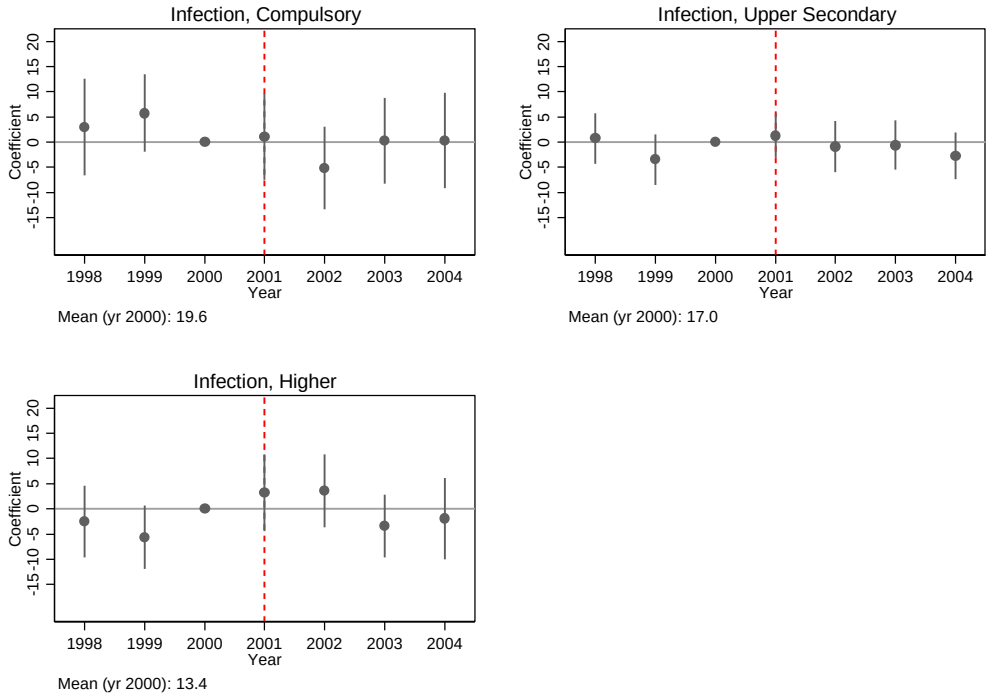
6.2.1 Heterogeneous effects

To gain an understanding of the effect of the reforms 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 to the impact of attending

[35] We investigated the effects on first-borns, who are less likely to have gained immunity through exposure to older siblings. For this group of 2- to 3-year olds, there is a significant increase in infections as they gain access to childcare. However, large standard errors for higher birth order children and 4- to 5-year olds suggest that our sample is too small for this type of sub-group analysis.

childcare, we study heterogeneous effects depending on mother's education level. For completeness, we do this for both age groups.

Figure 5 Infection-related hospitalization by maternal education level. Children ages 2–3 years.

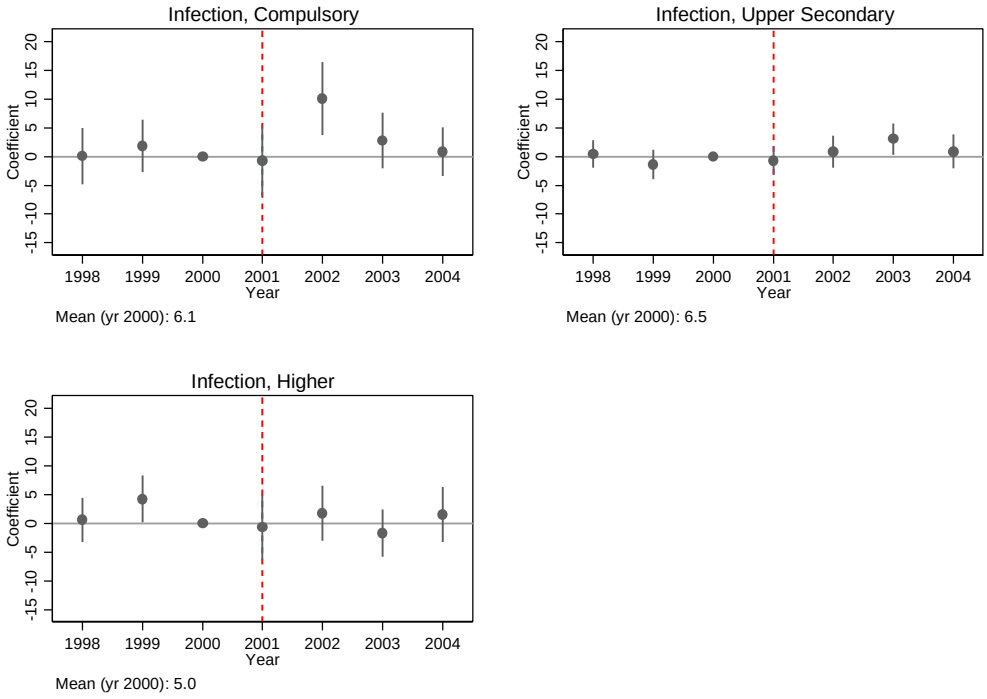


Note: All regressions control for child's sex, child's age (months at the end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother's region of birth (Nordic, non-Nordic), number of children ages 0–10 years in the family, municipal unemployment, and municipal and year fixed effects. Standard errors are clustered at the municipal level. Vertical lines show a 95-percent confidence interval.

Figure 5 shows the resulting estimates for children ages 2–3 years, and Figure 6 shows the estimates for children ages 4–5 years (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 all children are considered, remains. There are no statistically significant effects for any education level for the younger children. For older children, the results clearly show that the effect found for the whole group is driven by the 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 of parents with upper secondary schooling. Among the children of highly educated mothers,

however, there is a statistically significant effect before the reform, suggesting that the parallel-trends assumption might not hold for this group.

Figure 6 Infection-related hospitalization by maternal education level for children ages 4–5 years.



Note: All regressions control for child's sex, child's age (months at the end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother's region of birth (Nordic, non-Nordic), number of children ages 0–10 years in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level. Vertical lines show a 95-percent confidence interval.

There are at least two potential explanations for why children with low-educated mothers are more likely to need hospital care for infections as they gain access to childcare. First, it is possible 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, either because the children are less resilient and/or because they are less likely to receive appropriate preventive and primary care. Second, it is possible that children of mothers with low education are more likely to be exposed to childcare for the first time as the reform is implemented. According to the Parent Survey, the reform increased childcare attendance regardless of mothers' educational background after the reform. However, the children may have different previous exposure to childcare if long-term unemployment was more common among parents with low education.

During the pre-period, when these children were younger, childcare was only available for children whose parents were working or were full-time students. Hence, if their parents were long-term unemployed prior to the reform, they did not have access to childcare.

To investigate how likely children whose parents were unemployed when they were 4–5 years old were to have attended childcare when they were younger, we look at the share of mothers who either were unemployed or received student benefits when their children were 2–3 years old.

Table A 7 in the Appendix presents these shares by maternal education. There are, however, no indications that mothers with only compulsory education were more likely to be unemployed when their children were 2–3 years old than mothers with upper secondary education were. Hence, a more probable explanation for the higher risk of hospitalization among children of mothers with low levels of education is that they are more likely to need hospital care when they are first exposed to the childcare environment either because they are more vulnerable at the outset or because they do not receive as appropriate preventive care as other children. Another possible explanation is that low-SES children are more exposed to infections when they are in childcare because of differences in childcare quality and possibly higher rates of infections among the other children at the childcare center. Hall and Lindahl (2017) show that Swedish preschools differ greatly in rates of absences due to sickness.

6.3 Effects on child health in the medium term

Next, we turn to the medium-term effects and estimate equation 2, where we measure the health outcomes when children are aged 10–11. The results are presented in Table 4, where the bottom panel shows the effects on hospitalizations, and the top panel shows effects on drug prescriptions.[36] The parameter estimate *No access years* shows how many more children per 1,000 are hospitalized/prescribed a drug at ages 10–11 years if they experienced any parental unemployment during a year when they were aged 2–5 years

[36] We 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.

and did not have access to childcare as opposed to experiencing parental unemployment while having access to childcare.[37]

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 years, and the estimates are both statistically and economically insignificant. Turning to the results for prescriptions of ADHD medication or psycholeptics (*psychiatric*), our point estimate is negative, suggesting a reduction of approximately 10 percent compared to the population mean; this finding implies 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], which corresponds to a change of -24 percent to 7 percent. For antibiotics, we do not find evidence of any statistically significant effect, and the estimate is small relative to the population mean. However, for respiratory conditions, not having access to childcare increases the risk of needing prescriptions for respiratory problems at ages 10–11 years; an additional year with no access increases the likelihood of receiving drug prescription by 6 percent compared to the population mean. It is possible that, consistent with the hygiene hypothesis, increased exposure to childcare, and possibly to a greater variety of microorganisms, could have contributed to a reduction in asthma and allergies, which cause respiratory problems. Restricting this group of drugs to medications for asthma and allergy-related respiratory conditions (ACT groupings R1, R3, R6) in column (3), we find that children exposed to childcare are indeed less likely to be prescribed these treatments.

To further explore the hygiene hypothesis, we divide the sample by birth order. Since children with older siblings are likely to be exposed to a wider flora of microorganisms in their home environment than firstborns we expect to find that the effect of childcare attendance on asthma and allergies is stronger for first born children. The results presented in Table 5 clearly show that access to childcare only affected the prevalence of asthma and allergies among first born children. Children denied access for one year are 11 percent more likely to be prescribed medication for asthma or allergies. Dividing the sample by mothers' education level shows no differences across groups, which is expected since childcare attendance increased similarly among children regardless

[37] Table A 8 shows the corresponding results with different sets of controls. The estimates overall are stable to the inclusion of controls, but they change somewhat for antibiotics, any hospitalization, injuries and respiratory hospitalizations as parental controls are introduced.

mothers' educational background.[38] There is also a similar decrease in prescriptions for respiratory conditions (which includes medication for asthma and allergy) and a 5 percent reduction in antibiotics prescriptions. Because common colds or respiratory infections may be more serious for children with asthma and allergies it is not surprising that childcare access also affects these categories.

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

	(1) Any	(2) Respiratory	(3) Injury	(4) Infection	(5) Psychiatric
			Hospitalizations		
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	Asthma and allergy	Antibiotics	Psychiatric
No childcare access		7.178*** (2.563)	4.543** (1.894)	0.787 (2.179)	-1.210 (1.111)
N		308,623	308,623	308,623	308,623
Mean outcome		125.74	89.55	133.07	14.23

Note: Standard errors clustered at the municipality level. Control variables at age 2 years: 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 aged 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, interaction of years of unemployment and year of birth, interaction of year of birth and municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table 5. Prescriptions at age 10–11 years, by **birth** order

	(1) Mental	(2) Antibiotics	(3) Respiratory	(4) Asthma and allergy
		First born children		
No access, years	-1.126 (1.359)	7.354** (3.717)	16.667*** (3.599)	11.468*** (2.672)
N	130,127	130,127	130,127	130,127
Mean outcome	15.29	147.05	141.97	100.28
		Children with older siblings		
No access, years	-1.340 (1.317)	-2.744 (2.946)	1.222 (2.891)	0.204 (2.260)
N	178,496	178,496	178,496	178,496
Mean outcome	13.46	122.88	113.91	81.73

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.* p<0.10, ** p<0.05, *** p<0.01

6.4 Discussion of robustness

In this section, we discuss a number of issues that relate to the robustness of our results, and the credibility of making causal interpretations of the results.

[38] Results available upon request.

A crucial assumption in the analysis is that the control municipalities are a valid comparison group. In the analysis presented in Section 6.2 we showed that the results are not sensitive to including control variables suggesting that changes in the populations do not cause the results. We also show that there are no visible differential trends in our health measures before the reform. Nor are the results sensitive to excluding the largest municipality Stockholm from the estimations.[39] To further test whether there are some underlying differences in health trends between treated and control municipalities we conduct the same analysis on children of employed parents. The children of employed parents are not directly affected by the reform since they always had access and can thus serve as a placebo sample. They could however be indirectly affected since the environment at the childcare center may change if children of unemployed parents increase attendance. The results for children with employed parents are presented in appendix Table A 9 and Table A 10 and show no effects on children aged 4–5 years, for any of the health outcomes. This lends support to a causal interpretation of the effect of increased risk of hospitalization for infections found for 4- to 5-yearolds of unemployed parents as they gained access to childcare. However, for children 2–3 for whom we did not find any effects of childcare access, the placebo estimations show a statistically positive effect on hospitalizations for respiratory conditions 2003 and 2004 for children with employed parents. It is possible that a deterioration childcare environment drives the effect.

Another way to test the robustness of the results is to use alternative outcome measures and analyze subgroups. The outcome variables in the analysis measure whether a child has been in hospital or been prescribed a medicine at least once during the year. We also considered the effects on the total number of hospitalizations. Results point in a similar direction, but there is considerable noise in this measure. These results are not presented in the paper.

In Section 6.2.1 we presented some results on heterogeneous effect by mother's education level. Generally, the estimates become noisier, but the results showed that the increased risk of hospitalizations for infections was driven by children with mothers who have at most compulsory education. In order to interpret this as an effect of the reform, we need there to be a first stage. We have verified that the first stage is valid also for

[39] Results available upon request.

children whose mothers have compulsory education. We also investigated whether the medium-term effects, at age 10-11, differ with respect to maternal education. 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.

We have also analyzed whether the effects differ depending on the child's sex or on whether it is the mother or father who is unemployed. Given that the results we find seem to relate to exposure to infections and microorganisms while in childcare, there is little reason to expect there to be any gender differences. Our results, available upon request, do not show evidence of any such gender differences in the effects, although the effects for paternal unemployment are somewhat noisy.

7 Concluding comments

In this paper, we evaluate whether access to childcare for children with unemployed parents affects child health by investigating the short-term effects on hospitalization and medium-term effects on hospitalization and drug prescriptions. Using a nationwide reform that required municipalities to provide childcare for children with unemployed parents, we investigate the exogenous change in childcare access for this disadvantaged 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 high-quality center-based childcare.

The expected effects on health are ambiguous. In childcare, the child is attended by professional staff trained to detect early health problems and reduce the risk of accidents and poisoning. However, attending childcare reduces exposure to parental care and implies that the child receives less one-on-one adult attention, in addition to being part of a large group of children, which may increase exposure to both infectious and noninfectious microorganisms.

Overall, our results show that granting childcare access to children with unemployed parents has limited immediate effects on child physical health. Among toddlers (children aged 2–3 years), there is no evidence that access to childcare affects hospitalization in the short run. For preschool children (aged 4–5 years), we find that access to childcare led to an increase in hospitalizations related to infections for children with low educated

mothers. However, within one year after the reform, the hospitalization rate due to infections among children with unemployed parents falls back again.

In the longer term, at ages 10–11 years, our results indicate no effect of childcare access on hospitalizations, but there is evidence of a decrease in prescriptions of drugs related to respiratory conditions, in general, and asthma and allergy-related conditions in particular. To the extent that attending childcare increased these children's exposure to infectious and noninfectious microorganisms in early childhood, our results are in line with the hygiene hypothesis. This interpretation of the results is strengthened by the finding that we see this effect only for firstborn children and not for children with older siblings who are exposed to older siblings also in the home environment. For ADHD medication and psycholeptics (prescribed to treat anxiety or sleeping problems), results suggest that gaining access to childcare may increase mental health problems, but standard errors are large, and we cannot rule out zero or positive effects.

To conclude, it is not likely, based on the findings of this paper, that giving disadvantaged groups of children access to childcare will improve their immediate health outcomes. Neither do we find evidence that childcare access is harmful. Given previous evidence of the beneficial consequences for cognitive and noncognitive skills and the medium-term effects we find regarding allergies and asthma, providing childcare for children with unemployed parents is likely to be beneficial. A note of caution is warranted, however, since we cannot rule out adverse effects on behavior and mental health. Providing a better understanding of the role of childcare for children's mental health development is an important task for future research.

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Appendix

Selection of control municipalities

To estimate the effects of the reform, we need to identify a set of control municipalities where the reform did not significantly change 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 surveys conducted by the National School Board in 1998 and 2002. The surveys are answered by parents and pertain to the status of the parent and child at a specific point in time. This is not an exact measure of the enrollment rates of different groups because, for example, the status of the parent may change over the year, and there is no information on how long the parent has been unemployed. We define *pre-diff* as the difference in the enrollment rate between children of employed parents and children of unemployed parents in 1998 in a given municipality and *diff-diff* as the change in the enrollment difference between children of employed parents and children of 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 is placed in the middle of the red dot for the 75 nonreform municipalities with the smallest pre-reform enrollment difference (*pre-diff).* A slightly smaller yellow dot marks the 75 municipalities with the smallest change in enrollment difference (*diff-diff).* We choose as our control municipalities the 75 municipalities with the smallest sum of *pre-diff* and *diff-diff*. These are marked by a tiny black dot.

Figures

Figure A 1 Difference-in-differences from 1998 to 2002 and difference in childcare enrollment in 1998 according to parental employment status in Swedish municipalities.

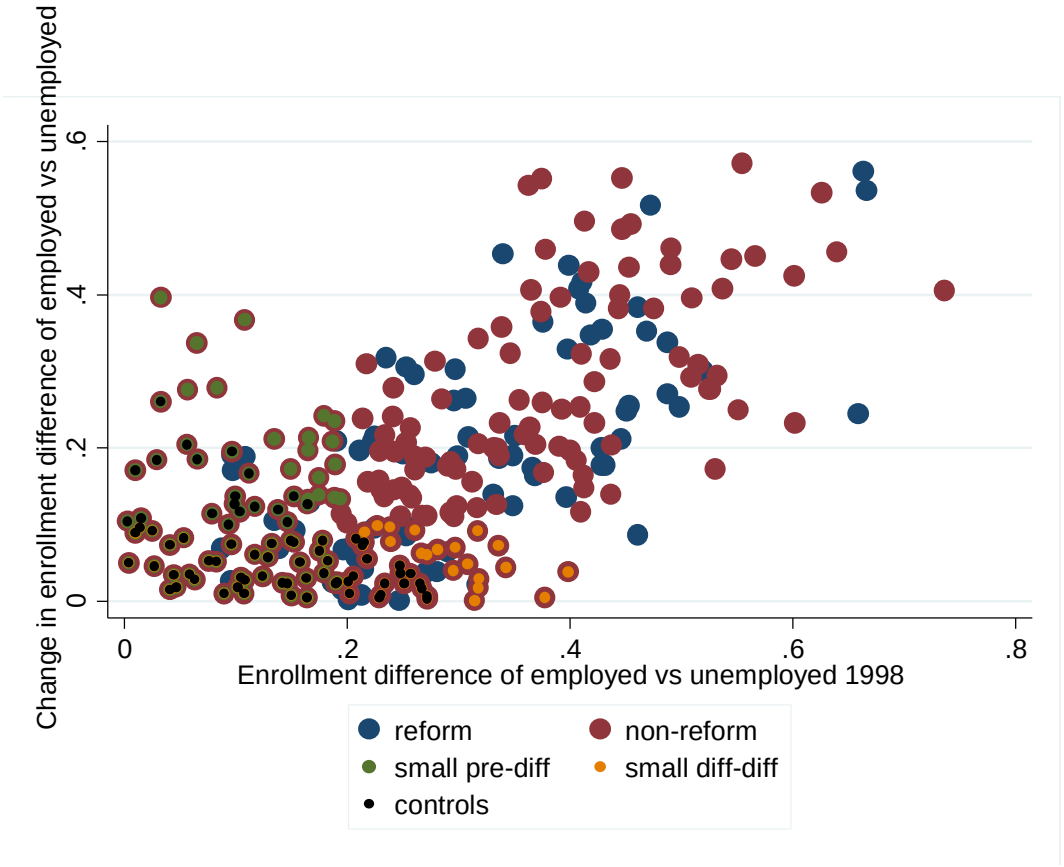


Figure A 2 Map of treatment and control municipalities

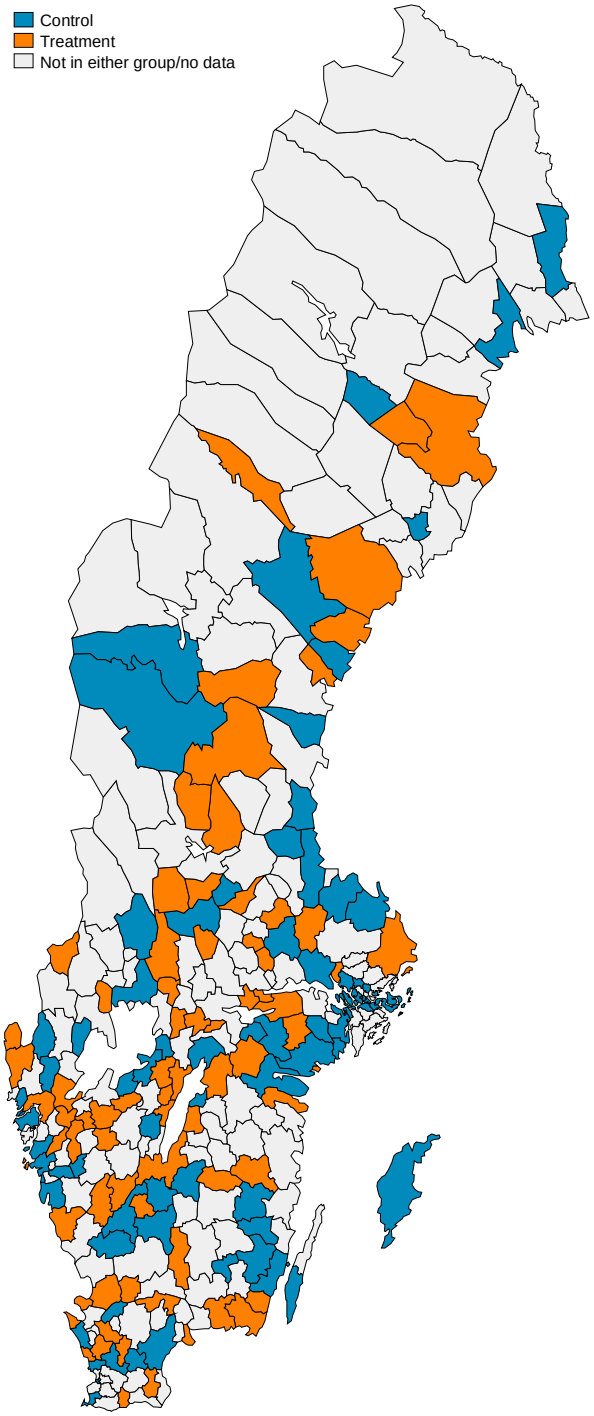


Figure A 3 Child-staff ratio in municipal childcare in the treatment and control municipalities for 1998–2005

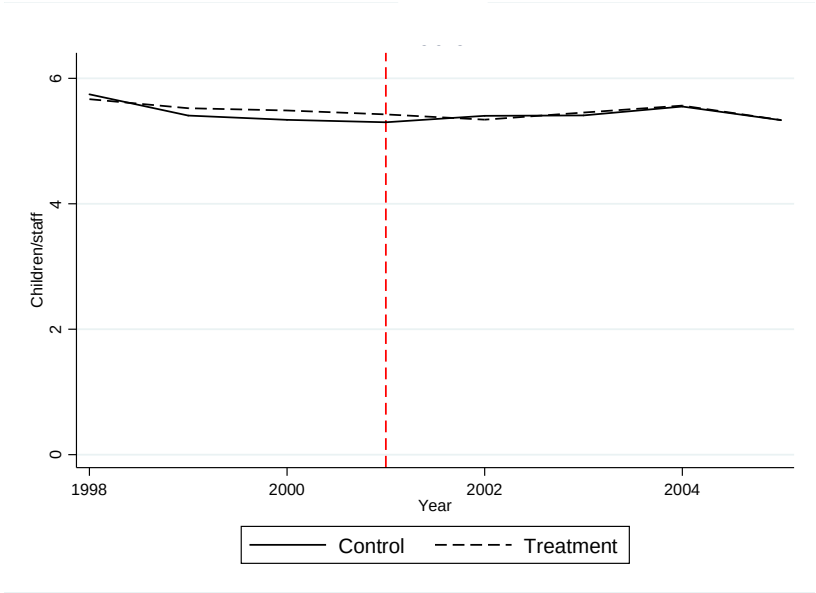
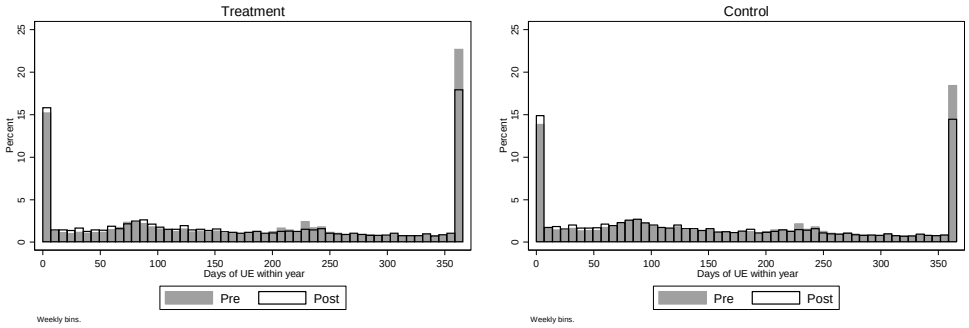


Figure A 4 Number of days per year for which children with unemployed parents experience parental unemployment in the treatment and control municipalities pre (1998–2000) and post (2002–2004) reform



Tables

Table A 1 Municipal-level characteristics in 2001

	Control	Treatment	All
Municipal budget, spending per capita			
Sports and recreation	951.45	963.23	1,007.52
	(321.29)	(318.78)	(335.83)
Childcare	4,598.17	4,034.55	4,324.60
	(796.13)	(547.01)	(779.36)
Family support	2,074.75	1,940.51	1,957.50
	(798.93)	(566.11)	(723.00)
Education	12,541.64	12,650.48	12,728.06
	(1,394.10)	(1,184.22)	(1,392.85)
Other municipal characteristics			
Left-wing majority	0.33	0.37	0.39
	(0.47)	(0.49)	(0.49)
Population share of 1- to 5-year-olds (%)	5.11	4.86	4.99
	(0.80)	(0.55)	(0.70)
Childcare participation	65.73	59.56	62.35
	(8.62)	(8.74)	(9.78)
Staff at childcare / child (municipal)	5.44	5.47	5.32
	(0.15)	(0.11)	(0.62)
Staff at childcare / child (private)	5.54	5.70	5.50
	(0.16)	(0.22)	(0.81)
Unemployment rate, 25- to 34-year-olds	10.39	11.11	11.15
	(3.61)	(3.24)	(3.63)
Population	45,389	20,738	30,827
	(100,929)	(202,500)	(58,499)
N	75	75	289

Table A 2 Childcare enrollment in reform and control municipalities according to the 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 used in the study

Variable	Definition	Example
Hospitalizations		
Hospitalization	=1 if admitted to hospital for any reason	
Infection	=1 if admitted to hospital with the ICD10 diagnosis code A00-A99, B10-B84, B90-B99, B00-B09, B85-B89	Infectious diarrhea, mononucleosis, chicken pox.
Respiratory	=1 if admitted to hospital with the ICD10 diagnosis code J07-J08; J19-J39; J48-J99	Upper and lower respiratory infections.
Injury and poisoning	=1 if admitted to hospital with the ICD10 diagnosis code S00-S99, T00-T35, T36-T65, T66-T98	Broken arm or ankle, medication overdose.
Psychiatric	=1 if admitted to hospital with diagnosis codes F00–F99	Insomnia, behavior disorder, anxiety, depression.
Prescriptions		
Antibiotics	=1 if prescribed a medication with ATC code J01	Ear infection, urinary infection.
Respiratory	=1 if prescribed a medication with ATC codes R01-R06	Asthma-related, cough.
Asthma and allergy	=1 if prescribed a medication with ATC codes R01, R03 and R06	
Psychiatric	=1 if prescribed a medication with ATC codes N06B, N06A, N05	ADHD, depression, insomnia

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

	Children ages 2–3 years			Children ages 4–5 years		
Any diagnosis						
1998	-2.328	-2.285	-2.043	-0.569	-0.453	-0.586
	(3.536)	(3.526)	(3.479)	(2.635)	(2.645)	(2.628)
1999	-2.129	-2.116	-2.333	0.717	0.768	0.622
	(3.603)	(3.598)	(3.614)	(2.272)	(2.271)	(2.259)
2001	-1.106	-1.062	-1.249	0.721	0.692	0.713
	(3.308)	(3.311)	(3.282)	(2.576)	(2.562)	(2.553)
2002	-3.081	-3.300	-3.139	3.988	3.908	3.913
	(3.651)	(3.660)	(3.677)	(2.777)	(2.810)	(2.805)
2003	-3.171	-3.582	-3.572	0.618	0.490	0.196
	(3.571)	(3.551)	(3.572)	(2.667)	(2.676)	(2.683)
2004	-0.882	-1.375	-1.523	3.918	3.729	3.395
	(3.702)	(3.722)	(3.787)	(2.461)	(2.475)	(2.467)
Respiratory						
1998	-2.378	-2.316	-2.251	-0.098	-0.057	-0.111
	(1.976)	(1.974)	(1.949)	(1.727)	(1.731)	(1.729)
1999	-0.990	-0.972	-0.998	-2.149	-2.120	-2.199
	(2.087)	(2.083)	(2.075)	(1.564)	(1.564)	(1.585)
2001	-0.201	-0.169	-0.322	1.077	1.036	1.097
	(1.763)	(1.763)	(1.753)	(1.693)	(1.683)	(1.699)
2002	-0.701	-0.815	-0.881	0.277	0.202	0.223
	(2.232)	(2.253)	(2.283)	(1.442)	(1.443)	(1.424)
2003	-3.417	-3.619	-3.616	-2.174	-2.286	-2.438
	(2.348)	(2.343)	(2.358)	(1.929)	(1.918)	(1.928)
2004	-1.466	-1.684	-1.725	0.397	0.267	0.162
	(2.365)	(2.362)	(2.382)	(1.503)	(1.505)	(1.471)
Injury						
1998	-1.308	-1.292	-1.199	-1.965*	-1.903	-1.992*
	(1.472)	(1.474)	(1.476)	(1.187)	(1.190)	(1.202)
1999	0.379	0.380	0.320	0.275	0.295	0.355
	(1.476)	(1.479)	(1.489)	(1.468)	(1.464)	(1.468)
2001	-1.337	-1.307	-1.245	-1.809	-1.839	-1.807
	(1.655)	(1.656)	(1.657)	(1.459)	(1.461)	(1.458)
2002	-1.712	-1.749	-1.615	-0.081	-0.106	-0.137
	(1.512)	(1.519)	(1.515)	(1.638)	(1.640)	(1.661)
2003	-1.360	-1.424	-1.345	-0.737	-0.741	-0.707
	(1.450)	(1.451)	(1.441)	(1.268)	(1.266)	(1.292)
2004	0.803	0.749	0.731	-0.646	-0.683	-0.760
	(1.598)	(1.599)	(1.602)	(1.275)	(1.278)	(1.288)
Infection						
1998	0.611	0.553	0.729	0.180	0.150	0.163
	(2.141)	(2.139)	(2.106)	(1.022)	(1.025)	(1.027)
1999	-1.658	-1.679	-1.837	0.358	0.334	0.296
	(2.090)	(2.085)	(2.112)	(0.934)	(0.931)	(0.927)
2001	2.323	2.328	2.196	-0.949	-0.947	-1.002
	(1.647)	(1.645)	(1.657)	(1.081)	(1.080)	(1.070)
2002	-0.700	-0.798	-0.587	2.477**	2.452**	2.421**
	(1.965)	(1.967)	(1.945)	(1.211)	(1.214)	(1.212)
2003	-0.685	-0.843	-0.805	1.743	1.716	1.611
	(1.588)	(1.611)	(1.593)	(1.154)	(1.149)	(1.138)
2004	-1.834	-2.077	-2.011	1.153	1.100	0.994
	(2.018)	(2.035)	(2.055)	(1.076)	(1.072)	(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 the municipality level.

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

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

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

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

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

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

Table A 7 Past experiences of children ages 4–5 years with unemployed parents during the post period

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

Table A 8 Hospitalizations and prescriptions at ages 10–11 years, including different sets of covariates.

		Prescriptions	
		Psychiatric	
No access, years	-1.021	-1.210	-1.210
	(1.100)	(1.111)	(1.111)
		Antibiotics	
No access, years	1.337	0.787	0.787
	(2.165)	(2.179)	(2.179)
		Respiratory	
	7.420***	7.178***	7.178***
	(2.596)	(2.563)	(2.563)
		Hospitalizations	
		Any diagnosis	
No access, years	-0.091	-0.233	-0.233
	(1.174)	(1.174)	(1.174)
		Respiratory	
No access, years	0.090	0.060	0.060
	(0.489)	(0.489)	(0.489)
		Injury	
No access, years	0.311	0.220	0.220
	(0.783)	(0.782)	(0.782)
		Infection	
No access, years	0.133	0.123	0.123
	(0.466)	(0.465)	(0.465)
		Psychiatric	
No access, years	-0.207	-0.203	-0.203
	(0.248)	(0.247)	(0.247)
		Prescriptions	
		Psychiatric	
No access, years	-1.021	-1.210	-1.210
	(1.100)	(1.111)	(1.111)
		Antibiotics	
No access, years	1.337	0.787	0.787
	(2.165)	(2.179)	(2.179)
		Respiratory	
No access, years	7.420***	7.178***	7.178***
	(2.596)	(2.563)	(2.563)
No access, years		Asthma and allergy	
N	308,623	308,623	308,623
Child controls	Yes	Yes	Yes
Parental controls	No	Yes	Yes
Municipal controls	No	No	Yes

Note: * p<0.10, ** p<0.05, *** p<0.01 All regressions control for municipal fixed effects. Control variables at age 2 years: Child: Sex, age in months at the end of the year, sibling order number; parents: education level, mother's age at first birth, non-Nordic ethnicity, number of children 0-10 years in the family; municipal: municipal UE rate (among 25- to 34-year-olds), municipality, year of birth, interaction of municipality and year of birth, interaction of years of unemployment and year of birth, interaction of year of birth and municipality.
Standard errors are clustered at the municipality level.

Table A 9 Difference-in-differences estimates, employed. Children age 2–3

	(1) Any	(2) Respiratory	(3) Injury	(4) Infection
2001	3.070	1.373	0.097	1.249
	(3.136)	(1.760)	(1.374)	(1.373)
2002	-0.222	0.100	0.004	0.001
	(2.798)	(1.805)	(1.599)	(1.445)
2003	2.352	3.806*	-0.702	-0.558
	(3.085)	(2.061)	(1.328)	(1.288)
2004	4.529	4.902**	-0.211	-0.390
	(3.021)	(1.926)	(1.328)	(1.304)
N	257,482	257,482	257,482	257,482

Note: * p<0.10, ** p<0.05, *** p<0.01 The reference period is 1998–2000. Control variables at age 2 years: Child: Sex, age in months at the end of the year, sibling order number; parents: education level, mother's age at first birth, non-Nordic ethnicity, number of children 0-10 years in the family; municipal: municipal UE rate (among 25- to 34-year-olds), municipality, year of birth, interaction of municipality and year of birth, interaction of years of unemployment and year of birth, municipality fixed effects, interaction of year of birth and municipality. Standard errors are clustered at the municipality level.

Table A 10 Difference-in-differences estimates, employed. Children age 4–5

	(1) Any	(2) Respiratory	(3) Injury	(4) Infection
2001	1.641	0.533	-0.406	-0.098
	(1.883)	(1.409)	(0.908)	(0.990)
2002	2.066	1.427	0.564	-0.391
	(2.293)	(1.278)	(1.130)	(1.033)
2003	2.732	1.024	0.182	0.427
	(1.931)	(1.518)	(1.118)	(0.944)
2004	0.667	1.509	-0.999	-0.430
	(2.211)	(1.413)	(1.183)	(0.682)
N	295,916	295,916	295,916	295,916

Note: * p<0.10, ** p<0.05, *** p<0.01 The reference period is 1998–2000. Control variables at age 2 years: Child: Sex, age in months at the end of the year, sibling order number; parents: education level, mother's age at first birth, non-Nordic ethnicity, number of children 0-10 years in the family; municipal: municipal UE rate (among 25- to 34-year-olds), municipality, year of birth, interaction of municipality and year of birth, interaction of years of unemployment and year of birth, municipality fixed effects, interaction of year of birth and municipality. Standard errors are clustered at the municipality level