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Age-specific Effects of Early Daycare on Children's Health

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Age-specific Effects of Early Daycare on Children's Health*

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Abstract

Over the past decades, the share of very young children in daycare has increased significantly in many OECD countries, including Germany. Despite the relevance of child health for child development and later life success, the effect of early daycare attendance on health has received little attention in the economic literature. In this study, I investigate the impact of a large daycare expansion in Germany on children's age-specific mental and physical health outcomes. Based on a unique set of administrative health records covering 90% of the German population over a period of ten years, I exploit exogenous variation in daycare attendance induced by the expansion. My results provide evidence for the substitution of illness spells from the first years of elementary school to the first years of daycare. Specifically, I find that early daycare attendance increases the prevalence of respiratory and infectious diseases and healthcare consumption when entering daycare (1–2 years) by 5–6 percent. At elementary school age (6–10 years), the prevalence decreases by similar magnitudes. I do not find evidence for an effect of daycare attendance on mental disorders, obesity, injuries, vision problems, or healthcare costs. Heterogeneity analysis indicates more pronounced effects for children from disadvantaged areas, earlier detection of vision problems, and a reduction in obesity in these children.

Keywords: child care, daycare expansion, physical health, mental health, education, administrative health records, difference-in-differences, event study

JEL classification: I10, I12, J13, C23

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1 Introduction

Since the early 2000s, the share of very young children (0–2 years) in daycare has increased significantly in many OECD countries.[1] Germany experienced one of the largest increases among all OECD countries from a 17% coverage rate in 2005 to 37% in 2018 (OECD, 2020). Along with this development, the body of literature studying the effects of (early) daycare attendance of children on their socio-emotional and cognitive outcomes has grown. Previous research shows that health is one of the most important determinants of socio-emotional and cognitive development during childhood, and of later educational achievements, health outcomes, and labor market outcomes during adulthood (e.g., Carneiro et al., 2007, Currie, 2020, Currie and Stabile, 2006, Heckman, 2007, Heckman et al., 2013, Peet et al., 2015). Additionally, child health *per se* matters – also because ill health produces direct costs to the healthcare system as well as indirect societal costs through, for example, labor productivity losses of parents. However, despite the relevance, the effect of early daycare attendance on health receives little attention in the literature.

The few studies that assess the effects of daycare exposure on child health are inconclusive in terms of the direction and magnitude of the effects. Programs targeting families from low socio-economic backgrounds generally benefit children's health (e.g., Conti et al., 2016, Hong et al., 2019) while the effects of universal daycare programs on health depend on the quality of the program, the counterfactual care mode, the considered age groups and the outcomes at measure. For example, a cheap, low-quality daycare expansion in Quebec has adverse health effects (e.g., Baker et al., 2008, 2019, Heckman et al., 2010, Kottelenberg and Lehrer, 2013), while daycare expansions providing better quality care have the potential to have null or positive effects on child health (e.g., Bosque-Mercader, 2022, Cornelissen et al., 2018, van den Berg and Siflinger, 2022).[2] Evidence on the health effects of early daycare entry (below age three) is particularly scarce, as most studies either focus on older children or on daycare expansions that affect all children below school age.

In this paper, I fill this research gap by analyzing the age-specific effects of early daycare[3] attendance of children on their mental and physical health. To overcome the endogeneity

[1] On average, the enrollment rate of 0–2-year-old children in daycare increased from 21% in 2005 to 32% in 2018 (e.g., OECD, 2020).

[2] For a literature review on the impact of universal early education programs, especially on health, see, e.g., Cascio (2015), Dietrichson et al. (2020), van Huizen and Plantenga (2018), or with a focus on Germany Spieß (2022).

[3] The term daycare describes all forms of formal child care provided by professionals outside the family.

of the decision to attend daycare at an early age, I exploit a large-scale expansion of publicly funded daycare in Germany. This expansion was induced by a federal reform that introduced a legal entitlement to a daycare slot for all children aged one year and older. Following the announcement of the reform in 2007, daycare coverage of under three-year-old children increased by about 17 percentage points between 2008 and 2018 in West Germany (Destatis, 2019). The reform generated large temporal and spatial variations in the expansion speed of daycare slots at the county level. Using this variation, I employ difference-in-differences and event-study approaches to identify causal effects.[4]

The analyses are based on administrative health records covering all individuals insured through the public health system in Germany (about 90% of the population) between 2009 and 2019. My sample includes children from birth cohorts 1999 to 2015 aged one to ten years, which amounts to about 11 million children. The data covers the outpatient register that contains all ambulatory care contacts, including all contacts with physicians, pediatricians, and therapists. Comprehensive diagnoses by practitioners based on the International Classification of Diseases (ICD-10) are recorded for each visit. Specifically, I consider physical and mental health outcomes, healthcare consumption, and costs. In terms of physical health, I analyze three sets of communicable diseases – *infections*, *respiratory diseases*, and *ear diseases* – and three non-communicable diseases – *obesity*, *injuries* and *vision problems.* For mental health, including socio-emotional outcomes, I consider the ICD-10 group of *mental and behavioral disorders.* To measure healthcare consumption, I assess the annual number of treatment cases and healthcare costs. *Ex-ante*, there is no clear prediction for the direction of the effects as the daycare expansion may affect these outcomes through several channels. Underlying channels include the earlier onset of an immunization process, formation of health habits, formation of socio-emotional and cognitive skills, and changes in the child's environment other than daycare attendance per se (e.g., health surveillance by the daycare teachers, increased maternal labor market participation and improved parental well-being).

My results provide evidence that early daycare attendance increases the prevalence of respiratory and infectious diseases at age one to two but decreases the prevalence at older ages. Specifically, a ten percentage point increase in the daycare coverage rate leads to an

[4]The expansion allows a clear treatment definition that does not require applying DiD estimators developed for staggered treatment implementations (e.g., Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020).

increase of 0.08 infections, 0.03 ear diseases, and 0.016 respiratory diseases per child per year at age one to two. These estimates correspond to a 5.7% increase for infections, 5.1% for ear diseases, and 5.6% for respiratory diseases compared to the sample means. The reductions in infections and respiratory diseases at elementary school age are of similar magnitude in absolute terms. In line with the hygiene hypothesis, which states exposure to viruses and bacteria at early ages initiates an immunization process that leads to more infections in the short-run but fewer infections at older ages (Strachan, 1989),[5] my results suggest a substitution of illness spells from elementary school to the first years of daycare. The increases in infections and respiratory diseases at age 1–2 years correspond to the decreases at elementary school age, suggesting that children who enter daycare earlier suffer from the same number of infections and respiratory diseases during their first ten years of life as children who enter daycare later. I do not find robust evidence of significant changes in mental health or obesity, while my results suggest null effects on injuries and vision problems. Healthcare consumption increases at ages 1–2 while it decreases at ages 3–5 and 6–8. Despite changes in the prevalence of diagnoses and the number of doctor visits, there is no clear effect on healthcare costs. The findings are robust to a large set of robustness checks, such as different definitions of the treatment status and the expansion period, the application of multiple hypothesis testing methods to obtain p-values accounting for the large number of outcomes, and plausibility checks of the common trend assumption. Heterogeneity analysis indicates more pronounced effects for children from disadvantaged areas, earlier detection of vision problems and a reduction in obesity in these children.

These results raise the question of whether substituting illness spells for infections and respiratory diseases from elementary school to the first years of daycare is beneficial. The daycare expansion appears to be neutral in terms of healthcare costs arising in the first ten years of life. My results suggest that the beneficial health effects for older children may reach beyond the study period, which aligns with previous literature on long-term daycare effects. I provide suggestive evidence from an additional analysis based on representative survey data from the German Socio-Economic Panel (SOEP) for spill-over effects on parents, i.e., that parental health deteriorates in the short run but improves in the long run. Furthermore, sickness absence at work of mothers with elementary school-age children is lower when

[5]Originally, the hygiene hypothesis was developed as an explanation for a reduction in hay fever and asthma diagnoses for children with many siblings as they are exposed to many microbial compounds early in life. Subsequently, the hygiene hypothesis was also related to a more general immunization process, not only affecting allergic illness but also other inflammatory diseases (e.g. Briggs et al., 2016, Oikonomopoulou et al., 2013, Schaub et al., 2006).

their children entered daycare at age 1–2 years. This, in turn, may increase productivity as mothers tend to work more hours when children are in elementary school compared to when children are below three years (Federal Institute for Population Research, 2020). In terms of spill-over effects on siblings, evidence from Daysal et al. (2022) points out that older siblings "bringing home" infections from daycare leads to worse health for younger siblings, who are particularly vulnerable below the age of one. Thus, moving infections to an earlier age when there are no younger siblings could benefit future younger siblings. When classifying the results, I also reflect on other factors such as the duration of illness spells at different ages and sickness absence at school or daycare. Overall, there is no evidence that changing the timing of infections to earlier years leads to detrimental effects that would challenge children's daycare entry at an early age.

My study contributes to the literature in several ways. First, this is the first study to specifically estimate the health effects of a universal daycare program for children below three. van den Berg and Siflinger (2022), who assess a reform that abolished daycare fees for children aged one to five years in one region in Sweden, evaluate the cumulative effect of attending daycare from age one until school entry. In Germany, prior to the reform, daycare attendance from age three was already almost universal; thus, the reform only shifted the daycare entry age to an earlier age. Second, I estimate age-specific effects by assessing instantaneous effects on child health (age one to two) as well as short-term effects (age three to five) and longer-term effects at elementary school age (age six to ten). Assessing age-specific effects is important as the effect of daycare may change over the life-course (e.g., Cattan et al., 2021, van den Berg and Siflinger, 2022).

Third, my detailed diagnosis data enables me to understand the potential heterogeneity of the health effects. Most previous studies rely on survey data that contain rather broad and subjective health measures. Survey data allow for assessing health and behavioral outcomes that cannot be measured otherwise and usually provide an extensive range of socio-economic characteristics. However, when measuring health and behavioral disorders, survey data are less detailed than administrative health records and are potentially subject to a reporting bias (e.g., Bound et al., 2001). In order to obtain a comprehensive understanding of the effects of daycare on health, it is essential to study various dimensions as different diseases could be differently affected. So far, there are only two studies assessing daycare's health effects using detailed administrative health records; namely, van den Berg and Siflinger

(2022) and Bosque-Mercader (2022).

Lastly, I study parental care as the counterfactual (i.e., alternative) care option. In Germany, prior to the expansion, there were almost no care options outside the family[6] and maternal labor market participation was low.[7] In other institutional contexts and countries the counterfactual care mode is different. For example, van den Berg and Siflinger (2022) study a reform that abolished daycare fees in one region in Sweden, which led to a switch from non-parental care to formal daycare arrangements. Thus, the Swedish reform led to a less drastic change for the children than the German reform, which induced a move from family care to daycare.

This paper is structured as follows. In the next section, I discuss the channels through which daycare affects child health in detail. After that, I describe the institutional setting, particularly the daycare expansion. In section 4, I present my data focusing on the construction of my sample and the outcome variables of interest. Next, I outline my empirical strategy and discuss the underlying assumptions. In section 6, I present my empirical results, discuss them and provide a heterogeneity analysis as well as an extensive set of robustness checks. Section 7 concludes.

2 Potential mechanisms

Since daycare attendance may affect health through several channels, it is difficult to anticipate the direction of the effects. First, communicable diseases such as infections, respiratory conditions, and ear problems are very prevalent among (young) children, which is in line with the hygiene hypothesis (Strachan, 1989). In fact, the results of van den Berg and Siflinger (2022), alongside evidence from the medical and epidemiological literature, suggest that there is an association between daycare attendance (at young ages) and the prevalence of communicable diseases (e.g. de Hoog et al., 2014, Kamper-Jørgensen et al., 2006, Watamura et al., 2010).[8] Specifically, van den Berg and Siflinger (2022) use detailed administrative health records for one region in Sweden and exploit a daycare reform that increased daycare exposure by reducing fees for public daycare. Their results suggest that daycare

[6]The main care actors of children below three are parents but also about 30% of children are cared for by other relatives, mainly grandparents (Barschkett, Spiess and Ziege, 2021).

[7]The daycare expansion in Germany is shown to increase labor market participation of mothers with young children (Müller and Wrohlich, 2020).

[8]Note that these studies do not take the endogeneity between daycare attendance and health into account and, thus, do not provide causal evidence.

attendance improves mental health at elementary school age and substitutes infections from elementary school ages to younger ages. Non-parental care arrangements serve as the counterfactual to public daycare in this setting.[9][10] Similarly, injuries are likely to happen more frequently when children interact with other children. For example, Barschkett, Koletzko and Spiess (2021) find that during Covid-19 lockdowns, when daycare centers were closed, the number of diagnoses for injuries reduced.[11] Furthermore, the expansion reduced child maltreatment (Sandner and Thomsen, 2020), which could be reflected in diagnosed injuries. Additionally, evidence from the Sure Start program – an early education program in the UK offering a range of services to support children and parents – supports the arguments on infectious diseases and injuries. Cattan et al. (2021) evaluate short- and medium-term health impacts using administrative health records on hospital admissions. They find exposure to Sure Start leads to an increase in hospitalizations at age one and a decrease at age 11–15. The main drivers of the increase in hospitalizations are infectious diseases in the short run, while in the long run, admissions due to accidents and injuries, infectious illnesses, and mental health-related conditions decrease.

Second, daycare teachers may play an essential role in children forming health habits (e.g., through movement habits and nutrition). Health habits are formed early in life; thus, childhood obesity is strongly correlated with adult overweight. Obesity has its onset often early in childhood[12] and is influenced by health behavior and general lifestyle. Being overweight or obese is an important determinant of skill development during childhood (e.g., Cawley and Spiess, 2008) and future health problems and chronic conditions (such as cardiovascular diseases and diabetes, Must et al., 1999). Interventions to prevent obesity are shown to be particularly effective in children younger than six (Davis and Christoffel, 1994, Waters et al., 2011). Additionally, eating habits – which are crucial causes of obesity – are likely developed early in life (e.g., Birch, 1999). Therefore, daycare attendance can influence health habits and, as a result, prevent obesity. Evidence from a universal daycare expansion during the 1990s for children three and older in Germany supports this argument.

[9] Lundin et al. (2008) show that the reduction in daycare fees had no effect on the labor market participation of Swedish women. This suggests that prior to the reform, children were already in non-parental care.

[10] Aalto et al. (2019) study the health effects of daycare on children with unemployed parents. They do not find an effect on hospitalization rates for children aged 2–3 years. However, the hospitalization rate due to infections increases for preschool-aged children, while there is no effect on the overall hospitalization rate. Furthermore, they provide evidence for the hygiene hypothesis due to reduced prescriptions for allergies and asthma at elementary school age.

[11] Note, this effect is a general "lockdown"-effect and cannot be interpreted causally as a daycare effect.

[12] At age 3 to 6, about 10.8% of girls and 7.3% of boys are overweight (Schienkiewitz et al., 2018).

Specifically, the reform was shown to positively affect children's physical health, i.e., a decline in physician recommendations for compensatory sport (Cornelissen et al., 2018) as well as fewer weight problems and better performance in the gross motor skills test (Lauber, 2015).

Third, there is evidence that daycare attendance is associated with the development of socio-emotional skills (e.g., Baker et al., 2008, Felfe and Lalive, 2018, Peter et al., 2016) and that the formation of socio-emotional skills is at least equally as important as the development of cognitive skills.[13] For example, Currie and Stabile (2006) point out that mental disorders have larger adverse effects on future reading and mathematics test scores than physical health problems. Furthermore, there is evidence that daycare can affect the salivary cortisol level in young children. Higher cortisol levels can be evidence of stress and decrease the antibody levels, which can result in greater illness frequency (Watamura et al., 2010).

Lastly, not only daycare attendance per se but also changes in the child's environment due to the expansion may affect their health. For example, some kind of health surveillance at daycare centers might track children's health (e.g., traces of abuse, detection of vision problems). Attending daycare most likely does not affect the likelihood of having eye problems but rather the *timing* of detecting such problems. Hong et al. (2019) provide evidence that attending a pre-kindergarten program in the US increases the probability of being diagnosed with vision problems, thus leading to earlier onset of treatment. Additionally, as Müller and Wrohlich (2020) show, the daycare expansion increased female labor market participation. On the one hand, employed parents need a doctor's note to take sick leave when their child is sick. On the other hand, employed parents have more time pressure than parents who are not employed. Thus, the incentives to take their child to the doctor more or less often for employed parents could go either way. Furthermore, Schmitz (2020) provides evidence that daycare attendance of children can have positive effects on parental well-being which in turn positively influences children (Berger and Spiess, 2011, Coneus and Spiess, 2012*b*).[14] Based on these contradicting predictions, it remains an empirical question whether the expansion affected children's health outcomes and whether they improved or

[13] There is a large body of literature assessing the effects of daycare on cognitive as well as socio-emotional (mostly measured by child development inidices such as the SDQ) skills. For an overview on this literature, see, e.g., Baker (2011), Elango et al. (2015).

[14] For a recent literature overview on the effects of daycare on various dimensions, including maternal labor market participation and child outcomes, with a focus on Germany, see Spieß (2022).

deteriorated in specific age groups.

One must further note that the effects depend on the counterfactual care mode. In Germany, the counterfactual care mode is family care (mainly provided by parents but also grandparents), while, for example, in Scandinavia, the counterfactual care mode is mostly non-parental care arrangements. Therefore, the differences in the children's environment when moving from family care to daycare is probably more significant than moving from non-parental care to daycare.

3 Institutional setting

In West Germany, traditionally, female labor market participation of mothers with young children is low (e.g., 35% in 2005 for mothers with children below the age of three, Müller and Wrohlich, 2020).[15] Besides incentives set by the tax and transfer system, one frequently quoted reason is the low supply of daycare for (young) children. Since 1996, every child aged three and older has been legally entitled for a daycare slot. As of 2022, almost all children visit a daycare center for at least one year before entering school (Destatis, 2022). Other policy reforms affecting the supply of all-day slots and daycare slots for children younger than three have only been initiated since the middle of the 2000s (Spieß, 2011). In 2005, the daycare expansion law ("Tagesbetreuungsausbaugesetz", TAG) was passed, aiming to expand daycare slots for children under the age of three (230,0000 additional slots in West Germany). In 2007, a summit of the federal government, the federal states, and the counties reinforced the aim of the 2002 EU-mandate[16] and set the target of a 35% daycare coverage rate for children under three years by 2013. Finally, in 2008 the law on support for children ("Kinderförderungsgesetz") was introduced, committing states to a gradual expansion of daycare supply for children below three years. The law also entailed a legal entitlement to every parent with a child aged one to three years to a subsidized daycare slot (in a daycare center or with a childminder) by August 2013.

These reforms induced a large expansion of publicly subsidized daycare slots in both West and East Germany. However, the expansions in East and West Germany differed in their

[15]Due to the division of Germany, social norms, as well as family policies developed differently in East and West Germany. Female labor market participation, as well as daycare coverage, is still much higher today in East Germany than in West Germany (e.g., Müller and Wrohlich, 2020).

[16]In 2002, the European Council set objectives regarding the provision of daycare in the "Barcelona objectives" (European Council, 2002). By 2013, all member states should provide daycare for at least 33% of children below three.

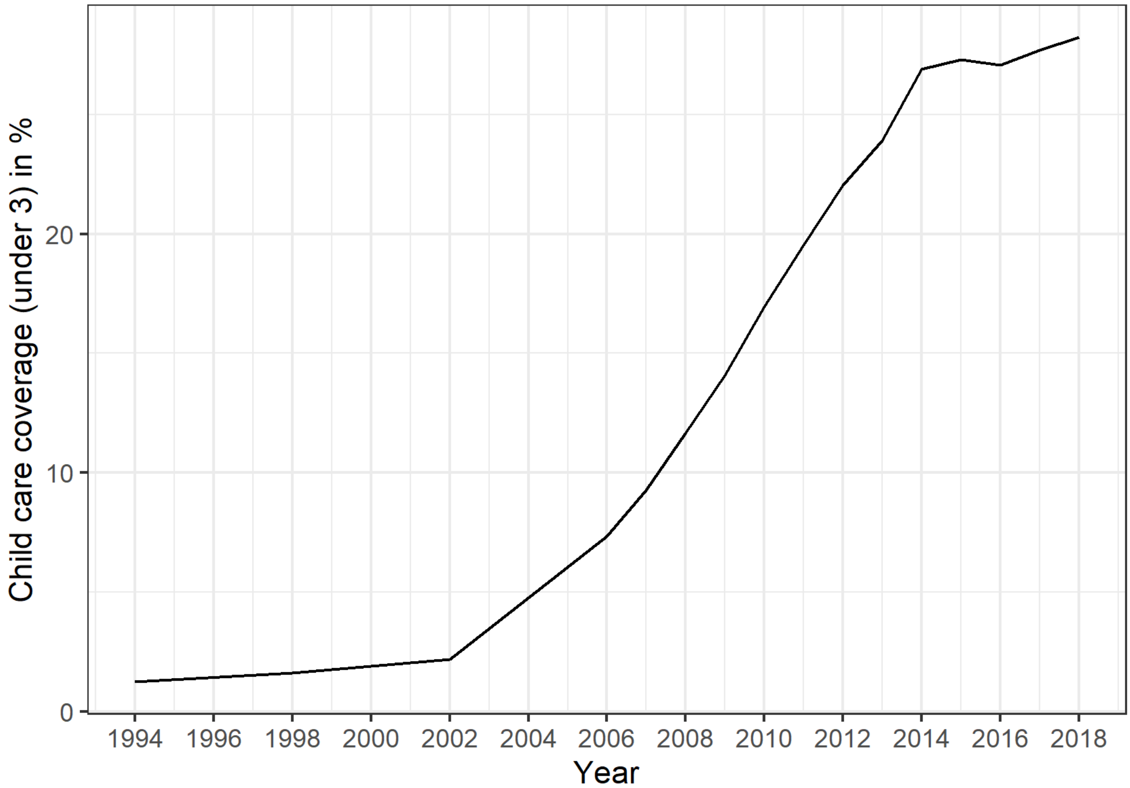
extent and the starting level. In West Germany, daycare coverage for children under three years increased from about 12% in 2008 to 29% in 2018, while it increased in East Germany from about 43% to 55% over the same period (Destatis, 2018).[17] I restrict the analysis to West Germany (excluding Berlin) as the situation in West and East Germany is not comparable and the expansion was significantly larger in West Germany. The development of daycare coverage in West Germany between 1994 and 2018 is depicted in Figure 1. It becomes visible that daycare coverage for children under three years was very low (below 5%) up until the early 2000s. From the mid-2000s, West Germany experienced a steep increase that ran flatter from 2014 onward. The increase experienced until 2018 was significant; however, the goal of a 35% coverage was not reached. Furthermore, the expansion created sizeable regional variation in the expansion speed. Figure 2 shows that, in 2008, the majority of West German counties had a coverage rate below 20%. In 2018, the majority of counties lay above 20%, many above 30%. Additionally, it becomes apparent that the expansion speed differed substantially across counties.

Germany is characterized by a publicly subsidized daycare system. One third of publicly funded daycare slots is provided by local authorities or municipalities, while private providers that are mostly publicly subsidized account for the remaining slots. Private providers include religious non-profit (one third), non-religious non-profit (17 percent), and other providers (15 percent) (Muehler, 2010, Spieß, 2022). Overall, 98% of providers are considered non-profit providers (Destatis, 2018). Generally, daycare is highly subsidized by the federal government, the states, and the municipalities, but the exact amount and source of funds varies by state. Compared to other OECD countries, Germany's public expenses relative to the GDP are slightly above average. Daycare fees typically range between 5 to 9% of net family income (Schmitz et al., 2017), which is below the OECD average (OECD, 2019*a*).[18] Most of the general objectives, strategies, and funding sources of daycare are determined at the federal level. However, operational planning and the implementation of objectives are managed by municipal governments and/or youth welfare offices. Thus, the structure and organization of daycare vary between states and communities (Hüsken, 2011, Müller and Wrohlich, 2020). Local authorities estimate the local demand for daycare slots

[17] Daycare coverage is defined as the share of children being in daycare, entailing daycare centers and childminders. The majority of children visit a daycare center. In 2018, only 5.4% were cared for by a childminder (Destatis, 2018). As there is ongoing, persistent, excess demand for daycare slots, I assume a full take-up of newly created daycare slots for children below three years in the subsequent analyses (Müller and Wrohlich, 2016, Wrohlich, 2008).

[18] For a more detailed overview of the organization and funding of German daycare centers, see, e.g., Huebener et al. (2020).

Figure 1: Daycare coverage (under 3) West Germany



Notes: The graph shows the daycare coverage rate for children below three years in West Germany.
Source: Destatis 1994-2018, own calculations.

and develop an expansion strategy accordingly. However, the procedure is not uniform across municipalities, thus leading to the observed differences in the expansion speed.

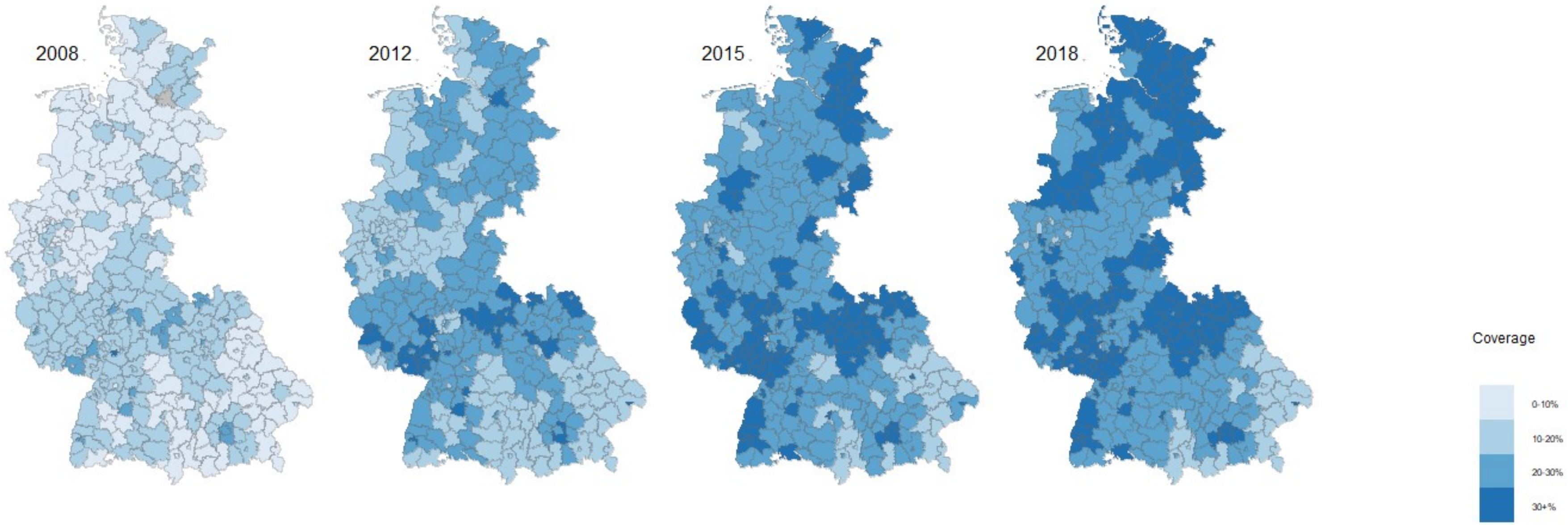
Previous work has established that the expansion increased female labor market participation (Müller and Wrohlich, 2020), increased fertility (Bauernschuster et al., 2016), reduced child maltreatment (Sandner and Thomsen, 2020), and improved children's socio-emotional skills Felfe and Lalive (2018).[19] Furthermore, even though daycare is universal and open to all families, the take-up rate of the scarce daycare slots for children below the age of three is higher among highly educated and non-migrant families (e.g., Jessen et al., 2020).

4 Data

For the analysis, I use administrative data covering 2009 through 2019,[20] collected by all public health insurers in Germany. The data are based on the database of claims of all

[19]For an overview of the effects of the daycare expansion, see, e.g., Rainer et al. (2013), Spieß (2022).
[20]Data are only available from 2009, and years from 2020 are excluded from the analysis due to the Covid-19 pandemic.

Figure 2: Daycare expansion during 2008 and 2018



Notes: The maps of West Germany show the daycare coverage rate for children below three years per county for different years in percent.
Source: Destatis 1994-2018, own calculations.

publicly insured individuals in Germany as collected by the Association of Statutory Health Insurance Physicians and then forwarded to the National Association of Statutory Health Insurance Physicians (Kassenärztliche Bundesvereinigung, KBV). In the data, physicians record a standardized diagnosis for each claim in order to be reimbursed by the health insurance. In Germany, health insurance is mandatory and characterized by public and private insurance systems. Nearly 90% of the German population is covered by one of the public health insurance funds.[21] Only individuals with earnings exceeding a certain threshold[22] and individuals in specific occupational groups (e.g., civil servants and self-employed) are allowed to opt out of the public system and sign up with a private insurance company instead. The health insurance of their parents covers children without extra fees.

4.1 Sample

I have access to data covering 2009 through 2019; thus, I am able to estimate age-specific health effects. To do so, I split the sample into four different age groups: toddlers (1–2 years), kindergarten-aged children (3–5 years), early elementary school-aged children (6–8 years), and older elementary school-aged children (9–10 years). Exploiting the daycare expansion described in section 3, I construct a treatment and a control group depending on the children's birth year and their county of residence when I first observe them. In the analysis, I focus on children born between 1999–2015. Comparing the number of public health insured children (Gesundheitsministerium, 2020) with official birth records for each birth cohort (Statista, 2021) suggests that I cover about 86% of all children born in Germany in the respective birth years.[23][24]

The data include information about all diagnoses patients received during the observed period. Each diagnosis constitutes a new entry meaning that the number of observations equals the number of diagnoses over the observed period. Thus, the sample is unbalanced

[21] Mandatory contributions from employers and employees combined with tax revenues are the primary financing sources of the German public health insurance. First, contributions are pooled in a Central Health Fund. Secondly, the contributions are reallocated to the sickness funds according to a morbidty-based risk adjustment scheme. For more information about the German health insurance system, see OECD (2019*b*).

[22] The income threshold for 2022 was 64,350 euro ($\approx 62,734$ dollar) per year.

[23] Note, there is only aggregated data on the number of publicly health insured individuals for 0–14-year-old children available for 2004–2020. To obtain an estimate of how many children are covered in the data, I add the official births for the respective birth cohorts that are 0–14 years old for each year between 2009 and 2019.

[24] The number of public health insured children in Germany also includes children who immigrated to Germany, while the official birth records do not include children who immigrated after birth. In contrast, the number on insured children does not include children who emigrated from Germany, while these are included in the birth records. Hence, the estimated share of 86% of children born in Germany might be imprecise as it suffers from the exclusion of emigrating/immigrating children in the official numbers.

because patients only appear if they received outpatient care, including a diagnosis. Based on this information, I construct a balanced sample with yearly information for all publicly insured children.[25] The final data set includes about 550,000–650,000 children per birth cohort resulting in about 11 million children overall. More detailed information on the data and the sample is provided in the Appendix section A.

4.2 Outcome variables

I define measures for physical and mental health using ICD–10 codes. Instead of estimating the effect for about 70,000 diagnoses categorized by the ICD-10 codes, I use broader 2–digit categories. Table 1 gives an overview of the considered diagnoses in this study. In addition to the aggregated set of diseases (2-digit level), I provide results for more narrowly defined diagnoses (3- and 4-digit levels of ICD-10 codes). In particular, I select diagnoses captured within the studied sets of diseases that belong to the 50 most frequently reported diagnoses by pediatricians (see ZI, 2015). These are in detail presented in Appendix section A.

Similar to van den Berg and Siflinger (2022), I assess three aspects of health: physical health (communicable and non-communicable diseases), mental health, and healthcare consumption. My core physical health measures for communicable diseases capture the following three sets of health conditions: *respiratory diseases* (ICD–10 codes J00–J99), *infections* (ICD-10 codes A00–B99), including any bacterial or viral infection, and *ear problems* (ICD–10 codes H60–H95), capturing diagnoses on the external ear, the middle ear, and the internal ear. These three sets of conditions of communicable diseases are mutually exclusive, meaning that the pediatrician (or another healthcare professional) settles on one ICD-10 code as a diagnosis. However, the conditions are closely related and could be in a causal relationship. In particular, many diagnoses concern contagious diseases common in childhood and often transmit among children; thus, they likely also spread in daycare centers. Many infections may accompany respiratory problems and cause subsequent ear problems. Furthermore, some respiratory diseases or ear problems concerning inflammations could result from infections. Hence, depending on the coding practice of the physician, the three conditions could fall under all three sets of diagnoses. Consequently, to capture all infections, it is essential to study all three groups. As additional measures for physical health (non-communicable diseases), I assess *obesity*, *vision problems*, and *injuries*. Obe-

[25] As outlined in Appendix section A, almost all children appear at least once during the 11-year observation period as they make use of early diagnostic tests.

sity is categorized in ICD-10 codes E65–E68. Additionally, I assess *injuries* (ICD-10 codes S00-S99), which include all kinds of injuries to all body parts (e.g., injuries to the head or knee). Lastly, I consider *vision problems* (ICD-10 codes H00-H59).

To assess the effects on *mental health* including behavioral problems, socio-emotional abilities, and mental health problems, I analyze the effect on ICD–10 codes F00-F99. Note, I measure socio-emotional skills by diagnosed mental and behavioral disorders, which is certainly more extreme than typical measures obtained from survey data (e.g., SDQ-Index). Thus, I rather focus on below-average socio-emotional developments than capturing children's full range of socio-emotional abilities. However, measures in survey data might also underreport socio-emotional development problems as survey respondents (mostly parents) are not professionally trained to recognize behavioral disorders and might have difficulties accepting that their child exhibits behavioral disorders.

To measure communicable diseases and injuries, I compute the annual number of diagnoses per child as a measure of the intensive margin in my main specification (count variables). For chronic conditions (obesity, mental disorders, vision problems), I use the extensive margin, which I construct as binary indicator variables that marks whether a child had a specific diagnosis at least once in a given year. This definition is analogous to Barschkett et al. (2022b) who work with the same data.

Lastly, I consider health care consumption and healthcare costs. Healthcare consumption is defined as doctor visits measured as treatment cases, aggregated at the calendar year level (official term: "Artzfälle"). One treatment case is defined as a treatment of an insured person by a doctor in a quarter, billed to one public health insurance fund.[26] Thus, if a child visits two different doctors in a quarter, she has two treatment cases in that specific quarter.[27] I aggregate quarterly cases to the calendar year level, thus counting the number of quarterly treatment cases per year. This means that a patient who visits only the same doctor every quarter would have a yearly count of four treatment cases, irrespective of the actual number of visits to this doctor per quarter. Consequently, the number of treatment cases underestimates the actual number of doctor visits. Similarly, healthcare costs are documented on the quarter level and include all costs billed from ambulatory care doctors.

[26]Since treatment cases are recorded this way in the data, I cannot define the variable differently for my application.

[27]If she visits only one doctor but switches the health insurance providers, she would also be assigned two doctor visits. However, since only 3% of children in my sample switch their health insurance provider, this issue is negligible.

I also aggregate the costs to the calendar year level and adjust them to 2009 fees.[28]

Summary statistics for the outcome variables, including the sample means for annual diagnoses as well as the prevalence of the diseases, are shown in Table 1. For all three sets of communicable diseases, the number of diagnoses per year, as well as the prevalence, decreases with age. On average, 1–2-year-old children have about 1.4 infections per year, while 9–10-year-old children have only about 0.7 infections per year. Respiratory diseases have the highest prevalence among all considered outcomes, e.g., 80% of 1–2-year-olds have a respiratory diagnosis at least once per year. In contrast, the likelihood of obesity increases with age, while the prevalence of mental disorders and vision problems increases until age 3–5 and is relatively constant across older age groups. The number of injuries is relatively stable across age groups. The annual number of treatment cases decreases with age; 1–2-year-olds have, on average, 6.3 treatment cases per year, while 9–10-year-olds have only 4.9 treatment cases per year. In line with decreasing treatment cases with age, healthcare costs are higher for younger children (on average 320 Euros per year for 1–2-year-old children) than for older children (249 Euros per year for 9–10-year-old children).

4.3 Control variables and daycare coverage rates

The KBV data only includes a few individual-level socio-demographic characteristics, including age, gender, year, birth month, and county of residence. Additionally, county-level ("Landkreise") information, such as the share of migrants and average household income, can be used for heterogeneity analyses. Furthermore, I extract information on the incidence of swine flu at the county level between 2009 and 2011 from the RKI Survstat dashboard (RKI, 2022) as there was considerable regional variation in the incidence across Germany during the swine flu epidemic.[29] I merge these data to the KBV data and use the swine flu incidences as control variables. The KBV data does not contain information about individual childcare arrangements, i.e., I do not observe if children attend daycare. Therefore, I merge the KBV data with county-level information on the share of children enrolled in daycare. As of 2022, after multiple county reforms that reorganized the counties, there are

[28] Fees are adjusted to 2009 fees. This adjustment accounts for the general increase in the fee level and specific changes to the medical system. (The time series "Honorarumsatz je Behandlungsfall in Euro" from 2009–2018 was used to adjust fees, KBV, 2019).

[29] The swine flu pandemic lasted from 2009 to 2010 (with cases still being prevalent in 2011) and was particularly prevalent among children. For example, in 2009, about half of all swine flu cases occurred in children under 15 (RKI, 2010*a*). The incidence differed across regions and age groups (e.g., Buda et al., 2010, RKI, 2010*b*).

Table 1: Outcomes

	1-2 years	3-5 years	6-8 years	9-10 years
Communicable diseases				
Infections (no. per year)	1.39 (1.59)	1.00 (1.29)	0.78 (1.11)	0.66 (1.04)
Infections (prevalence)	0.63 (0.48)	0.53 (0.50)	0.46 (0.50)	0.40 (0.49)
Ear diseases (no. per years)	0.58 (1.13)	0.84 (1.47)	0.45 (1.09)	0.28 (0.84)
Ear diseases (prevalence)	0.33 (0.47)	0.39 (0.49)	0.24 (0.43)	0.16 (0.37)
Respiratory diseases (no. per year)	2.85 (2.67)	2.65 (2.75)	1.85 (2.35)	1.58 (2.22)
Respiratory diseases (prevalence)	0.81 (0.39)	0.77 (0.42)	0.65 (0.48)	0.58 (0.49)
Non-communicable diseases				
Mental disorders (no. per year)	0.31 (0.87)	0.87 (1.63)	1.057 (2.15)	1.03 (2.38)
Mental disorders (prevalence)	0.18 (0.38)	0.37 (0.48)	0.33 (0.47)	0.27 (0.45)
Obesity (no. per year)	0.02 (0.21)	0.04 (0.32)	0.06 (0.40)	0.10 (0.50)
Obesity (prevalence)	0.01 (0.12)	0.02 (0.15)	0.03 (0.18)	0.05 (0.22)
Injury (no. per year)	0.22 (0.56)	0.19 (0.54)	0.19 (0.57)	0.24 (0.65)
Injury (prevalence)	0.17 (0.37)	0.14 (0.35)	0.13 (0.34)	0.16 (0.37)
Vision problems (no. per year)	0.59 (1.16)	0.82 (1.56)	0.79 (1.60)	0.71 (1.45)
Vision problems (prevalence)	0.34 (0.47)	0.38 (0.49)	0.34 (0.47)	0.32 (0.47)
Healthcare consumption				
Treatment cases	6.33 (3.84)	6.14 (4.04)	5.28 (7.46)	4.92 (8.91)
Healthcare costs	320 (313)	287 (320)	245 (393)	249 (450)
Observations	9,042,454	16,840,400	17,167,518	11,674,867

Notes: Reported are means and standard deviations in parentheses. "No. per year" indicates count variables, i.e. contains the number of diagnoses per year. "Prevalence" indicates dummy variables, i.e. indicates the share of children who had at least one diagnosis per year. Costs are fee-adjusted.
Source: KBV 2009–2019, own calculations.

401 counties in Germany. Since 2006, the German Statistical Office has provided data on daycare coverage annually. Before 2006, only data for 1994, 1998, and 2002 are available. I restrict the analysis to West Germany (323 counties).

5 Empirical strategy

To estimate the effect of the daycare expansion on children's health outcomes, I exploit spatial and temporal variation in the daycare expansion by employing difference-in-difference (DiD) and event study (ES) approaches. Specifically, I compare health outcomes of children born before and after the expansion from counties where daycare expanded a lot (treatment group) and counties with little or no increase in daycare coverage (control group). A similar design is also used by, e.g., Havnes and Mogstad (2011), Müller and Wrohlich (2020), and

Bauernschuster et al. (2016). Recent research has identified problems with DiD with staggered implementation utilizing two-way fixed effects and created new estimators to address these issues (e.g., Borusyak et al., 2021, Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). I address this issue in section 6.3. Below, I describe my main empirical strategy and discuss potential threats to identification and alternative specifications to validate my results.

The daycare expansion generated a steady increase in the coverage rate from the mid-2000s until 2014 on the national level (see Figure 1). Specifically, the expansion started in the mid-2000s, peaked between 2008 and 2012, and substantially slowed from 2014 onward. However, Figure 2 reveals considerable heterogeneity on the regional (county) level in the total coverage rate and the expansion speed. In my main specification, a generalized DiD, I use the heterogeneous treatment intensity across counties and regress the outcomes directly on the daycare coverage rate in each county controlling for year and county fixed effects and a set of control variables. The advantage of directly regressing the outcomes on the daycare coverage rate is that i) I do not need to make assumptions on the definition of treatment and control group (definition of affected cohorts and counties), and ii) I use the whole variation in treatment intensity across counties. With this approach, I closely follow Müller and Wrohlich (2020).

Specifically, I estimate in a Two Way Fixed Effects (TWFE) framework the following equation:

$$Y_{it} = \alpha_c + \psi_j + \theta cc_{cjt} + X_{it}\beta + \varepsilon_{ijt} \tag{1}$$

where Y_{it} represents the health outcomes of child i at age t.[30] cc_{cjt} is the average child care coverage rate in county c for birth cohort j at age $t \in age[1,2]$[31] with θ being the coefficient of interest. α_c and ψ_j refer to county and birth cohort fixed effects, respectively, and X_{it} is a vector of control variables containing age, gender, and birth cohort dummies interacted with the swine flu incidence in 2009, 2010, and 2011.

[30] In the main specification, I exclude extreme outliers in the dependent variables (count variables), i.e., observations in the top 99.9999 percentile. In robustness checks, I include all observations. The results do not change and are available upon request.

[31] Note, due to data limitations, I only observe child i's county of residence from 2009 onward. Thus, for children born before 2009, I use the county of residence observed in 2009. I use the observed county of residence for all other children when they are 1–2 years old. This assumption is plausible as in my sample, only 8% of children move and only 1% of children move from a treatment to a control county as defined in section B.

Two important aspects must be considered to validly estimate the effect of the daycare expansion within a DiD approach. First, the variance-weighted common trend assumption needs to hold, i.e., in the absence of the expansion, health outcomes across counties should have evolved in parallel conditional on covariates. If the identifying assumption does not hold, the effects cannot be interpreted causally. The validity of this assumption cannot be tested directly. However, I perform several checks on the plausibility of this assumption.

First, I follow Havnes and Mogstad (2011) and Bauernschuster et al. (2016) and specify a standard DiD framework (Equation (B.1)) where the variable of interest is an interaction term between the treatment indicator $Treat_i$ (child lives in a county where daycare expanded a lot) and the reform indicator $Post_i$ (child was born after the expansion). The DiD approach controls for unobserved differences between children from treatment and control counties as well as between children born in different years. In the Appendix section B, I outline the DiD framework in more detail. Based on the DiD framework, I provide event study graphs drawing on similar regressions as presented in Equation (B.1) in Appendix section B to verify the plausibility of the common trend assumption. In this specification, the $Post$-indicator is replaced by a $Cohort$-indicator, which includes the birth year of children.[32] If no pre-trends are present, the coefficients on the interaction between $Cohort_i$ and $Treat_i$ should be small and insignificant for all birth cohorts born before the expansion. The identification in an event study approach is robust to time-varying treatment effects (Goodman-Bacon, 2021). Graphical evidence is shown in section 6.[33] Second, I implement a placebo regression employing a chronic disease (diabetes mellitus) as an outcome variable, as diabetes should be unaffected by exposure to daycare or other environmental factors.

The second issue involves the correct calculation of standard errors. In order to correct for possible serial correlation of the error terms, I report heteroskedasticity-consistent standard errors clustered at the county level. This leads to asymptotically valid inference; however, in finite samples like mine (323 clusters), the problem may still be present (Cameron and Trivedi, 2005). Therefore, I additionally estimate wild-bootstrapped clustered standard errors with 9,999 repetitions (Cameron et al., 2008). Under the common trend assumption and the assumption that the marginal effect of an additional daycare slot is constant, θ

[32] In an alternative specification, I use the percentage point increase between 2008 and 2012 as a continuous treatment variable. The DiD and event study results are very similar.

[33] Due to data availability, the event-study results are only presented for 6–8-year-old children because, for this age group, a sufficiently large number of pre and post-birth cohorts are available. It seems plausible that the common trend assumption's plausibility can be extrapolated to the other age groups.

can be interpreted as the causal effect of an increase in the daycare coverage rate on the outcomes of interest. The parameter θ is an Intention-To-Treat (ITT) parameter, as it measures the effect of increasing access to rather than actual use of daycare.

Because I control for county-specific fixed effects, the daycare expansion does not need to be unrelated to time-invariant county characteristics. However, it is useful to understand the determinants of the expansion across counties. In Appendix Table C.1, I investigate differences in socio-demographic characteristics between treatment and control counties in 2008. The definition of treatment and control group is based on the DiD framework explained in the Appendix section B. I can depict only minor differences between treatment and control counties for most characteristics. Interestingly, at the beginning of the expansion period, treatment counties exhibit a slightly higher daycare coverage rate (12.5 vs. 10.7%). Furthermore, the unemployment rate is slightly higher in control counties (5.9 vs. 6.7%), the share of migrants is higher in control counties (6.0 vs. 10.1%), the population density is almost three times as large in control counties compared to treatment counties (299.5 vs. 847.9) and GDP per capita is also higher in control counties (26,435 vs. 33,429 Euros). Thus, in some aspects treatment and control counties are fairly comparable in their socio-demographic composition, while they differ in others. Since the characteristics that exhibit differences are likely stable across the observation period, I control for the differences with the county fixed effects.

6 Empirical results

The following section describes and discusses the results of my main specification. Furthermore, I provide evidence for the robustness of my results, present alternative specifications, and show results for more narrowly defined outcomes and from heterogeneity analyses.

6.1 Generalized DiD results

Table 2 reports the results of the daycare expansion on children's health, obtained from estimating Equation (1). The first three panels display results for the three sets of communicable diseases: Infections, ear diseases, and respiratory diseases. Panels four to seven show the results for mental disorders, obesity, injury, and vision problems. Lastly, the bottom two panels represent the results for healthcare consumption measured by annual treatment cases and healthcare costs. Column 1 shows the results for all children aged 1–10 years. Columns 2 to 5 report age-specific results, i.e., for 1–2 year-old children (column 2), 3–5 year old children (column 3), 6–8-year-olds (column 4), and 9–10-year-olds (column 5). To account for the large number of outcomes and the finite number of clusters, I also report adjusted p-values (known as q-values) for multiple hypotheses testing following Benjamini and Hochberg (1995) to control for the false discovery rate (i.e., the expected proportion of rejections that are type I errors)[34] and wild-bootstrapped clustered standard errors (Cameron et al., 2008).[35] Additionally, the last row in each panel shows the sample mean.

Communicable diseases. For all three sets of communicable diseases, being affected by the expansion leads to a positive instantaneous effect, i.e., an increase in the number of diagnoses for 1–2-year-old children, and negative effects in the long run, i.e., a decrease across older age groups. Specifically, the effects for 1–2-year-old children are highly significant (on the 0.01% significance level) and amount to 0.008 for infections, 0.003 for ear diseases, and 0.016 for respiratory diseases. This means that an increase in the daycare coverage rate by ten percentage points increases the number of diagnoses by 0.08, 0.03, and 0.16, respectively. These estimates correspond to a 5.7% increase for infections, 5.1% for ear diseases, and 5.6% for respiratory diseases compared to the sample means. While the effects on 3–5-year-olds are small and statistically not significant, the results depict negative and significant effects for infections and respiratory diseases for elementary school children (3.9% and 2.2% for 6–8-year-olds and 6.0% and 3.8% 9–10-year-olds, respectively, compared to the sample means). The effects on ear diseases for older children are statistically not significant. The effects when pooling all age groups together (1–10-year-old children) are small and insignificant for infections and respiratory diseases, suggesting that the expansion leads to a shift of illness spells from elementary school age to early daycare age. The total number of infections and respiratory diseases children suffer during their first ten years of life is not affected. For ear diseases, there is a small and positive effect for the pooled age group suggesting that a ten percentage point increase in daycare slots leads to an increase of 3.6% in the number of ear diseases children are affected by between one and ten years.

Non-communicable diseases. The estimates for mental disorders go in the same direction

[34]Compared to familywise error rate controlling methods such as the Bonferroni correction, this approach has greater power and reduces the penalty to testing additional hypotheses (Anderson, 2008, Benjamini and Hochberg, 1995).

[35]To estimate the wild-bootstrapped clustered standard errors, I apply the R-package *fwildclusterboot*, which is based on the method developed by Roodman et al. (2019). Due to computational power, the estimation only works in the samples for specific age groups.

Table 2: Generalized DiD Results

	Age: 1-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases					
Infections	0.001	0.008***	0.001	−0.003**	−0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
q-value; boot strapped p-value	0.191	0.000; 0.000	0.613; 0.352	0.009; 0.001	0.000; 0.000
Sample Mean (no. per year)	0.924	1.394	1.000	0.777	0.665
Ear diseases	0.002*	0.003***	−0.001	−0.00001	−0.001*
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
q-value; boot strapped p-value	0.037;	0.001; 0.000	0.619; 0.437	0.977; 0.974	0.045; 0.034
Sample Mean (no. per year)	0.558	0.583	0.84	0.454	0.284
Respiratory diseases	−0.0002	0.016***	−0.001	−0.004*	−0.006***
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)
q-value; boot strapped p-value	0.910;	0.000; 0.000	0.774; 0.698	0.049; 0.017	0.001; 0.000
Sample Mean (no. per year)	2.207	2.854	2.653	1.852	1.583
Non-communicable diseases					
Mental disorders	−0.001*	0.001**	0.0001	−0.001**	−0.001***
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)
q-value; boot strapped p-value	0.037;	0.011; 0.004	0.774; 0.777	0.022; 0.004	0.000; 0.000
Sample Mean (prevalence)	0.305	0.177	0.37	0.329	0.275
Obesity	0.0002*	0.0001+	0.0001*	0.00005	−0.0005***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
q-value; boot strapped p-value	0.037;	0.094; 0.068	0.349; 0.040	0.703; 0.497	0.000; 0.000
Sample Mean (prevalence)	0.031	0.014	0.024	0.033	0.051
Injury	−0.0003*	0.0005*	−0.0003	−0.0001	0.0002
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
q-value; boot strapped p-value	0.060;	0.021; 0.068	0.576; 0.040	0.745; 0.497	0.422; 0.000
Sample Mean (no. per year)	0.204	0.216	0.19	0.189	0.239
Vision problems	0.0002	0.001***	0.0003	−0.0004	−0.001*
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
q-value; boot strapped p-value	0.426;	1; 0.068	0.613; 0.040	0.216; 0.497	0.017; 0.000
Sample Mean (prevalence)	0.349	0.34	0.381	0.341	0.322
Healthcare consumption					
Treatment cases	−0.006**	0.011**	−0.012***	−0.006*	−0.005+
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)
boot strapped p-value		0.001	0.001	0.011	0.086
Sample Mean (no. per year)	5.638	6.331	6.135	5.279	4.911
Healthcare costs	−0.754***	1.499***	−0.064	−0.495**	−0.918***
	(0.176)	(0.164)	(0.183)	(0.165)	(0.260)
boot strapped p-value		0.000	0.736	0.001	0.000
Sample Mean (no. per year)	271.106	319.964	287.281	244.466	249.115
Control for age + gender	yes	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes
Birth cohorts	2000-2014	2008-2014	2006-2014	2003-2011	2000-2009
Observations	54,152,621	8,522,322	14,117,165	13,979,553	10,605,774

Notes: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. q-values are p-values adjusted for multiple hypothesis testing following Benjamini and Hochberg (1995). Boot strapped p-values are calculated based on wild-bootstrapped clustered standard errors accounting for a finite number of clusters.
Source: KBV 2009–2019, own calculations.

as the estimates for communicable diseases, i.e., an increase in the short run and a decrease in the long run. In particular, there is a positive effect for 1–2-year-old children (5.6%) and negative effects for children in elementary school (3% and 3.6%). For obesity, most point estimates are positive but not statistically significant, with 9–10-year-old children being an exception (decrease of 9.8%). Finally, I obtain mostly insignificant estimates for injuries and vision problems across all age groups.

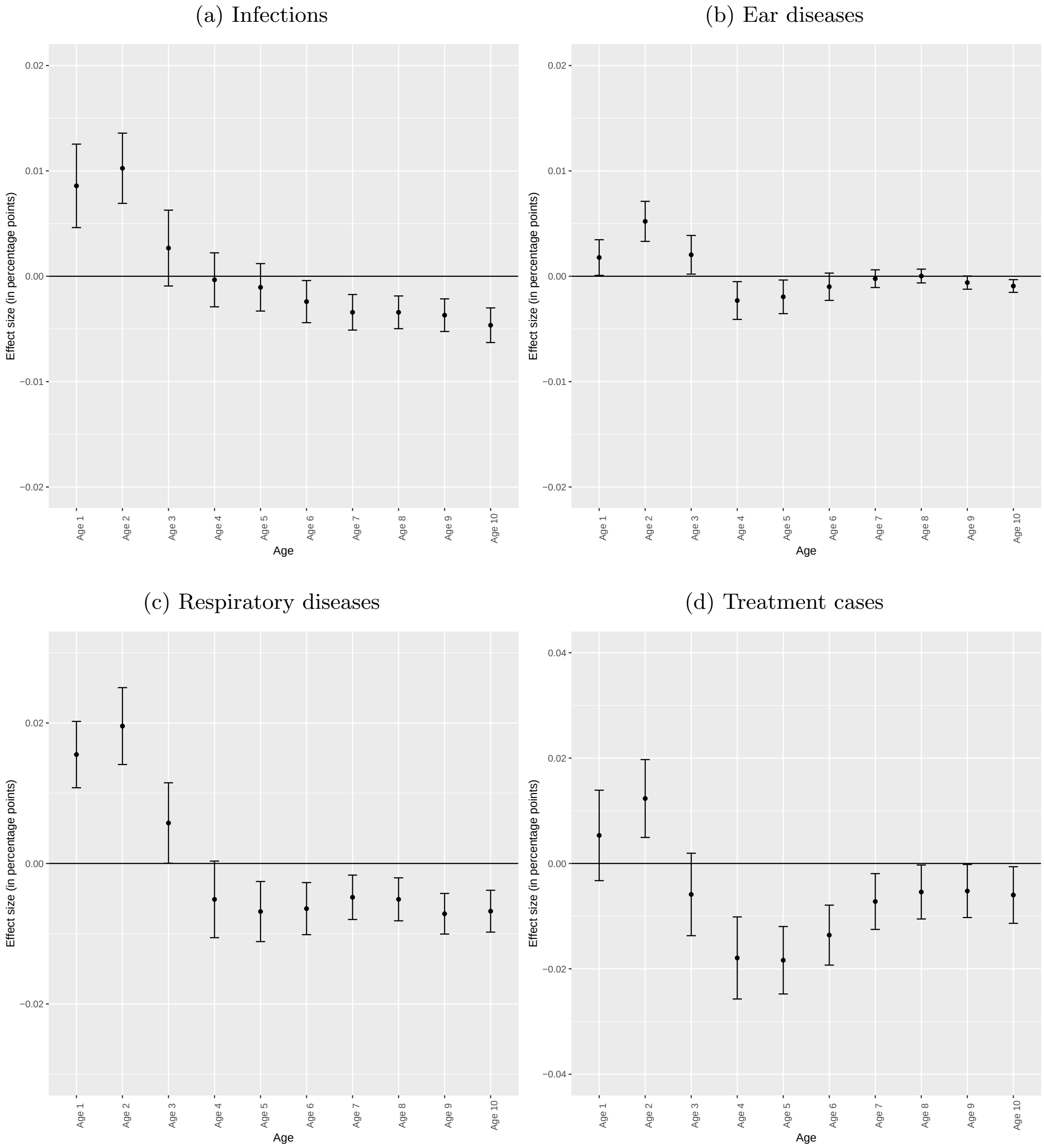
Healthcare consumption. For treatment cases, the results point out an increase for 1–2-year-olds (1.7%) and a decrease for all other age groups (2% for 3–5-year-olds and 1.1% for 6–8-year-olds). The decreases for older age groups outweigh the increase at age 1–2 years, suggesting that, overall, children between one and ten visit a doctor less often when exposed to daycare (reduction of 1.1%). The estimates for healthcare costs point in the same direction as for healthcare consumption: positive for 1–2-year-olds and negative for 3–10-year-olds. The effects are all highly significant (except for 3–5-year-olds) and range between -2% (6–8 years) and 4.7% (1–2 years). Overall, there is a slight decrease of 2.8% (1–10 years).

6.2 Age-specific results

To better understand the effects of daycare across the age distribution, I plot the coefficients for all age groups separately. Figure 3 displays the results age-specific results for infections, ear diseases, respiratory diseases, and treatment cases, i.e., for all outcome variables that prove to be significantly and robustly[36] affected by the expansion. The underlying estimates, as well as the results for the remaining outcomes, are shown in Appendix Table D.1. Similar to the pooled results, estimates for all communicable diseases are positive and significant at ages one to three. From age five or six, estimates turn negative and significant for respiratory diseases and infections, respectively. I only depict significant negative effects for ear diseases at ages four, five, and ten. The effects on treatment cases are positive and significant at age two but negative and significant for all age groups from age four. Interestingly, effects do not fade out with age, but effect sizes are relatively stable for the different age groups.

[36]See section 6.3 for a discussion on robustness of the results.

Figure 3: Age-specific results



Notes: The graphs show age-specific effects for 1–10 year old children for infections (panel (a)), ear diseases (panel (b)), respiratory diseases (panel (c)) and treatment cases (panel (d)). The estimates are based on Equation (1).
Source: KBV 2009–2019, own calculations.

6.3 Robustness

Standard DiD results. Appendix Table E.1 reports the standard DiD results obtained from estimating Equation (B.1) for children aged 3–10 years.[37] Generally, the DiD results show a similar picture as the generalized DiD results, i.e., decreases in communicable diseases

[37]Due to data limitations, it is not possible to apply this approach for 1–2-year-old children.

and treatment cases in the long run and mostly insignificant or small estimates for injuries and vision problems. However, the table shows a different picture for mental disorders, obesity, and healthcare costs compared to the generalized DiD results. For obesity and mental disorders, all coefficients are positive but mostly statistically insignificant. Despite the significant changes in the frequency of diagnoses and healthcare consumption, I do not depict a significant effect of the expansion on healthcare costs for most age groups. Only the estimate for 9–10-year-old children is statistically significant, suggesting an increase in healthcare costs. Since the DiD estimates on obesity, mental disorders, and healthcare costs contradict my main results, I refrain from interpreting the results further.

Common trend assumption. In order to causally interpret my results, the common trend assumption needs to hold. I provide evidence for the plausibility of this assumption with the event study graphs presented in the Appendix in section E, which are estimated based on Equation (E.1). Figure E.1 presents event-study results for 6–8-year-old children for the three sets of communicable diseases. For infections and respiratory diseases, the point estimates to the left side of the cutoff are mostly insignificant, suggesting a common trend in the absence of expansion. To the right side of the cutoff, the point estimates are negative for both diseases. For respiratory diseases, almost all estimates are statistically significant, while most are only marginally significant or insignificant for infections. This might be explained by a lack of statistical power when estimating the results separately for all birth cohorts. As expected from the insignificant results in the DiD, there is no trend visible for ear diseases – neither before nor after the cutoff. For the other diseases, there is also no trend visible (Figure E.2), suggesting that the common trend assumption is plausible. For treatment cases and healthcare costs, there is also a common trend visible before the reform and in line with the DiD results negative (but insignificant) point estimates after the cutoff for treatment cases (Figure E.3).

To further prove the validity of this assumption, I relax the assumption of constant marginal effects by adding a quadratic term in child care coverage to Equation (1). The estimates point in the same direction as the main results, but standard errors are larger for some estimates, reducing statistical significance (Appendix Table E.2). Furthermore, I exclude the $phase - in$ dummy in the regressions for 6–10-year-old children in the DiD. Results do not change compared to the main results (Appendix Table E.3). Finally, I conduct a placebo analysis to provide evidence that my results reflect a reform effect and not just some underlying time trend. I choose diabetes mellitus as an outcome variable, a chronic disease that should not be affected by environmental factors such as daycare attendance. The results show for all age groups very small coefficients which are statistically not significant (Appendix Tables E.4 and E.5). To ensure that the results are not driven by secular changes between urban and rural areas coinciding with the reform, I further drop all cities (Kreisfreie Städte) with more than 500,000 inhabitants. The results hardly change compared to the main results (Appendix Tables E.6 and E.7). In summary, these tests support the plausibility of the common trend assumption, thereby supporting the causal interpretation of my results.

Heterogeneous treatment effects. Recent developments in the DiD literature indicate that TWFE estimators may be subject to biases in staggered treatment implementations under heterogeneity in groups and time. This stems from the fact that the TWFE estimator is a weighted sum of the average treatment effects (ATE) in each group and period. Weights sum to one but individual weights may be both positive and negative (e.g., Borusyak et al., 2021, Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). In the context of the daycare expansion, heterogeneity of treatment effects across time is possible, and the staggered rollout might assign negative weights to some treatment effects.[38] Despite the literature's focus on discrete treatment variables, Callaway et al. (2021) point out that the issue of negative weights can also arise in specifications with continuous treatment definitions. Thus, the estimates of my main specification (generalized Did) may be subject to biases arising from negative weights. However, no adjusted estimator is currently available for continuous treatment definitions. In contrast, in my standard DiD framework, by construction, the treatment implementation is not staggered, i.e., the treatment is only implemented at one point in time. Thus, I do not face the problem of time-specific treatment effects. To assess whether the treatment effect varies over time, I estimate the standard DiD specification in Equation (B.1) with different definitions of the expansion period. The results displayed in Appendix Tables E.8-E.16 are very similar to the baseline results suggesting that varying treatment effects over time are not relevant in this context. Despite being unable to account for negative weights in the generalized DiD, I am confident that the potential bias is negligible, as the standard DiD framework largely confirms the generalized DiD results.

[38]In contrast, treatment effect heterogeneity across groups is not relevant as there is an excess demand for daycare slots across the country.

Alternative treatment definitions. In my baseline specification of the DiD, I define treatment and control counties by ordering counties by their percentage point increase during the main expansion period and separating the sample at the 30th/70th percentile. In order to test the sensitivity of my results toward this assumption, I run the analysis with other definitions. Specifically, I compare the upper 50% with the lower 50%, the upper 40% with the lower 40%, the upper 35% with the lower 35%, the upper 25% with the lower 25% and the upper 20% with the lower 20%. Finally, I estimate a specification where I use the percentage change increase within the main expansion period instead of a dummy, indicating that a country was above or below a cutoff. Appendix Tables E.17 – E.25 display the results for the different outcomes. The coefficients for ear diseases, injury, and vision problems remain insignificant across most specifications. The results on infections, respiratory diseases, obesity, and treatment cases are, in terms of significance, hardly different from the main results across the different treatment definitions. However, the point estimates increase in magnitude with increasing percentile, i.e., the point estimates are the smallest in the most conservative definition (median separation). This can be explained by the fact that the median separation includes comparing children from counties with very similar expansion rates (just below the median vs. just above the median). In contrast, separation at other percentiles entails the comparison of children from counties that exhibit larger differences in the expansion speed. In line with the uncertain results on mental disorders and healthcare costs from the DiD and generalized DiD estimations, the results are only significant in some specifications.

Extensive/Intensive margin. My main specification investigates the reform's effect on the intensive margin (number of diagnoses) for communicable diseases and injuries as well as the extensive margin for chronic conditions. Alternatively, I investigate the extensive margin for communicable diseases and injuries alongside the intensive margin for chronic conditions. The results are presented in Appendix Tables E.26 and E.27. The direction and significance of the effects are very similar to the main results. Thus, the intensive and extensive margins are affected when considering communicable diseases.[39] This finding suggests that the number of diagnoses and the share of children affected by at least one communicable

[39] Note, the definition of the intensive margin also includes children with zero diagnoses. Thus, in theory, the effects could be driven entirely by the extensive margin (children switching from zero to at least one diagnosis). However, the relative changes (percentage change compared to the sample means) are smaller in the extensive margin than in the intensive margin, suggesting that the main driver of the change in communicable diseases are children experiencing more diagnoses rather than children switching from zero to non-zero diagnoses.

disease per year changes (positive effect at age 1–2 and negative effect at elementary school age). Interestingly, while the extensive margin of vision problems is unaffected, the intensive margin displays significant effects in both the generalized DiD and the standard DiD. Children aged 1–2 years suffer from significantly more vision problems, while children in elementary school are significantly less affected when exposed to daycare. The effects in the intensive margin might be driven by the effect on conjunctivitis as described in section 6.4, which is not a chronic condition but an infectious disease. Thus, counting the number of diagnoses might be a more suitable measure.

6.4 Detailed diagnoses

So far, I have presented results for quite broad sets of diseases. In this section, I provide results for more narrowly defined diagnoses (3- and 4-digit levels of ICD-10 codes) within these sets of diseases.

Infections. Within infections, intestinal infectious diseases are responsible for about 29% of infections of 1–2-year-old children. Herein other gastroenteritis and colitis of infectious and unspecified origin (e.g. "abdominal influenza") account for about 81%. Intestinal infections and, therein, gastroenteritis are the only subgroups among the studied subgroups for which I can depict significant increases for 1–2-year-old children and decreases for elementary school-aged children (Appendix Table F.1). Namely, a 10 percentage point increase in the coverage rate leads to an 11% and 9% increase in intestinal infections and gastroenteritis diagnoses for 1–2-year-old children. The reductions at age 9–10 years amount to 10% for intestinal infections and 12% for gastroenteritis.

Ear diseases. For the aggregated set of ear diseases, I find significant increases at age 1–2 years but no sizable changes in 3–10-year-old children. However, the more detailed analysis reveals that the reform led to a significant increase in middle ear infections (diseases of middle ear and mastoid) at age 1–2 years and decreases at older ages. The increase at age 1–2 years is most evident for the nonsuppurative and otitis media subgroup (Appendix Table F.1).

Respiratory diseases. Increases in respiratory diseases at age 1–2 years are particularly pronounced for acute upper respiratory infections (herein acute upper respiratory infections of multiple and unspecified sites), other acute lower respiratory infections (herein acute bronchitis), and other diseases of the upper respiratory tract. The decrease in respiratory diseases for elementary school-age children can be mainly attributed to other acute lower respiratory infections (herein acute bronchitis) and other diseases of the upper respiratory tract (herein allergic rhinitis), which account combined for one-third of all respiratory conditions for 9–10-year-old children (Appendix Table F.2). Within the group of chronic lower respiratory diseases, there is a significant increase in asthma at age 1–2 years and a significant decrease at age 6–8 years.

Non-communicable diseases. The analysis of the effects of the expansion on the aggregated set of mental disorders does not reveal clear effects. However, when looking into frequent subgroups within the generalized DiD framework, I find evidence that the expansion increases the prevalence of disorders of psychological development and decreases the prevalence of behavioral and emotional disorders, with onset usually occurring in childhood and adolescence at age 1–2 years. In addition, I depict a significant decrease in behavioral and emotional disorders at elementary school age, with onset usually occurring in childhood and adolescence. Similarly, despite null effects on the aggregated set of vision problems, the detailed analysis provides evidence for an increase in the prevalence of disorders of conjunctiva (herein conjunctivitis) at age 1–2 years and a decrease at elementary school age (Appendix Table F.3).[40]

6.5 Effect heterogeneity

The health effects of the daycare expansion may be heterogeneous across different groups of children. I address this question by exploring whether the effects of the expansion on health outcomes are heterogeneous by gender and by areas with different socio-economic status.

Gender. Appendix Table G.1 shows the results of estimating Equation (1) for girls and boys separately. For most outcomes, there are no gender differences in the impact of the expansion on health. The exception presents injuries: While girls have, in general, a lower prevalence of injuries than boys (e.g., 0.172 injuries per year at age 6–8 years for girls vs. 0.206 injuries per year for boys), entering daycare earlier significantly increases the risk of injuries for girls at age 1–2 years while there is no such effect for boys.[41] The effect

[40]Note, conjunctivitis is mainly caused by viruses that likely spread in daycare centers. The results on conjunctivitis are in line with the findings on communicable diseases.

[41]To test whether the effects are statistically significantly different between the groups, I add an interaction term between *cc* and a binary variable indicating whether the child lived in a high/low income county in a separate regression based on equation 1. The coefficient on this interaction term is significant for injuries at age 1–2 years.

corresponds to a 5.1% increase following a ten percentage point increase in the coverage rate.

Socio-economic background. Daycare in Germany is universal and open to everyone. However, the take-up rate of the scarce daycare slots for children below the age of three is higher among highly educated and non-migrant families (e.g., Jessen et al., 2020). Many large-scale early childhood interventions are found to benefit more disadvantaged populations (see Almond et al. (2018) for a review) and I now turn to study whether the expansion's impacts vary by socio-economic background. Due to data limitations, I do not observe socio-economic characteristics at the individual level, but I can observe average household income and the share of migrants at the county level. To construct a measure of the income level (share of migrants), I sort the counties by their average household income (share of migrants) and separate the sample at the top/bottom 30th percentile to compare counties with high and low household income (share of migrants).

The results in Appendix Tables G.2 and G.3 reveal that children from lower socio-economic background (low-income counties, counties with high shares of migrants) drive the results on communicable diseases. Specifically, the effects on infections are more pronounced for children from low-income counties while the effects on ear and respiratory diseases are larger for children from counties with high shares of migrants. Similarly, early daycare reduces obesity in elementary school children from counties with high shares of migrants and leads to an earlier detection of vision problems in young children from low-income counties.[42] There are no sizable differences between high and low-income counties and between counties with high and low shares of migrants for the remaining outcomes. The absence of differences by socio-economic status in some outcomes (e.g., mental disorders) could be explained by the fact that even in low-income areas and areas with high shares of migrants, more highly educated and non-migrant families take advantage of the supply of daycare slots. Another explanation could be differences in daycare quality (e.g., differences in group sizes) between more and less advantaged counties. Individual-level information on the socio-economic background could provide more precise estimates of the socio-economic differences of the impact of the reform.

[42]The observed differences are statistically significant for infections, ear diseases and obesity but not for respiratory diseases and vision problems.

6.6 Discussion of the results

In sum, the results on communicable diseases provide evidence of a daycare-driven intertemporal substitution of illness spells from the first years of the elementary school towards the first years of daycare. More precisely, on the one hand, children suffer more frequently from infections, ear, and respiratory diseases when entering daycare at age 1–2. On the other hand, at elementary school age, children fall sick less often with these conditions. Interestingly, in total, between ages one and ten, children who enter daycare earlier suffer from the same number of infections and respiratory diseases as children who enter daycare later. In contrast, there is a small positive effect for the overall age group for ear diseases, suggesting that children who enter daycare at age 1 or 2 suffer from more ear diseases up until age ten than children who enter daycare later. The results are intuitive as children in daycare are in close contact with other children and, therefore, exposed to many viruses and bacteria. Exposure to viruses and bacteria leads to worse health in the short run but initiates the immunization process earlier, leading to fewer infections in the longer run.

My results are in line with the hygiene hypothesis, as well as the results from van den Berg and Siflinger (2022), Cattan et al. (2021) and the medical literature (e.g., Enserink et al., 2013).[43] The results of van den Berg and Siflinger (2022) are similar in that they also find increases in infections and respiratory diseases following daycare exposure in the short run and better health in the long run. Their study finds more pronounced effects on ear diseases in the long run, while my effects mainly hold for infections and respiratory diseases. However, the three sets of conditions are closely related and different reporting practices and daycare environments in Sweden and Germany could explain the differences. Furthermore, my analysis of the more narrowly defined outcomes also reveals effects of the expansion on a subgroup of ear diseases, namely otitis media. Similarly, Cattan et al. (2021) provide evidence that exposure to Sure Start, among other things also entailing daycare, leads to an increase in hospital admissions due to infectious illnesses at age one and a decrease in later childhood and adolescence. In contrast to my results, there is evidence that a large-scale daycare reform in Quebec led to adverse effects on health both in the short and long run. The effects are mainly driven by children who had access to daycare at very young ages (Baker et al., 2008, 2019, Kottelenberg and Lehrer, 2013). Differences in daycare quality

[43]Note, Enserink et al. (2013) does not control for selection into daycare and can therefore not be interpreted causally. However, it provides evidence for an association between attending daycare and catching infections, which is stronger at younger ages.

might drive these contrasting results: While daycare in Sweden is considered high-quality (e.g., Bremberg, 2009), the expansion in Quebec was relatively cheap and is considered low-quality care (e.g., Kottelenberg and Lehrer, 2013). Findings from other studies analyzing smaller and targeted programs that entail daycare but also other components, such as home visits (e.g., Perry Preschool Program and Abecedarian Program) show positive effects on short- and long-run health outcomes (e.g. Conti et al., 2016).

To better understand the effect sizes, I compare my estimates with estimates found for other factors that influence health. Age, for example, is a critical determinant of infections, ear, and respiratory diseases (compare Table 2). Additionally, education, air pollution and second-hand smoke are known to be critical factors influencing child and adolescent health (e.g., Coneus and Spiess, 2012*a*, Hawkins et al., 2016, Huebener, 2018). Comparing effect sizes from the literature with my effects reveals that the observed increase in 1–2-year-old children and reductions in elementary school-aged children in the prevalence of communicable diseases appear sizable. One additional year of education leads to three to four times bigger effect sizes than a ten percentage point higher daycare coverage rate at age 1–2 years (e.g., Huebener, 2018). In comparison, one year in age or smoke-free legislation leads to about twice as large effect sizes for communicable diseases (e.g., Table 2, Hawkins et al., 2016). Specifically, Hawkins et al. (2016) denote reductions of 8–12 percent in emergency department visits of children associated with asthma, respiratory infections, and ear infections following smoke-free legislation.

My results provide little evidence that the daycare expansion affects mental health. In section 6.4, I evaluate whether certain common mental disorders are differently affected, which could lead to the overall null effect. Here, I provide evidence that children affected by the reform might suffer more often from development disorders at young ages but less often at elementary school age. However, I do not find significant and robust effects for other subgroups. My findings contrast van den Berg and Siflinger (2022), who point out substantial decreases in the prevalence of mental disorders for almost all age groups. These differences could arise due to the differences in the counterfactual and the timing of entering daycare: While in Sweden, the reform led to a change from informal care into daycare for all age groups, the expansion in Germany caused a switch from mainly home care into daycare only for the children below the age of three. Children in informal daycare could benefit from a switch to formal daycare where care actors are potentially more qualified.

In Germany, almost all children from age three onward were in daycare before the reform. Therefore, only the timing of entering daycare changed, not the daycare environment.

My analysis suggests that entering daycare does not affect the prevalence of obesity, vision problems, or injuries. The null effects on vision problems contrast findings from Hong et al. (2019), who suggest that attending a pre-kindergarten program in the US increases the probability of being diagnosed with vision problems. One reason for the different findings could be that the findings in Hong et al. (2019) relate to children from low-income families. At the same time, the expansion in Germany, in principle, affected all children, but particularly children from higher socio-economic backgrounds (Jessen et al., 2020). My heterogeneity analysis supports this argument, as in low-income areas, children are more likely to be diagnosed with vision problems at age 1–2 years.

According to my results, the daycare expansion has no clear effects on the prevalence of obesity. However, I observe a reduction in obesity among elementary school aged children from counties with a high share of migrants. Lauber (2015) points out that children at the margin, i.e., children whose daycare usage is affected by regional daycare provision, gain from enrollment at 30 months or earlier. Specifically, they show significantly fewer weight problems. Also, D'Onise et al. (2010) find in a meta-study that daycare/preschool attendance leads to a reduction in obesity. Lauber (2015) and D'Onise et al. (2010) study daycare attendance of pre-school aged children. In their cases, children who do not attend daycare may not attend daycare at all before entering school. In my study, the expansion affects daycare attendance of 1–2-year-old children and almost all children attend daycare from age three. These age differences might explain the differences in the results.

In line with more communicable diseases at young ages and fewer at older ages, I provide evidence that the daycare expansion led to more healthcare consumption at ages 1–2 and less healthcare consumption in the long run. This finding matches the results of van den Berg and Siflinger (2022). Similarly, Cattan et al. (2021) find more hospital admissions in the short run but fewer in later childhood and adolescence. However, the effects on healthcare consumption are relatively small in magnitude (+1.7% for 1–2 years and −2% and −1.1% for 3–5 years and 6–8-year-olds, respectively for the generalized DiD results assuming a ten percentage point increase in the daycare coverage rate).[44] One potential explanation could be that the effect of some parents taking their child more often to the

[44]For comparison, the increase in respiratory diseases at age 1–2 corresponds to 5.6%.

doctor to get a sick note while others are taking it less often to the doctor for time reasons cancel each other out. Another reason could be that these considerations might not have changed substantially due to the reform. Even though mothers' labor force participation has increased, the effects are quite small in size (Müller and Wrohlich, 2020). However, despite the change in healthcare consumption and frequency of diagnoses, I do not detect sizable effects of the expansion on healthcare costs. This result is quite surprising, but a potential explanation lies in the billing system of the German public healthcare system: Physicians get reimbursed only once for patients that show up multiple times with the same diagnoses during one quarter. Thus, more frequent doctor visits for the same diagnoses during one quarter are not captured in healthcare costs.

The results from my heterogeneity analysis, namely the absence of gender difference for most outcomes and more pronounced effects for children from disadvantaged areas, align with the findings from the literature (Almond et al., 2018, Bosque-Mercader, 2022, Hong et al., 2019, van den Berg and Siflinger, 2022). However, in terms of gender differences, some studies find daycare to be more beneficial for boys (e.g., the literature on targeted early childhood interventions and evidence on the Sure Start program in the UK, Carneiro and Ginja, 2016, Cattan et al., 2021, Conti et al., 2016, Gray-Lobe et al., 2021).

6.7 Implications of the results

My results raise the question of whether the substitution of illness spells of infections and respiratory diseases from the first years of elementary school to the first years of daycare is beneficial from a welfare perspective. In terms of healthcare costs arising in the first ten years of life, the daycare expansion appears to be neither beneficial nor costly. However, to evaluate the welfare effects of the reform in terms of health, other aspects such as duration of illness spells at different ages, sickness absence at school/daycare, severity and long-term health effects, and spill-over effects to siblings or parents need to be considered.

Severity and long-term health effects. On the one hand, some diseases, e.g., acute respiratory infections, might be particularly dangerous for very young children (e.g., Kamper-Jørgensen et al., 2006) and might lead to more hospitalizations and antibiotic prescriptions. In turn, higher antibiotic intake in children may also have adverse long-term effects on cognitive development and other health outcomes such as obesity (e.g., Baron et al., 2020, Mbakwa et al., 2016). Similarly, the medical literature mostly finds adverse long-term effects (e.g., an increase in asthma) following severe respiratory infections in children below 12 months (e.g., Carraro et al., 2014). However, as daycare in Germany starts for most children earliest when they turn one year old, this potential channel is less relevant in this study.

On the other hand, the effect sizes for infections and respiratory diseases at elementary school age are relatively stable and negative across older age groups suggesting that there are effects beyond age ten. A small strand of the literature evaluates long-term health effects of universal daycare reforms and find mixed results (e.g., Baker et al., 2019, Bosque-Mercader, 2022, Breivik, 2020, Haeck et al., 2018). For example, Bosque-Mercader (2022) finds a lower prevalence of asthma following a daycare expansion in Spain in young adults aged 11–27 years. Similarly, Haeck et al. (2018) find that the increase in asthma prevalence following the Quebec expansion is offset in the long-run. My results (section 6.4) also suggest an increase in asthma in the short-run and a decrease in the long-run. The evidence on long-run health is mixed concerning other health outcomes, such as health care consumption. Taken together, the stable negative coefficients on communicable diseases across elementary school age combined with the literature finding improved long-term health outcomes suggest that the impact of the reform is not limited to the time horizon studied in this paper but may reach adolescence and adulthood. These potential improvements in health beyond the study period highlight the benefits of early daycare attendance.

Spill-over effects to siblings and parents. In 2010 in Germany, the average age difference between the first and second child was about four years (Pötzsch, 2012). Thus, if children enter daycare before the age of three, it is more likely that they do not have younger siblings yet. As shown by Daysal et al. (2022), younger siblings have a significantly higher likelihood of being hospitalized before age one for respiratory conditions and to experience worse long-run outcomes in terms of health, education and labor market success than older siblings. They argue that one explanatory channel is older siblings "bringing home" infections from daycare. Therefore, if older siblings catch infections below three years when they do not yet have siblings, this potentially improves the health outcomes of younger siblings. Unfortunately, as I cannot link siblings in my data, this empirical question remains to be answered by future research.

To investigate potential spillover effects on parents, I conduct an additional analysis drawing on data from the German Socio-Economic Panel (SOEP).[45] I construct a sample including

[45] More information on the SOEP can be found in Goebel et al. (2019).

children between one and ten years observed between 2009 and 2019, including the socio-economic characteristics of the parents. As outcome variables, I use mothers' and fathers' general health, the number of doctor visits in the past three months, the number of days missed at work due to sickness in the previous year, and the number of days missed at work due to sickness of the child in the previous year. In a simple OLS framework, I regress the explanatory variable, a variable indicating if a child attended daycare when she was below three years, and a set of control variables[46] on the outcome variables. The results are presented in Table H.1 and can be interpreted as associations between children's early daycare attendance and parental health. However, due to endogeneity issues, the results of this additional analysis do not provide causal estimates.

Specifically, there is evidence of a negative association between parental health and children's daycare attendance when children are 1–2 years old (Table H.1). This suggests that also parents suffer from infections young children "bring home" from daycare. In line with child health improving with age, fathers of children of age 6–8 years also seem to benefit from better health when their children enter daycare early. In contrast, there is no such correlation visible for mothers. However, for mothers of young children, sickness absence at work due to illness (of their child) is positively correlated with children having been in daycare when they were below three years. Similarly, there is a negative relationship between the number of days missed at work due to the child's illness when these children are 3–5 years old. No such effects are visible for fathers, which is in line with mothers bearing most of the care work for young children in Germany. Generally, the results provide evidence that parents' illness load and sickness absence at work also increase in the short-run and decrease in the long run. As maternal hours worked increase with child age (Federal Institute for Population Research, 2020), decreasing sickness absence when children are older may increase productivity, thereby enhancing welfare.

Sickness absence at school/daycare. Shifting illness spells may also entail a shift in the timing of absenteeism at school or daycare. The reduction in infectious diseases at elementary school age combined with mothers' reduced sickness absence at work due to child health when children are at elementary school age suggests that children's sickness absence at elementary school decreases. On the one hand, substituting infections from elementary

[46] The set of control variables includes parental education, survey year, cohabitation status, birth order, parental labor force status, parental migration background, household income, parental age, child sex, the federal state of residence, age of siblings and all-day daycare/school attendance.

school to daycare age might be desirable as the sickness absence of students in schools may be reduced. Sickness absence at school is associated with worse educational and labor market outcomes (e.g., Cattan et al., 2017), emphasizing the benefit of reducing sickness absence at school. On the other hand, sickness absence at daycare centers could disrupt the relationship between children and daycare teachers as well as fellow children in daycare, thus harming children's early development. Future research could investigate the trade-off between sickness absence in school and daycare to find the "optimal" timing for infections.

Duration of illness spells. Differences in the duration of illness spells by age may provide arguments in favor of or against the change in the timing of illness spells. I do not observe illness/symptom duration in my data. Instead, Thompson et al. (2013) provide an overview of the duration of infectious illnesses (e.g., common cold symptoms, respiratory tract infection symptoms, earache, sore throat, cough) obtained from various medical studies. Age patterns vary by symptoms. For example, common cold symptoms resolve on average after 1.5 weeks in infants, 2.1 weeks in three-year-old children, and 1.3 weeks in seven-year-olds. For respiratory tract infections, time to symptom resolution is relatively similar from infants to elementary school-aged children (about 6-9 days, depending on the study). Thus, the medical literature does not provide strong evidence that illness duration varies substantially by age. Whether there is also no age pattern in this particular setting remains an empirical question for future research.

7 Conclusion

This paper provides novel insights into the causal effects of a large-scale daycare expansion for children below three on a multi-dimensional and comprehensive set of health outcomes. For identification, I exploit temporal and spatial variation in the expansion speed across German counties and employ difference-in-differences approaches.

My empirical results, based on administrative health records from Germany, provide evidence that early daycare attendance does not affect the illness load of children overall, but leads to a substitution of illness spells (respiratory, ear and infectious diseases) from elementary school age to the first years of daycare. The observed change in communicable diseases appears sizable in light of other reforms or factors that affect health. For example, one year in age or smoke-free legislation produces about twice as large effect sizes. I do not find significant and robust changes in mental health or obesity. For injury and vision problems, I detect null effects. Healthcare consumption increases at ages 1–2 years while it decreases at ages 3–5 and 6–8 years. Despite changes in the prevalence of diagnoses and the number of doctor visits, there is no clear evidence of the effects on healthcare costs. The findings are robust to a large set of robustness checks such as different definitions of the treatment status and the expansion period, the application of multiple hypothesis testing methods to obtain valid p-values, and plausibility checks of the common trend assumptions.

Heterogeneity analysis indicates no gender differences in the expansion's impact but more pronounced effects for children from disadvantaged areas. This finding aligns with consistent evidence that children from disadvantaged backgrounds can benefit disproportionately from access to daycare in a range of dimensions, including health (e.g., Almond et al., 2018). The impact of the expansion likely works through several mechanisms, including the earlier onset of an immunization process (hygiene hypothesis), formation of health habits, formation of socio-emotional and cognitive skills, and changes in the child's environment other than daycare attendance *per se* (e.g., increased maternal labor market participation).

Evidence from additional analysis and the literature reveals that the effects are not bound to children directly affected by the expansion but may spill over to siblings and parents. Namely, an additional analysis using survey data (SOEP) indicates that parents of children who enter daycare before the age of three suffer from worse health in the short run but benefit from improved health when children are older. Similarly, sickness absence from work decreases with the age of the children, which may increase productivity (maternal labor market participation increases with child age), thereby enhancing welfare. Additionally, younger siblings may benefit from older siblings entering daycare earlier as the shift of illness spells may reduce the number of infections older siblings have and "bring home" after younger siblings are born (Daysal et al., 2022). Furthermore, other factors such as long-term health effects, duration of illness spells at different ages or sickness absence at school or daycare do not provide evidence that changing the timing of infections to earlier years leads to detrimental effects that would challenge the daycare entry age of children. Assessing and contrasting the costs and (health) benefits of the reform, including a more precise analysis of spill-over effects on parents and siblings, are avenues for future research. Additionally, relying on additional data sources such as prescription and inpatient registers could shed light on the effects of early daycare attendance on other health dimensions including severe illness treated in hospitals and antibiotic intake.

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Appendices

A Additional information on the data

Procedure to balance the sample

Analogous to Barschkett et al. (2022b), first I create variables indicating the number of times an outcome, for example, respiratory conditions, was diagnosed in a specific period. Additionally, I create variables that measure the extensive margin, i.e., indicate if a patient had at least one relevant diagnosis per year. Secondly, I aggregate the data to a yearly level so that each patient appears only once per year. Finally, I balance the data by imputing information for patients without outpatient care in a specific year. By definition, all outcome variables are zero as the patient did not receive a relevant diagnosis during this year.

Detailed information about the number of children in the data

Children who did not receive any outpatient care during the 11-year observation period are not included in my sample. However, Kamtsiuris et al. (2007) states that 95% of 0–2-year-old children visit a pediatrician at least once per year. Additionally, more than 90% of children make use of individual early diagnostic tests. Thus, given that I observe individuals over 11 years, the share of children not receiving any outpatient care should be negligible. In the dataset, children are identified via a unique patient ID based on name, first name, and date of birth. Note, I only observe the ID, not the underlying information. Due to errors in recording name, first name, and birth date throughout the billing process, some patients have multiple IDs, i.e., they have one correct ID, and then other IDs that were created due to an error in the spelling of the name or date of birth. The majority of these "wrong" IDs only appear once or twice during the observation period. These errors should not be systematic, thusly not threatening my identification strategy.

Detailed information on outcome variables

Respiratory diseases. In Germany, the most frequent diagnosis code of all ICD-10 codes used by pediatricians is Acute upper respiratory infections of multiple and unspecified sites (J06). Other respiratory diseases that are also among the top 50 diagnoses used by pediatricians are Asthma (J45), Acute bronchitis (J20), Vasomotor and allergic rhinitis (J30), Acute tonsillitis (J03), Other respiratory disorders (J98), Acute nasopharyngitis (common cold, J00), Bronchitis, not specified as acute or chronic (J40), Acute pharyngitis (J02), Acute laryngitis, tracheitis (J04) and Chronic rhinitis, nasopharyngitis and pharyngitis (J31) (ZI, 2015).

Infectious diseases. Common infections occurring in childhood are Other infectious diseases (B99), Viral infection of unspecified sites (B34), and Other gastroenteritis and colitis of infectious and unspecified origin (A09) (ZI, 2015).

Ear problems. Here, the most common diagnoses occurring among children are Suppurative and unspecified otitis media (H66), Other hearing loss (H91), and Nonsuppurative otitis media (H65) (ZI, 2015).

Vision problems. I study Conjunctivitis (H10), Visual disturbances (H53), and Visual impairment, including blindness (binocular or monocular) (H54), which belong to the most frequent vision diagnoses among children in Germany (ZI, 2015).

Mental health. The most frequent mental health diagnoses among children in Germany are Specific developmental disorders of speech and language (F80), Hyperkinetic disorders (attention deficit/hyperactivity disorder (ADHD), F90), Unspecified disorders of psychological development (F89), Specific developmental disorders of motor function (F82), Other behavioral and emotional disorders with onset usually occurring in childhood and adolescence (F98), and Mixed specific developmental disorders (F83) (ZI, 2015).

B Standard difference-in-differences framework

In order to be able to test for the plausibility of the common trend assumption, I follow Havnes and Mogstad (2011) and Bauernschuster et al. (2016) and specify a standard DiD framework. I define 2008–2012 as the main expansion period. Defining 2008 as the starting year gives municipalities some time to adjust to the 2007 announcement of a legal entitlement to a daycare slot for all children aged one year and older from 2013 onward. Furthermore, 2008–2012 was the period with the greatest growth in daycare coverage. Therefore, post-reform cohorts born 2007–2011 were affected with full force, whereas the phase-in cohorts born 2005–2006 were affected to a lesser extent. The expansion did not affect cohorts born before 2005 (pre-reform cohorts). In robustness checks, I use different definitions of the main expansion period to ensure that my results are robust to changes in the exact choice of the expansion period.

To divide counties into the treatment and control groups, I order counties according to the percentage point increase in daycare coverage rates from 2008–2012. This definition allows for a clear treatment definition that does not require applying DiD estimators (e.g., Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020) for staggered treatments. I then separate the sample at the 30th/70th percentile, the upper 30% constituting the treatment counties and the bottom 30% the control group. Children from counties between the 30th and 70th percentile are excluded from the analysis. Figure B.1 depicts daycare coverage rates before, during and after the expansion in treatment and control counties. The graphs move almost in parallel until 2008, while treatment counties experience a steeper increase in daycare coverage from 2008 onward. Thus, I compare counties that distinctly differ in their expansion speed within the main expansion period. In robustness checks, I provide evidence that my results are robust to changes in the definition of the treatment group by choosing cutoffs other than the 30th/70th percentile (e.g., median).

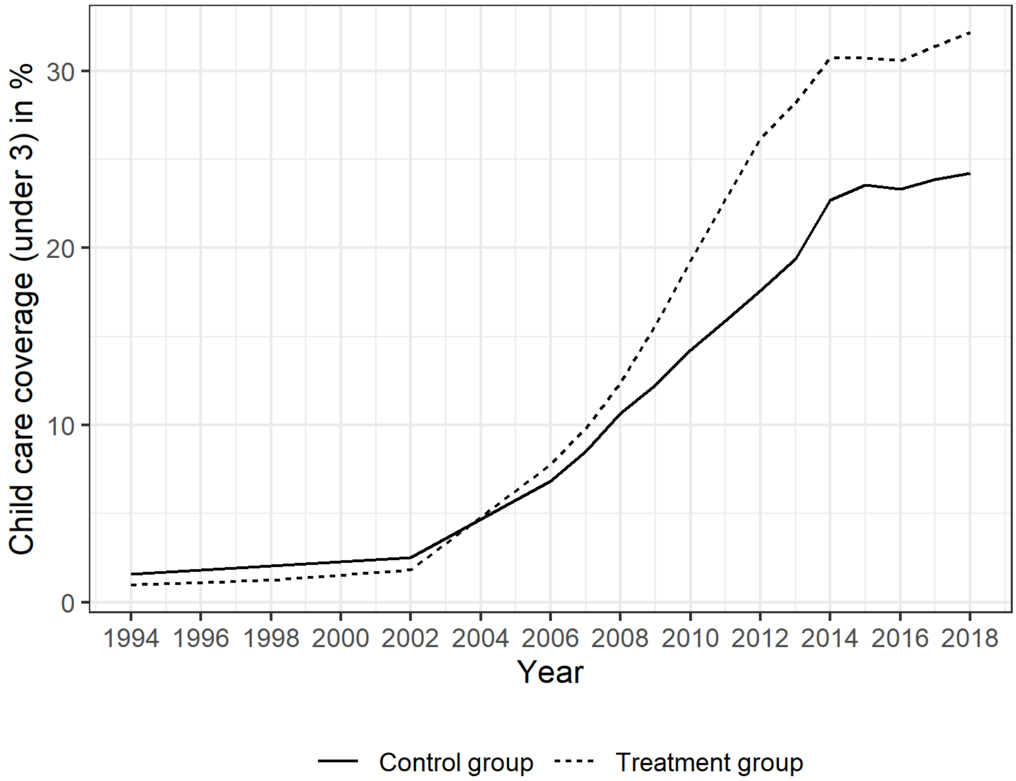
My regression model, estimated by OLS, can be defined as

$$\begin{aligned} Y_{it} = \psi_t + \gamma_1 Treat_i + \gamma_2 Post_t + \gamma_3 Phasein_t + \gamma_4 (Treat_i \times Phasein_t) \\ + \theta(Treat_i \times Post_t) + X_{it}\beta + \varepsilon_{it} \end{aligned} \tag{B.1}$$

where $Treat_i$ is a dummy variable that indicates whether child i lived in a treatment county. In an alternative specification, I use the percentage point change between 2008 and 2012 as a continuous treatment variable. This continuous treatment variable reduces the information loss in the standard DiD design and relaxes the assumption of the treatment status of counties. $Phasein_t$ is 1 if child i was born in year $t \in [2005, 2006]$[47] and $Post_t$ turns 1 if child i was born in year $t \in [2007, 2011]$. All other variables are the same as in Equation (1). Interacting $Treat_i$ and $Post_t$ marks all children affected by the expansion, i.e., children born between 2007 and 2011 and living in a treatment county. Thus, θ is the coefficient of interest and captures the expansion's intention-to-treat (ITT) effect. I interpret this as an ITT effect, as my model estimates the reduced form impact on all children from post-reform cohorts who reside in the treatment area. The benefit of estimating an ITT

[47]Note, the $Phase - in$ dummy is excluded in the analysis for 3–5-year-old children, as data is only available from 2006 on for three-year-old children.

Figure B.1: Daycare expansion during 1994 and 2018

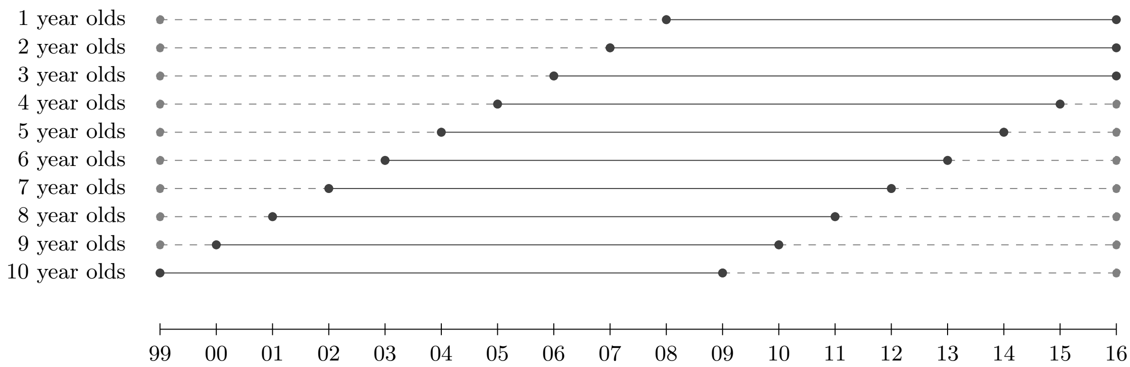


Note: The graph shows the daycare coverage rate for children below three years in West Germany comparing treatment and control counties.
Source: Destatis 1994–2018, own calculations

effect is that it captures the full reform impact. Thus, not only is the effect on treated children portrayed but also potential spill-over effects on, for example, siblings that were themselves not affected by the reform, peer effects on other children who were not attending daycare, and changes in both formal and informal care arrangements (e.g., grandparental care (Barschkett et al., 2022a)). However, the ITT averages the effect over all children in treated counties. Therefore, the effect size is difficult to interpret and needs to be weighed against the size of the expansion. To do so, I compute the treatment-on-the-treated (TT) effect by scaling the ITT with the first-stage results. In the first stage, I estimate the same model as in Equation (B.1) with the daycare coverage rate in county c for birth cohort t on the left-hand side. θ then gives the change in the daycare coverage rate for affected counties. I arrive at the TT by calculating TT = ITT/first stage. The TT represents the effect of daycare exposure (per daycare spot) on children born in post-reform cohorts who live in the treatment area.

As the KBV data are only available from 2009 onward, the standard DiD approach can only be applied to children three years and older. This is because a pre-period is missing for younger children, as only birth cohorts from 2007/08 onward are observed. Figure B.2 in the appendix shows data availability for the different age groups and birth cohorts. As there is considerable variation in the daycare expansion speed during and after the main expansion period between the different counties, the generalized DiD approach can also be applied to the youngest age group, namely the 1–2-year-olds. Thus, the instantaneous effects of the reform can be assessed. With the standard DiD approach, only the longer-term effects can be evaluated.

Figure B.2: Data availability by age group and birth cohort



Note: The graph shows data availability for different birth cohorts and age groups,.

Source: KBV 1999–2016, own calculations

C County characteristics

Table C.1: Descriptive statistics treatment vs. control counties

	Control counties (N = 97)	Treatment counties (N = 97)	P-Value
Daycare coverage rate			
mean (sd)	10.654 (5.084)	12.522 (4.871)	0.010
Unemployment rate			
mean (sd)	6.699 (3.041)	5.906 (2.362)	0.044
Share of population U3			
mean (sd)	2.498 (0.190)	2.385 (0.2256)	0.000
Average age			
mean (sd)	42.479 (1.110)	42.745 (1.2860)	0.125
Share of migrants			
mean (sd)	10.081 (4.701)	5.967 (2.635)	0.000
Fertility rate			
mean (sd)	1.397 (0.109)	1.403 (0.105)	0.732
Infant mortality			
mean (sd)	3.622 (1.967)	3.474 (2.099)	0.614
Life expectancy			
mean (sd)	80.061 (1.001)	79.980 (0.787)	0.536
Female employment rate			
mean (sd)	44.294 (3.948)	45.991 (3.397)	0.002
Household income			
mean (sd)	1,604.526 (209.639)	1,575.454 (168.331)	0.288
Population density			
mean (sd)	847.856 (836.735)	299.536 (440.367)	0.000
GDP per capita			
mean (sd)	33.429 (12.006)	26.435 (12.118)	0.000
Excess nitrogen			
mean (sd)	79.434 (24.904)	70.934 (27.067)	0.024

Notes: Means (standard deviations) and p-values testing for the difference between the groups are reported.
Source: INKAR 2008, own calculations.

D Results by age group

Table D.1: Generalized DiD Results by age group

	Age: 1	Age: 2	Age: 3	Age: 4	Age: 5	Age: 6	Age: 7	Age: 8	Age: 9	Age: 10
Communicable diseases										
Infections	0.009^{***}	0.010^{***}	0.003	-0.0003	-0.001	-0.002^{*}	-0.003^{***}	-0.003^{***}	-0.004^{***}	-0.005^{***}
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ear diseases	0.002^{*}	0.005^{***}	0.002^{*}	-0.002^{*}	-0.002^{*}	-0.001	-0.0002	0.00002	-0.001^{+}	-0.001^{**}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Respiratory diseases	0.016^{***}	0.020^{***}	0.006^{*}	-0.005^{+}	-0.007^{**}	-0.006^{***}	-0.005^{**}	-0.005^{**}	-0.007^{***}	-0.007^{***}
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Non-communicable diseases										
Mental disorders	0.001	0.001^{***}	0.001	-0.0002	-0.001^{+}	-0.001^{**}	-0.001^{*}	-0.001^{**}	-0.001^{***}	-0.001^{***}
	(0.0005)	(0.0004)	(0.0005)	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Obesity	0.0001^{+}	0.0001^{+}	0.0001^{+}	0.0001^{+}	0.0001	0.00002	0.0001	$-\ 0.0001$	-0.0003^{**}	-0.001^{***}
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Injury	-0.0003	-0.0003	-0.0004^{+}	-0.0001	-0.0003	-0.0003	-0.0002	0.0001	0.0002	0.0004^{+}
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Vision problems	0.001^{***}	0.002^{***}	0.001^{**}	-0.0001	-0.0003	-0.0002	-0.0002	-0.001^{*}	-0.0004^{+}	-0.001^{*}
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Healthcare consumption										
Treatment cases	0.005	0.012^{**}	-0.006	-0.018^{***}	-0.018^{***}	-0.014^{***}	-0.007^{**}	-0.005^{*}	-0.005^{*}	-0.006^{*}
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Healthcare costs	0.002^{*}	0.005^{***}	0.002^{*}	-0.002^{*}	-0.002^{*}	-0.001	-0.0002	0.00002	-0.001^{+}	-0.001^{**}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Control for gender	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control for swine flu	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Birth cohorts	2008-2018	2007-2017	2006-2016	2005-2015	2004-2014	2003-2013	2002-2012	2001-2011	2000-2010	1999-2009
Observations	4,287,667	4,754,773	5,278,596	5,801,293	5,760,578	5,725,600	5,708,062	5,733,983	5,806,102	5,868,892

Note: $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$. Robust standard errors clustered on county-level are in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

E Robustness

Standard DiD results

Table E.1: DiD Results

	Age: 3-10	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases				
Infections	−0.004	−0.013	−0.027**	−0.009
	(0.011)	(0.012)	(0.010)	(0.009)
q-value; boot strapped p-value	0.738; 0.734	0.581; 0.265	0.023; 0.004	0.581; 0.313
TT	−0.8%	−3.4%	−6.1%	−2.7%
Ear diseases	0.016+	0.005	−0.006	0.001
	(0.008)	(0.008)	(0.005)	(0.003)
q-value; boot strapped p-value	0.118; 0.041	0.581; 0.487	0.337; 0.183	0.524; 0.683
TT	5.7%	1.3%	−2.3%	0.6%
Respiratory diseases	−0.031	−0.025	−0.050*	−0.024
	(0.021)	(0.020)	(0.020)	(0.016)
q-value; boot strapped p-value	0.257; 0.135	0.581; 0.196	0.036; 0.005	0.684; 0.144
TT	−2.7%	−2.1%	−4.5%	2.8%
Non-communicable diseases				
Mental disorders	0.003	0.007*	0.004	0.002
	(0.004)	(0.003)	(0.003)	(0.003)
q-value; boot strapped p-value	0.478; 0.406	0.128; 0.013	0.370; 0.258	0.581; 0.495
TT	1.7%	4.6%	2.3%	1.5%
Obesity	0.003***	0.001	0.002**	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
q-value; boot strapped p-value	0.006; 0.000	0.581; 0.350	0.014; 0.001	0.208; 0.025
TT	12.9%	10.3%	11.0%	7.6%
Injury	−0.002	−0.001	−0.001	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)
q-value; boot strapped p-value	0.277; 0.192	0.635; 0.627	0.699; 0.603	0.581; 0.426
TT	−1.5%	−1.3%	−0.9%	−1.5%
Vision problems	−0.006*	−0.002	−0.001	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
q-value; boot strapped p-value	0.118; 0.034	0.581; 0.483	0.802; 0.799	0.581; 0.434
TT	−3.0%	−1.3%	−0.5%	1.1%
Healthcare consumption				
Treatment cases	0.001	−0.062**	−0.062*	0.015
	(0.031)	(0.023)	(0.029)	(0.029)
boot strapped p-value	0.946	0.004	0.028	0.593
TT	0.03%	−2.6%	−1.7%	0.6%
Healthcare costs	5.840+	−0.319	3.016	9.016**
	(3.425)	(1.302)	(2.126)	(2.965)
boot strapped p-value	0.080	0.785	0.147	0.002
TT	3.9%	−0.3%	2.1%	6.8%
Control for age + gender	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes
Birth cohorts	2000-2011	2006-2011	2003-2011	2000-2009
First stage	59.7%	38.8%	57.2%	57.2%
Observations	21,215,410	5,235,062	7,903,346	5,990,518

Note: +p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation B.1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. q-values are p-values adjusted for multiple hypothesis testing following Benjamini and Hochberg (1995). Boot strapped p-values are calculated based on wild-bootstrapped clustered standard errors accounting for a finite number of clusters. The ITT is calculated by scaling the coefficient with the pre-treatment mean. The TT is calculated by diving ITT/first stage, where the first stage estimates amount to 59.7% for the age group 3–10, 38.8% for 3–5, 57.2% for 6–8 and 52.7% for 9–10. The coefficients show the effect of living in a fast-expanding county and being born after the reform on the respective disease. *Source:* KBV 2009–2019, own calculations.

Event-Study graphs

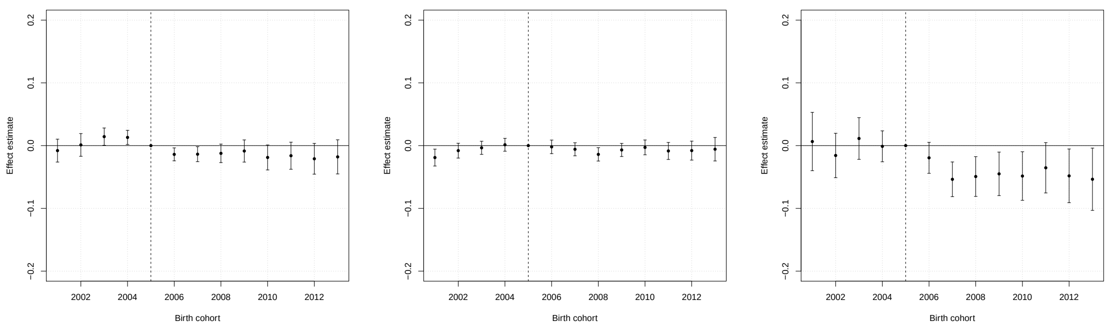
The event-study graphs are estimated based on the following specification

$$Y_{it} = \psi_t + \theta(Treat_i \times Cohort_i) + X_{it}\beta + \varepsilon_{it} \qquad \text{(E.1)}$$

where $Cohort_i$ represents the birth year of child i, where 2005 serves as the reference cohort, all other variables are the same as in Equation (B.1).

Figure E.1: Event study: Communicable diseases

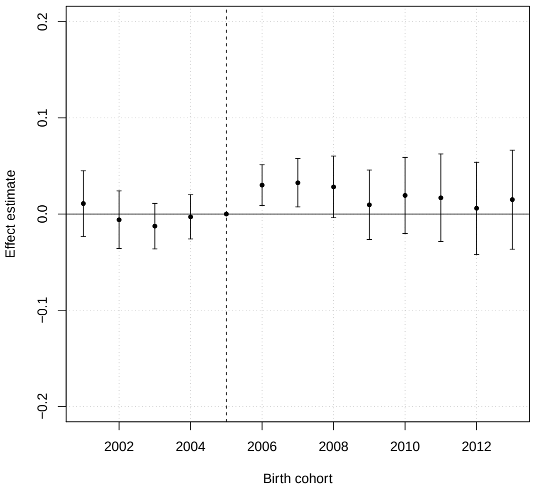
(a) Infections (b) Ear diseases (c) Respiratory diseases



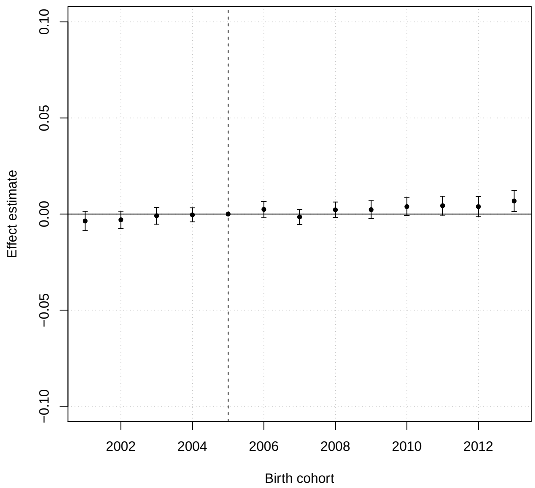
Note: The graphs show event-study estimates for 6-8 year old children for infections (panel (a)), ear diseases (panel (b)) and respiratory diseases (panel (c)). The estimates are based on Equation (E.1). *Source:* KBV 2009–2019, own calculations

Figure E.2: Event study: Other diseases

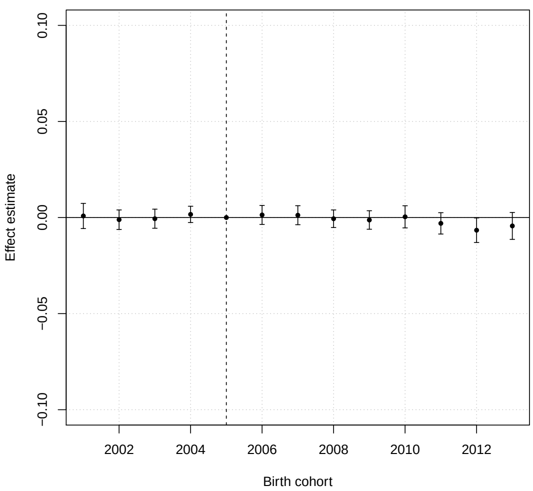
(a) Mental disorders



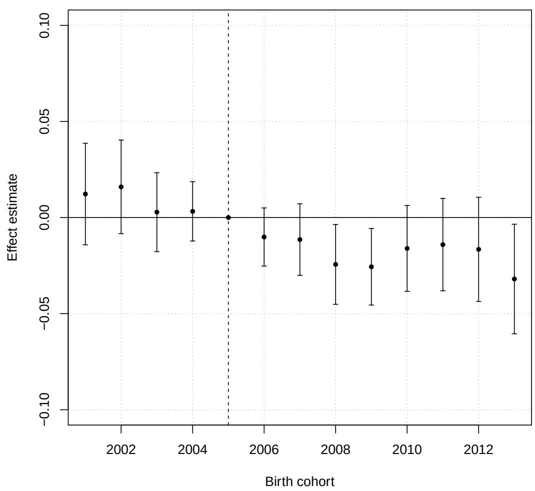
(b) Obesity



(c) Injury



(d) Vision problems

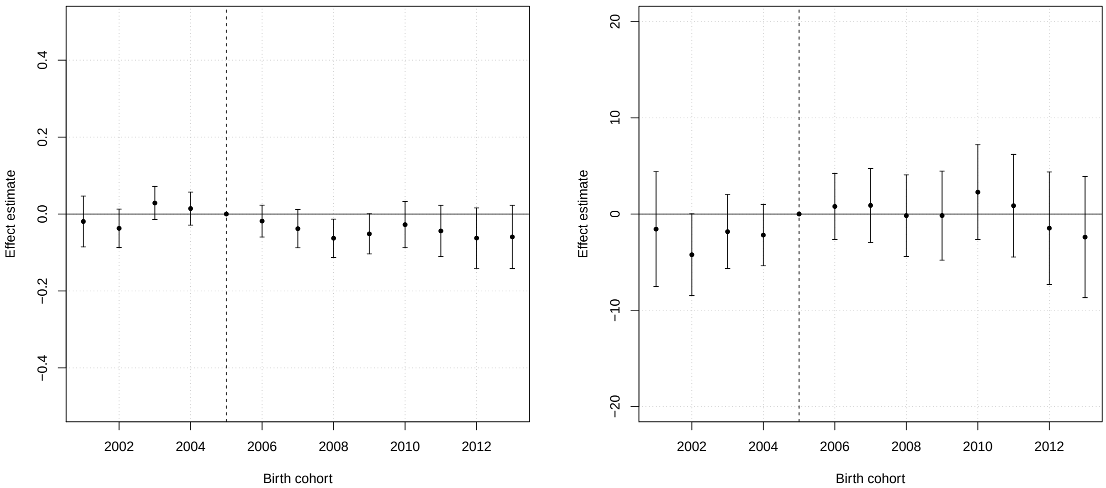


Note: The graphs show event-study estimates for 6-8 year old children for mental disorders (panel (a)), obesity (panel (b)), injury (panel (c)) and vision problems (panel (d)). The estimates are based on Equation (E.1). *Source:* KBV 2009–2019, own calculations

Figure E.3: Event study: Healthcare consumption and costs

(a) Treatment cases

(b) Healthcare costs



Note: The graphs show event-study estimates for 6-8 year old children for treatment cases (panel (a)) and healthcare costs (panel (b)). The estimates are based on Equation (E.1). *Source:* KBV 2009–2019, own calculations

Quadratic term

Table E.2: Generalized DiD Results: Squared term childcare coverage rate

	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases				
Infections	0.011***	0.009***	0.001	−0.002
	(0.003)	(0.003)	(0.002)	(0.001)
Pre-Treatment Mean	1.394	1	0.777	0.665
Ear diseases	0.001	0.001	0.0005	0.0001
	(0.002)	(0.002)	(0.001)	(0.0005)
Pre-Treatment Mean	0.583	0.84	0.454	0.284
Respiratory diseases	0.017***	0.001	−0.003	−0.006**
	(0.004)	(0.005)	(0.002)	(0.002)
Pre-Treatment Mean	2.854	2.653	1.852	1.583
Non-communicable diseases				
Mental diseases	−0.0001	−0.0002	−0.001*	−0.002***
	(0.001)	(0.001)	(0.0005)	(0.0004)
Pre-Treatment Mean	0.177	0.37	0.329	0.275
Obesity	−0.00002	0.0002	0.0002	−0.0004*
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Pre-Treatment Mean	0.014	0.024	0.033	0.051
Injury	0.002***	−0.001*	−0.0003	0.0001
	(0.0004)	(0.0004)	(0.0003)	(0.0004)
Pre-Treatment Mean	0.216	0.19	0.189	0.239
Vision problems	0.0003	0.001+	−0.001+	−0.001+
	(0.001)	(0.001)	(0.0004)	(0.0004)
Pre-Treatment Mean	0.34	0.381	0.341	0.322
Healthcare consumption				
Treatment cases	0.005	−0.016**	0.001	0.005
	(0.007)	(0.006)	(0.007)	(0.006)
Pre-Treatment Mean	6.331	6.135	5.279	4.911
Healthcare costs	1.780***	0.154	0.353	−0.052
	(0.307)	(0.333)	(0.260)	(0.437)
Pre-Treatment Mean	319.964	287.281	244.466	249.115
Control for age + gender	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009
Observations	8,522,334	14,117,183	13,979,566	10,605,784

Note: +p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1. The term cc^2 is added. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Exclusion of phase-in dummy

Table E.3: DiD Results: Without Phase-in dummy

	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases			
Infections	-0.013	-0.017^{*}	-0.007
	(0.012)	(0.008)	(0.007)
Pre-Treatment Mean	1.003	0.78	0.665
Ear diseases	0.005	-0.006	0.002
	(0.008)	(0.004)	(0.003)
Pre-Treatment Mean	0.929	0.461	0.289
Respiratory diseases	-0.025	-0.043^{**}	-0.016
	(0.020)	(0.015)	(0.014)
Pre-Treatment Mean	2.951	1.951	1.634
Non-communicable diseases			
Mental disorders	0.007^{*}	0.001	0.001
	(0.003)	(0.003)	(0.003)
Pre-Treatment Mean	0.881	0.959	0.975
Obesity	0.001	0.002^{**}	0.002^{*}
	(0.001)	(0.001)	(0.001)
Pre-Treatment Mean	0.025	0.032	0.05
Injury	-0.001	-0.001	-0.002
	(0.002)	(0.001)	(0.002)
Pre-Treatment Mean	0.199	0.2	0.248
Vision problems	-0.002	-0.0002	0.002
	(0.003)	(0.002)	(0.002)
Pre-Treatment Mean	0.374	0.334	0.32
Healthcare consumption			
Treatment cases	-0.062^{**}	-0.048^{*}	0.011
	(0.023)	(0.023)	(0.025)
Pre-Treatment Mean	6.439	5.35	4.936
Healthcare costs	-0.319	1.664	6.231^{*}
	(1.302)	(1.730)	(2.569)
Pre-Treatment Mean	304.959	247.604	247.59
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,235,062	7,903,346	5,990,518

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation B.1 excluding the phase-in dummy. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on the respective disease. *Source:* KBV 2009–2019, own calculations.

Placebo Regression: Diabetes

Table E.4: Placebo Regression (generalized DiD): Diabetes

	Age: 1-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Infections	−0.00002	0.00003	−0.00001	−0.00005	−0.0001
	(0.00002)	(0.00003)	(0.00003)	(0.00004)	(0.00004)
Pre-Treatment Mean	0.002	0.001	0.001	0.002	0.003
Control for age + gender	yes	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes
Birth cohorts	2000-2014	2008-2014	2006-2014	2003-2011	2000-2009
Observations	54,152,607	8,522,318	14,117,165	13,979,538	10,605,769

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. Diabetes is coded as a dummy variable (indicating if a child had at least once per year a Diabetes diagnosis). The estimates are based on the specification in Equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on diabetes. *Source:* KBV 2009–2019, own calculations.

Table E.5: Placebo Regression (DiD): Diabetes

	Age: 3-10	Age: 3-5	Age: 6-8	Age: 9-10
Infections	−0.00001	0.0002	0.0001	−0.0002
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Pre-Treatment Mean	0.003	0.002	0.002	0.003
Control for age + gender	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes
Birth cohorts	2000-2011	2006-2011	2003-2011	2000-2009
Observations	21,215,384	5,235,058	7,903,335	5,990,512

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. Diabetes is coded as a dummy variable (indicating if a child had at least once per year a Diabetes diagnosis). The estimates are based on the specification in Equation B.1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on diabetes. *Source:* KBV 2009–2019, own calculations.

Exclusion of cities with more than 500,000 inhabitants

Table E.6: Generalized DiD Results: Exclusion of big cities

	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases				
Infections	0.009^{***}	0.002	-0.003^{***}	-0.004^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
Pre-Treatment Mean	1.385	0.999	0.772	0.658
Ear diseases	0.003^{**}	-0.001	-0.0001	-0.001^{+}
	(0.001)	(0.001)	(0.0004)	(0.0003)
Pre-Treatment Mean	0.584	0.849	0.455	0.284
Respiratory diseases	0.014^{***}	-0.001	-0.004^{*}	-0.005^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
Pre-Treatment Mean	2.875	2.68	1.854	1.581
Non-communicable diseases				
Mental diseases	0.001^{***}	0.0001	-0.001^{*}	-0.001^{***}
	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Pre-Treatment Mean	0.177	0.372	0.327	0.273
Obesity	0.0001	0.0001^{+}	0.0001	-0.0004^{***}
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pre-Treatment Mean	0.014	0.023	0.032	0.05
Injury	0.0005^{*}	-0.0003	0.00000	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Pre-Treatment Mean	0.219	0.193	0.192	0.244
Vision problems	0.001^{***}	0.0003	-0.0005^{+}	-0.001^{**}
	(0.0003)	(0.0004)	(0.0003)	(0.0002)
Pre-Treatment Mean	0.338	0.386	0.345	0.325
Healthcare consumption				
Treatment cases	0.007^{*}	-0.014^{***}	-0.005^{+}	-0.007^{*}
	(0.003)	(0.004)	(0.003)	(0.003)
Pre-Treatment Mean	6.328	6.16	5.28	4.901
Healthcare costs	1.391^{***}	-0.065	-0.549^{**}	-1.004^{***}
	(0.175)	(0.204)	(0.183)	(0.252)
Pre-Treatment Mean	319.179	287.256	241.689	244.404
Control for age + gender	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009
Observations	7,153,668	11,998,338	12,020,533	9,197,199

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1. Children residing in cities with more than 500,000 inhabitants are excluded. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table E.7: DiD Results: Exclusion of big cities

	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases			
Infections	−0.011	−0.033***	−0.017*
	(0.011)	(0.010)	(0.008)
Pre-Treatment Mean	0.996	0.773	0.658
Ear diseases	0.005	−0.007	−0.0002
	(0.009)	(0.005)	(0.004)
Pre-Treatment Mean	0.938	0.461	0.288
Respiratory diseases	−0.034	−0.055*	−0.029
	(0.021)	(0.022)	(0.018)
Pre-Treatment Mean	2.98	1.953	1.631
Non-communicable diseases			
Mental disorders	0.008*	0.003	0.001
	(0.003)	(0.004)	(0.003)
Pre-Treatment Mean	0.383	0.303	0.261
Obesity	0.0004	0.002**	0.002+
	(0.001)	(0.001)	(0.001)
Pre-Treatment Mean	0.025	0.031	0.048
Injury	−0.0004	−0.001	−0.002
	(0.002)	(0.002)	(0.003)
Pre-Treatment Mean	0.202	0.204	0.252
Vision problems	−0.002	−0.001	0.002
	(0.003)	(0.003)	(0.003)
Pre-Treatment Mean	0.379	0.338	0.323
Healthcare consumption			
Treatment cases	−0.070**	−0.070*	0.00002
	(0.026)	(0.033)	(0.033)
Pre-Treatment Mean	6.466	5.35	4.925
Healthcare costs	0.083	3.431	8.307**
	(1.410)	(2.295)	(3.096)
Pre-Treatment Mean	305.295	244.719	242.862
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	4,208,643	6,443,579	4,942,566

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation B.1. Children residing in cities with more than 500,000 inhabitants are excluded. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on the respective disease. *Source:* KBV 2009–2019, own calculations.

Different Expansion period definitions

Table E.8: DiD Results Infections: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	−0.012	-0.025^{**}	−0.008
	(0.011)	(0.009)	(0.008)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,474	6,914,050	5,882,942
Exp. period: 2009–2012	−0.013	-0.020^{*}	−0.006
	(0.012)	(0.010)	(0.009)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,917	7,811,109	5,913,604
Exp. period: 2009–2013	−0.011	-0.017^{+}	−0.005
	(0.012)	(0.010)	(0.009)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	6,154,171	7,924,942	5,996,757
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on infections. *Source:* KBV 2009–2019, own calculations.

Table E.9: DiD Results Ear diseases: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	0.014^{+}	0.003	0.004
	(0.007)	(0.005)	(0.003)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,469	6,914,046	5,882,937
Exp. period: 2009–2012	−0.007	−0.002	0.002
	(0.007)	(0.005)	(0.004)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,916	7,811,105	5,913,600
Exp. period: 2009–2013	−0.001	0.001	0.005
	(0.008)	(0.004)	(0.004)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	6,154,168	7,924,939	5,996,749
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on ear disease. *Source:* KBV 2009–2019, own calculations.

Table E.10: DiD Results Respiratory diseases: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	-0.028	-0.051^{**}	-0.023
	(0.019)	(0.019)	(0.017)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,465	6,914,045	5,882,937
Exp. period: 2009–2012	-0.029	-0.039^{+}	-0.008
	(0.019)	(0.020)	(0.016)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,909	7,811,105	5,913,599
Exp. period: 2009–2013	-0.022	-0.037^{+}	-0.0004
	(0.020)	(0.019)	(0.015)
Observations	6,154,155	7,924,936	5,996,750
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on respiratory disease. *Source:* KBV 2009–2019, own calculations.

Table E.11: DiD Results Mental disorders: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	0.005^{+}	0.003	0.002
	(0.003)	(0.003)	(0.003)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,474	6,914,045	5,882,936
Exp. period: 2009–2012	0.004	0.003	0.002
	(0.003)	(0.003)	(0.003)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,915	7,811,106	5,913,600
Exp. period: 2009–2013	0.004	0.001	0.002
	(0.003)	(0.003)	(0.003)
Observations	6,154,172	7,924,942	5,996,744
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on mental disorders. *Source:* KBV 2009–2019, own calculations.

Table E.12: DiD Results Obesity: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	0.001	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	7,715,602	12,448,997	10,605,640
Exp. period: 2009–2012	0.0004	0.003***	0.002*
	(0.001)	(0.001)	(0.001)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	9,241,260	13,979,440	10,605,640
Exp. period: 2009–2013	0.001	0.003***	0.003*
	(0.001)	(0.001)	(0.001)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	10,817,715	13,979,440	10,605,640
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on obesity. *Source:* KBV 2009–2019, own calculations.

Table E.13: DiD Results Injury: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	−0.002	−0.002	−0.004
	(0.002)	(0.002)	(0.002)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,474	6,914,045	5,882,936
Exp. period: 2009–2012	−0.002	−0.001	−0.006*
	(0.002)	(0.002)	(0.002)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,915	7,811,106	5,913,600
Exp. period: 2009–2013	−0.001	−0.001	−0.004+
	(0.002)	(0.002)	(0.002)
Observations	6,154,172	7,924,942	5,996,744
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on injuries. *Source:* KBV 2009–2019, own calculations.

Table E.14: DiD Results Vision problems: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	-0.005^{+}	-0.002	0.002
	(0.003)	(0.003)	(0.002)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	7,715,602	12,448,997	10,605,640
Exp. period: 2009–2012	-0.003	-0.001	0.0004
	(0.003)	(0.003)	(0.003)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	9,241,260	13,979,440	10,605,640
Exp. period: 2009–2013	-0.003	-0.002	0.001
	(0.003)	(0.003)	(0.003)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	10,817,715	13,979,440	10,605,640
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on vision problems. *Source:* KBV 2009–2019, own calculations.

Table E.15: DiD Results Treatment cases: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	-0.062^{**}	-0.074^{*}	-0.006
	(0.022)	(0.029)	(0.029)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,473	6,914,043	5,882,939
Exp. period: 2009–2012	-0.078^{***}	-0.085^{**}	-0.018
	(0.023)	(0.027)	(0.029)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,916	7,811,107	5,913,601
Exp. period: 2009–2013	-0.086^{***}	-0.087^{**}	-0.008
	(0.024)	(0.027)	(0.028)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	6,154,169	7,924,935	5,996,752
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on treatment cases. *Source:* KBV 2009–2019, own calculations.

Table E.16: DiD Results healthcare costs: Different expansion period definitions

	Age: 3-5	Age: 6-8	Age: 9-10
Exp. period: 2008–2011	−1.267	0.314	6.901*
	(1.340)	(2.189)	(3.055)
Birth cohorts	2006-2010	2003-2010	2000-2009
Observations	4,296,470	6,914,049	5,882,939
Exp. period: 2009–2012	−1.009	−0.337	3.290
	(1.475)	(2.009)	(2.738)
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,181,913	7,811,105	5,913,602
Exp. period: 2009–2013	−1.597	0.066	4.715
	(1.444)	(2.198)	(3.051)
Birth cohorts	2006-2012	2003-2011	2000-2009
Observations	6,154,169	7,924,938	5,996,753
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the expansion period. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on healthcare costs. *Source:* KBV 2009–2019, own calculations.

Different Treatment definitions

Table E.17: DiD Results Infections: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	-0.002	-0.011	-0.002
	(0.009)	(0.009)	(0.007)
Observations	9,241,248	13,979,422	10,605,626
upper 40 vs. lower 40%	-0.008	-0.021^{*}	-0.008
	(0.010)	(0.009)	(0.008)
Observations	7,162,809	10,828,710	8,212,083
upper 35 vs. lower 35%	-0.010	-0.025^{**}	-0.009
	(0.011)	(0.009)	(0.008)
Observations	6,107,240	9,224,953	6,992,512
upper 25 vs. lower 25%	-0.019	-0.030^{**}	-0.011
	(0.014)	(0.011)	(0.010)
Observations	4,085,745	6,147,066	4,646,346
upper 20 vs. lower 20%	-0.018	-0.032^{**}	-0.014
	(0.014)	(0.011)	(0.011)
Observations	3,406,869	5,105,933	3,849,284
percentage change	-0.001	-0.003^{*}	-0.001
	(0.002)	(0.001)	(0.001)
Observations	9,241,248	13,979,422	10,605,626
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on infections. *Source:* KBV 2009–2019, own calculations.

Table E.18: DiD Results ear diseases: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	0.006	−0.001	0.001
	(0.006)	(0.004)	(0.003)
Observations	9,241,241	13,979,415	10,605,625
upper 40 vs. lower 40%	0.007	−0.004	−0.002
	(0.007)	(0.005)	(0.003)
Observations	7,162,812	10,828,713	8,212,080
upper 35 vs. lower 35%	0.005	−0.006	−0.001
	(0.007)	(0.005)	(0.003)
Observations	6,107,238	9,224,951	6,992,508
upper 25 vs. lower 25%	0.006	−0.003	0.004
	(0.009)	(0.005)	(0.004)
Observations	4,085,741	6,147,065	4,646,343
upper 20 vs. lower 20%	0.002	−0.005	0.004
	(0.009)	(0.006)	(0.004)
Observations	3,406,865	5,105,931	3,849,280
percentage change	0.001	−0.0003	0.0004
	(0.001)	(0.001)	(0.0004)
Observations	9,241,254	13,979,421	10,605,627
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on ear diseases. *Source:* KBV 2009–2019, own calculations.

Table E.19: DiD Results respiratory diseases: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	−0.020	−0.034*	−0.018
	(0.014)	(0.015)	(0.013)
Observations	9,241,241	13,979,415	10,605,625
upper 40 vs. lower 40%	−0.015	−0.039*	−0.022
	(0.016)	(0.017)	(0.014)
Observations	7,162,802	10,828,708	8,212,081
upper 35 vs. lower 35%	−0.017	−0.046*	−0.024
	(0.018)	(0.019)	(0.015)
Observations	6,107,230	9,224,947	6,992,509
upper 25 vs. lower 25%	−0.033	−0.050*	−0.024
	(0.023)	(0.024)	(0.019)
Observations	4,085,736	6,147,064	4,646,344
upper 20 vs. lower 20%	−0.031	−0.054*	−0.029
	(0.025)	(0.026)	(0.020)
Observations	3,406,862	5,105,930	3,849,280
percentage change	−0.003	−0.006*	−0.003
	(0.003)	(0.003)	(0.002)
Observations	9,241,241	13,979,415	10,605,625
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on respiratory diseases. *Source:* KBV 2009–2019, own calculations.

Table E.20: DiD Results mental disorders: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	0.04^{+}	0.002	0.001
	(0.002)	(0.002)	(0.002)
Observations	9,241,246	13,979,423	10,605,626
upper 40 vs. lower 40%	0.004^{+}	0.001	−0.0001
	(0.002)	(0.003)	(0.003)
Observations	7,162,807	10,828,712	8,212,079
upper 35 vs. lower 35%	0.006^{**}	0.001	0.0003
	(0.003)	(0.003)	(0.003)
Observations	6,107,233	9,224,949	6,992,509
upper 25 vs. lower 25%	0.007^{+}	0.004	0.003
	(0.004)	(0.004)	(0.004)
Observations	4,085,740	6,147,065	4,646,343
upper 20 vs. lower 20%	0.007^{+}	0.004	0.003
	(0.004)	(0.004)	(0.004)
Observations	3,406,864	5,105,931	3,849,280
percentage change	0.002^{+}	0.002	0.003
	(0.001)	(0.002)	(0.002)
Observations	9,241,246	13,979,423	10,605,626
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on mental disorders. *Source:* KBV 2009–2019, own calculations.

Table E.21: DiD Results obesity: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	0.0002	0.001*	0.001
	(0.0005)	(0.001)	(0.002)
Observations	9,241,244	13,979,425	10,605,619
upper 40 vs. lower 40%	0.0004	0.002*	0.002+
	(0.001)	(0.001)	(0.001)
Observations	7,162,804	10,828,715	8,212,074
upper 35 vs. lower 35%	0.0005	0.002**	0.002+
	(0.001)	(0.001)	(0.001)
Observations	6,107,232	9,224,952	6,992,502
upper 25 vs. lower 25%	0.001	0.003***	0.004**
	(0.001)	(0.001)	(0.001)
Observations	4,085,738	6,147,066	4,646,343
upper 20 vs. lower 20%	0.001	0.003***	0.004**
	(0.001)	(0.001)	(0.001)
Observations	3,406,863	5,105,937	3,849,280
percentage change	0.0002	0.0005*	0.0003
	(0.0002)	(0.0002)	(0.0003)
Observations	9,241,244	13,979,425	10,605,619
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on obesity. *Source:* KBV 2009–2019, own calculations.

Table E.22: DiD Results injury: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	−0.0001	−0.001	−0.002
	(0.001)	(0.001)	(0.002)
Observations	9,241,251	13,979,422	10,605,622
upper 40 vs. lower 40%	0.0004	−0.001	−0.002
	(0.002)	(0.002)	(0.002)
Observations	7,162,811	10,828,713	8,212,076
upper 35 vs. lower 35%	−0.0002	−0.002	−0.003
	(0.002)	(0.002)	(0.002)
Observations	6,107,237	9,224,951	6,992,505
upper 25 vs. lower 25%	−0.001	−0.002	−0.004
	(0.002)	(0.002)	(0.003)
Observations	4,085,744	6,147,064	4,646,344
upper 20 vs. lower 20%	−0.0004	−0.002	−0.006*
	(0.002)	(0.002)	(0.003)
Observations	3,406,868	5,105,933	3,849,283
percentage change	−0.0001	−0.0001	−0.0004
	(0.0002)	(0.0002)	(0.0003)
Observations	9,241,251	13,979,422	10,605,622
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on injuries. *Source:* KBV 2009–2019, own calculations.

Table E.23: DiD Results vision problems: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	−0.0003	0.001	0.002
	(0.002)	(0.002)	(0.002)
Observations	9,241,251	13,979,427	10,605,630
upper 40 vs. lower 40%	−0.002	−0.002	0.0001
	(0.002)	(0.002)	(0.002)
Observations	7,162,810	10,828,721	8,212,084
upper 35 vs. lower 35%	−0.002	−0.001	0.001
	(0.003)	(0.003)	(0.002)
Observations	6,107,236	9,224,958	6,992,513
upper 25 vs. lower 25%	−0.001	0.0001	0.004
	(0.003)	(0.003)	(0.003)
Observations	4,085,740	6,147,071	4,646,348
upper 20 vs. lower 20%	−0.003	−0.001	0.003
	(0.004)	(0.004)	(0.003)
Observations	3,406,863	5,105,937	3,849,285
percentage change	−0.001	-0.002^{*}	−0.002
	(0.001)	(0.001)	(0.001)
Observations	9,241,251	13,979,427	10,605,630
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a dummy variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on vision problems. *Source:* KBV 2009–2019, own calculations.

Table E.24: DiD Results treatment cases: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	-0.038^{*}	-0.048^{*}	0.001
	(0.019)	(0.022)	(0.022)
Observations	9,241,251	13,979,423	10,605,628
upper 40 vs. lower 40%	-0.040^{+}	-0.062^{*}	-0.00003
	(0.021)	(0.025)	(0.025)
Observations	7,162,812	10,828,713	8,212,081
upper 35 vs. lower 35%	-0.066^{**}	-0.082^{**}	-0.007
	(0.023)	(0.027)	(0.027)
Observations	6,107,240	9,224,950	6,992,509
upper 25 vs. lower 25%	-0.068^{*}	-0.059^{+}	0.010
	(0.027)	(0.034)	(0.033)
Observations	4,085,743	6,147,062	4,646,343
upper 20 vs. lower 20%	-0.070^{*}	-0.078^{*}	-0.023
	(0.029)	(0.037)	(0.035)
Observations	3,406,866	5,105,931	3,849,280
percentage change	-0.008^{*}	-0.010^{**}	-0.001
	(0.003)	(0.004)	(0.004)
Observations	9,241,251	13,979,423	10,605,628
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on treatment cases. *Source:* KBV 2009–2019, own calculations.

Table E.25: DiD Results healthcare costs: Different treatment definitions

	Age: 3-5	Age: 6-8	Age: 9-10
upper 50 vs. lower 50%	0.586	0.764	3.878^{+}
	(1.040)	(1.595)	(2.229)
Observations	9,241,249	13,979,424	10,605,629
upper 40 vs. lower 40%	0.351	0.819	4.761^{+}
	(1.151)	(1.820)	(2.569)
Observations	7,162,807	10,828,712	8,212,082
upper 35 vs. lower 35%	−0.080	1.075	5.257^{+}
	(1.249)	(1.956)	(2.829)
Observations	6,107,235	9,224,949	6,992,509
upper 25 vs. lower 25%	−0.604	3.479	10.217^{**}
	(1.404)	(2.430)	(3.239)
Observations	4,085,742	6,147,066	4,646,343
upper 20 vs. lower 20%	−0.361	2.433	7.578^{*}
	(1.505)	(2.650)	(3.376)
Observations	3,406,865	5,105,932	3,849,280
percentage change	0.005	0.165	0.945^{*}
	(0.157)	(0.257)	(0.367)
Observations	9,241,249	13,979,424	10,605,629
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The outcome variable is a count variable. The estimates are based on the specification in Equation B.1 with varying definitions of the *Treat*-Variable. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on healthcare costs. *Source:* KBV 2009–2019, own calculations.

Extensive margin

Table E.26: Generalized DiD Results: Extensive/intensive margin

	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases				
Infections	0.002***	0.001	−0.001*	−0.002***
	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Sample Mean	0.63	0.534	0.456	0.404
Ear diseases	0.001***	−0.0001	−0.00000	−0.0003*
	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Sample Mean	0.327	0.394	0.239	0.164
Respiratory diseases	0.002***	−0.0003	−0.001*	−0.001***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Sample Mean	0.81	0.772	0.648	0.585
Non-communicable diseases				
Mental disorders	0.001	−0.0005	−0.003	−0.006**
	(0.001)	(0.002)	(0.002)	(0.002)
Sample Mean	0.312	0.867	1.057	1.031
Obesity	0.0002*	0.0003*	−0.0001	−0.001***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Sample Mean	0.022	0.043	0.063	0.098
Injury	0.0003*	−0.0001	−0.00005	0.0002+
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Sample Mean	0.165	0.14	0.132	0.161
Vision problems	0.002**	−0.0004	−0.003**	−0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Sample Mean	0.592	0.816	0.791	0.709
Control for age + gender	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009
Observations	8,522,309	14,117,159	13,979,527	10,605,758

Note: +p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): infections, ear diseases, respiratory diseases, injuries. The following variables are count variables (annual number of diagnoses): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table E.27: DiD Results: Extensive/intensive margin

	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases			
Infections	-0.003	-0.009^{*}	-0.003
	(0.004)	(0.004)	(0.004)
Pre-Treatment Mean	0.536	0.457	0.404
Ear diseases	-0.0004	-0.003	-0.0004
	(0.002)	(0.002)	(0.001)
Pre-Treatment Mean	0.424	0.244	0.168
Respiratory diseases	-0.004^{+}	-0.009^{**}	-0.003
	(0.002)	(0.003)	(0.003)
Pre-Treatment Mean	0.798	0.663	0.594
Non-communicable diseases			
Mental disorders	0.025^{*}	0.029^{+}	0.025
	(0.010)	(0.017)	(0.017)
Pre-Treatment Mean	0.881	0.959	0.975
Obesity	0.001	0.003	0.001
	(0.001)	(0.002)	(0.003)
Pre-Treatment Mean	0.043	0.061	0.095
Injury	-0.001	-0.0001	-0.001
	(0.001)	(0.001)	(0.001)
Pre-Treatment Mean	0.146	0.14	0.166
Vision problems	-0.009	-0.021^{*}	-0.015^{+}
	(0.009)	(0.010)	(0.009)
Pre-Treatment Mean	0.821	0.78	0.704
Control for age + gender	yes	yes	yes
Control for swine flu incidence	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes
Birth cohorts	2006-2011	2003-2011	2000-2009
Observations	5,235,062	7,903,346	5,990,518

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): infections, ear diseases, respiratory diseases, injuries. The following variables are count variables (annual number of diagnoses): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation B.1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a living in a fast-expanding county and being born after the reform on the respective disease.
Source: KBV 2009–2019, own calculations.

F Detailed diagnoses

Table F.1: Generalized DiD Results: Detailed Diagnoses (infections and ear diseases)

	Age: 1-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Infections					
Intestinal infectious diseases (A00-A09)	−0.0002	0.003***	0.001+	−0.001*	−0.001***
	(0.0002)	(0.001)	(0.0004)	(0.0002)	(0.0002)
q-value	0.452	0.004	0.209	0.040	0.001
Pre-Treatment Mean	0.159	0.272	0.182	0.122	0.103
Other gastroenteritis and colitis of	−0.0002	0.002**	0.0004	−0.001**	−0.001***
infectious and unspecified origin (A09)	(0.0002)	(0.001)	(0.0004)	(0.0003)	(0.0002)
q-value	0.635	0.046	0.724	0.066	0.001
Pre-Treatment Mean	0.129	0.219	0.149	0.101	0.085
Other viral diseases (B25-B34)	0.0004	0.002	−0.001	−0.0004	−0.001*
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0005)
q-value	0.559	0.150	0.667	0.432	0.086
Pre-Treatment Mean	0.215	0.356	0.264	0.164	0.128
Viral infection of unspecified site (B34)	0.0005	0.002	−0.0005	−0.0004	−0.001+
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0005)
q-value	0.635	0.248	0.920	0.730	0.173
Pre-Treatment Mean	0.208	0.348	0.256	0.158	0.123
Other infectious diseases (B99-B99)	0.001**	0.002*	0.001	−0.0002	−0.0004
	(0.0003)	(0.001)	(0.001)	(0.0003)	(0.0003)
q-value	0.015	0.067	0.447	0.490	0.240
Pre-Treatment Mean	0.146	0.271	0.181	0.106	0.075
Other and unspecified infectious diseases (B99)	0.001**	0.002*	0.001	−0.0002	−0.0004
	(0.0003)	(0.001)	(0.001)	(0.0003)	(0.0003)
q-value	0.026	0.109	0.724	0.730	0.302
Pre-Treatment Mean	0.146	0.271	0.181	0.106	0.075
Ear diseases					
Diseases of middle ear and mastoid (H65-H75)	0.001	0.002**	−0.002**	−0.001*	−0.0004*
	(0.0005)	(0.001)	(0.001)	(0.0003)	(0.0002)
q-value	0.363	0.006	0.112	0.109	0.075
Pre-Treatment Mean	0.372	0.475	0.59	0.275	0.152
Suppurative and unspecified otitis media (H66)	−0.0002	0.0004	−0.002***	0.0001	0.00004
	(0.0002)	(0.001)	(0.0004)	(0.0002)	(0.0001)
q-value	0.635	0.590	0.004	0.730	0.823
Pre-Treatment Mean	0.17	0.271	0.255	0.115	0.068
Nonsuppurative otitis media (H65)	0.001+	0.002**	−0.0005	−0.0004*	−0.0002+
	(0.0003)	(0.001)	(0.0005)	(0.0002)	(0.0001)
q-value	0.316	0.036	0.724	0.116	0.208
Pre-Treatment Mean	0.165	0.193	0.286	0.116	0.055
Other disorders of ear (H90-H95)	0.001**	0.0002	0.001*	0.001*	−0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0001)
q-value	0.015	0.384	0.121	0.040	0.376
Pre-Treatment Mean	0.09	0.048	0.134	0.088	0.062
Other hearing loss (H91)	0.00003	−0.00001	0.0002	0.00003	−0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
q-value	0.834	0.927	0.807	0.845	0.723
Pre-Treatment Mean	0.023	0.009	0.037	0.023	0.016
Control for age + gender	yes	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes
Birth cohorts	2000-2014	2008-2014	2006-2014	2003-2011	2000-2009
Observations	51,857,093	7,369,329	14,118,601	13,982,062	10,608,646

Note: +p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level are in parentheses. All variables are count variables including the annual number of diagnoses. The estimates are based on the specification in Equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table F.2: Generalized DiD Results: Detailed Diagnoses (respiratory diseases)

	Age: 1-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Respiratory diseases					
Acute upper respiratory infections (J00-J06)	−0.0004	0.005**	−0.001	0.001	−0.001+
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
q-value	0.705	0.006	0.667	0.256	0.121
Pre-Treatment Mean	0.998	1.422	1.225	0.824	0.664
Acute nasopharyngitis (common cold, J00)	−0.001*	0.0001	0.001	0.0005	−0.0002
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
q-value	0.160	0.927	0.724	0.506	0.723
Pre-Treatment Mean	0.145	0.254	0.179	0.108	0.082
Acute pharyngitis (J02)	−0.0002	−0.0003	−0.00000	0.0002	−0.0001
	(0.0003)	(0.001)	(0.0005)	(0.0004)	(0.0003)
q-value	0.635	0.783	1	0.730	0.823
Pre-Treatment Mean	0.12	0.139	0.137	0.109	0.099
Acute tonsillitis (J03)	−0.0003	0.0005	−0.0002	0.001*	0.0003
	(0.0003)	(0.001)	(0.0004)	(0.0003)	(0.0002)
q-value	0.635	0.495	0.920	0.080	0.271
Pre-Treatment Mean	0.171	0.152	0.222	0.066	0.123
Acute laryngitis, tracheitis (J04)	0.0001	0.0003	0.001	0.00004	−0.0003+
	(0.0002)	(0.0004)	(0.0004)	(0.0002)	(0.0001)
q-value	0.635	0.521	0.724	0.905	0.173
Pre-Treatment Mean	0.054	0.079	0.068	0.043	0.036
Acute upper respiratory infections of multiple and unspecified sites (J06)	−0.001	0.004**	−0.001	−0.0001	−0.001*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
q-value	0.635	0.046	0.795	0.932	0.057
Pre-Treatment Mean	0.639	1.018	0.794	0.493	0.39
Other acute lower respiratory infections (J20-J22)	−0.001	0.005***	0.001	−0.001	−0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
q-value	0.498	0.000	0.667	0.225	0.017
Pre-Treatment Mean	0.275	0.462	0.363	0.199	0.144
Acute bronchitis (J20)	−0.0002	0.004***	0.0004	−0.001	−0.001**
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)
q-value	0.830	0.001	0.920	0.380	0.047
Pre-Treatment Mean	0.248	0.415	0.328	0.179	0.129
Other diseases of upper respiratory tract (J30-J39)	−0.0002	0.003**	−0.001	−0.002**	−0.002***
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.001)
q-value	0.718	0.018	0.447	0.019	0.001
Pre-Treatment Mean	0.406	0.355	0.495	0.378	0.361
Vasomotor and allergic rhinitis (J30)	−0.0001	0.0005	−0.0002	−0.001**	−0.002***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
q-value	0.834	0.326	0.884	0.046	0.001
Pre-Treatment Mean	0.121	0.022	0.069	0.151	0.215
Chronic rhinitis, nasopharyngitis and pharyngitis (J31)	−0.0001	0.002	0.0001	0.0002	0.0002
	(0.0004)	(0.001)	(0.001)	(0.0003)	(0.0002)
q-value	0.838	0.248	0.968	0.730	0.436
Pre-Treatment Mean	0.101	0.189	0.119	0.074	0.055
Chronic lower respiratory diseases (J40-J47)	−0.001	0.002+	−0.001	−0.003**	−0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
q-value	0.452	0.108	0.668	0.019	0.503
Pre-Treatment Mean	0.327	0.34	0.351	0.312	0.309
Bronchitis, not specified as acute or chronic (J40)	0.0001	−0.001	−0.0005	0.0004	0.001
	(0.0003)	(0.001)	(0.002)	(0.001)	(0.001)
q-value	0.834	0.582	0.948	0.730	0.302
Pre-Treatment Mean	0.1	0.178	0.125	0.07	0.057
Asthma (J45)	−0.001	0.002*	−0.0003	−0.002***	−0.001
	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)
q-value	0.588	0.109	0.948	0.008	0.271
Pre-Treatment Mean	0.189	0.118	0.175	0.211	0.231
Other diseases of the respiratory system (J95-J99)	0.0002	0.002	0.001	0.0004	−0.0002
	(0.0005)	(0.001)	(0.001)	(0.0003)	(0.0003)
q-value	0.705	0.175	0.447	0.256	0.503
Pre-Treatment Mean	0.132	0.231	0.169	0.095	0.069
Other respiratory disorders (J98)	0.0002	0.002	0.001	0.0005	−0.0001
	(0.0005)	(0.001)	(0.001)	(0.0003)	(0.0003)
q-value	0.830	0.272	0.849	0.421	0.796
Pre-Treatment Mean	0.13	0.229	0.167	0.094	0.068
Control for age + gender	yes	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes
Birth cohorts	2000-2014	2008-2014	2006-2014	2003-2011	2000-2009
Observations	51,857,121	7,369,332	14,118,563	13,982,067	10,608,649

Note: +p<0.1; *p<0.05; **p<0.01; ***p<0.001.Robust standard errors clustered on county-level are in parentheses. All variables are count variables including the annual number of diagnoses. The estimates are based on the specification in equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table F.3: Generalized DiD Results: Detailed Diagnoses (mental disorders and vision problems)

	Age: 1-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Mental disorders					
Disorders of psychological development (F80-F89)	−0.0001	0.001**	0.0002	−0.0005^{+}	−0.001*
	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
q-value	0.718	0.044	0.668	0.136	0.033
Pre-Treatment Mean	0.22	0.116	0.305	0.234	0.154
Specific developmental disorders of speech and language (F80)	0.00001	0.0004^{+}	0.0002	−0.0003	−0.0003
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)
q-value	0.977	0.143	0.884	0.421	0.271
Pre-Treatment Mean	0.163	0.056	0.261	0.175	0.085
Specific developmental disorder of motor function (F82)	−0.0002	0.001*	−0.0004^{+}	−0.0003^{+}	−0.0001
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0001)
q-value	0.508	0.109	0.439	0.305	0.796
Pre-Treatment Mean	0.05	0.045	0.059	0.054	0.033
Mixed specific developmental disorders (F83)	−0.0001	0.0002*	0.00001	−0.00001	−0.00004
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
q-value	0.635	0.123	0.990	0.932	0.823
Pre-Treatment Mean	0.022	0.008	0.024	0.029	0.021
Unspecified disorder of psychological development (F89)	0.0001	0.00004	−0.00001	0.00005	−0.00004
	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
q-value	0.635	0.927	0.990	0.845	0.823
Pre-Treatment Mean	0.029	0.022	0.034	0.032	0.023
Behavioural and emotional disorders with onset usually occurring in childhood and adolescence (F90-F98)	−0.001***	−0.001*	−0.0005^{+}	−0.001*	−0.001***
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
q-value	0.000	0.000	0.209	0.040	0.001
Pre-Treatment Mean	0.112	0.036	0.092	0.135	0.154
Other behavioural and emotional disorders with onset usually occurring in childhood and adolescence (F98)	−0.0004***	−0.001*	−0.0001	−0.0003*	−0.001***
	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
q-value	0.006	0.046	0.884	0.066	0.001
Pre-Treatment Mean	0.046	0.014	0.041	0.058	0.057
Vision problems					
Disorders of conjunctiva (H10-H13)	0.0004**	0.002***	−0.0005*	−0.0002	−0.0003**
	(0.0001)	(0.0003)	(0.0002)	(0.0001)	(0.0001)
q-value	0.015	0.000	0.184	0.244	0.597
Pre-Treatment Mean	0.143	0.418	0.195	0.096	0.071
Conjunctivitis (H10)	0.0004**	0.002***	−0.001*	−0.0001	−0.0003**
	(0.0001)	(0.0003)	(0.0002)	(0.0001)	(0.0001)
q-value	0.027	0.000	0.411	0.506	0.045
Pre-Treatment Mean	0.139	0.242	0.19	0.092	0.008
Diseases of the eye and adnexa (H53-H54)	−0.0002	−0.0003	0.0004	−0.0002	−0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
q-value	0.452	0.927	0.333	0.334	0.631
Pre-Treatment Mean	0.073	0.025	0.078	0.084	0.081
Visual disturbances (H53)	−0.0002	−0.0003	0.0003	−0.0001	0.00003
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)
q-value	0.635	0.409	0.724	0.730	0.946
Pre-Treatment Mean	0.065	0.023	0.069	0.076	0.074
Visual impairment including blindness (H54)	0.00001	0.00002	0.00003	−0.00005	0.00000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
q-value	0.919	0.927	0.968	0.828	0.976
Pre-Treatment Mean	0.011	0.003	0.014	0.011	0.011
Control for age + gender	yes	yes	yes	yes	yes
Control for swine flu incidence	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes
Birth cohorts	2000-2014	2008-2014	2006-2014	2003-2011	2000-2009
Observations	21,221,456	5,235,816	7,905,234	5,993,036	

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level are in parentheses. All variables are dummy variables. The estimates are based on the specification in Equation 1. Outliers are excluded, i.e. the top 0.00001% in terms of number of diagnoses. The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

G Heterogeneity

Table G.1: Results by gender

	Girls				Boys			
	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases								
Infections	0.008^{***}	0.002	-0.002^{**}	-0.004^{***}	0.008^{***}	0.001	-0.003^{**}	-0.004^{***}
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Pre-Treatment Mean	1.387	1.008	0.821	0.718	1.412	0.998	0.736	0.614
Ear diseases	0.003^{***}	−0.001	−0.0003	-0.001^{*}	0.003^{**}	−0.0005	0.0002	−0.0005
	(0.001)	(0.001)	(0.0005)	(0.0003)	(0.001)	(0.001)	(0.001)	(0.0004)
Pre-Treatment Mean	0.534	0.816	0.452	0.289	0.634	0.868	0.457	0.28
Respiratory diseases	0.015^{***}	−0.001	-0.004^{*}	-0.005^{***}	0.016^{***}	−0.002	-0.004^{*}	-0.006^{***}
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Pre-Treatment Mean	2.679	2.513	1.738	1.473	3.045	2.8	1.968	1.693
Non-communicable diseases								
Mental disorders	0.001^{**}	0.0001	-0.001^{+}	-0.001^{**}	0.001^{**}	0.0001	-0.001^{**}	-0.001^{***}
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Pre-Treatment Mean	0.162	0.323	0.262	0.217	0.193	0.418	0.395	0.332
Obesity	0.0001^{+}	0.0002^{**}	0.0001	-0.0005^{***}	0.0001	0.00002	0.00002	-0.0004^{***}
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pre-Treatment Mean	0.015	0.027	0.035	0.052	0.013	0.02	0.03	0.051
Injury	0.001^{**}	−0.0003	0.00003	0.0003	0.0003	−0.0003	−0.0002	0.00004
	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Pre-Treatment Mean	0.195	0.165	0.172	0.227	0.239	0.215	0.206	0.252
Vision problems	0.001^{***}	0.001	−0.0003	−0.0004	0.001^{***}	0.0002	-0.0005^{+}	-0.001^{**}
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
Pre-Treatment Mean	0.331	0.376	0.342	0.33	0.351	0.388	0.341	0.314
Healthcare consumption								
Treatment cases	0.010^{**}	-0.011^{**}	-0.006^{*}	−0.004	0.010^{**}	-0.014^{***}	-0.006^{*}	-0.005^{+}
	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Pre-Treatment Mean	6.129	5.888	5.058	4.769	6.573	6.402	5.507	5.055
Healthcare costs	1.384^{***}	−0.004	-0.513^{**}	-0.750^{**}	1.572^{***}	−0.199	-0.504^{*}	-1.095^{***}
	(0.167)	(0.184)	(0.160)	(0.242)	(0.184)	(0.220)	(0.213)	(0.315)
Pre-Treatment Mean	308.688	268.586	219.012	222.848	333.192	306.602	269.764	275.062
Control for age + gender	yes	yes	yes	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009	2008-2014	2006-2014	2003-2011	2000-2009
Observations	4,169,396	6,919,385	6,882,828	5,223,170	4,306,952	7,144,969	7,060,343	5,353,428

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1 (separately for boys and girls). The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table G.2: Results by household income on county level

	Top 30th percentile household income				Bottom 30th percentile household income			
	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases								
Infections	0.001	−0.002	−0.001	−0.004*	0.011**	0.0004	−0.004**	−0.004**
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Pre-Treatment Mean	1.353	0.96	0.758	0.647	1.448	1.04	0.803	0.694
Ear diseases	0.002^{+}	−0.002	0.0004	−0.001	0.0001	0.001	0.00004	−0.001^{+}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Pre-Treatment Mean	0.55	0.796	0.432	0.269	0.628	0.895	0.482	0.304
Respiratory diseases	0.012***	−0.004	−0.001	−0.005^{+}	0.012*	0.003	−0.003	−0.005
	(0.003)	(0.004)	(0.002)	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)
Pre-Treatment Mean	2.701	2.459	1.724	1.476	3.09	2.911	2.028	1.736
Non-communicable diseases								
Mental disorders	0.001	0.0001	−0.001^{+}	−0.001^{+}	0.001*	0.0003	−0.001	−0.001^{+}
	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)
Pre-Treatment Mean	0.171	0.358	0.32	0.266	0.18	0.377	0.338	0.284
Obesity	0.0001	0.0001	0.0002	−0.0005*	0.00003	0.0002	0.0001	−0.0004*
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Pre-Treatment Mean	0.013	0.022	0.03	0.048	0.015	0.026	0.035	0.055
Injury	0.0003	0.00002	0.0001	0.0005	0.0003	−0.001	−0.0005^{+}	−0.0005
	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Pre-Treatment Mean	0.206	0.18	0.18	0.231	0.228	0.201	0.199	0.251
Vision problems	0.0003	−0.001	−0.001	−0.001*	0.001**	0.001^{+}	0.0001	−0.0002
	(0.0005)	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.0005)
Pre-Treatment Mean	0.344	0.373	0.332	0.317	0.341	0.391	0.353	0.326
Healthcare consumption								
Treatment cases	0.010^{+}	−0.013*	−0.006	−0.003	0.004	−0.014*	−0.005	−0.008
	(0.005)	(0.006)	(0.004)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Pre-Treatment Mean	6.15	5.891	5.096	4.763	6.547	6.417	5.523	5.12
Healthcare costs	1.399***	−0.026	−0.306	−0.482	1.036***	−0.044	−0.232	−1.341**
	(0.273)	(0.330)	(0.245)	(0.345)	(0.232)	(0.317)	(0.325)	(0.510)
Pre-Treatment Mean	314.933	278.307	240.285	249.829	327.273	299.153	253.064	254.179
Control for age + gender	yes	yes	yes	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009	2008-2014	2006-2014	2003-2011	2000-2009
Observations	2,537,818	4,235,551	4,168,113	3,171,942	2,506,540	4,205,943	4,186,757	3,166,076

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1 (separately for children from counties in the top 30 income percentile and children from counties in the bottom 30 income percentile). The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

Table G.3: Results by share of migrants on county level

	Top 30th percentile share of migrants				Bottom 30th percentile share of migrants			
	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Communicable diseases								
Infections	0.005^{*}	0.0004	−0.003	-0.006^{*}	0.008^{***}	0.001	−0.001	-0.002^{*}
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Pre-Treatment Mean	1.415	1.008	0.791	0.686	1.376	0.985	0.754	0.636
Ear diseases	0.005^{***}	0.001	0.001	−0.0004	0.001	-0.003^{*}	−0.001	-0.001^{*}
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.0004)
Pre-Treatment Mean	0.561	0.793	0.44	0.28	0.605	0.874	0.466	0.29
Respiratory diseases	0.027^{***}	0.016^{**}	−0.005	-0.009^{**}	0.009^{*}	−0.006	−0.001	−0.002
	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)
Pre-Treatment Mean	2.853	2.61	1.863	1.606	2.882	2.674	1.837	1.57
Non-communicable diseases								
Mental disorders	0.0004	0.0005	-0.002^{*}	-0.002^{*}	0.001	-0.001^{+}	−0.001	-0.001^{*}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.001)	(0.0004)	(0.0005)
Pre-Treatment Mean	0.177	0.36	0.329	0.276	0.186	0.382	0.331	0.277
Obesity	0.0001	0.0001	0.0001	-0.001^{***}	0.00004	0.0002^{+}	−0.00002	−0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pre-Treatment Mean	0.015	0.026	0.037	0.059	0.014	0.022	0.029	0.046
Injury	0.001^{*}	0.0003	0.00002	0.001	−0.0000	-0.001^{+}	−0.0002	−0.0001
	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Pre-Treatment Mean	0.209	0.177	0.171	0.219	0.219	0.201	0.204	0.256
Vision problems	0.001^{*}	0.0004	−0.0004	−0.0002	0.001^{+}	−0.00003	−0.0001	−0.0003
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.001)	(0.0004)	(0.0004)
Pre-Treatment Mean	0.336	0.36	0.319	0.304	0.345	0.397	0.363	0.343
Healthcare consumption								
Treatment cases	0.030^{***}	0.015^{+}	-0.009^{*}	−0.006	−0.002	-0.027^{***}	-0.007^{+}	−0.003
	(0.006)	(0.008)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Pre-Treatment Mean	6.237	5.983	5.186	4.864	6.475	6.267	5.37	4.972
Healthcare costs	1.818^{***}	0.635	-0.818^{*}	-1.648^{**}	1.006^{***}	-0.660^{*}	-0.759^{**}	-0.610^{+}
	(0.264)	(0.415)	(0.316)	(0.575)	(0.213)	(0.255)	(0.251)	(0.314)
Pre-Treatment Mean	314.976	279.585	243.168	253.364	330.728	295.077	247.763	248.453
Control for age + gender	yes	yes	yes	yes	yes	yes	yes	yes
Control for KKZ + Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Birth cohorts	2008-2014	2006-2014	2003-2011	2000-2009	2008-2014	2006-2014	2003-2011	2000-2009
Observations	2,522,457	4,206,470	4,207,269	3,182,483	2,418,955	4,126,573	4,111,397	3,086,171

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Robust standard errors clustered on county-level in parentheses. The following variables are count variables: infections, ear diseases, respiratory diseases, injuries (annual number of diagnoses), treatment cases and costs. Costs are fee-adjusted. The following variables are dummy variables (indicating if a child had at least once per year a particular diagnosis): mental disorders, obesity, vision problems. The estimates are based on the specification in Equation 1 (separately for children from counties in the top 30 share of migrants percentile and children from counties in the bottom 30 share of migrants percentile). The coefficients show the effect of a one percentage point increase in the daycare coverage rate on the respective disease. *Source:* KBV 2009–2019, own calculations.

H Additional analysis: SOEP

Table H.1: Additional analysis: Parental health

	Age: 1-2	Age: 3-5	Age: 6-8	Age: 9-10
Maternal health	-0.0754*	-0.00462	0.0419	-0.0245
	(0.0384)	(0.0345)	(0.0476)	(0.0573)
Observations	8823	8187	4825	2650
Mother: Number of doctor visits	0.201+	-0.119	0.119	0.0321
	(0.120)	(0.213)	(0.202)	(0.188)
Observations	7218	8180	4819	2634
Mother: Number of days missed at work due to sickness	4.087***	-0.447	1.498+	0.0430
	(1.203)	(1.164)	(0.883)	(1.745)
Observations	3493	4908	3151	982
Mother: Number of days missed at work due to child's sickness	1.278**	-1.264***	-0.629	0.537
	(0.400)	(0.371)	(0.428)	(0.549)
Observations	1357	2059	1142	271
Paternal health	-0.0640+	0.0267	0.183***	-0.110
	(0.0387)	(0.0416)	(0.0501)	(0.0673)
Observations	7150	6136	3967	2142
Father: Number of doctor visits	0.0675	-0.0318	-0.274	-0.152
	(0.113)	(0.177)	(0.172)	(0.202)
Observations	5414	6118	3958	2134
Father: Number of days missed at work due to sickness	0.222	-0.345	-1.419	-1.570
	(1.103)	(0.954)	(1.131)	(1.245)
Observations	3567	4510	3064	921
Father: Number of days missed at work due to child's sickness	0.702	0.119	-0.585	-1.017
	(0.498)	(0.275)	(0.577)	(0.969)
Observations	645	781	416	77

Note: $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001. Standard errors in parentheses. The estimates are based on a simple OLS regression where "daycare attendance at age 1–2 years" is the explanatory variable. The set of control variables includes parental education, survey year, cohabitation status, birth order, parental labor force status, parental migration background, household income, parental age, child sex, federal state of residence, age of siblings and if all-day daycare/school is attended. *Source:* SOEP, v37, own calculations.