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Mathias Huebener
DIW Berlin and IZA

Astrid Pape
Freie Universität Berlin

C. Katharina Spiess
DIW Berlin and Freie Universität Berlin

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Parental Labour Supply Responses to the Abolition of Day Care Fees*

This paper provides evidence that low private contributions to highly subsidised day care constrain mothers from working longer hours. We study the effects of a reform that abolished day care fees in Germany on parental labour supply. The reform removed private contributions to highly subsidised day care in the year before children enter primary school. We exploit the staggered reform across states with a difference-in-differences approach and event studies. Although participation in day care is almost universal for preschoolers, we provide evidence that the reform increases the intensity of day care use and the working time of mothers by about 7.1 percent. Single mothers, mothers with no younger children, mothers in denser local labour markets, and highly educated mothers react strongest. We find no evidence for labour supply responses at the extensive margin, and no evidence of responses in paternal labour supply. The effects on maternal labour supply fade-away by the end of primary school as mothers in the control group also gradually increase their labour supply as their children grow older.

JEL Classification: J13, J22, J38

Keywords: labour supply, child care costs, difference-in-differences, event study

Corresponding author:
Mathias Huebener
DIW Berlin
Mohrenstraße 58
10117 Berlin
Germany
E-mail: mhuebener@diw.de

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I Introduction

One of the most constraining factors for maternal labour supply is access to affordable day care: With the increasing availability of highly subsidised day care in many developed economies since the 1970s, maternal labour supply also increased (e.g. OECD, 2019). Still, many mothers of young children do not work or they work part-time, which is associated with long-term negative consequences for their careers and pensions (e.g. Fasang, Aisenbrey, & Schömann, 2012; Manning & Petrongolo, 2008). Policy-makers across countries continue to increase day care subsidies to promote maternal employment, either through an increased supply of subsidised care or through reductions of parental contributions to day care.

Many studies show that the availability of subsidised day care can effectively promote maternal employment (e.g. Carta & Rizzica, 2018; Bauernschuster & Schlotter, 2015; Fitzpatrick, 2012; Cascio, 2009; Baker, Gruber, & Milligan, 2008).[1] Still, although many parents must pay some fees for day care, we know very little about the effects of small private contributions to day care in highly subsidised systems on parental employment decisions. This paper studies parental labour supply effects of a reform that abolished day care fees in Germany, a country with a relatively high rate of part-time working mothers (OECD, 2019). The reform abolished private contributions to day care in a highly subsidised setting for preschoolers, i.e. to children in the year before entering primary school. Day care participation for these children is near universal. The reform was implemented across Germany's states at different points in time, starting in 2006. We exploit the reform variation across states and time with a difference-in-differences approach. Thereby, we compare the labour supply of parents with preschool children in treatment states before and after fees were abolished, contrasting these changes to parental labour supply in states without a fee abolition reform. We also use the fact that the reform affected only preschoolers: We estimate effects for all parents with children up to the age of 10. This event study type approach across children's age allows for identifying any anticipation effects (younger children are not yet treated) and the evolution of effects as children of treated parents enter primary school. The main analysis

[1] The effects depend on the availability of alternative care modes or the existing level of affordable day care. An additional strand of the literature studies effects of childcare tax credits on maternal labour supply. However, these tax credits apply to day care systems that are hardly subsidised (e.g. Blau & Robins, 1988; Blundell, Duncan, McCrae, & Meghir, 2000; Herbst, 2010).

relies on data from the German Micro Census, annually sampling one percent of all German households. The rich data provides detailed information on more than 328,000 families.

Theoretically, the effect of lower day care fees can be ambiguous (e.g. Fitzpatrick, 2012). For parents who initially do not use day care and who are out of the labour force, lower day care costs increase the incentive to take up work. Lower costs lower the reservation wage and increase the net benefits of working. For parents who are already using day care and who are involved in paid work, reducing day care fees generates an income and a substitution effect. The net benefits of an additional hour of work increase, as the increasing shadow price of leisure makes market work more attractive (substitution effect). At the same time, the overall available household income increases as well, which may lead to a reduction in market work hours (income effect). Which effect dominates is *ex-ante* ambiguous.

The paper provides evidence that further cost reductions increase maternal labour supply at the intensive margin. Average cost reductions of about 65 euro per months (in 2010 euro, 86 USD) increase the usage of day care by about three hours per month and maternal market working hours by about 1.5 hours per month. Full-time employment increases by 7.2 percent (1 percentage point, with a baseline of 14 percent). Event study results show that maternal labour supply reacts right at the age of the child at which fees are abolished. The increase in working hours of mothers exiting marginal employment (less than ten hours per week) is only short-lived in the year of free day care. Effects on full-time employment persist as children enter primary school, but the statistical difference vanishes after about four years because the labour supply of unaffected mothers catches up as their children grow older. We find the strongest reactions by mothers without further younger children, single mothers, and mothers living in more urban areas (typically with denser local labour markets that may allow short-term adjustments of labour supply). Effects are also larger for highly educated mothers (with a close attachment to the labour market) and mothers' whose children enter school systems in federal states with more afternoon care, i.e. a higher all-day schooling share (though these differences are not statistically significant). Effects at the extensive margin are very small and, in most specifications, insignificant. We also cannot find any evidence of paternal labour supply responses because the vast majority of fathers already worked full-time before the reform. The findings pass a large set of robustness checks, including significant changes to the set of federal states considered in the analysis (e.g. excluding East German states or never-reformers).

Our paper makes several important contributions to the large literature on subsidised day care and labour supply: First, and foremost, we contribute new insights into effects of *removing* private contributions to day care in an already highly subsidised system. Most of the existing literature studies effects of *introducing* highly subsidised care. The context, initial maternal labour supply, and the available amount of subsidised day care matter for the magnitude of estimated labour supply effects of day care fee reforms (see e.g. Bauernschuster & Schlotter, 2015; Brewer, Cattan, Crawford, & Rabe, 2016; Cattan, 2016).[2] With many industrialised countries already providing subsidised day care, we move on and contribute an answer to the highly policy-relevant question of whether *further* fee reductions can still promote maternal labour supply. We add to a very small set of studies examining labour supply effects of further fee *reductions* in other contexts (Lundin, Mörk, & Öckert, 2008; Bettendorf, Jongen, & Muller, 2015; Givord & Marbot 2015; and Brewer et al. 2016), but our study is one of the first on day care fee *abolitions*.

Second, we estimate the day care fee effects of a universal programme in which day care attendance is near universal for affected preschoolers. Many previous studies evaluate targeted programmes or programmes with lower take-ups; due to non-universal take-up, they can only estimate intend-to-treat effects of day care subsidies. As the programme we analyse is universal, we can perform numerous heterogeneity analyses to better understand who responds the most to day care fee abolitions. We can also estimate day care fee effects on labour supply net of childcare availability constraints, as almost all children already participate in day care.[3]

Third, we trace the dynamics of the effects, as children grow older. Most previous studies focus on short-term effects, but cost-benefit considerations require an understanding of whether effects persist beyond the years of subsidised day care. Moreover, we also study the effects of the reform on paternal labour supply, a dimension rarely considered in the previous literature. Finally, we

[2] In a summary of non-quasi-experimental studies, Blau & Currie (2006) report elasticities for the price of day care for maternal labour force participation range from 0.06 to -3.40, suggesting a positive impact of lower day care costs on maternal labour supply. These estimates cannot account for endogeneity and selection problems: Day care costs are only observed for households using day care, which is related to mothers' working decision. Another strand of the literature employs structural models. For example Müller, Spiess, & Wrohlich (2013) amd Wrohlich (2004), show that without the already available subsidies, maternal labour supply would be substantially lower.

[3] In a representative survey, 91% of parents report that they had a choice between at least two day care centres (Camehl, Schober, & Spiess, 2018).

provide cost-benefit considerations and reveal that the abolition of day care fees is an effective, but not a very efficient, policy tool to support mothers with preschoolers in the labour market.

As highlighted above, the effects of day care costs on maternal labour supply receive significant research attention. Studies often estimate the effect of subsidised or free care provision compared to a *counterfactual situation of little or no subsidised care*. In environments with low maternal labour market attachment and a limited supply of affordable day care, studies report large positive effects of lower day care costs on maternal labour supply. The introduction of centre-based care for a lower daily fee in Quebec, Canada, increased the share of mothers working by 14.5 percent, while day care enrolment increases from 40 to more than 60 percent (Baker et al., 2008; Lefebvre, Merrigan, &Verstraete, 2009). In a US study, Gelbach (2002) uses variation in enrolment in free preschool related to the quarter of birth to estimate the effect of free day care on maternal labour supply. For single mothers whose youngest child is treated, the probability to work increases by 6 percent, while working hours increase by 10 percent. The same setting with more recent data is exploited by Fitzpatrick (2012). She finds labour supply increases only for single mothers: Employment increases by 15 to 20 percentage points. She attributes parts of the different findings to substantial changes in the labour market environment for women. Cascio (2009) exploits the staggered expansion of kindergarten subsidies expanding the supply of seats for children in US public schools. She finds that single mothers' labour supply is particularly responsive. Similar results are found by Goux & Maurin (2010), who exploit age discontinuities in eligibility for free preschool in France. Nollenberger & Rodriguez-Planas (2015) analyse the expansion of free preschool to 3-year-olds in Spain. Maternal employment increases by 10 percent. Carta & Rizzica (2019) analyse a reform extending access to subsidised day care to 2-year-olds in Italy. Labour force participation increased by about 6 percentage points and employment by 5 percentage points, with large differences depending on labour market conditions and family income. Brewer et al. (2016) exploit the introduction of free day care in England, distinguishing between part- and full-time free day care. While free half-day care does not affect maternal labour supply, free full-day care increases the probability for mothers of entering the labour force by 5 percentage points.[4]

[4] Another strand of the literature studies expansions of publicly subsidised day care (e.g. for Norway, see, e.g., Havnes & Mogstad, 2011, and for Germany, see, e.g., Bauernschuster & Schlotter, 2015; Müller & Wrohlich, 2018), which is not very informative for the debate on day care fees.

In contrast to these studies, we study effects of day care fee reductions in *a counterfactual environment that already provides highly subsidised day care*. In Germany, enrolment in day care in the year before entering primary school is near universal; still most mothers only work part-time. Only a few previous studies consider a similar context. Lundin et al. (2008) analyse a cap on the price for day care in Sweden, which cut private costs more than half. They find that this affected neither day care enrolment nor maternal labour supply, as both were already high pre-reform. Bettendorf et al. (2015) analyse a 2005 reform in the Netherlands that cut average parental fees in half. The reform increased day care enrolment by 15 percentage points, while maternal labour force participation increased by 2 percentage points. A reform in France increased subsidies for day care by 50 percent. Givord & Marbot (2015) estimate that maternal labour force participation increased by around 1 percentage point in the short-term. Gathmann & Sass (2018) show that a relative *increase* in day care costs – resulting from a subsidy for home-based care in one federal state – reduces day care attendance by 8 percentage points, with no effects on maternal labour supply. Busse & Gathmann (2018) provide first evidence on effects of day care fee abolitions for Germany. Using data from the German Socio-Economic Panel (SOEP), they focus on effects on children, but also report insignificant effect estimates on maternal labour supply at the extensive working margin. Using a much larger data set and event studies, we document effects mainly at the intensive margin of maternal employment and provide estimates of short- and medium-run effects. Our rich data allow detailed heterogeneity analyses. The dynamics and heterogeneity of the effects prove to be very important for cost-benefit considerations, which we provide at the end of our analysis.

The study is structured as follows. Section II provides the institutional background with respect to the parental labour market and the day care structure. Section III describes the data and outlines our empirical strategy. Section IV reports the main findings on maternal labour supply. We analyse effect heterogeneities and the effects on fathers in Section V. In Section VI, we perform several robustness checks. We discuss our findings and conclude in Section VII.

II Institutional Background

Female labour force participation in Germany has substantially increased over the last decades: While in 1965, only 39.3 percent of all women aged 15 to 65 participated in the labour force, this share rose to 70.8 percent in 2016. Germany now ranks third within the European Union (Merkle,

1994, Bundesagentur für Arbeit, 2017). *Maternal* labour force participation, however, is only slightly above European average (OECD, 2019). In 2015, around 63 percent of mothers whose youngest child was aged between three and five were part of the labour force; of these, only 30 percent worked full-time. Paternal labour supply is constantly very high with most fathers working full-time.

Increases in maternal employment were possible through the increased supply of publicly funded day care since the 1990s. An important contributor was the introduction of a legal claim for a four hours slot in 1996 for children aged three or older (Bauernschuster & Schlotter, 2015; Spiess, 2008). Still, publicly funded day care coverage varies by children's age: After 2000, enrolment is almost universal for children above the age of three. Below age three, the share for children in day care has seen a substantial expansion, especially in West Germany, from below 5 percent in 1990 to about 29.4 percent in 2018 (Statistisches Bundesamt, 2018; Seils, 2013).

Most day care centres are operated by non-profit organisations or municipalities. In comparison to the US market and some European markets, there is not much competition among day care providers in Germany (e.g. Artz & Welsch, 2014; Spiess, 2008) and the share of for-profit providers is low at about 2 percent (Statistisches Bundesamt, 2018). Day care in Germany is part of the child and youth welfare system. The federal government has legislative and organisational authority over this system, setting the framework for day care with federal law.[5] The actual implementation of it is in the responsibility of each federal state. Municipalities and the federal states share the responsibility for day care funding, with municipalities ensuring the provision of day care. Unlike in most other countries, the federal government does not have a direct role in the basic funding of day care services. This results in substantial regional variation in the level and structure of day care fees (see Schmitz, Spiess, & Stahl, 2017, and Appendix Table B.1 for an overview). The federal law only suggests that day care fees should consider household income, the number of children, and the number of hours spent in day care in the fee structure. The majority of states structure fees based on household income, family size, and the number of hours. Economically deprived households, i.e. mainly households receiving public transfers, are typically exempted from fees or their fees are covered by welfare agencies.

[5] "Child and Youth Welfare Act", *Kinder- und Jugendhilfegesetz.*

Because states and municipalities regulate day care fees, fees are usually not a signal of day care quality. Each state administers its own regulations for minimum quality standards. The child-teacher ratio is one of the few indicators that are precisely, albeit differently, regulated across states. Moreover, all German states have implemented pedagogical guidelines. The level of other quality regulations and the specific pedagogical guidelines vary across states. Consequently, day care quality varies across regions and day care centres (e.g. Stahl, Schober, & Spiess, 2018).[6] There is also no overall national accreditation system like that administered by the National Association for the Education of Young Children in the United States (e.g. Xiao, 2010), which consumers may use as a source of information. Furthermore, there are no quality ratings and improvement systems, as found in many US states (e.g. Herbst, 2018).

Day care is highly subsidised by the states, the municipalities, and the federal government. Before the day care fee reform, on average, 75-80 percent of the costs of non-profit providers are covered by public funds, about 10 percent by the providers themselves, and the rest by parents (Spiess, 2008). Public expenses for day care increased from 8.6 billion euro in 1995 to 25.4 billion euro in 2014, which amounts to 0.9 percent of GDP (Statistisches Bundesamt, 2017). This is only slightly above the OECD average of 0.8 percent (OECD, 2016). Scandinavian countries, such as Norway, spend a substantially larger share, about 2 percent of GDP. Still, day care requires some private contributions. Day care fees typically amount to about 5 to 9 percent of net family income (Schmitz, Spiess, & Stahl, 2017). The OECD average is 12 percent for partnered parents, while expenses are particularly high in the US at 25 percent (OECD, 2016).

Starting in 2006, German federal states started to abolish day care fees for preschoolers, i.e. for children in the last year of day care before primary school (see Appendix Table B.1). The political arguments that are typically brought forward for the abolition of day care fees are to financially support families as well as to facilitate the use and benefits of day care, independent of the financial background of the household.[7] Moreover, it was argued that day care offers education and, thus, should be free, just like school education. Two city-states, Berlin and Hamburg, along with four

[6] Surprisingly, several studies show that, in general, parents report a relatively high level of satisfaction with day care, although this varies by quality aspects and is related to actual levels of quality as assessed by parents (Camehl, Stahl, Schober, & Spiess, 2015).

[7] See, e.g., Behörde für Soziales, Familie, Gesundheit und Verbraucherschutz (2009) for Hamburg. The abolition of day care fees is not a reform that is related to one political party only: Christian Democrats, Social Democrats, and the Green Party were each responsible for the introduction of free day care in at least one federal state.

larger states – Hesse, Lower Saxony, North-Rhine Westphalia, and Rhineland-Palatinate – with both rural and urban populations were the first states to abolish fees for preschoolers. These states are part of the treatment group in our analysis.[8] Three other states adopted free day care and then subsequently reimposed fees in later years. We omit these states from the main analysis and discuss their role in the robustness section. Appendix Figure A.1 presents the rollout of the reform across federal states.[9] All other states did not change the fee systems in the period we analyse. After 2013, more German states reduced and abolished day care fees, or announced plans for such reforms in the coming years (BMFSFJ, 2019).

III Data and Empirical Approach

A. German Micro Census

Our main analysis uses data from the German Micro Census (RDC, 2019). This annual survey draws a representative sample of one percent of all German households. Participation is mandatory and only few questions are answered on a voluntary basis. The dataset is particularly well suited for our analysis because it contains rich information on household structures and labour market outcomes. Further, the number of observations is large and, due to mandatory participation, selective non-response or attrition is not an issue. We use the scientific use file, a 70 percent random sample of the data, which, however, restricts information on the date of birth and the municipality of individuals.

We use the waves 2005 through 2013, covering the main treatment period.[10] We study five main outcomes: Parents' labour force participation (working or actively looking for a job), whether they engage in market work, their typical working hours, whether they work full-time (more than 30 hours), or whether they work more than marginally (more than 10 hours). We include mothers with children up to age ten (end of primary school) in order to assess short- and medium-run

[8] The two city-states of Berlin and Hamburg, as well as Rhineland-Palatinate also abolished day care fees for younger children. These changes, however, were administered in later years. We include a variable in our model that accounts for fee abolitions beyond the last year. Our findings are also robust to excluding these three states from the sample (see sensitivity checks in Section VI.B).

[9] For more details, see Deutscher Bundestag Wissenschaftliche Dienste (2016); Ministerium für Bildung, Jugend und Sport des Landes Brandenburg (2013); Schmitz et al. (2017).

[10] Since 2005, the Micro Census interviews are carried out throughout the year. Before 2005, interviews were conducted in April, which may result in some seasonal dependences for labour market outcomes compared to individuals interviewed from 2005 onward.

effects. Because our data is cross-sectional, we assume that school-aged children went to day care in the same state they are living in now. This assumption is reasonable, as mobility across states in Germany is rather low.[11] Descriptive statistics are reported in Table 1. Our samples comprise 328,299 mother-child observations overall, and 192,792 mother-child observations where the mother participates in the labour force (65 percent).[12] Overall, 63 percent are working, with 13.5 average weekly working hours (conditional on labour force participation 22.6), 15 percent work full-time (conditional 25 percent), and 51 percent (conditional 86 percent) work more than 10 hours per week.

B. Supplementary data: SOEP and official statistics

While the German Micro Census provides a very large sample to analyse labour market outcomes of parents, the data lacks information on children's day care participation and parental day care expenses. Therefore, we complement our analysis with two other data sources.

First, we employ the German Socio-Economic Panel Study (SOEP, see Goebel et al. 2018). This annual representative household panel study interviews about 33,000 individuals in 11,000 households on a broad range of topics. It also collects information on day care arrangements and day care expenses. Specifically, we use the same timeframe (2005-2013). Due to the very detailed information on the birth month and school entry, we are able to accurately define the last year of day care prior to entering school. This is our basis to assess the fee abolition reform effect on day care expenses and day care attendance. The data on day care expenses is available for three waves (2005, 2009, 2013) and adjusted for inflation. As we focus only on children in the last day care year, the number of observations is comparably small.

We also use administrative statistics of child and youth welfare at the state-year level (Statistisches Bundesamt, 2018, covering the years 2006-2013) to analyse reform effects on day care attendance and hours of care (day care dosage).

[11] Less than 7 percent of all children aged one to ten move to a different state over the course of at least six years (own calculations based on SOEP data, see Section III.B).

[12] In a second step of the analysis, we focus on mothers participating in the labour force. We also remove families receiving social benefits (recipients of *Arbeitslosengeld I and II)*, as they are typically exempted from day care fees.

C. Empirical strategy

The day care fee abolition reform was introduced at different points in time across federal states. This variation allows us to compare day care choices and labour supply of parents in states with day care fee abolitions before and after the reform. To capture any general changes over time, we can contrast the before-after comparisons in reform states to before-after comparisons in states without reforms. As a starting point for the analysis, we employ a difference-in-differences (DiD) model with a reform dummy, state-fixed effects (δ_s), and cohort-fixed effects (θ_c):

$$\mathcal{Y}_{isc} = \beta \times Reform_{sc} + \delta_s + \theta_c + X'_{isc}\kappa + \xi_{isc} \quad (1)$$

The variable $\mathcal{Y}_{\mathrm{isc}}$ is the outcome of individual *i* in state *s* born in birth cohort *c*. The variable *Reform* takes the value of 1 for children of birth cohort *c* in state *s* who are exempt from fees in their final year of day care before school entry (see Appendix Table B.1). The *X*-vector denotes individual or state-time varying control variables, which we specify in detail later. The error term ξ captures idiosyncratic variations. Standard errors allow for heteroscedasticity and clustering at the state-year level.

The main assumptions for a causal interpretation of the reform estimates is the common trend assumption and no simultaneous co-treatments. One of our key concerns for the identification of fee abolition effects regards the expansion of subsidised day care *availability*. Its relevance for maternal labour supply is well-established in the literature (see e.g. for Germany Müller & Wrohlich, 2018). We carefully address two potential threats to our identification in the robustness section: First, the substantial expansion of publicly subsidised day care for children below the age of three (e.g. Spiess, 2011); second, the expansion of full-day care for all children from age three onward, along with two day care-expansion laws from 2004 and 2008.[13] Most importantly for our analysis, we need to rule out that these changes rather than the fee abolition reform drive effects on parental labour supply. Our robustness checks in Section VI provide confidence that the difference-in-differences approach can separate effects of state-dependent day care fee abolitions from general trends in day care availability. We use the parsimonious model in eq. 1 to estimate

[13] *Tagesbetreuungsausbaugesetz,* 2005, *Kinderförderungsgesetz,* 2008 (see, e.g., Schober & Spiess; 2013; Schober & Stahl, 2016).

reform effects on day care expenses of parents and the use of day care in data from the SOEP and in official statistics.[14]

For our analysis of parental labour supply that we observe independent of children's age, we also make use of the treatment variation across children's age. The treatment only affects children in the final year of day care of certain cohorts, while children in earlier cohorts and younger children are not (yet) treated.[15] We set up the analysis as an event study across children's age. We estimate the effects on parents of younger children who will be exempt from fees in the final day care year (but are not yet treated), on parents of affected children in the last day care year, and on parents of older children who were exempt from fees before they entered primary school. Thereby, we compare parental labour supply of parents of children at a specific age in treatment states before and after the reform, contrasting the differences with general changes of parental labour supply in non-reform states. This approach has the advantage that the evolution of effects across children's age can be traced: If effects set-in in the final year of day care, we can rule-out that unobserved co-treatments on younger children drive the findings; second, we can learn about the dynamics of maternal labour supply effects as children grow older.

Therefore, we adjust the standard difference-in-differences model from eq. 1 and estimate regression models of the following form:

$$y_{iascw} = \beta_a \times Reform_{sc} + \gamma_a + \theta_c + \rho_w + \sum_z \delta_{sz} \times \mathbb{1}_{DistrictSize_{i=z}} + X_i'\kappa + S_{sc}'\lambda + \mu_0 + \epsilon_{iascw} \quad (2)$$

We now consider labour supply outcome y of parent i with a child of age a, born in cohort c, residing in state s and participating in survey wave w. The coefficients β_a are of key interest, estimating parental labour supply responses separately by children's age a. The model also includes age-group-fixed effects (γ_a) to account for general differences in parental labour supply by children's age. We also include a set of child cohort-fixed effects (θ_c) and survey wave-fixed effects (ρ_w) to account for any shocks or changes over time between birth cohorts of children and surveys that are common across regions, such as changes in economic conditions, or federal law

[14] The variables included in the *X*-vector varies between the SOEP-based individual level analysis, and the aggregated administrative data. The control variables are specified in the table notes.

[15] One state removed fees for the final three years in day care, two other states removed day care fees for younger children in subsequent years. We control for additional free day care years in the main analysis and also exclude these states in a robustness check. We draw the same conclusions.

changes in family support. We account for regional differences with a set of region-fixed effects (δ_{sz}). As we do not observe regions smaller than the states in the scientific use file, we interact state dummies with a set of district size dummies to account for smaller regional differences even within federal states, as their size can be taken as constant in the observation period. Such regional differences may include, e.g., labour market opportunities, day care infrastructure, and social norms.

Due to data limitations, we cannot observe the birth month of the child and the time of school entry exactly (only the year of birth). We group children aged five to seven years *not in school* as those who are most likely in their final year of day care. Using data from the SOEP, we see that 98 percent of children experience their last day care year in this age group. About 2 percent of four-year-olds, 41 percent of five-year-olds, and 90 percent of six- and seven-year-olds who are not in school are in their last day care year. Some children enter school at this age already, such that we assign children aged five to seven years and in school to the group of school starters. The other groups are children below age three, three to four years (both groups may attend day care, but not in the final year), and eight to ten years (i.e. primary school children). The grouping clarifies the treatment assignment, but also increases the precision of the estimates.

We then add a vector of individual socio-economic control variables, *X*, to the model. It comprises indicators for maternal migration background, maternal education (low, middle, and high secondary schooling), maternal age in years, whether the partner is living in the household, and the gender of the child. We then include a vector *S* of state-level controls at the federal state-year level that account for possible time-varying differences across regions that are not captured by region fixed-effects. It includes the female labour force participation rate[16], the coverage rate of children in day care below age three, the share of primary school children in all-day schooling[17],

[16] States in which women have a closer labour market attachment may be more likely to pass the day care fee abolition reform. Therefore, it would be sensible to control for it. However, if the reform affects maternal labour force participation, controlling for it would bias the reform effect estimates (i.e. bad control variable). Note that the share of mothers with children in the treated age group is small among all women aged 15-64; moreover, our reform effect estimates on maternal labour force participation is very small and, in most cases, insignificant. The main findings are not sensitive to including the female labour force participation as a control variable.

[17] In contrast to the day care expansion and the increase in the full-day care slots, we are less concerned that the all-day primary schooling expansion confounds effect estimates, as it affects *older* children in primary school after the treatment and its expansion is not correlated with the day care fee abolition. In Appendix Figure A.2, using data from Kultusministerkonferenz (2011, 2015), we show that treatment and control states experience a comparable expansion of all-day schooling, with treatment states starting from a higher level. We test whether the state share of primary

and a variable that accounts for years of free day care for some individuals in states that expanded the final fee-free year to include earlier years. As we only observe the year of birth of the child and the interview year, the treatment status of the cohort around the implementation is somewhat unclear. We account for this treatment uncertainty with a dummy indicating this first cohort, but the results are not sensitive to removing the cohort from the sample (see Section VI.B).

To assess whether treatment and control observations are comparable in terms of their socio-economic characteristics, we estimate the model in eq. 2 without socio-economic and state-level controls using children's, maternal, and household characteristics as the dependent variable. The results are reported in Table 2. Almost all characteristics of mothers with children in the final year of day care are balanced, both in the full sample and in the subsample of mothers participating in the labour force. Only one in twenty tests turns significant, which is what we would also expect by chance. We test the joint orthogonality of the socio-economic characteristics based on the model in equation 2, using the reform indicator as the dependent variable (right-hand-side balancing test as described in, e.g., Bruhn & McKenzie, 2009; Pei, Pischke, & Schwandt, 2019). The joint F-test suggests that the socio-economic characteristics are jointly orthogonal to the treatment. Still, note that we also control for maternal education, in addition to other socio-economic characteristics, in our main specification.

Our model is specified parsimoniously. A saturated model could interact all covariates with the set of age-group dummies. This is equivalent to estimating the effects for each age group separately; we show in Appendix Table B.2 that we reach the same conclusions when we do so. As expected, the separate estimations are estimated less precisely.

The causal effect interpretation of the resulting estimates rests on the common trend assumption. While this assumption cannot be tested directly, we can perform checks on its plausibility. To do so, we adjust eq. 2 in the spirit of a Granger causality test (Granger, 1969): We substitute the reform dummy by a set of indicators for the years preceding and following the reform. We discuss details of the Granger causality test in Section IV.C. We already anticipate that we find small and

school children in all-day schools correlates with the day care fee abolition (based on eq. 1, coefficient 0.05, *p*-value 0.14), but cannot find a significant or systematic relationship.

insignificant estimates on pre-reform periods for children in the final year of day care, providing plausibility for the common trend assumption.

The error term ϵ captures idiosyncratic variations. Inference is based on heteroscedasticity robust standard errors that allow for clustering of the error term at the state-year level (117 clusters). We show in the robustness section that our conclusions are robust to clustering standard errors at the state-birth cohort level or the state level (13 clusters). We account for the small number of state-clusters in our statistical inference performing wild cluster bootstrapping procedures (Cameron et al., 2008, see Section VI for details).

IV Results

A. Effects on day care expenses and day care use

We first characterise the distribution of pre-reform fees and the effect of the reform on private day care expenses (i.e. the first stage) using data from the SOEP. Figure 1 shows the distribution of day care expenses graphically. Before the reform, these expenses amount to about 100 euro per child per month, with a maximum of about 400 euro (i.e. in 2010-USD about 133 and 532 USD). The majority pays less than 200 euro. After the reform, the share of families reporting expenses below 25 euro increases substantially from below 20 percent to over 60 percent. Note that the information refers to all day care related expenses, not only fees: i.e. parental reporting may also include private contributions for meals or additional contributions that day care facilities may collect. However, this only refers to a small amount of the overall expenses (Schmitz et al., 2017).

Table 3 presents average monthly day care expenses in control states and the pre-reform expenses in treatment states (columns 1-2). Parents spent on average 76 and 92 euro per child, respectively, while half-day care is less expensive than full-day care. The abolition of day care fees reduces expenses substantially: The pre-post difference in treatment states amounts to 56 euro. Considering the changes in control states with a basic difference-in-differences model (see eq. 1), the change amounts to 65 euro (column 5). On average, expenses for a half-day slot decline by 56 euro per child, for a full-day slot by 87 euro. In relation to equivalent household income, expenses drop on average from seven to two percent.

We next investigate effects on the use of day care in administrative data and SOEP data, using the baseline model of eq. 1 (see Table 4). Panel A reports the findings in administrative data. We find

no effect on participation in day care (extensive margin, column 1), probably because this share is already close to one.[18] However, we find effects on the use of day care at the intensive margin (columns 2-5): Children aged six years spend about 0.7 hours more in day care per week. We estimate an increase in full-day care (35 hours or more per week) by 3.2 percentage points that is mirrored by a similar reduction in care for less than 25 hours per week. These findings suggest that day care fee abolitions cause some parents to shift from half-day care to full-day care.

Panel B reports the findings for families in the SOEP data. The advantage of the data is that we can clearly identify children in the final day care year, but the drawback is that the number of observations is much smaller. Again, we cannot find any change in day care attendance that is associated with the fee abolition reform (column 1). Also in the SOEP data, children are more likely to be in full-day care when they are affected by the fee abolition reform (column 3, the other outcomes are not captured by the SOEP questionnaire).

In sum, children affected by the fee abolition reform are no more likely to participate in day care, but they are more likely to be in full-day care.

B. Effects on maternal labour supply

Before we report the empirical estimation results on labour supply effects, we provide some descriptive graphical evidence. Figure 2 plots the maternal labour supply outcomes across children's age. Mothers of children in non-reform states and mothers in treatment states *before* the reform are part of the control group. Mothers of children affected by the reform are in the treatment group (i.e. younger children that *will be* treated in the final day care year and older children that were treated are also part of the treatment group).[19] While the share of mothers participating in the labour force increases, as expected, with children's age, the share of mothers with and without day care fees is almost identical before age five. Children aged five or six years are likely to be in their final day care year. By age seven, most children are already in school. Maternal labour force participation in the treatment sample is almost identical for children below age five. We see a very

[18] Note that the sample means for children aged six are lower than the day care attendance rates in official statistics that only report day care attendance rates up to age five (and below age six). The shares are calculated from the number of children in day care (divided by the full cohort size of children), but at age six about half of the children already entered primary school.

[19] The graphs are based on Stata's *marginsplot*, and are net off region- and child cohort-fixed effects.

small increase, if any, in maternal labour force participation at age five that vanishes at age six. Thereafter, the treatment group is statistically identical to the control group.

To better detect graphically any labour supply reactions of mothers at the intensive margin, we condition the sample on mothers in the labour force. The share of mothers' working is expectedly high and follows a similar age-trend before age five. For children aged five and six, we observe a small increase in the employment of the treatment group, which coincides with the final day care year for which fees were abolished. None of these differences are statistically significant. When children enter primary school, the employment shares of the treatment and control group intersect again. For working hours and full-time employment, we again observe similar age-patterns in employment before the final day care year and a substantial increase in the treatment group for the final year in day care.[20] This increase persists throughout primary school, but as the labour supply of mothers in the control group also increases at the intensive margin as their children grow older, the difference vanishes at around age nine. The first graphical results suggest that the abolition of day care fees mainly affects the intensive margin of maternal labour supply in the short run.

We now turn to the estimation of our empirical model. In Table 5, Panel A, we build up the empirical model from eq. 2 and, first, report the effects on mothers with children in the final day care year (estimates for mothers with younger and older children are in the model, but only reported for our preferred specification in Panel B). We start with a model without socio-economic and state-level controls (column 1), then sequentially add them to the model (columns 2-3). First, note that the estimated coefficients are very similar across these three specifications for the five outcome variables we consider. The estimates on maternal labour force participation and whether mothers work in the market are very small and insignificant. This corroborates the first graphical evidence that labour supply responses at the extensive margin are minimal.

The estimates on maternal working hours suggest an overall increase of about 0.4 hours per week (2.5 percent), a 1 percentage point (7.1 percent) increase in full-time employment (more than 30 hours per week), and no significant increase in the share of mothers working more than 10 hours

[20] The age-pattern in working hours and full-time employment result from conditioning the sample on mothers in the labour force. Working mothers with younger children, typically work at a higher intensity. This is a familiar pattern that is documented across countries (OECD, 2019). In Germany, 22 percent of coupled mothers of children below age three work 40 to 44 hours, while less than 11 percent with children aged three to five do so. This pattern is similar in Austria and Italy.

(i.e. a reduction in marginal employment). As most of the labour supply reactions occur at the intensive margin, we condition our sample on labour force participation in column 4. Maternal working hours increase by about 0.8 hours per week (3.7 percent), full-time employment by about 2.5 percentage points (11.4 percent), and the share of mothers working more than 10 hours increases by 1.1 percentage points (1.3 percent). Accordingly, the day care fee reform lowers the share of women in marginal employment by about 7.3 percent (1.1 percentage point decrease from a baseline share of 15 percent). These short-term effects on maternal working hours and full-time employment are plausible if compared to the estimated reform effects on the daily day care dosage as reported in Table 4: While children spend on average 2.2 percent more time in day care per week, mothers work on average 2.5 percent more hours/week. The share in full-day care increases by 7.2 percent, the share of mothers working full-time increases by 7.1 percent.

In Panel B, we present the estimates of our preferred specification for each of the four outcome variables considering mothers with children across all age groups. We report the coefficient estimates for mothers of children below age three, aged three to four, children in the last year of day care (where treatment occurs), primary school starters, and children further advanced in primary school. Note that the main estimate for mothers with children in their last day care year is identical to the coefficients in the last column of Panel A. Across outcomes, there is no labour supply reaction of mothers with children before their last year in day care. This first proposes balanced pre-trends and no anticipation effects of maternal labour supply to a cost reduction in day care in the final year. In the final year of day care, maternal working hours increase, along with the share of full-time employed mothers and the share of mothers working more than ten hours.

The effects on maternal working hours and full-time employment persist after children enter primary school, but vanish for children aged eight to ten years; the effects on marginal employment vanish immediately. For children aged eight to ten, one state (North Rhine-Westphalia) does not (yet) contribute to the treatment group, as the reform was only implemented in 2011, meaning that treated children are still too young to be captured by the data. Thus, the effects on maternal labour supply may vanish because the sample composition changes. However, the findings appear to be very similar after removing this state from the analysis (see robustness Section VI.B). Consequently, we conclude that a short-term abolition of day care fees creates short-term responses, but do not otherwise persist in longer-term.

C. Evolution of effects over time

In this section, we study how the effects evolve over time. In the spirit of a Granger causality test, we decompose the estimated effects in the last day care year into pre-reform and post-reform effects. This exercise is interesting for two reasons: First, we test the main identification assumption, i.e. for common pre-reform trends between treatment and control states. Second, we can better understand the lag between the day care fee reform and parental labour supply responses, identifying whether these effects persist for later cohorts as well, i.e. whether effects last.

For this purpose, we interact the *Reform* dummy in eq. 2 with dummies on the distance to the introduction of the fee reform, reporting the coefficients for children in the last day care year. The cohort preceding the reform is the baseline cohort to which we compare the estimates. We summarise the estimates in Figure 3. Across outcomes, we see balanced pre-trends as the estimated coefficients vary around zero. This supports the main identification assumption of variance weighted common trends (Goodman-Bacon, 2018).[21] About two to three years after the reform, maternal working hours and full-time employment grow to new levels. On average, mothers work one more hour per week and they are about 4 percentage points more likely to work full-time.

V Further Results

A. Effect heterogeneity

The day care fee abolition reform was a universal programme affecting all preschoolers in the treatment states. We now study heterogeneities in maternal labour supply reactions to the day care fee abolition to better understand whose labour supply is most elastic to day care cost changes. We interact the reform dummy of eq. 2 with dummies indicating the subgroups and include the baseline categories in the model (Table 6).

First, we find that single mothers react 2.5 times more with their working hours than mothers with cohabiting partners. This finding follows patterns in the previous literature in which single mothers

[21] Goodman-Bacon (2018) provides a theorem under which reform estimates of a two-way fixed effects model, i.e. a difference-in-differences model with multiple treatment states and reform periods, is a variance-weighted average of each possible before and after comparison between control units, treatment units, not-yet-treated units, and already treated units. If treatment effects vary over time, the DiD estimate may be biased. Event study approaches that decompose the effects are still unbiased. We provide further robustness checks in addition to this event study approach, as proposed by Goodman-Bacon (2018), in the sensitivity Section VI.

also react stronger to an increased *availability* of subsidised day care (e.g. Gelbach, 2002; Cascio, 2009; Fitzpatrick, 2012). In addition, in households where the father is not working, mothers react more strongly (single mothers are excluded from this analysis).

We then check for differences by maternal education. Mothers with higher secondary schooling degrees react stronger than mothers with lower levels of education. This may be because their labour market attachment before birth is stronger, such that any relaxation of constraints (here, a reduction in the opportunity costs of market work) leads to a stronger labour supply reaction. Further, more highly educated mothers may be more able to react to fee abolitions due to differences in their job characteristics. Harnisch, Müller, & Neumann (2018) show that underemployment (desired working hours exceed actual hours) is much less prevalent for highly educated women in Germany, also due to job characteristics. Using our data from the German Micro Census, we see that highly educated mothers work in larger companies (measured by the number of employees) and more often in the public sector. Both factors allow for more flexible adjustments in working hours (Zapf & Weber, 2017).

Next, we interact the reform dummy with an indicator for whether mothers are living in more rural or more urban areas (more than 60,000 inhabitants). Urban areas may provide better local labour market opportunities for short-term adjustments, day care centres may provide more flexible opening hours, and social norms may be more supportive of maternal full-time employment. We find that women in urban areas are significantly more responsive to the fee abolition reform.

We also check whether labour supply effects of mothers of pre-schoolers differ by the availability of all-day schooling in (subsequent) primary school. The rational behind this is the following: Forward-looking mothers may adjust their working hours more if day care is also granted as children enter primary school in the following year. In Germany, primary school typically lasts half-day, while all-day schools also offer afternoon care (and educational activities). Based on the current share of children in all-day schools and other forms of institutional care of primary school-aged children (e.g. *Horte*, data provided by Kultusministerkonferenz, 2011, 2015) at the federal state level, we estimate effects for availability above and below median. Maternal labour supply responses are stronger if the all-day primary school availability is above median (though this difference is statistically not significant). Finally, we estimate the effects separately by household income (which is partly endogenous, because we only observe the current household income

category). First, we find effects along the full distribution of household income, while the effects appear strongest for the lowest income group (household income below 60 percent of the median), as their share of household resources committed to day care costs is the largest for this income group (Schmitz et al., 2017). However, the effects on maternal labour supply are also relevant in higher income brackets, because fees typically increase with household income (though at a lower rate).

B. Paternal labour supply

Does paternal labour supply also react to changes in day care fees? The division of labour of families is still rather traditional in Germany, i.e. after childbirth, women often exit the labour force for some years or mostly work part-time (e.g. Lauber et al., 2014). As children grow older, maternal labour force participation increases gradually. Fathers, in contrast, maintain high levels of full-time employment throughout. We estimate the effects of the fee abolition reform on fathers' labour supply and summarise the main findings in Figure 4. As expected, there is little variation in paternal labour supply by children's age and no differences emerge following the abolition of day care fees. Overall, we conclude that paternal labour supply is unresponsive to the abolition. This result is similar to other studies analysing the link between paternal labour supply and childcare (e.g. Andresen & Havnes, 2018; Gambaro, Marcus, & Peter, 2019).

VI Robustness Checks

A. Potential confounders

The causal interpretation of the estimates relies on the common trend assumption, for which we provide several plausibility checks. Still, we need to assume that no other treatments coincide with the fee abolition reform. We are most concerned with two potential threats regarding the availability of day care, which we discuss in the following.

First, the period under investigation saw a substantial expansion of publicly subsidised day care for children younger than three (e.g. Spiess, 2011). In Appendix Figure A.3, Panel A, we plot the increasing share of children in day care during our observation period across states. This expansion is similar across states that abolished fees and in states that did not. It increases at a higher pace after 2010 in treatment and control states alike. For children aged three or older, treatment states show a lower share of children in day care in 2006 and a catch-up until 2013. We investigate

whether this catch-up is related to the fee abolition reform using the two-way fixed effects model specified in eq. 1. Appendix Table B.3 reports the findings. As shown in column 1, the change in day care attendance for children below age three and aged three to four cannot be attributed to the fee abolition reform.

Recall that for children in their final year of day care (aged six years, Table 4), we cannot find any change in day care attendance at the extensive margin that is associated with the fee abolition reform. We conclude that the general day care expansion and the availability of new day care slots was a universal trend not associated with the fee abolition reform. We also control for the share of children below age three in the main analysis of parental labour supply: the coefficients barely change. This reassures that changes are orthogonal to the abolition of fees.

Our second concern regards the expansion of full-day care for children from age three onward, which came along with two day care expansion laws in 2004 and 2008 (e.g. Schober & Stahl, 2016). For our analysis, we need to rule out that the increase in the share of children in full-day care moderates the effects on maternal labour supply rather than the fee abolition reform. In Appendix Figure A.4, we plot the change in full-day care between 2006 and 2013 for children of different ages for states with and without fee abolition reforms. In Panel A, we see that the full-day care change for children aged three is highly predictive for the change of full-day care of children aged four in treatment and control states alike. We again employ our difference-in-differences model from eq. 1 to check whether changes at the intensive margin of day care are general trends or whether they might be related to the timing of the fee reform across states. In Appendix Table B.3 columns 2-5, we see, for children below age three and aged three to four that the reform dummy is not associated with longer day care hours or the use of full-day care.

When we group children aged three to four and compare their full-day change to children aged five to six (the target group of the fee abolition reform), we find that the increase at age five to six is somewhat stronger in treatment states (Appendix Figure A.4, Panel B). As the expansion in lower ages is highly predictive for the expansion in higher ages and as the fee reform does not correlate with full-day use in lower ages, we attribute increases in the full-day use of children aged five to six to the fee abolition reform.

B. Specification choices

In this section, we assess the sensitivity of our findings to several empirical decisions. We provide these checks for the sample of mothers in the labour force, for whom we derive our main findings, and for all mothers. These checks are reported in Appendix Tables B.4 and B.5.

First, we assess the impact of the first treatment cohort for our findings: The fees were typically abolished for day care years starting in August. As we do not observe the exact interview date, individuals interviewed early in the year are falsely assigned to treatment, even though only the following cohort will be exempted from fees. The graphical evidence in Figure 3 shows that the maternal labour supply reaction can be observed two years after the reform. In the main analysis, we include an indicator for the first cohort. If we exclude the cohort from the sample, the results are almost identical.

We now assess the sensitivity of findings to the inclusion of certain states. Goodman-Bacon (2018) suggests that estimates from difference-in-differences models with multiple treatment states and reform periods are a variance-weighted average of each possible before and after comparison between control units, treatment units, not yet-treated units, and already treated units. One implication that can be drawn from this theorem is that unbiased effect estimates require the variance-weighted common trend assumption to hold. While our main results pass several plausibility checks for common trends (e.g. event studies), it is worth assessing the sensitivity of the findings to changes in the treatment and control groups. First, we drop East German states from the analysis, who serve mainly as control states (out of the five East German states in the sample, only Berlin abolished day care fees). While maternal labour force participation and the availability of day care below age three, is substantially higher in East Germany, the results on fee reform effects are similar to the main findings.

Goodman-Bacon (2018) also proposes to remove all never-changing states from the sample in a robustness check. Note that this robustness check cuts the sample size into half. Identification is now entirely based on states with earlier and later fee abolitions. Recall from the event study in Figure 3 that effects set in about two years after treatment but remain fairly stable over time. This lagged reaction may induce some downward bias when the counterfactual for later treatment states is drawn from earlier treatment states. Still, we find the same pattern in the effects for mothers in the labour force (although the estimates are, as expected, smaller and less precise).

Next, we alter the sample to include states that abolished fees, but then reintroduced them shortly after. With a lag in the labour supply responses of mothers, we would expect that adding these states to the sample would slightly lower reform effect estimates. This is indeed what is observed, but the conclusions are the same. We also remove the three states that abolished fees for younger children in subsequent years from the treatment group; our findings are the same.

Finally, we adjust the error term structure that we assume for inference. While we account for a clustered structure of standard errors at the state-year level in the main analysis (117 clusters), we alternatively cluster standard errors at the state-child year of birth level (91 clusters) and at the state level (13 clusters). Further, we calculate *p*-values based on wild-cluster bootstrapping procedures to account for the small number of clusters. As recommended for a small number of clusters, we use Webb weights with a uniform 6-point distribution to reduce the discreteness of p-values (Webb, 2013).[22] Our findings are robust to these adjustments.

VII Discussion and Conclusion

We provide novel evidence that even small private day care contributions prevent mothers from working longer hours, even if public day care systems are already highly subsidised and widely used. We analyse a fee abolition reform in Germany that reduced day care fees by an average of 65 euro per month. We analyse universal day care fee reductions in an environment with near-universal day care enrolment, which has advantages compared to other studies that can only estimate intention-to-treat effects of more targeted programmes. By also considering the dynamics of labour supply reactions, a rigorous set of heterogeneity analyses, and paternal labour supply next to maternal labour supply, we are able to draw a more comprehensive picture of the effects of lower day care costs on parental labour supply in a universal day care system.

We find that children affected by the reform spend on average 0.7 hours (2.2 percent) per week more in day care. They are 3.2 percentage points (7.2 percent) more likely to attend full-day care. Maternal working hours increase on average by 0.4 hours per week (2.5 percent), full-time employment by 1 percentage point (7.1 percent). Single mothers, mothers without other younger children, highly educated mothers, and mothers in more urban areas are most responsive. The

[22] We use the Stata command *boottest* as proposed by Roodman, MacKinnon, Nielsen, & Webb (2018) and test under H_0 as recommended.

effects persist as children enter primary school, but vanish when children are about four years older because the labour supply of non-treated mothers catches up. Fathers do not respond to fee abolitions.

The effects presented in this paper are larger than the zero-effects of a day care price cut studied by Lundin et al. (2008). However, the day care costs they study change in an environment with high maternal labour supply and a particularly high rate of full-time employment. Studies from North America (e.g. Fitzpatrick, 2012; Cascio, 2009, Baker et al., 2008; Gelbach, 2002) find substantially larger effects, which is mainly explained by the treatment intensity: These studies mainly analyse the change from offering little or no subsidised care in a non-universal day care systems toward a more comprehensive universal publicly funded day care system. Compared to expansions of subsidised day care (e.g. Bauernschuster & Schlotter, 2015; Müller & Wrohlich, 2018 for Germany), fee abolitions have a small effect on parental labour supply. Our results confirm that the effects of childcare costs on maternal labour supply are context-dependent.

The day care fee abolition may only increase parental employment if the supply side of day care can react flexibly to increased demand for day care at the extensive margin (new day care slots for children previously not in day care) or at the intensive margin (increase in daily childcare hours). Our study shows reactions at the intensive margin: the hours children spend in day care increase. The general expansion of day care in Germany generated increased demand for day care teachers in a context of day care teacher shortages (Autorengruppe Fachkräftebarometer, 2014), which may have constrained more flexible responses to an increased demand. Moreover, given the shortages of day care teachers, the fee abolitions may also have lowered day care quality, which may deter some parents from prolonging day care hours. The effects might be somewhat different in another policy environment. With a higher supply-side elasticity, we would expect even larger effects of day care fee abolitions.

Why do we not find effects at the extensive margin? Removing private contributions to day care lowers the reservation wage, thus the incentive to take up work should increase. With already low private contributions before the reform, participation in day care was near universal; still, maternal labour force participation was only at 68 percent. This suggests that reservation wage

considerations may be dominated by social norms on maternal employment or high opportunity costs for mothers if they had to increase the time away from their children.[23]

With respect to the *effectiveness* of the policy, we still conclude that maternal labour supply responses are remarkable at the intensive margin - given the small size of the treatment and the already large amount of day care subsidies. But what about the *efficiency* of the reform if policy makers aim at raising parental labour supply with day care fee abolitions? The reform increases public expenditures, but the increase in maternal employment also generates additional tax revenues. In the context of limited public resources, cost-benefit considerations allow for assessing the efficiency of the reform. For this assessment, we compare the average drop in day care fees of about 65 euro per month to the increase in tax revenues resulting from increased employment. We estimate an increase in full-time employment of about 1 percentage point in the full population. The difference between females' full-time and part-time annual pre-government income amounts to 17,000 euro (Statistisches Bundesamt, 2014), i.e. we estimate that average annual maternal gross income increases by 170 euro on average. We find that the increase in employment lasts for up to four years, such that the total increase in gross income sums to 680 Euros. If we assume an average taxation and social security rate for this additional income of about 30 percent, government revenues increase by 204 euro. These public benefits are compared to an average increase in public day care expenses of 780 euro for one year (65 euro per month). Note that this back-of-the-envelope calculation of additional day care expenses is rather a lower bound estimate, as the use of full-day care also increases, which further increases public expenditures. The cost-benefit analysis suggests that the government refinances at most 30 percent of its expenses through increased tax revenues from maternal employment.[24] The major efficiency loss occurs because of substantial windfall gains: Many families do not change their labour supply in response to day care fee abolitions. Almost all preschool children attend day care, but 32 percent of mothers are out of the labour force and do not enter as fees are abolished. Still, they benefit from an increase in disposable family income. In the group of employed mothers, the substantial effect heterogeneity

[23] Theoretically, the income effect of day care fee reductions could dominate the substitution effect, such that labour supply effects could also be negative. In none of the heterogeneity checks do we find any evidence for this case and we are also not aware of any other study providing according empirical evidence.

[24] Note that we only consider monetary dimensions. The reform could also affect other dimensions that are not taken into account in this cost-benefit consideration, such as family well-being (e.g. Baker et al., 2008; Barnett, 2011; Felfe & Zierow, 2014; Loeb, Bridges, Bassok, Fuller, & Rumberger, 2007). Furthermore, fiscal multiplier effects are not considered.

suggests additional, substantial windfall gains. If day care cost reductions mainly aim at incentivising parental labour supply, tax credits on childcare expenses - which link childcare subsidies to labour supply - might be more efficient than universal day care fee abolitions.

Our findings are highly policy relevant. Many countries have increased subsidised childcare substantially in an attempt to support mothers to work. Policy-makers in many countries acknowledge the negative long-term consequences of childbirth on women's earnings, promotion chances, and even pensions. Countries continue to increase public childcare subsidies, either through an increased supply of subsidised care or through private fee reductions as can currently be observed in Japan, the UK, and further federal states in Germany. With limited public resources, it is critical to identify *effective and efficient* policy tools that support mothers in the labour market. In sum, we conclude that the abolition of day care fees is an effective tool that increases full-time maternal employment. However, the windfall gains of a fee abolition reform are very large, such that abolishing day care fees cannot be considered as an efficient policy tool to increase maternal labour supply.

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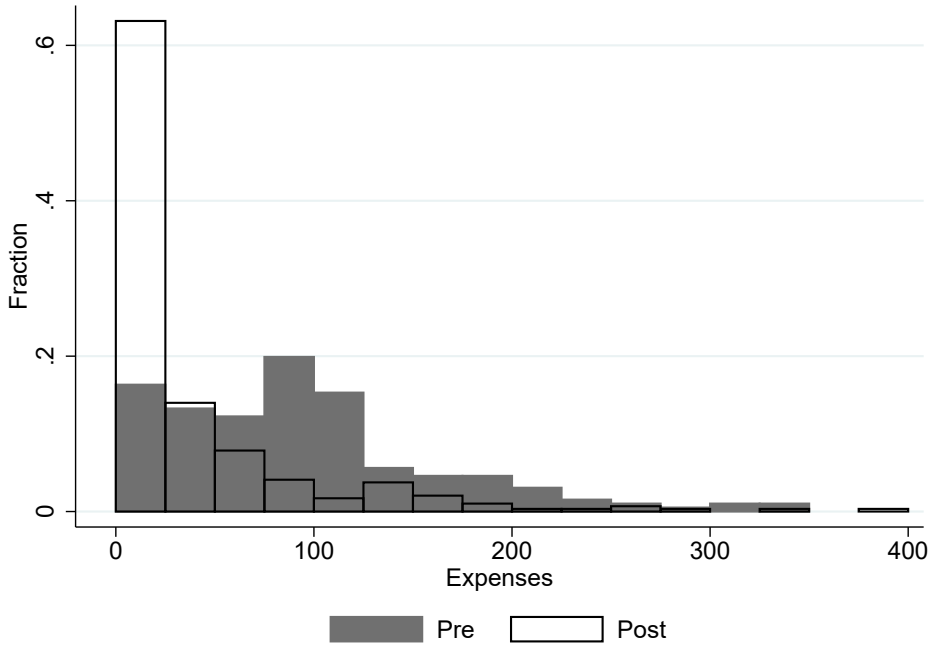
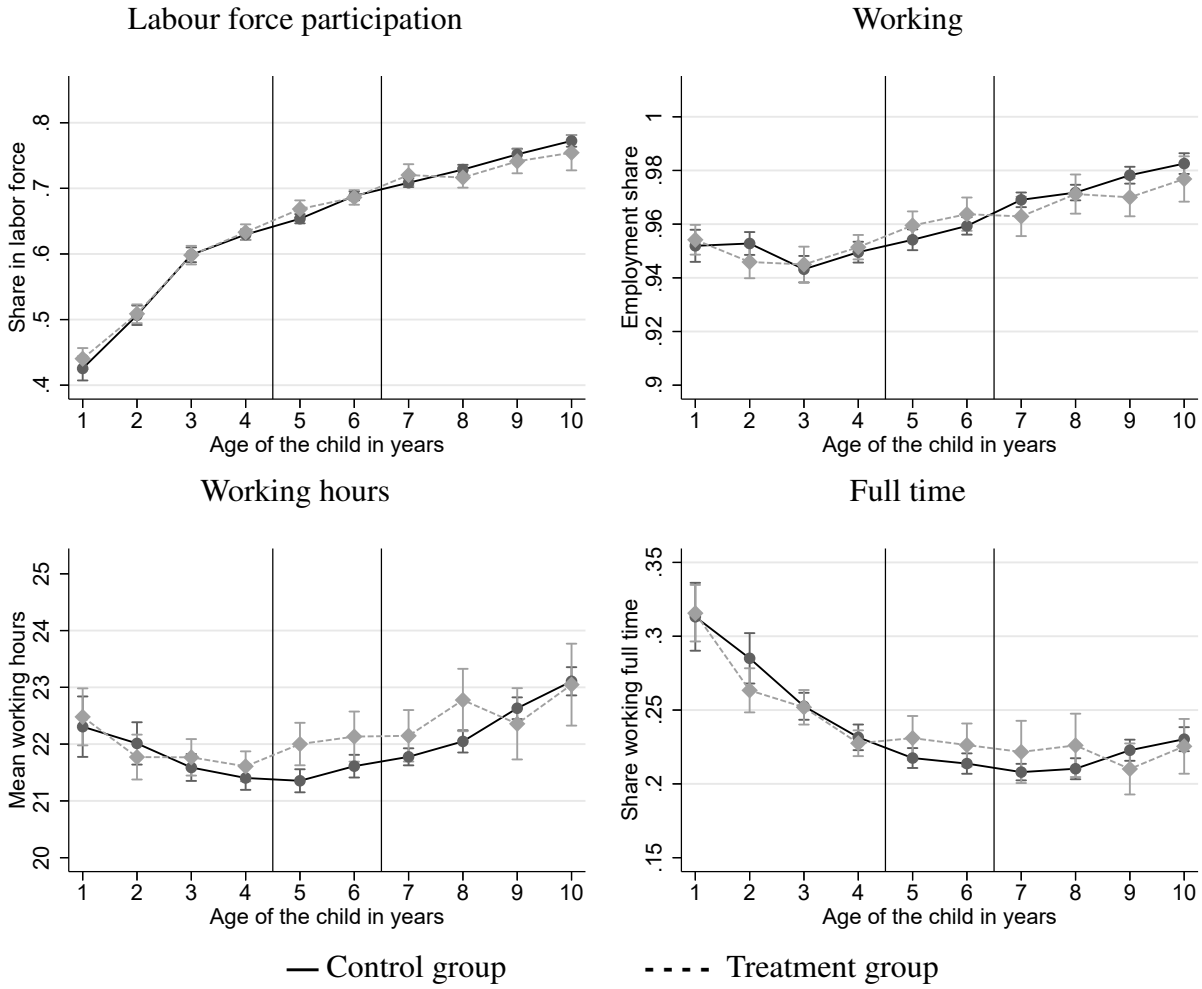
Figures

Figure 1. Private expenses for children in the final day care year



Notes: The histogram plots the distribution of private day care expenses in euro (2010-prices) in the final year of day care for treatment states given in Table B.1 before and after the day care fee abolition reform. Values larger than zero after the fee abolition can be attributed to private expenses for meals, extra childcare center activities, or reporting bias. Moreover it may be attributed to expenses if day care is used beyond the day care hours for which fees were abolished.
Source: Own calculations based on SOEP v33 (2005, 2009, 2013).

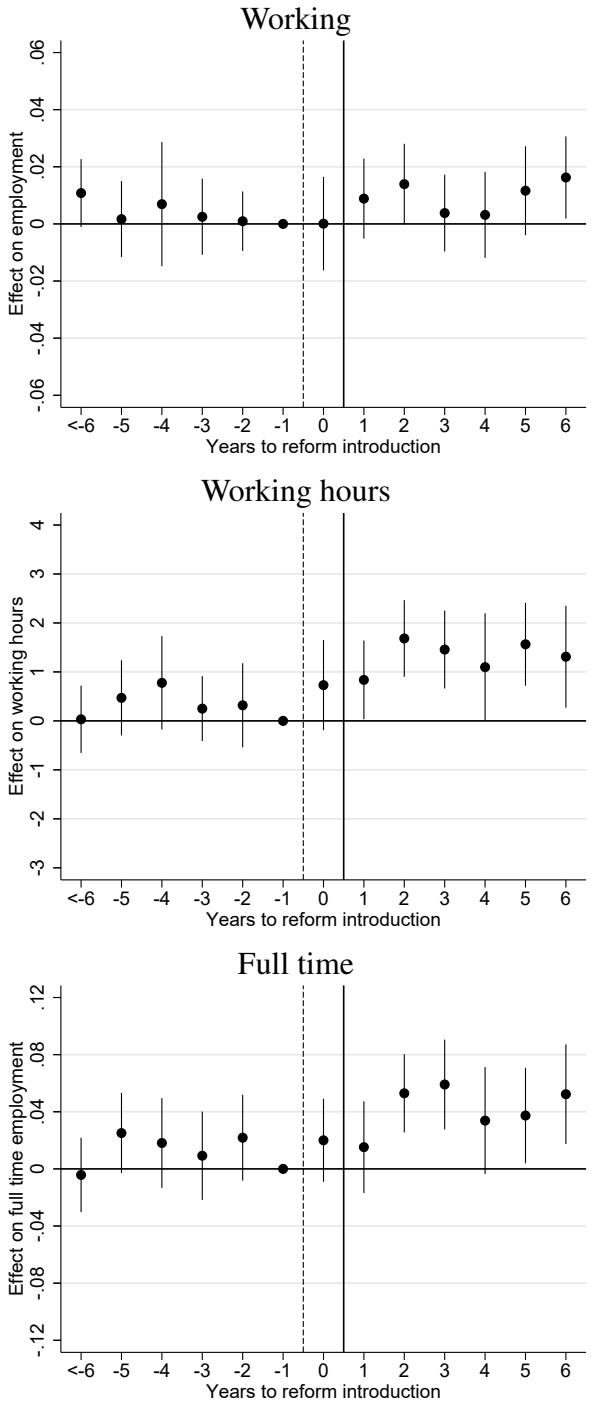
Figure 2. Maternal labour market outcomes by child age



Notes: The figure plots maternal labour market outcomes by the age of the child. The marginsplots are net of region and child cohort fixed effects. Working, working hours, and full time are conditional on labour force participation. The treatment group comprises mothers with children who have been treated with free day care in the last day care year and those who will be treated as they become older. The control group comprises mothers of children in control states and non-treated cohorts in treatment states. The black vertical bars enclose the age range in which the vast majority of families in treatment states receive free day care in the last day care year.

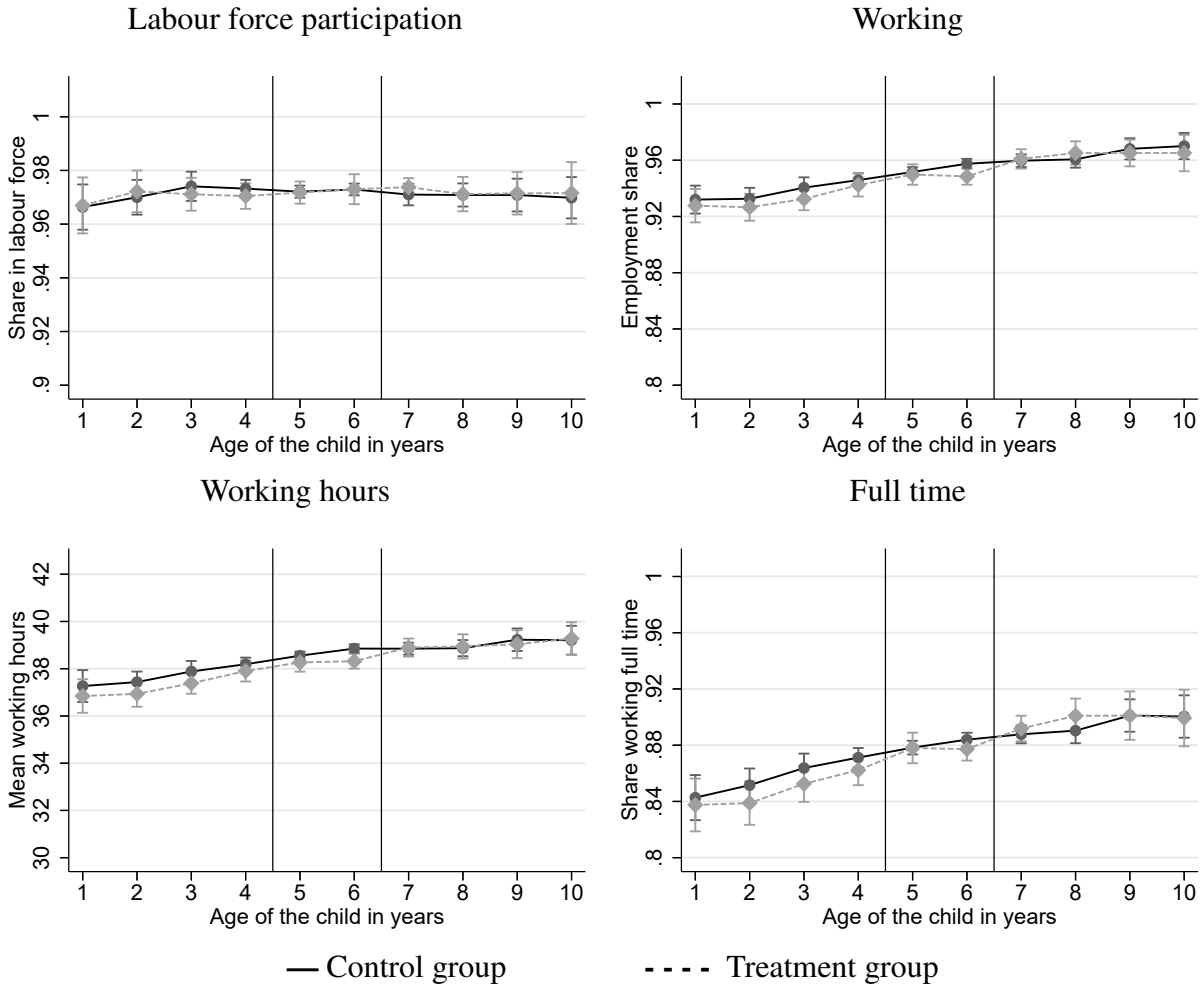
Source: RDC (2019), own illustration based on German Micro Census (2005-2013).

Figure 3. Event study of maternal labour supply responses to the abolition of day care fees



Notes: The figure plots coefficient estimates from an interaction of the reform with indicators on the time difference to the reform. The black vertical bar indicates the year after which day care is free of charge in treatment states in the final year of day care. The dashed line indicates the initial introduction, but due to data limitations with respect to birthday and interview day information, treatment assignment in year 0 is fuzzy.
Source: RDC (2019), own illustration based on German Micro Census (2005-2013).

Figure 4. Paternal labour market outcomes by child age



Notes: The figure plots paternal labour market outcomes by the age of the child. The marginsplots are net of region and child cohort fixed effects. Working, working hours, and full time are conditional on labour force participation. The treatment group comprises all children which have already been treated with free day care in the last day care year and those who will be treated as they become older. The control group comprises fathers of children in control states and non-treated cohorts in treatment states. The black vertical bars enclose the age range in which the vast majority of families in treatment states receive free day care in the last day care year.
Source: RDC (2019), own illustration based on German Micro Census (2005-2013).

Tables

Table 1: Descriptive statistics

	Sample			
	All mothers		Mothers in the labour force	
	Sample mean	s.d.	Sample mean	s.d.
Outcomes of maternal labour supply				
In labour force (D)	0.63	(0.48)	1.00	(0.00)
Working (D)	0.58	(0.49)	0.96	(0.19)
Working hours/week	13.45	(14.75)	22.56	(12.70)
Works full time (D)	0.15	(0.36)	0.25	(0.44)
Works more than 10 hours/week (D)	0.51	(0.50)	0.86	(0.35)
Children's characteristics				
Age in years	5.17	(3.15)	5.62	(3.05)
Female (D)	0.49	(0.50)	0.49	(0.50)
Year of birth	2003.28	(4.12)	2002.97	(4.08)
Maternal characteristics				
Age in years	35.22	(6.08)	36.29	(5.66)
Migration background (D)	0.17	(0.38)	0.11	(0.31)
Lower secondary schooling (D)	0.29	(0.46)	0.21	(0.41)
Middle secondary schooling (D)	0.38	(0.48)	0.41	(0.49)
Upper secondary schooling (D)	0.33	(0.47)	0.38	(0.49)
Household characteristics				
Partner living in household	0.86	(0.34)	0.88	(0.33)
Household net income in Euro	3256.07	(2081.40)	3638.34	(2168.80)
Institutional characteristics				
Share eligible for free final day care year (D)	0.27	(0.44)	0.24	(0.43)
Cohort share in day care below age 3	0.12	(0.12)	0.12	(0.13)
Cohort share in all-day schooling (primary school)	0.15	(0.19)	0.15	(0.19)
State maternal labour force participation	0.70	(0.06)	0.70	(0.06)
District population size (in 10,000)	14.06	(17.92)	13.01	(17.48)
Urban area (pop. larger than 60,000, D)	0.59	(0.49)	0.56	(0.50)
Number of observations	328,299		192,792	

Notes: The table provides descriptive statistics. Dummy variables are indicated with *D*. Standard deviations are reported in parentheses.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).

Table 2: Balancing of individual characteristics

	Sample			
	All mothers		Mothers in the labour force	
	$\beta_{lastyear}$	s.e.	$\beta_{lastyear}$	s.e.
Children's characteristics				
Children's age in years	0.005	(0.005)	0.001	(0.006)
Child is female	-0.000	(0.006)	-0.007	(0.008)
Maternal characteristics				
Age in years	-0.042	(0.066)	0.126	(0.079)
Migration background	0.006	(0.005)	0.001	(0.005)
Lower secondary schooling	-0.002	(0.006)	-0.007	(0.006)
Middle secondary schooling	-0.006	(0.006)	-0.010	(0.008)
Higher secondary schooling	0.008	(0.006)	0.017**	(0.007)
Household characteristics				
Partner living in household	0.001	(0.004)	0.005	(0.005)
Household net income in Euro	12.943	(25.283)	40.477	(33.889)
	F-stat	*p-value*	*F-stat*	*p-value*
Test for joint orthogonality	0.90	0.53	1.33	0.23

Notes: The table reports effect estimates of a fee abolition in the final day care year on child and family characteristics. The results are based on OLS regressions of model 2. The test for joint orthogonality of the child and family characteristics is based on the specification in eq. 2. The treatment indicator is moved to the left-hand side. An F-test tests for the joint significance of the socio-economic characteristics (*right-hand-side balancing test*, as described in, e.g., Pei, Pischke, and Schwandt, 2019; Bruhn and McKenzie, 2009). Standard errors are clustered at the state $\times$ year level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).

Table 3: Changes in day care expenses for children in the final day care year

	Control states	Treatment states			
Dependent variable	Mean (1)	Pre-reform mean (2)	Post-reform mean (3)	Difference col. (3)-(2) (4)	DiD (5)
Day care expenses in Euro	76.29	92.08	35.51	-56.57***	-64.52***
				(10.096)	(6.662)
Expenses for half-day care	67.76	83.48	30.05	-53.429***	-56.212***
				(10.841)	(7.796)
Expenses for full-day care	93.22	114.92	43.05	-71.867***	-86.865***
				(17.821)	(14.395)
Share of day care expenses on monthly household net income	2.81	3.49	1.14	-2.35**	-1.78***
				(0.411)	(0.344)
Share of day care expenses on equiv. monthly net household income	5.82	7.19	2.42	-4.77***	-3.63***
				(0.850)	(0.709)
Number of observations	403	184	281	465	868

Notes: Columns (1)-(3) report day care expenses in control states (Mecklenburg-Vorpommern, Brandenburg, Sachsen-Anhalt, Thüringen, Bayern, Bremen, Baden-Württemberg) and treatment states (Hamburg, Niedersachsen, Nordrhein-Westfalen, Hessen, Rheinland-Pfalz) for children in the final year of day care. Estimates in column (4) result from a regression of the dependent variable on a post-dummy. Estimates in column (5) result from a simple Difference-in-Differences (DiD) model, i.e. a regression with treatment and control states, year and state fixed effects. Note that the estimates should be interpreted as lower-bound effects on costs if day care use intensity increases with day care subsidies. Standard errors are clustered at the state $\times$ year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on SOEP v33 (2005, 2009, 2013)

Table 4: Effects day care fee abolitions on day care use

	Dependent variable:				
		conditional on day care use			
	In day care (1)	Weekly hours in day care (2)	≥35 hours/week (3)	25-35 hours/week (4)	less than 25 hours/week (5)
Panel A: Administrative data					
Children aged 6 years					
Last year in day care free	0.009	0.723***	0.032**	0.010	-0.043**
	(0.022)	(0.200)	(0.015)	(0.026)	(0.017)
Sample mean	0.48	33.93	0.44	0.36	0.21
Number of state-year-age cells	104	104	104	104	104
Number of observations	5,271,194	5,271,194	5,271,194	5,271,194	5,271,194
Panel B: SOEP					
Children in last year of day care					
Last year in day care free	-0.014	—	0.059*	—	—
	(0.015)		(0.032)		
Sample mean	0.95		0.34		
Number of observations	3,269		3,121		

Notes: The table reports estimates from a regression of the dependent variable on the treatment indicator and year- and state-fixed effects. The share of 6-year old children in day care is calculated from the number of children in day care (divided by the full cohort size of children). Official statistics only report day care attendance rates up to age five (and below age six), which are at about 96 percent. At age six, the sample mean is 0.48 because about half of the children already entered primary school. Standard errors are clustered at the state × year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on Kinder- und Jugendhilfestatistik (2006-2013, Panel A), SOEP v33 (2005-2013, Panel B).

Table 5: Main results - Effect of day care fee abolitions on maternal labour supply

Panel A: Developing the empirical model

	Estimated treatment effect in last day care year of ...					
	All mothers				Mothers in labour force	
Dependent variable	Mean (last year)	Basic model (1)	+ socio-econ. controls (X) (2)	+ state-level controls (S) (3)	Mean (last year)	Full model (4)
Mother in labour force	0.67	0.006	0.006	0.004	1.00	—
		(0.006)	(0.007)	(0.006)		
Mother works	0.60	0.004	0.005	0.001	0.96	0.005
		(0.007)	(0.007)	(0.007)		(0.003)
Mother's working hours	13.50	0.406**	0.469**	0.342*	21.78	0.807***
		(0.188)	(0.190)	(0.196)		(0.195)
Mother works full time	0.14	0.010**	0.012***	0.010**	0.22	0.025***
		(0.004)	(0.004)	(0.004)		(0.007)
Mother works $\geq$ 10 hours	0.53	0.006	0.007	0.004	0.85	0.011**
		(0.007)	(0.007)	(0.007)		(0.005)
Number of observations	328,299				192,792	

Panel B: Event study across child age

	Dep. variable for mothers in the labour force:			
Independent variables	Mother works (1)	Mother's working hours (2)	Mother works full time (3)	Mother works $\geq$ 10 hours (4)
Pre-treatment				
Reform $\times$ below age 3	-0.003	0.534	0.019	0.002
	(0.003)	(0.353)	(0.014)	(0.006)
Reform $\times$ 3-4	0.001	0.321	0.005	0.007
	(0.003)	(0.196)	(0.007)	(0.005)
Treatment				
Reform $\times$ Last year (free)	0.005	0.807***	0.025***	0.011**
	(0.003)	(0.195)	(0.007)	(0.005)
Post-treatment				
Reform $\times$ school starter	-0.001	0.643***	0.022**	0.000
	(0.004)	(0.226)	(0.009)	(0.006)
Reform $\times$ primary school	-0.002	0.474	0.012	0.000
	(0.003)	(0.352)	(0.012)	(0.007)

Notes: The table reports treatment effect estimates on maternal labour market outcomes of a fee abolition in the final day care year at different ages of the child. The results are based on OLS regressions of model 2. The sample includes all mothers of children age 0-10 years who are participating in the labour force and not on social benefits. Standard errors are clustered at the state $\times$ year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).

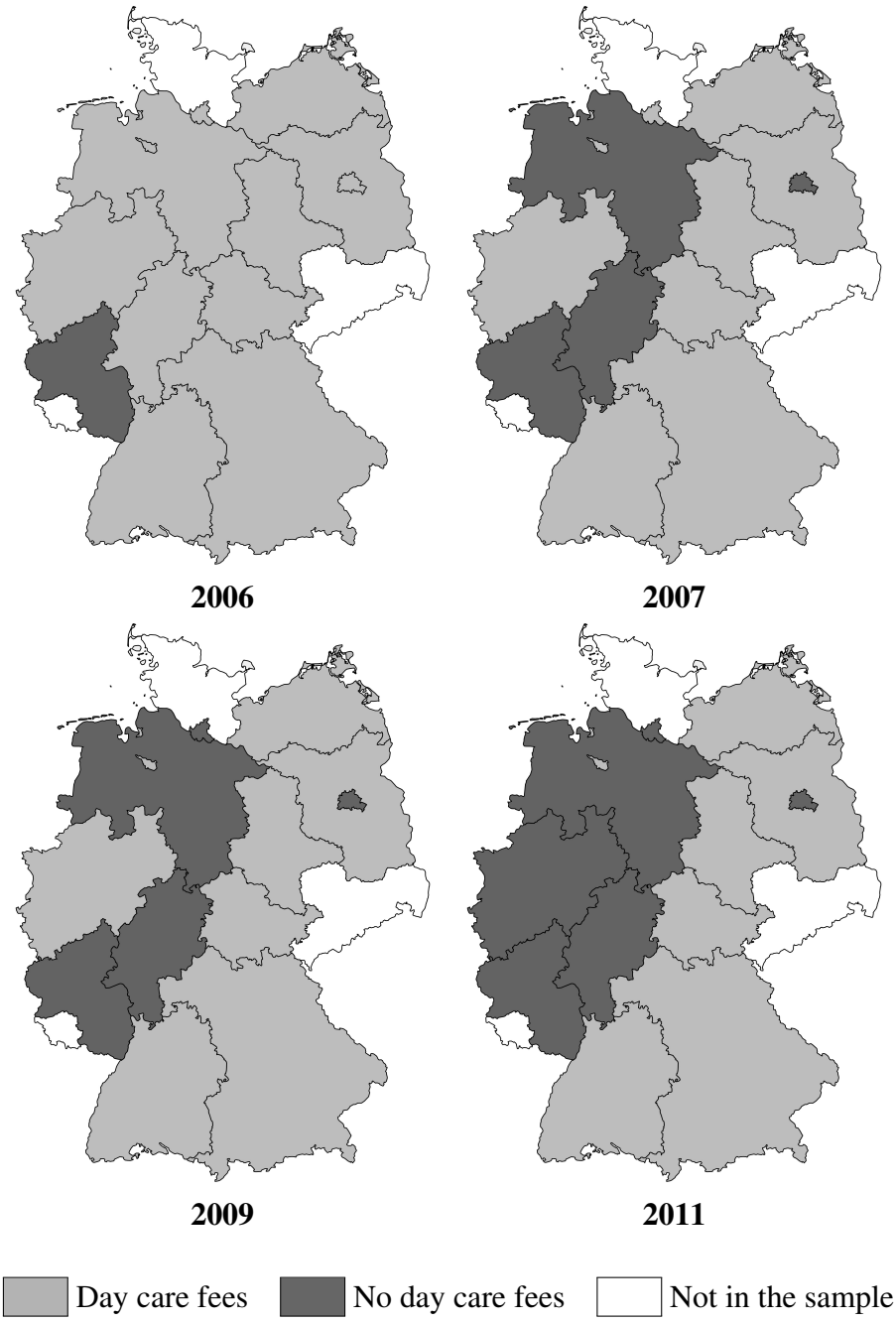
Table 6: Effect heterogeneity

	Dep. variable:			
	Mother works	Mother's working hours	Mother works full time	Mother works $\geq$ 10 hours
	when child is in the last day care year			
Independent variable: Treatment × ...	(1)	(2)	(3)	(4)
Single mothers	0.004	1.606***	0.053***	0.008
	(0.007)	(0.440)	(0.017)	(0.009)
Cohabiting mothers	0.005	0.682***	0.020**	0.011**
	(0.003)	(0.214)	(0.008)	(0.006)
p-value coefficient difference	0.877	0.069	0.080	0.747
Father is not working	0.008	1.534***	0.054***	0.007
	(0.006)	(0.396)	(0.015)	(0.009)
Father works	0.004	0.600***	0.017**	0.011*
	(0.003)	(0.217)	(0.008)	(0.006)
p-value coefficient difference	0.561	0.040	0.027	0.715
Mothers with higher secondary schooling	0.005	0.907***	0.026***	0.013**
	(0.003)	(0.212)	(0.008)	(0.005)
Mothers with lower secondary schooling	0.007	0.357	0.017	-0.000
	(0.006)	(0.378)	(0.014)	(0.011)
p-value coefficient difference	0.695	0.179	0.492	0.275
Mothers without children below age 3	0.007*	0.960***	0.030***	0.012**
	(0.004)	(0.208)	(0.008)	(0.005)
Mothers with children below age 3	-0.005	-0.071	-0.009	0.007
	(0.005)	(0.456)	(0.015)	(0.010)
p-value coefficient difference	0.066	0.035	0.013	0.618
Living in more rural area	0.007	0.073	0.006	0.007
	(0.005)	(0.198)	(0.009)	(0.008)
Living in more urban area	0.005	1.157***	0.033***	0.013**
	(0.003)	(0.242)	(0.009)	(0.005)
p-value coefficient difference	0.617	0.000	0.009	0.398
All-day primary school below median	0.008*	0.652***	0.020**	0.007
	(0.004)	(0.218)	(0.008)	(0.006)
All-day primary school above median	0.002	1.030***	0.032***	0.015**
	(0.003)	(0.286)	(0.011)	(0.006)
p-value coefficient difference	0.178	0.213	0.277	0.294
HH income below 60% of median	0.008	1.584***	0.063***	0.003
	(0.010)	(0.422)	(0.014)	(0.014)
HH income 60%- 100% of median	0.015***	0.674**	0.018**	0.018**
	(0.005)	(0.299)	(0.009)	(0.009)
HH income below 100%-150% of median	0.001	0.717**	0.015	0.008
	(0.004)	(0.279)	(0.010)	(0.008)
HH income above 150% of median	-0.001	0.900***	0.032***	0.012**
	(0.004)	(0.295)	(0.011)	(0.006)

Notes: The table reports treatment effect estimates on maternal labour market outcomes of a fee abolition in the final day care year at different ages of the child. The results are based on OLS regressions of model 2. The reform indicator is interacted with dummies for different characteristics (baseline dummy included in the model). Standard errors are clustered at the state × year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).

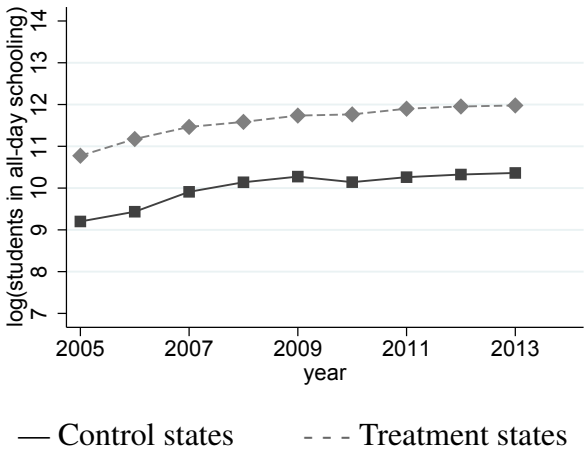
Appendix

Figure A.1. Timing of day care fee abolition reform across federal states



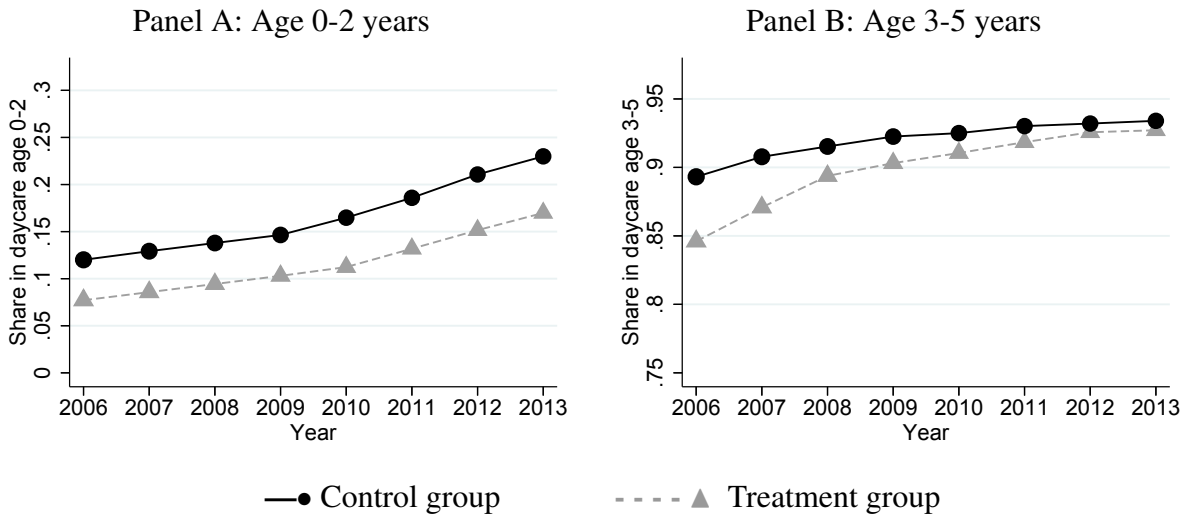
Notes: The figure plots the variation in timing of day care fee abolition in the final year of day care across federal states. States excluded from the main analysis abolished day care fees and re-introduced them shortly after (Schleswig-Holstein, Saxony and Saarland). These states are included in a robustness check.
Source: Own illustration.

Figure A.2. Change in all-day schooling in primary school (2005-2013)



Notes: The figure plots the log number of students in primary school between 2005 and 2013 for states that abolished day care fees (treatment states), and states that did not (control states). The upward trend in treatment and control states stems from a national programme encouraging states to expand all-day primary school offers.
Source: Own illustration based on Kultusministerkonferenz (2011, 2015).

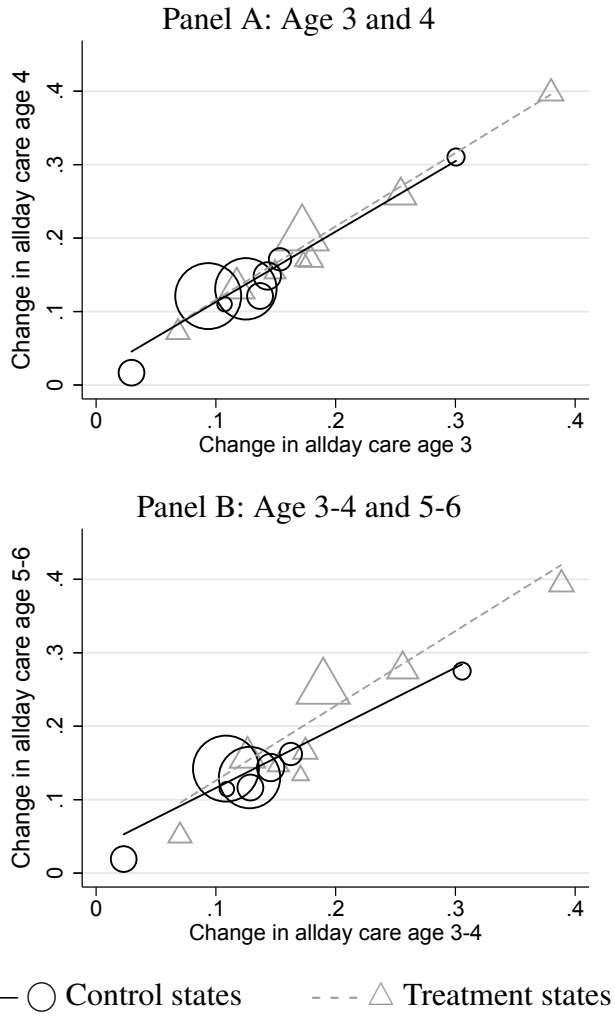
Figure A.3. Share in day care age 0-2 and 3-5 years



Notes: The figure plots the evolution of the share of children in day care for the ages 0-2 years (Panel A) and 3-5 years (Panel B).

Source: Own illustration based on Kinder- und Jugendhilfestatistik (2006-2013).

Figure A.4. Change in full-day childcare usage (2006-2013)



Notes: The figure plots the change in full-day childcare use between 2006 and 2013 for states that abolished day care fees (treatment states), and states that did not (control states). The size of the markers represents the size of the federal state, the lines represent state-size weighted linear fits. While there is a national expansion in full-day care use for children from age 3 onward, the increase in the final year of day care (age 5-6) is larger in treatment states. We show in Table 4 that this larger increase can be attributed to the day care fee abolition reform.
Source: Own illustration based on Kinder- und Jugendhilfestatistik (2006-2013).

Table B.1: Day care fee regulations

Federal state	Fee administration level	Fees dependent characteristics	Day care fee abolition	First cohort born in	Time covered by fee abolition
Treatment states					
Berlin	Federal state	Income	**Jan. 07: last year**	2002	Full-day
		Children in household	Jan. 10: last two years		Full-day
		Hours of care	Jan. 11: last three years		Full-day
Hamburg	Federal state	Income, Family size, Hours of care	**Sep. 09: last three years**	2004	Five hours
Hesse	Municipalities	Income, Family size, Hours of care, Age of child	**Aug. 07: last year**	2002	Five hours
North Rhine-Westphalia	Youth welfare office	Income Children in household	**Aug. 11: last year**	2006	Full-day
Rhineland-Palatinate	Provider	Income	**Jan. 06: last year**	2001	Full-day
		Children in household	Sep. 07: last two years		Full-day
			Sep. 08: last three years		Full-day
			Sep. 09: last four years		Full-day
			Aug. 10: last five years		Full-day
Lower Saxony	Municipalities	Income	**Aug. 07: last year**	2002	Full-day
Control states					
Baden-Württemberg	Provider	Children in household Hours of care	-		
Bavaria	Provider	Hours of care			
Brandenburg	Provider	Income Children in household Hours of care	-		
Bremen	City-wide	Income Children/people in household Hours of care	-		
Mecklenburg-Vorpommern	Provider and municipalities	not specified	-		
Saxony-Anhalt	Municipalities	Hours in care Children in childcare	-		
Thuringia	Municipalities	Income Hours in care	-		
Not incl. in the main analysis					
Saarland	Provider	Income Children in household	up to Jul. 2011: last year		Full-day
Saxony	Municipalities	Age Children in childcare	Mar. 2009 - Dez. 2010: last year		Full-day
Schleswig-Holstein	Provider	Income Children in household	Aug. 2009 - Jul. 2010: last year		Full-day

Notes: The table summarises the day care fee regulations across federal states between 2005 and 2013. *Fee administration level* refers to the governmental level responsible for setting day care fees. *Day care fee abolitions* in bold refer to the last day care year.
Source: Information is based on Deutscher Bundestag Wissenschaftliche Dienste (2016), Bertelsmann Stiftung (2017), Ministerium für Bildung, Jugend und Sport des Landes Brandenburg (2013), and Schmitz, Spieß, and Stahl (2017).

Table B.2: Comparison of event study results to separate regressions

	Coefficient on *Reform* × *last day care year*			
	Sample			
	All mothers		Mothers in the labour force	
Dep. variable	Combined (main)	Separate regressions	Combined (main)	Separate regressions
Mother in labour force	0.004	0.013*	—	—
	(0.005)	(0.008)		
Mother works	0.001	0.000	0.005	0.007
	(0.007)	(0.010)	(0.003)	(0.006)
Mother working hours	0.342*	0.177	0.807***	0.681*
	(0.196)	(0.274)	(0.195)	(0.373)
Mother works full-time	0.010**	0.009	0.025***	0.023*
	(0.004)	(0.007)	(0.007)	(0.013)
Number of observations	328,299	46,605	192,792	28,408

Notes: The table reports estimates on the effects of a day care fee abolition on maternal labour market outcomes for mothers with children in the last year of day care. The *combined* model refers to our main specification as specified in eq. 2, which considers all mothers of children up to age 10. *Separate regressions* refers to a model that estimates eq. 2 on a sample of children in the last year of day care (i.e. a = [age 5-7, in school]). Standard errors are clustered at the state × year level and are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: RDC (2019), own calculations based on based on German Micro Census (2005-2013).

Table B.3: Changes in day care use

	Dependent variable:				
		conditional on day care use			
	In day care (1)	Weekly hours in day care (2)	≥35 hours/week (3)	25-35 hours/week (4)	less than 25 hours/week (5)
Placebo: Children aged 0-2 years (fees will only be abolished in final day care year around age 5-6)					
Last year in day care free	0.014	-0.093	-0.009	0.012	-0.003
	(0.025)	(0.447)	(0.025)	(0.012)	(0.020)
Sample mean	0.26	35.48	0.55	0.25	0.19
Number of state-year-age cells	312	312	312	312	312
Number of observations	14,873,086	14,873,086	14,873,086	14,873,086	14,873,086
Placebo: Children aged 3-4 years (fees will only be abolished in final day care year around age 5-6)					
Last year in day care free	0.012	0.224	0.014	-0.009	-0.006
	(0.010)	(0.198)	(0.010)	(0.011)	(0.013)
Sample mean	0.90	33.52	0.42	0.34	0.23
Number of state-year-age cells	208	208	208	208	208

Notes: The table reports estimates from a regression of the dependent variable on the treatment indicator and year- and state-fixed effects. Regressions also include children's age-in-years fixed effects. Standard errors are clustered at the state × year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations based on Kinder- und Jugendhilfestatistik (2006-2013).

Table B.4: Robustness checks, mothers in the labour force

	Dep. variable:					
	Mother in labour force	Mother works	Mother's actual working hours	Mother's normal working hours	Mother works full time	Mother works $\geq$ 10 hours
Without first affected cohort (N=185,065)	—	0.005*	0.810**	0.832***	0.026***	0.012**
		(0.003)	(0.321)	(0.206)	(0.008)	(0.005)
Without East Germany (N=165,134)	—	0.007*	0.627**	0.597***	0.017**	0.009
		(0.003)	(0.313)	(0.221)	(0.008)	(0.006)
Without never changers (N=96,486)	—	0.007*	0.561	0.548*	0.016	0.005
		(0.004)	(0.403)	(0.291)	(0.010)	(0.006)
Including states with re-introduced fees (N=214,906)	—	0.005*	0.620**	0.644***	0.018***	0.011**
		(0.003)	(0.270)	(0.176)	(0.007)	(0.004)
Excluding states without fees for younger children (N=163,553)	—	0.004	0.478**	0.628***	0.019***	0.010*
		(0.003)	(0.237)	(0.172)	(0.007)	(0.005)
Excluding NRW(N=144,369)	—	0.006*	0.519**	0.702***	0.020***	0.013**
		(0.004)	(0.243)	(0.204)	(0.007)	(0.006)
Excluding self-employed(N=166,613)	—	0.005*	0.360	0.620***	0.017**	0.009*
		(0.003)	(0.228)	(0.182)	(0.007)	(0.005)
Clustering at state-birth year level (N=192,792)	—	0.005*	0.805**	0.807***	0.025***	0.011*
		(0.003)	(0.312)	(0.243)	(0.009)	(0.006)
Clustering at state level (N=192,792)	—	0.005*	0.805***	0.807***	0.025***	0.011***
		(0.002)	(0.231)	(0.169)	(0.007)	(0.003)
p-value for clustering at state level		0.067	0.004	0.000	0.003	0.003
p-value for wild cluster bootstrap		0.186	0.104	0.012	0.005	0.001

Notes: The table reports OLS regression results of the $\beta_{lastyear}$ coefficient from eq. 2. Standard errors are clustered at the state $\times$ year level, if not stated differently. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).

Table B.5: Robustness checks, full sample

	Dep. variable:					
	Mother in labour force	Mother works	Mother's actual working hours	Mother's normal working hours	Mother works full time	Mother works $\geq$ 10 hours
Without first affected cohort (N=314,393)	0.005	0.002	0.525***	0.361*	0.010**	0.005
	(0.007)	(0.007)	(0.189)	(0.194)	(0.004)	(0.007)
Without East Germany (N=261,673)	0.011	0.015**	0.647***	0.533**	0.009*	0.015**
	(0.008)	(0.007)	(0.200)	(0.209)	(0.005)	(0.007)
Without never changers (N=174,446)	0.003	0.006	0.487**	0.335	0.006	0.007
	(0.008)	(0.008)	(0.215)	(0.246)	(0.006)	(0.008)
Including states with re-introduced fees (N=326,263)	0.004	0.007	0.561***	0.430**	0.010**	0.010*
	(0.006)	(0.006)	(0.192)	(0.168)	(0.004)	(0.005)
Excluding states without fees for younger children (N=244,946)	0.009	0.011**	0.496***	0.526***	0.012***	0.013**
	(0.006)	(0.006)	(0.168)	(0.160)	(0.004)	(0.006)
Excluding NRW (N=210,398)	0.013*	0.016**	0.576***	0.673***	0.014***	0.018***
	(0.006)	(0.006)	(0.189)	(0.191)	(0.005)	(0.007)
Excluding self-employed (N=256,762)	0.012**	0.015***	0.446***	0.559***	0.010**	0.015***
	(0.006)	(0.005)	(0.158)	(0.156)	(0.004)	(0.005)
Clustering at state-birth year level (N=328,299)	0.006	0.004	0.579***	0.406**	0.010*	0.006
	(0.006)	(0.007)	(0.215)	(0.191)	(0.006)	(0.007)
Clustering at state level (N=328,299)	0.006	0.004	0.579***	0.406**	0.010**	0.006
	(0.006)	(0.008)	(0.134)	(0.163)	(0.005)	(0.007)
Wild cluster bootstrap (N=328,299)	0.334	0.651	0.001	0.028	0.04)	0.410
P-value wild cluster bootstrap	0.4665	0.7728	0.0160	0.0430	0.0260	0.6346

Notes: The table reports OLS regression results of the $\beta_{lastyear}$ coefficient from eq. 2. Standard errors are clustered at the state $\times$ year level, if not stated differently. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2019), own calculations based on German Micro Census (2005-2013).