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Parental Labour Supply Responses to the Abolition of Day Care Fees

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Abstract

This paper provides evidence that low private contributions to highly subsidised day care constrain mothers from working longer hours. We study the effects of reforms that abolished day care fees in Germany on parental labour supply. The reforms 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 reforms increase 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 most strongly. 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.

Keywords: Labour Supply, Childcare Costs, Family Policy, Event Study
JEL: J13, J22, J38

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I Introduction

Many developed economies have experienced substantial increases in female labour force participation since the 1970s (e.g. OECD, 2019). A major driver for this improvement is the increased availability of subsidised day care (e.g. Müller & Wrohlich, 2019). Still, the proportion of mothers working full-time is comparably low, with severe long-term negative consequences for female careers and pensions (e.g. Fasang, Aisenbrey, & Schömann, 2012; Manning & Petrongolo, 2008). Research provides evidence that changes in social norms contribute to gradual increases in female working hours, especially of mothers (e.g. Maurin & Moschion, 2009; Nicoletti, Salvanes, & Tominey, 2018). Yet, could policy-makers further help women increase their working hours by reducing day care costs further in systems that subsidise day care heavily already? This question is at the heart of this study.

A large literature shows 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] However, 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 series of reforms that abolished day care fees in Germany, a country with a relatively high rate of part-time working mothers (OECD, 2019). The reforms abolished private contributions to day care in a highly subsidised setting for preschoolers, i.e. for children in the year before entering primary school. Day care participation for these children was already near universal. The reforms were 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 with parental labour supply in states without a fee abolition reform. We also use the fact that the reforms affected only preschoolers. We estimate effects for all parents with children up to the age of ten. This event study type approach across children's ages 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

[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 the 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).

main analysis 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 month (in 2010, equivalent to 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 all-day childcare, i.e. a higher share of all-day schooling (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 the effects of *removing* private contributions to day care in an already highly subsidised system. Most of the existing literature studies the effects of *introducing* highly subsidised care. The context, initial maternal labour supply, and available amount of subsidised day care are relevant factors in the magnitude of the 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; Brewer et al., 2016), but our study is one of the first on day care fee *abolitions*. We find that policies can still support women in increasing their working intensity.

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 estimate day care fee effects on labour supply net of childcare availability constraints as almost all children already participate in day care. It also reduces concerns related to the selection and the quality of care relative to outside options. We can also perform numerous heterogeneity analyses to better understand who responds the most to day care fee abolitions.[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 reforms on paternal labour supply, a dimension rarely considered in the previous literature.

[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 decisions. Another strand of the literature employs structural models. For example, Müller, Spiess, & Wrohlich (2013) and Wrohlich (2004) show that without the already available subsidies, maternal labour supply would be substantially lower.

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

Finally, we 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 of 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 proportion of mothers working by 14.5 percent, while day care enrolment increased 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 of working 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 three-year-olds in Spain. Maternal employment increased by 10 percent. Carta & Rizzica (2019) analyse a reform extending access to subsidised day care to two-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 all-day care increases the probability of mothers of entering the labour force by 5 percentage points.[4]

[4] Another strand of the literature studies expansions of publicly subsidised day care (for Norway, see, e.g., Havnes & Mogstad, 2011, and for Germany, see, e.g., Bauernschuster & Schlotter, 2015; Müller & Wrohlich, 2018), as well as expansions in day care hours (e.g. Lefebvre & Merrigan, 2008; Dhuey, Lamontagne, & Zhang, 2019; Müller & Wrohlich, 2020). Neither strand is very informative for the debate on day care fees.

In contrast to these studies, we study the 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 of day care in Sweden that cut private costs by 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 the first evidence of the effects of day care fee abolitions in 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. We assess the efficiency of the day care fee abolition reforms in Section VI, and perform several robustness checks in Section VII. We discuss the findings and conclude in Section VIII.

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 proportion 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 the 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-hour 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. Since 2000, enrolment has been almost universal for children above the age of three. Below age three, the proportion of 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 proportion 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 through federal law. The actual implementation of it is 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 the structure of day care fees should take into account household income, the number of children, and the number of hours spent in day care. The majority of states do 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.

Because states and municipalities regulate day care fees, fees are usually not an indication 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).[5] There is also no overall national accreditation system, such as 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 reforms, on average, 75-80 percent of the costs of non-profit providers were 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. Nonetheless, day care does require 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).

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.[6] Moreover, it was argued that day care offers education and, thus, should be free, just like school education. Fees were mainly abolished to relieve families from further costs related to children. Arguments relating to maternal employment were barely raised in the public discussion of day care fee abolitions. Two city-states, Berlin and Hamburg, along with four larger states – Hesse, Lower Saxony, North-Rhine Westphalia and Rhineland-Palatinate – with both rural and urban populations were the first states to abolish

[5] 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).

[6] 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.

fees for preschoolers. These states are part of the treatment group in our analysis.[7] Three other states adopted free day care and then subsequently re-imposed 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.[8] Adopting states are concentrated in West Germany. After 2013, more German states reduced or abolished day care fees or announced plans for such reforms in the coming years (BMFSFJ, 2019).[9] The timing of the reforms does not systematically relate to maternal employment levels (Appendix Figure A.2).[10] The reforms were introduced in states with day care quality (as proxied by the child-teacher ratio) similar to West German control states (Jugendinstitut, 2008). However, their quality is slightly higher than in the East German control states. Appendix Figure A.3 shows that there is no systematic relation between the timing of fee abolitions and day care quality.

Once children are enrolled in day care, only very few children change the day care centre, especially towards the final years of day care. Thus, a change of the day care centre due to state-wide fee abolitions and for only one year prior to school entry is highly unlikely. If parents wanted to increase the day care hours in response to the day care fee abolitions, they would typically approach the management of the day care centre to find out whether it had sufficient capacity. This is typically agreed upon if the requested day care extension is small, or if only a few parents request longer day care hours.

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 a few questions are answered on a voluntary basis. The dataset is particularly well-suited to our analysis because it contains rich information on household

[7] Berlin and Rhineland-Palatinate also abolished day care fees for younger children in later years. In Section III.C, we describe how we deal with this issue empirically.

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

[9] In Thuringia and Brandenburg, for example, parents of children in their last year of day care are exempted from day care fees since 2018. Furthermore, several East German states reduced and abolished fees further as part of a large federal programme that provides about 5.5 billion euro of federal funding through 2022 for quality improvements in day care and for fee reductions (*KiTa-Qualitäts- und -Teilhabeverbesserungsgesetz – KiQuTG*).

[10] The later adoption, together with further fee reductions following the federal funding programme makes it unlikely that East Germany did not implement the reform because female labour supply was already high and, therefore, the policy was not needed there to be true.

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.[11] 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 effects. Because our data is cross-sectional, we assume that school-aged children went to day care in the same state in which they are now living. This assumption appears reasonable, as mobility across states in Germany is low. Less than 3 percent of all children move to a different state before entering primary school; less than 7 percent move before they have turned 10.[12] One concern is whether families move toward a treatment state in response to the treatment. This seems unlikely because the costs of permanently moving to a different state are high compared with the benefit of saving the relatively low day care fees for one year. Still, we analyse how many of the children in a treatment state who are not yet enrolled in school actually moved there from a control state. We find that before and after the abolition of day care fees, this proportion is constant at 1.5 percent.

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).[13] Overall, 63 percent are working, with 13.5 average weekly working hours (conditional on labour force participation 22.6 percent), 15 percent work full-time (conditional 25 percent), and 51 percent (conditional 86 percent) work more than 10 hours per week.

[11] 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.
[12] Own calculations and analyses based on SOEP data (see Section III.B for details).
[13] 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.

B. Supplementary data: SOEP and official statistics

While the German Micro Census provides a very large sample by which to analyse the 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).

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 the 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 compare the before-after comparisons in reform states with 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, while the results are also robust to clustering standard errors at the state level using wild cluster bootstrap procedures (Cameron et al., 2008, see Section VII for details).

We use the parsimonious model in equation (1) to estimate reform effects on day care expenses of parents and the use of day care in data from the SOEP and in official statistics.[14] The causal interpretation of the β reform estimates is based on the common trend assumption and requires no simultaneous co-treatments. One of our key concerns for the identification of fee abolition effects relates to the expansion of subsidised day care *availability*. Its relevance to maternal labour supply is well-established in the literature (see, e.g. for Germany, Müller & Wrohlich, 2020). We carefully address two potential threats to our identification in the Robustness Section VII: first, the substantial expansion of publicly subsidised day care for children below the age of three (e.g. Spiess, 2011); second, the expansion of all-day care for all children from age three onward, along with two day care expansion laws from 2004 and 2008.[15] 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 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.

For our analysis of parental labour supply that we observe independent of children's age, we can also take into account the treatment variation across children's ages. The treatment only affects children in the final year of day care in certain cohorts, while children in earlier cohorts and younger children are not (yet) treated.[16] 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 with general changes of parental labour supply in non-reform states. Estimating fee abolition effects across children's ages has at least two advantages. First, we trace the pre-treatment evolution of effects and can assess whether co-treatments on younger children drive the effects

[14] The variables included in the *X*-vector vary between the SOEP-based individual level analysis and the aggregated administrative data. The control variables are specified in the table notes.
[15] *Tagesbetreuungsausbaugesetz*, 2005, *Kinderförderungsgesetz*, 2008 (see, e.g., Schober & Spiess; 2013; Schober & Stahl, 2016).
[16] Two other states removed day care fees for younger children in subsequent 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 two states from the sample (see sensitivity checks in Section VII.B).

in the final year of day care. For example, if the expansion of subsidised day care for children below the age of three coincides with day care fee abolitions in the last year of day care, we would expect to detect effects for children below the age of three. However, if effects set in in the final year of day care, we can rule out that related day care reforms that also affect children of younger ages coincide with the fee abolition reform. A second advantage is that we can learn about the dynamics of the effects on maternal labour supply as children grow older and enter school.

Therefore, we extend the difference-in-differences model from equation (1) and estimate regression models of the following form:

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

We now consider labour supply outcome *y* of parent *i* with a child of age *a*, born in cohort *c*, and residing in state *s*. 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) to account for any shocks or changes over time between birth cohorts of children that are common across regions, such as changes in economic conditions, or federal law changes in family support. We account for regional differences with a set of regional 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.

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, the gender of the child and the year in which the household was surveyed. 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 regional fixed effects. It includes the female labour force participation rate[17], the coverage rate of children in day care below age

[17] 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. a bad control variable). Note that

three, the proportion of primary school children in all-day schooling[18], and a variable that accounts for years of free day care for some individuals in states that expanded the fee-free final year to include earlier years. As we observe only 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 VII.B).

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 have already entered school at this age, so we assign children aged five to seven 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 identification strategy does not rely on comparisons of children across these age groups. The grouping just clarifies the treatment assignment but also increases the precision of the estimates. One concern could be that parents delay school entry in response to day care fee abolitions, thereby endogenously selecting the sample. We find no support for endogenous selection by estimating the model in equation (1) for all children aged five and six on an indicator of being enrolled in school (age five: coefficient estimate 0.0008, p-value 0.828; age six: coefficient estimate 0.0016, p-value 0.929).

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 we would expect, the separate estimations that are equivalent to the model fully

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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.

[18] In contrast to the day care expansion and the increase in 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.4, 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 school children in all-day schools correlates with the day care fee abolition (based on equation 1, coefficient estimate 0.05, *p*-value 0.14) but cannot find a significant or systematic relationship.

interacted with children's age group are estimated less precisely. Note that the model does *not* rely on comparisons between older and younger children for the estimation of treatment effects as one could do it in a triple-differences setting. This, however, would require one to assume *a priori* that one age group was not affected (which is probably not plausible due to possible anticipation effects on younger children and lasting treatment effects on older children). Instead, we estimate the effects on children of *each* age group to also learn about anticipation effects and treatment effect dynamics.

The causal effect interpretation of the resulting estimates rests on the assumption that the reform indicator is orthogonal to the error term ϵ conditional on the covariates. In our difference-in-differences setting, it assumes a common trend between treatment and control observations had the treatment not occurred. While this assumption cannot be tested directly, we can perform checks on its plausibility. To do so, we adjust equation (2) in the spirit of a Granger causality test (Granger, 1969): we substitute the reform dummy with 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 will find small and insignificant estimates on pre-reform periods for children in the final year of day care, supporting the common trend assumption. Another way to assess the randomness of the reform indicator is to compare treatment and control observations in terms of their socio-economic characteristics. We estimate the model in equation (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 is 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. Note, however, that we also control for maternal education, in addition to other socio-economic characteristics, in our main specification.

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) because our treatment occurs at the federal state level and our data is sampled from a randomly drawn cross-section every year. 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 VII 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, about 133 USD and 532 USD, respectively). The majority pay less than 200 euro. After the reform, the proportion 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; 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 all-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 equation 1), the change amounts to 65 euro (column 5). On average, expenses for a half-day slot decline by 56 euro per child, and for an all-day slot by 87 euro. In relation to equivalent household income, expenses drop on average from seven to two percent.

We next investigate the effects on the use of day care in administrative data and SOEP data, using the baseline model of equation 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.[19] However, we find effects on the use

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

of day care at the intensive margin (columns 2-5): children aged six spend about 0.7 hours more in day care per week. We estimate an increase in all-day care (35 hours or more per week) of 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 all-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 all-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 all-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 ages. 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).[20] While the proportion of mothers participating in the labour force increases, as expected, with children's age, the proportion 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 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 proportion of mothers working is expectedly high and follows a similar age profile until their children reach the age of five. For

[20] The graphs are based on Stata's *marginsplot* and are net of regional and child cohort fixed effects.

mothers of 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 is statistically significant. When children enter primary school, the employment proportions 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.[21] 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 equation (2) and first report the effects on mothers with children in the final day care year.[22] We start with a model without socio-economic and state-level controls (column 1), then sequentially add them to the model (columns 2-3). 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 proportion of mothers working more than 10 hours (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 proportion of mothers working more than 10 hours by 1.1 percentage points (1.3 percent). Accordingly, the day care fee reform lowers the proportion of women in marginal employment by about 7.3 percent (a 1.1-percentage-point decrease from a baseline share of 15 percent). These short-term effects

[21] The age pattern in working hours and full-time employment results 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.

[22] Estimates for mothers with younger and older children are in the model but only reported for our preferred specification in Panel B.

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 per week. The proportion of all-day care increases by 7.2 percent, and the proportion of mothers working full-time increases by 7.1 percent.[23]

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 suggests balanced pre-trends and no anticipation effects on maternal labour supply of a cost reduction in day care in the final year.[24] In the final year of day care, maternal working hours increase, along with the proportion of full-time employed mothers and the proportion 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. Most likely, this can be justified by an employment catch-up of non-treated mothers as children grow older (see Fig, 2). Alternatively, effects on maternal labour supply may vanish because the sample composition changes. 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. However, our findings appear to be very similar after removing this state from the analysis (see Section VII.B). Consequently, we conclude that a short-term abolition

[23] We also estimate these effects for an alternative specification of full-time employment defined as working more than 35 hours per week, which corresponds to our definition of all-day care. This reduces the effect to 6.4 percent (0.7 percentage points).

[24] The absence of anticipation effects in maternal employment could relate to higher day care costs for younger children that typically increase with household income and hours of care. Thereby, opportunity costs of working longer hours increase before the last day care year. Research also suggests that mothers of young children discount the future more strongly. Some studies provide evidence of hyperbolic discounting of mothers (Schneider, 2019). Accordingly, the marginal utility that mothers draw from spending more time with their children before the last year in day care, may outweigh the marginal utility drawn from (strongly) discounted career and monetary benefits of working minus the increased current fees.

of day care fees creates short-term responses, which do not otherwise persist in the longer term as non-treated mothers catch-up as their children grow older.

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 the effects last.

For this purpose, we interact the *Reform* dummy in equation (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).[25] 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 equation (2) with dummies indicating the subgroups and include the baseline categories in the model (Table 6).

[25] 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 Section VII.

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 also react more strongly 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. This pattern aligns with findings in the literature of a higher labour supply elasticity of women to shocks on their spouses' labour supply, especially if young children live in the household (e.g. Halla, Schmieder & Weber, 2020).[26]

We then check for differences by maternal education. Mothers with higher levels of secondary schooling react more strongly than mothers with lower levels of education. This may be because their labour market attachment before giving birth is stronger, so 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 on mothers of pre-schoolers differ by the availability of all-day schooling in (subsequent) primary school. The rationale 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 proportion of children in all-day schools and other forms of institutional care of primary school-aged children (e.g. *Horte*, data provided by

[26] Single mothers are excluded from this analysis.

Kultusministerkonferenz, 2011, 2015) at the federal state level, we estimate the effects for availability above and below the median. Maternal labour supply responses are stronger if the all-day primary school availability is above the median (though this difference is statistically not significant).

Finally, we estimate the effects separately by household income (which is partly endogenous because we observe only the current household income category). First, we find effects along the full distribution of household income. 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 in families is still fairly 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 Cost-benefit considerations

Day care fee abolitions increase public expenditures, but they also cause increases in maternal employment that generate additional fiscal revenues. In the context of limited public resources, we now turn to cost-benefit considerations that allow assessing the *efficiency* of the reform.

Our cost-benefit considerations are described in detail in Appendix C.[27] The analysis is based on detailed information on gross and net earnings of mothers using SOEP data.[28] Our estimated average responses to day care fee abolitions suggest that mothers in the labour force increase their working hours by 0.78 hours, the share working full-time increases by 2.3 percentage points. These effects could result from (a) most mothers adjusting their working hours rather marginally, or from (b) few mothers adjusting their working hours substantially, while other mothers react much less (or scenarios in between). Both scenarios could lead to different cost-benefit results due to specificities of the German tax and transfer system (such as progressivity of taxes or ”spousal splitting”). We therefore consider both scenarios.

In the first scenario of a *minimum reaction* of mothers, we first calculate working hours increases for mothers switching to full-time employment (2.3 percent), and the other mothers such that the weighted average is a 0.78 hours response to day care fee abolitions. Based on hourly wages, we calculate the related annual increase in gross earnings. Based on the *fiscal share*[29] in maternal earnings, we then calculate the resulting annual fiscal benefit for both groups, full-time shifters and other mothers. Based on their group sizes of 2.3 and 97.7 percent, we calculate the weighted average annual fiscal benefit. Assuming that the effects last for up to four years (compare Figure 2), we estimate that mothers in the labour force contribute a total discounted annual fiscal benefit of 632 euros. While only mothers in the labour force, i.e. 66 percent contribute to the additional fiscal revenues, day care fee abolitions apply to all families and amount to total fiscal costs of 774 euro. Overall, in the *minimum reaction scenario*, we estimate that the fee abolition reforms are financed by about 54 percent through increased fiscal revenues resulting from maternal employment increases (see Figure 5 and Appendix C for details).

In the second *maximum reaction* scenario, we assume that the 2.3 percent of full-time shifting mothers adjust their working hours substantially, from average part-time employment to average full-time employment, i.e. by 19 hours per week. We assume that the other mothers increase weekly working hours by 0.35, such that the weighted average effect on working hours

[27] We focus on monetary dimensions, abstracting from potential reform effects on other non-monetary dimensions that could be affected by changes in maternal employment or changes in day care expenditures, such as family well-being (e.g. Baker et al., 2008; Barnett, 2011; Felfe & Zierow, 2014; Loeb, Bridges, Bassok, Fuller, & Rumberger, 2007) or child development (Busse & Gathmann, 2018).

[28] We analyse information on mothers with 5-9 year old children between 2005 and 2013 to mimic the sample composition of our main analysis using the Micro Census. The Micro Census does not contain the required earnings information.

[29] The fiscal share covers taxes and social security contributions. In the German context they all have to be considered as fiscal gains as the state, comprising for instance, the old age pension funds, benefits.

is again 0.78. Because of much larger increases in working hours, the fiscal benefit is now much larger for this group at 4,170 euro. As the group is only small, the weighted total fiscal benefit over 4 years from mothers in the labour force increases to 640 euro as compared to 632 euro in the *minimum reaction* scenario. Given the same share participating in the labour force, and the same total fiscal costs, we estimate a fiscal benefit-cost ratio of about 55 percent in the *maximum reaction* scenario.

Our analysis also reveals substantial heterogeneities in maternal labour supply responses by maternal cohabitation status, maternal education, and between less and more urbanised areas. These subgroups may differ in their hourly earnings and family constellations that could lead to substantial variations in the fiscal share in their earnings, and the total fiscal benefit. To characterize the cost-efficiency for these subgroups, we conduct the same analyses for each of these groups based on the group-specific point estimates on full-time employment and working hours effects, fiscal shares in earnings and fee abolition effects in day care expenses (as day care fees varied e.g. by socio-economic characteristics). Expectedly, we find substantial heterogeneity in the fiscal benefit-cost ratio: For example, our estimates suggest that employment reactions of single mothers (and the related fiscal revenues) cover the costs of day care fee abolitions, for highly educated mothers the fiscal benefit-cost ratio is at about 80 percent. All estimated refinancing shares are summarized in Figure 5.

Overall, the cost-benefit considerations suggest that the government finances on average 54 percent of its expenses through increased revenues from maternal employment. 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 34 percent of mothers are out of the labour force and do not enter it as fees are abolished. Nevertheless, they benefit from an increase in disposable family income. In the group of employed mothers, the substantial effect heterogeneity suggests additional and 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 even more efficient than universal day care fee abolitions.

VII Robustness Checks

A. Potential confounders

The causal interpretation of the labour supply estimates relies on the common trend assumption, for which we provide several plausibility checks. Even so, we need to assume that

no other treatments coincide with the fee abolition reforms. 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, *universal* expansion of publicly subsidised day care for children younger than three (e.g. Spiess, 2011). In Appendix Figure A.5, Panel A, we plot the increasing proportion of children in day care during our observation period. 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. The level difference is mainly attributed to regional differences between East and West Germany. For children aged three to four, treatment states show a steeper increase in day care attendance between 2006 and 2013. With East Germany in the control group, the baseline level is higher in 2006 and grows more slowly. Most important for our analysis is whether any change in day care use for younger children is related to the fee abolition reform in the last year, which we assess using the two-way fixed effects model specified in equation (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 reforms. 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 reforms. As the day care expansion for younger children relates to maternal labour force participation and working hours, we also check graphically the evolution of these outcomes for mothers of younger children. Appendix Figure A.6 shows that maternal labour supply outcomes evolve very similarly between treatment and control states, with a level shift because of the East German states in the control group.[30] This provides evidence that treatment and control states evolved similarly over time. Moreover, we detected no effect of fee abolitions on the labour supply of mothers of younger children. Eventually, we control for the proportion of children below age three in the main analysis of parental labour supply: the coefficients barely change. This reassures us that changes are orthogonal to the abolition of fees. In sum, we are confident that the general day care expansion and the availability of new day care slots was a universal trend not associated with fee abolitions.

Our second concern relates to the expansion of all-day care for children from age three onward, which coincided 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 proportion of children in

[30] Our main findings are robust to excluding East Germany, as we show in Section VII.B.

all-day care moderates the effects on maternal labour supply rather than the fee abolition reform. In Appendix Figure A.7, we plot the change in all-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 all-day care change for children aged three is highly predictive of the change in all-day care of children aged four in treatment and control states alike. We again employ our difference-in-differences model from equation (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 all-day care.

When we group children aged three to four and compare their all-day change with children aged five to six (the target group of the fee abolition reforms), we find that the increase at age five to six is somewhat stronger in treatment states (Appendix Figure A.7, Panel B). As the expansion at lower ages is highly predictive of the expansion at higher ages and as the fee reforms do not correlate with all-day use at lower ages, we attribute increases in the use of all-day care of children aged five to six to the fee abolition reforms.

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.

A large literature documents that maternal labour supply decisions interact with spouses' labour market outcomes, at least as long as the later changes (see, e.g., Halla, et al., 2020). While we cannot find effects of day care fee abolitions on paternal labour supply, we consider spouses' labour supply as additional control variables in our analysis of maternal labour supply responses.[31] Including spouses' labour force participation and working hours in the regressions[32] does not alter our main results on maternal labour supply responses.

Next, 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,

[31] This is not our preferred specification because spouses' labour supply characteristics are potential outcomes and are often considered as "bad control variables" (see, e.g., Angrist & Piscke, 2009).
[32] To account for missing information (e.g. if spouses are not living in the same household), we set their labour market outcomes to zero and include dummy variables for missing information.

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 exclude from the analysis the East German states, which serve mainly as control states.[33] While maternal labour force participation and the availability of day care below age three are substantially higher in East Germany, the results for fee reform effects are similar to the main findings.

Goodman-Bacon (2018) also suggests removing all never-changing states from the sample in a robustness check. Note that this robustness check cuts the sample size in 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. Nevertheless, 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 we observe, but the conclusions are the same.

[33] Of the five East German states in the sample, only Berlin abolished day care fees.

We are also concerned that our treatment effects in the final year of day care could partially be driven by the two of the six treatment states that abolished fees for younger children in later years, although they account for only 18 percent of observations in our treatment group. In our main analysis, we control for additional free day care years. Alternatively, we remove the two states from the analysis altogether and reach the same conclusions. Reassuringly, in the main analysis, we estimate significant effects only for parents in the final year of day care, not earlier. Therefore, we are confident about capturing the effects of day care fee abolitions in the final day care year and not any dynamic effects from fee abolitions for younger children.

As we cannot precisely measure whether children are in their final year of preschool in the Micro Census, we chose to assign families with children aged 5-7 years not in school to the group of treated children in the last year. However, about 41 percent of five-year-olds who are not in school are in their last day care year (based on own calculations in the SOEP). We remove children aged five years from the "last year" group and assign them to the group of younger children. As the share of treated families increases, our main effects on working hours and full-time work increase for working mothers, supporting the notion that the further reduction of day care fees in highly-subsidised systems increases maternal labour supply, especially at the intensive margin.[34]

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).[35] Our findings are robust to these adjustments.

VIII Discussion and Conclusion

We provide novel evidence that small private day care contributions prevent mothers from working longer hours, even if public day care systems are already highly subsidised and widely

[34] As an alternative check, we estimate the effect on day care fee expenses in the SOEP on a sample of families with children aged 5-7 not in school (equivalent to the labour supply analysis, instead of children in their last year of day care). The estimate of day care fee abolitions on day care expenses is then -32.55 euro (p-value<0.01).
[35] We use the Stata command *boottest* as proposed by Roodman, MacKinnon, Nielsen, & Webb (2018) and test under H_0 as recommended.

used. We analyse fee abolition reforms 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 all-day care. Maternal working hours increase on average by 0.4 hours per week (2.5 percent) and 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 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 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, 2020 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. Moreover, we cannot rule out that day care centres inflated private contributions to lunches or activities in response to fee abolitions. This, however, would be captured in our effect estimates on fee reductions and again suggests that our resulting estimates are rather lower-bound estimates.[36] 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; however, maternal labour force participation was at only 68 percent. This suggests that reservation wage considerations may be dominated by social norms regarding maternal employment or high opportunity costs for mothers if they had to increase the time away from their children.[37]

With respect to the *effectiveness* of the policy, we 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. Our detailed cost-benefit considerations suggest that the fiscal costs are financed by 54 percent through additional fiscal revenues from increased maternal employment. However, we document substantial heterogeneity in the efficiency of the policy. If day care cost reductions mainly aim at incentivising parental labour supply, we conclude, with respect to the *efficiency* of the policy, that other policy tools, such as tax credits on childcare expenses that link childcare subsidies to labour supply, could be even more efficient than universal day care fee abolitions.

A large literature documents the career penalties of part-time work (e.g. Weeden, Cha, & Bucca, 2016, Manning & Petrongolo, 2008). While our findings propose that day care fee abolitions increase maternal work intensity in the short-term, future research could further

[36] The short-run estimates on maternal labour supply are conservative as they also include families with five year old children who are partially not yet in their last year of day care. We show in the robustness section that this introduces some downward-bias in the estimates of *short-run* labour supply responses of mothers in the labour force.

[37] 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; nor are we aware of any other study providing according empirical evidence.

investigate dynamic effects on maternal earnings and careers. Such effects could persist longer if temporary increases in working hours help mothers enter a higher career trajectory.

Our findings are highly policy-relevant. Many countries have increased subsidised childcare substantially in an attempt to support mothers returning 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 fee abolition reforms are large, so that abolishing day care fees for all cannot be considered as an efficient policy tool to increase maternal labour supply.

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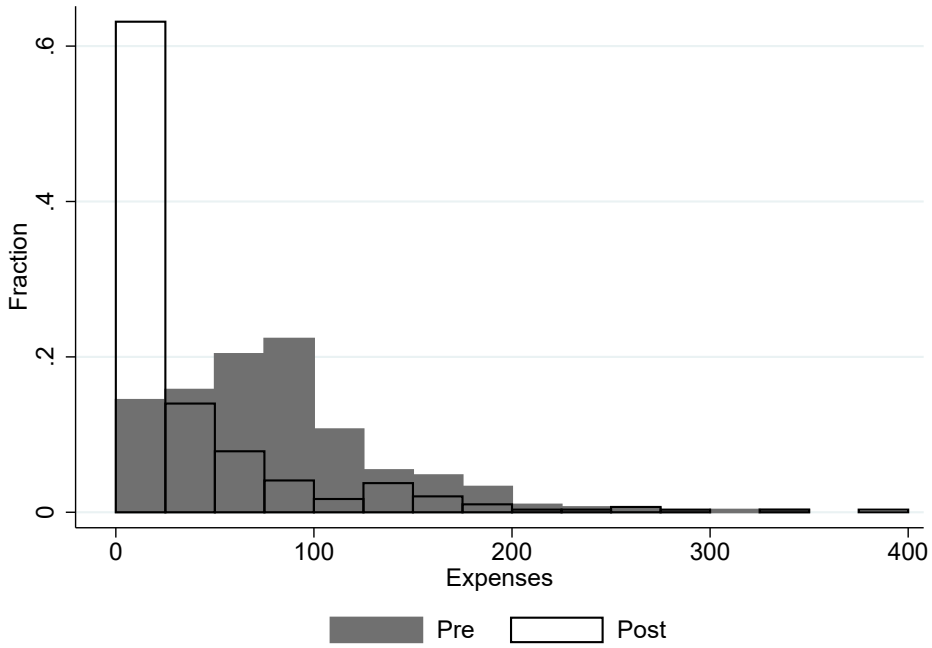
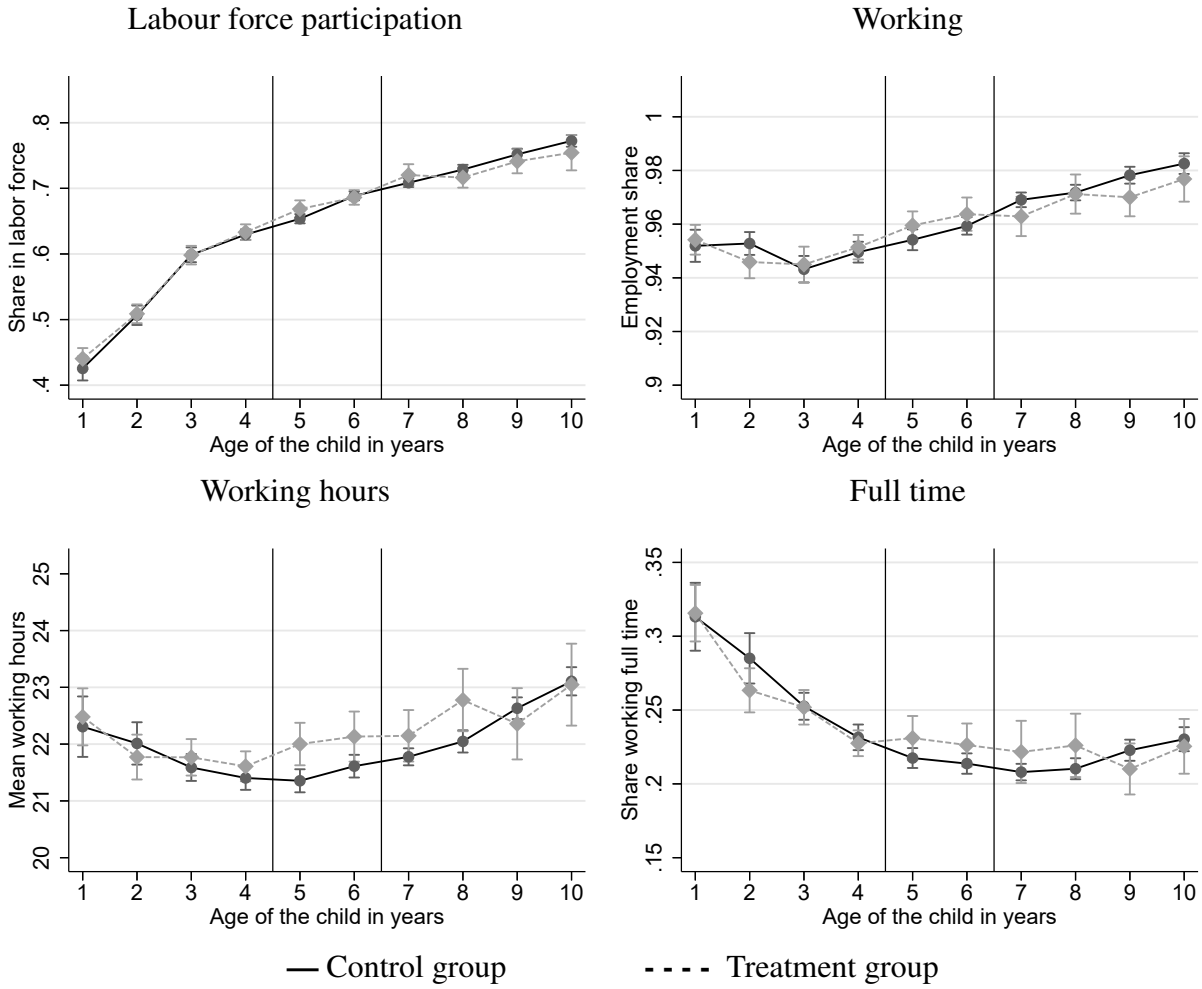
Figures

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. They may also 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).

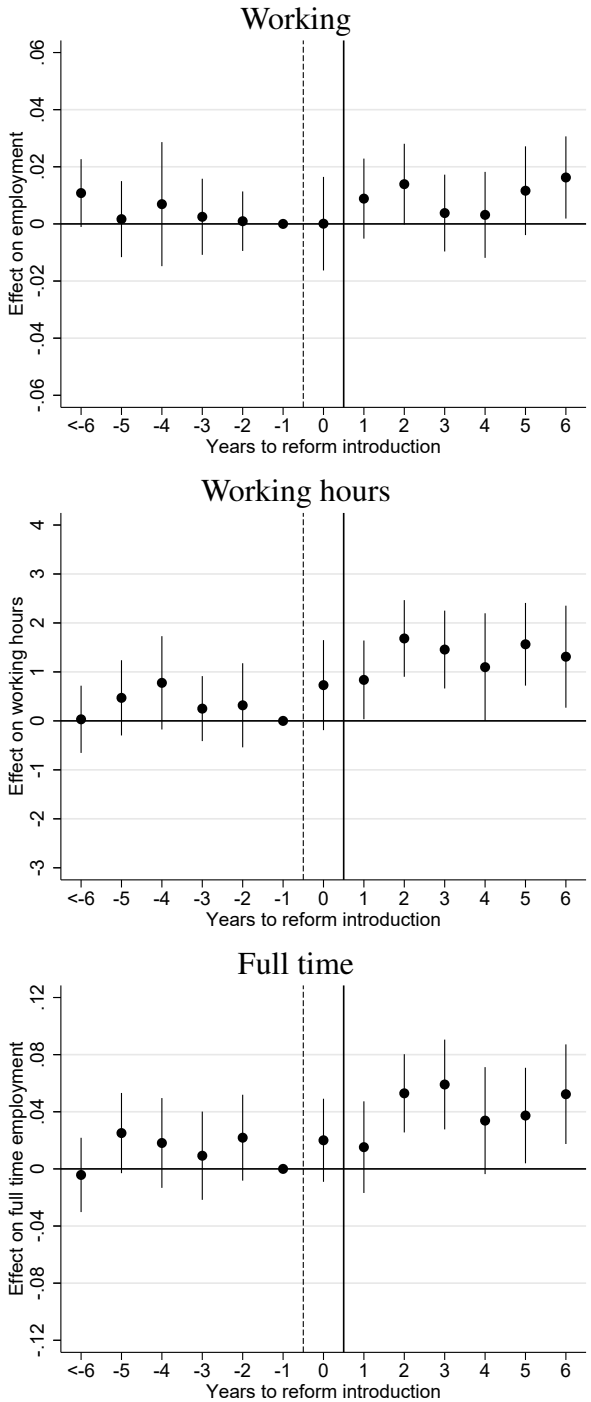
Figure 2. Maternal labour market outcomes by child age



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.

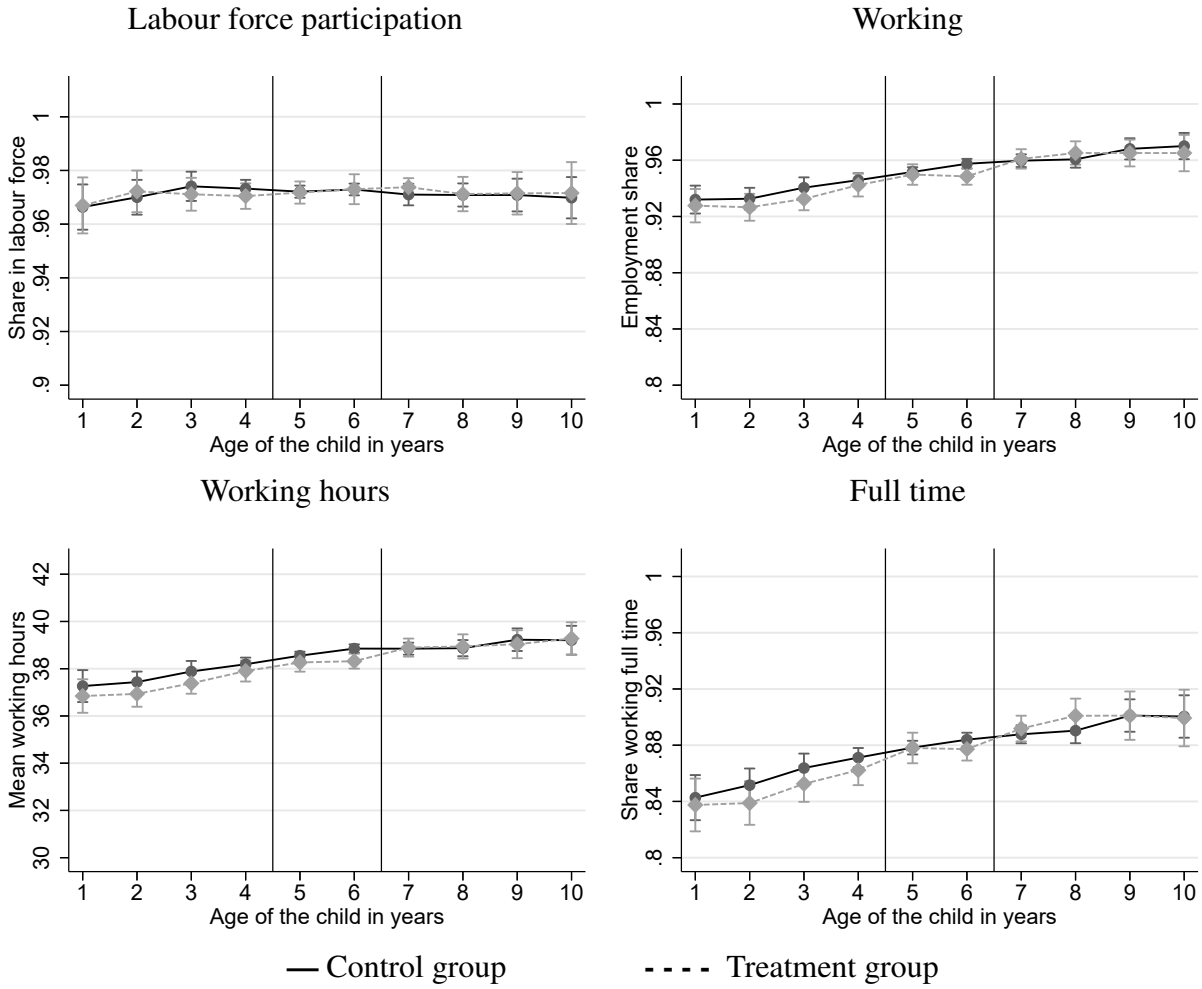
Source: RDC (2019), own illustration based on German Micro Census (2005-2013).

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).

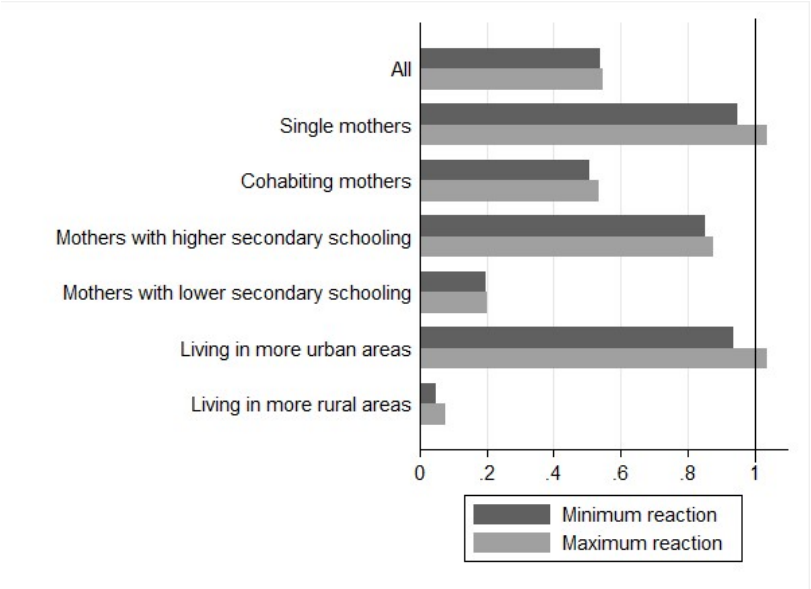
Figure 4. Paternal labour market outcomes by child age



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).

Figure 5. Fiscal benefit-cost ratios resulting from day care fee abolitions



Notes: The figure plots the fiscal benefit-cost ratio defined as fiscal gain / fiscal costs resulting from day care fee abolition for the overall population and subgroups. Compare Appendix C for details.
Source: Own calculations based on SOEP v33 (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 all-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 have 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	0.009	0.006	0.007	1.00	—
		(0.006)	(0.007)	(0.006)		
Mother works	0.60	0.009	0.005	0.005	0.96	0.005
		(0.007)	(0.007)	(0.007)		(0.003)
Mother's working hours	13.50	0.462**	0.540**	0.419**	21.78	0.782***
		(0.186)	(0.195)	(0.194)		(0.198)
Mother works full time	0.14	0.010**	0.012***	0.010**	0.22	0.023***
		(0.004)	(0.004)	(0.004)		(0.007)
Mother works $\geq$ 10 hours	0.53	0.009	0.010	0.008	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	-0.004	0.501	0.017	0.002
	(0.003)	(0.353)	(0.014)	(0.006)
Reform $\times$ 3-4	0.000	0.293	0.004	0.007
	(0.003)	(0.194)	(0.007)	(0.005)
Treatment				
Reform $\times$ Last year (free)	0.005	0.782***	0.023***	0.011**
	(0.003)	(0.198)	(0.007)	(0.005)
Post-treatment				
Reform $\times$ school starter	-0.002	0.620***	0.021**	0.001
	(0.004)	(0.226)	(0.009)	(0.006)
Reform $\times$ primary school	-0.002	0.453	0.011	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).

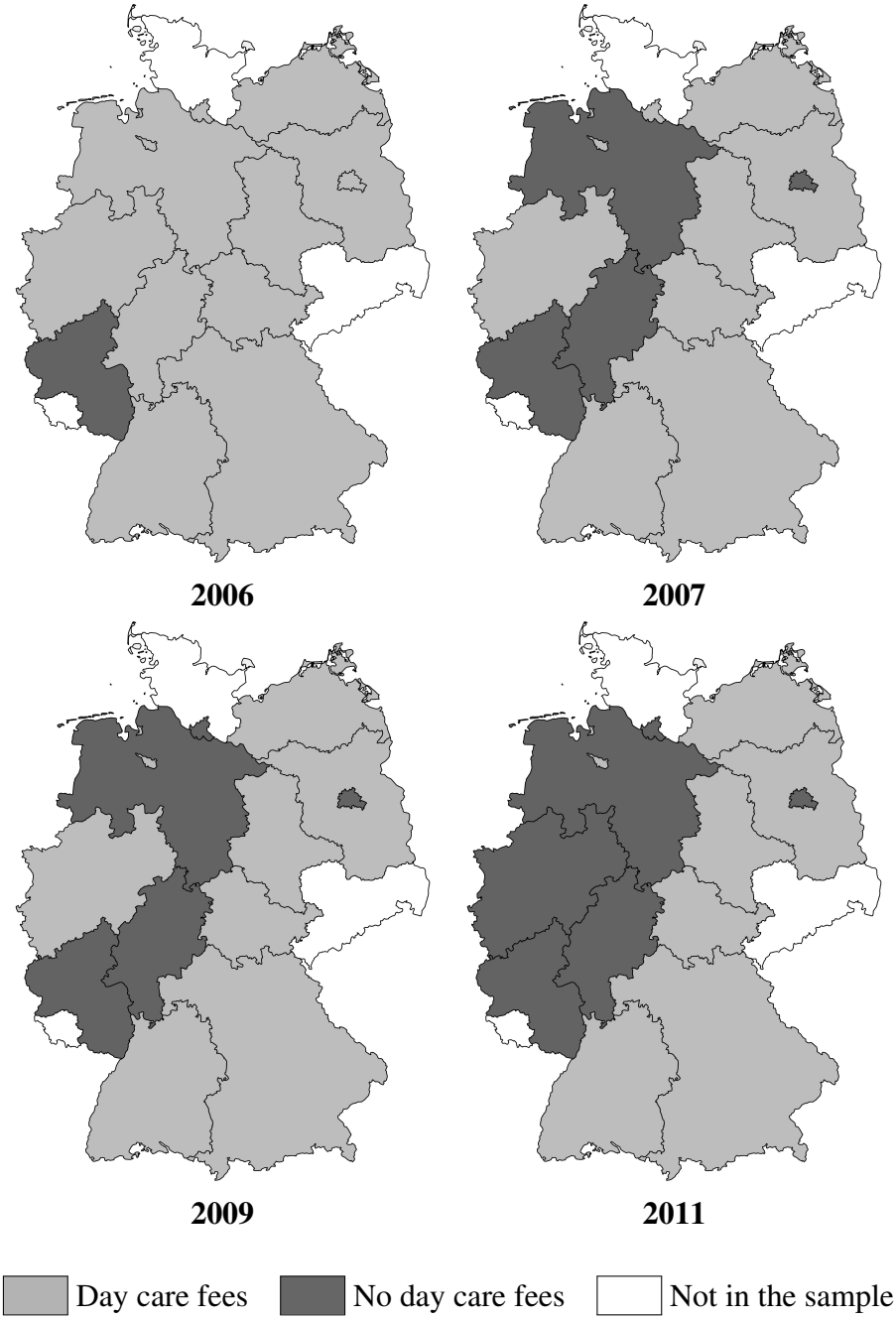
Table 6: Effect heterogeneity

	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).

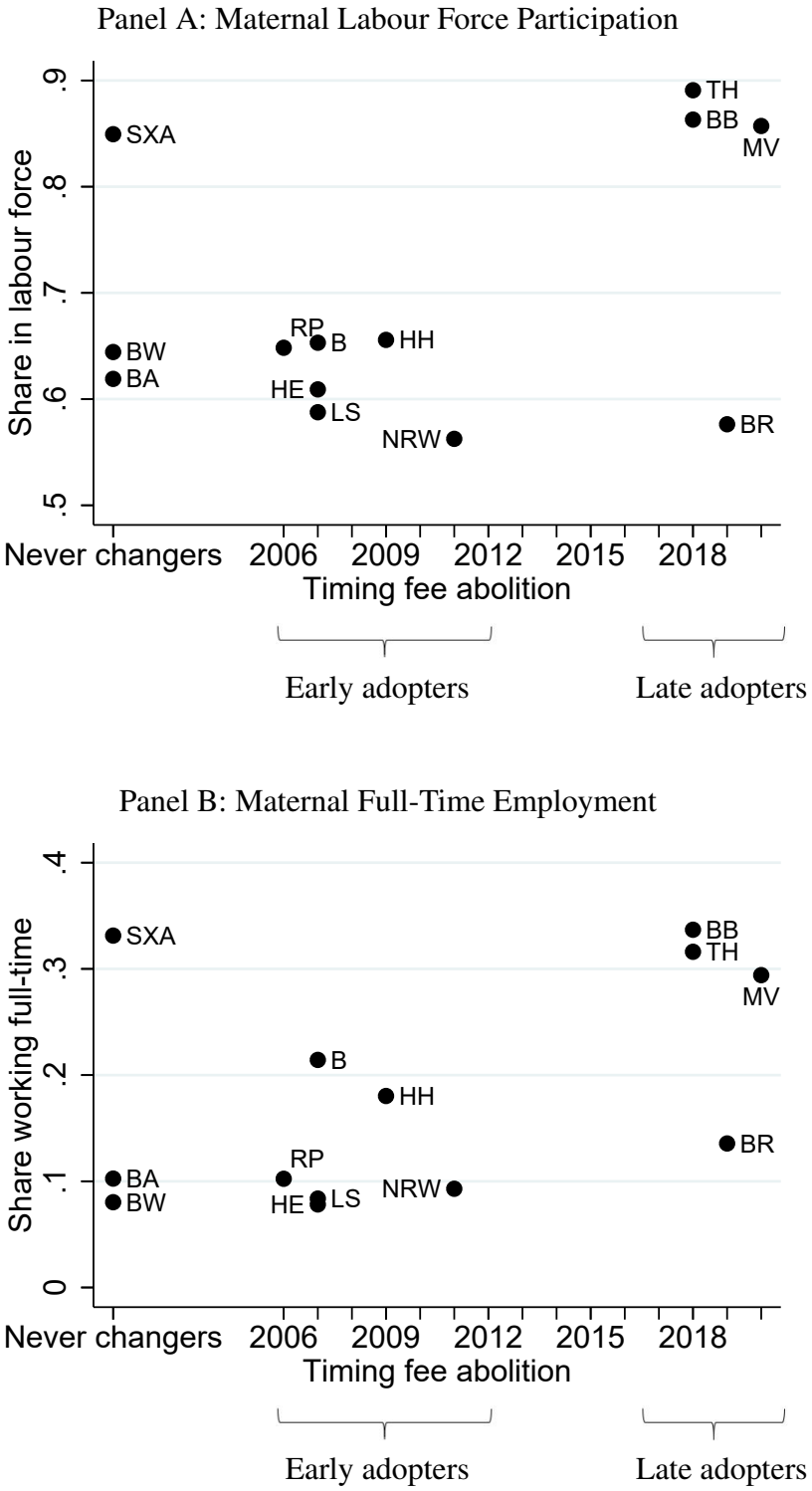
Appendix A

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



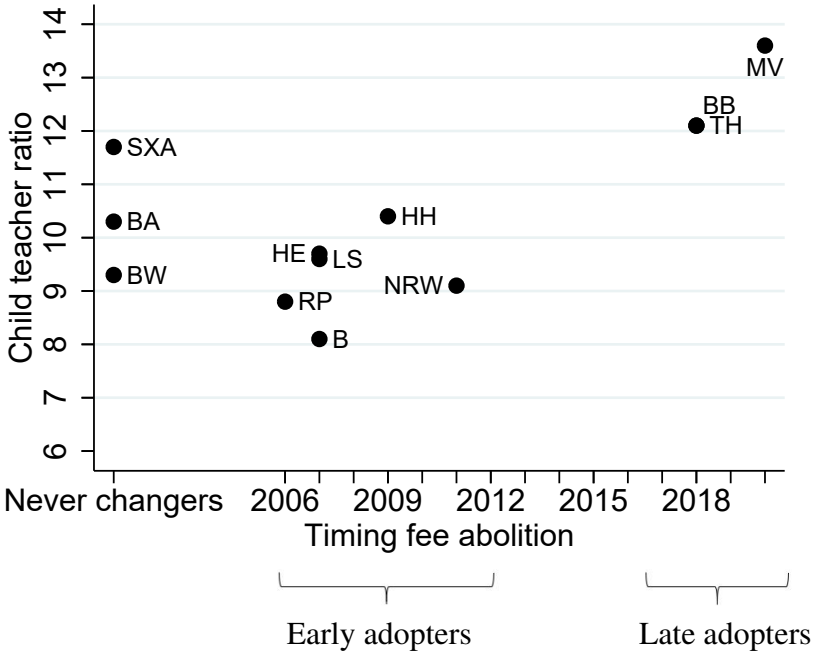
Notes: The figure plots the variation in timing of day care fee abolition in the final year of day care across federal states. States excluded from the main analysis abolished day care fees and re-introduced them shortly after (Schleswig-Holstein, Saxony and Saarland). These states are included in a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

Figure A.2. Maternal labour force participation and full-time employment



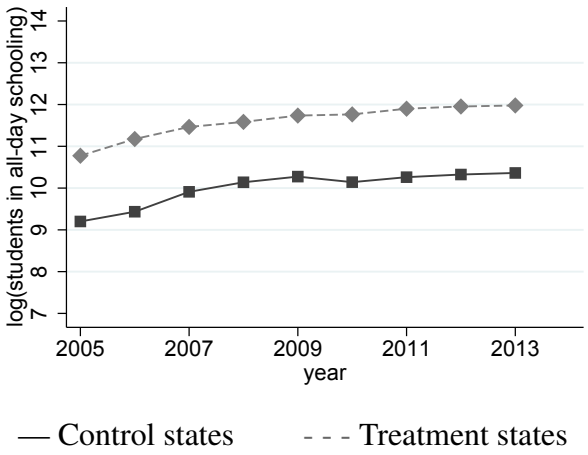
Notes: The figure plots pre-treatment labour force participation and full-time employment of mothers of children aged 5-7 who are not in school in 2005 by state and year of fee abolition. Abbreviations as in Table B.1. Three states that abolished and re-introduced day care fees (not included in our analysis) are omitted from this figure.
Source: Own calculations based on German Micro Census (2005).

Figure A.3. Day care quality



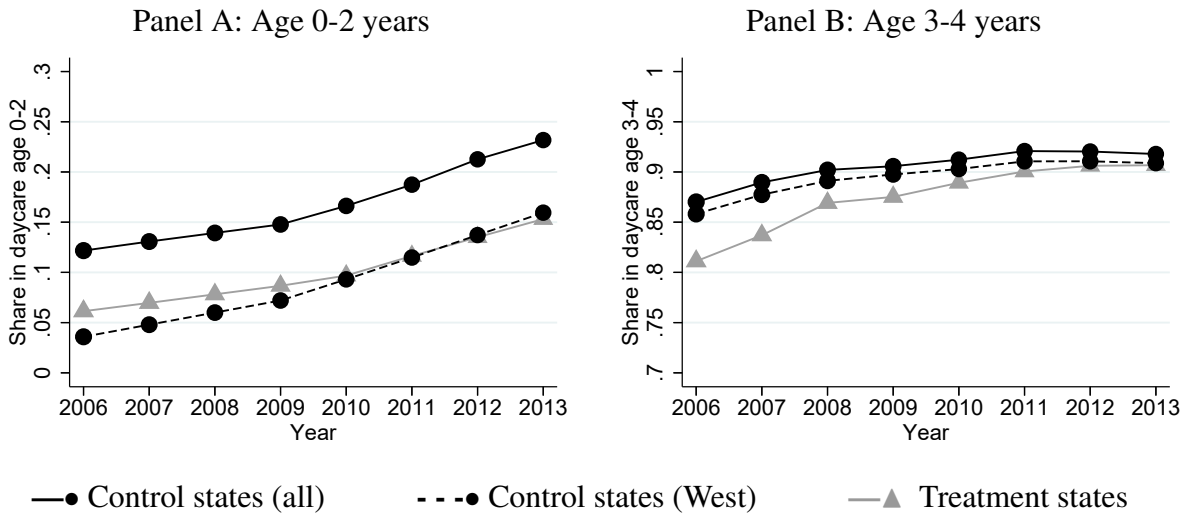
Notes: The figure plots the child-teacher ratio as one indicator of structural day care quality in 2006 (earliest available year) by state and year of fee abolition. Abbreviations as in Table B.1. Data missing for Bremen.
Source: Own calculations based on Deutsches Jugendinstitut (2008).

Figure A.4. 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).

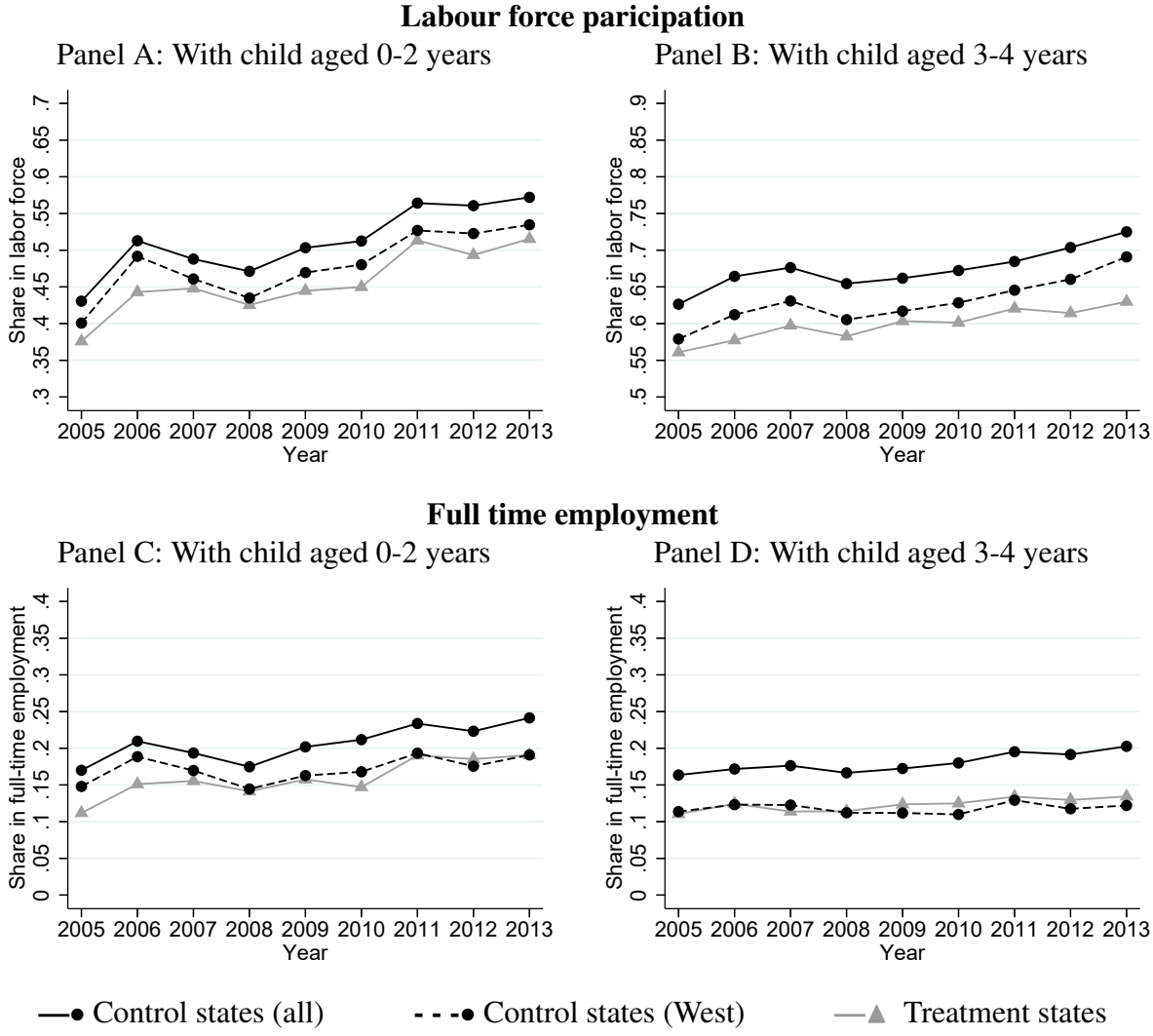
Figure A.5. Share in day care age 0-2 and 3-4 years



Notes: The figure plots the evolution of the share of children in day care for the ages 0-2 years (Panel A) and 3-4 years (Panel B).

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

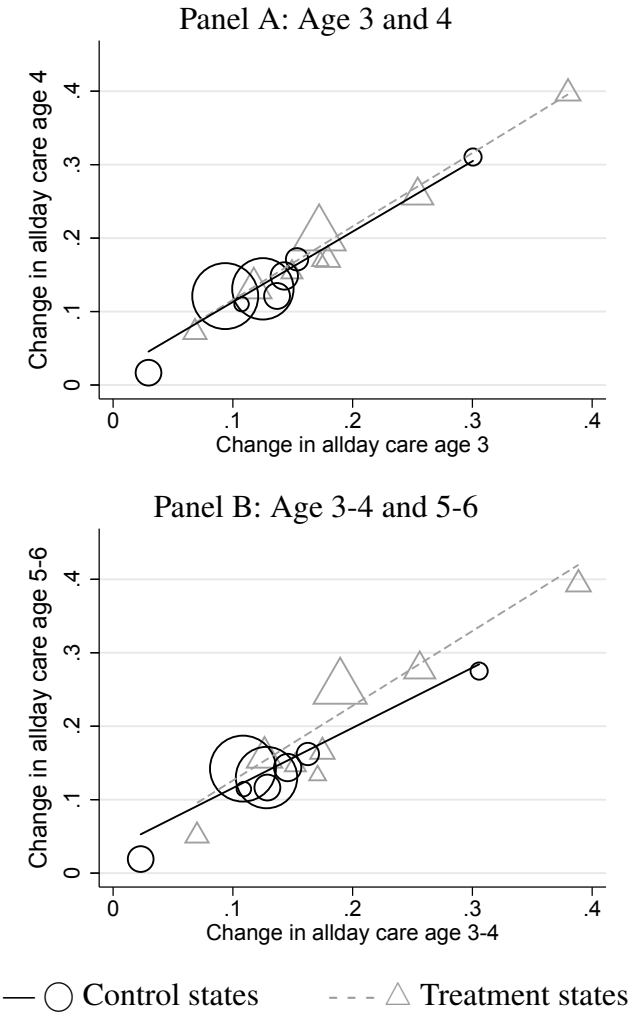
Figure A.6. Maternal labour supply



Notes: The figure plots the evolution of maternal labour force participation and maternal full time employment for mothers with children aged 0-2 years (Panel A) and 3-4 years (Panel B) separately for treatment states, control states, and control states from West Germany.

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

Figure A.7. Change in all-day childcare usage (2006-2013)



Notes: The figure plots the change in all-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, and the lines represent state-size weighted linear fits. While there is a national expansion in all-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).

Appendix B

Table B.1: Day care fee regulations

Federal state	Fee administration level	Fees dependent characteristics	Day care fee abolition for last day care year	First cohort born in	Time covered by fee abolition
Treatment states					
Berlin[a] (B)	Municipality	Income Children in household Hours of care	**Jan. 07**	2002	All-day
Hamburg (HH)	Municipality	Income, Family size, Hours of care	**Sep. 09**	2004	Five hours
Hesse (HE)	Municipality	Income, Family size, Hours of care, Age of child	**Aug. 07**	2002	Five hours
North Rhine-Westphalia (NRW)	Youth welfare office	Income Children in household	**Aug. 11**	2006	All-day
Rhineland-Palatinate[b] (RP)	Provider	Income Children in household	**Jan. 06**	2001	All-day
Lower Saxony (LS)	Municipality	Income	**Aug. 07**	2002	All-day
Control states					
Baden-Württemberg (BW)	Provider	Children in household Hours of care	-		
Bavaria (BA)	Provider	Hours of care			
Brandenburg (BB)	Provider	Income Children in household Hours of care	-		
Bremen (BR)	City-wide	Income Children/people in household Hours of care	-		
Mecklenburg-Vorpommern (MV)	Provider and municipality	not specified	-		
Saxony-Anhalt (SXA)	Municipality	Hours in care Children in childcare	-		
Thuringia (TH)	Municipality	Income Hours in care	-		
Not incl. in the main analysis					
Saarland	Provider	Income Children in household	Up to Jul. 2011		All-day
Saxony	Municipality	Age Children in childcare	Mar. 2009 - Dez. 2010		All-day
Schleswig-Holstein	Provider	Income Children in household	Aug. 2009 - Jul. 2010		All-day

Notes: The table summarises 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. [a] Berlin abolished fees for the second and third last year in 2010 and 2011, respectively. [b] Rhineland-Palatinate abolished fees for the second, third, fourth and fifth last year in 2007 through 2010, respectively.
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	0.007	0.013*	—	—
	(0.006)	(0.008)		
Mother works	0.005	0.000	0.005	0.007
	(0.007)	(0.010)	(0.003)	(0.006)
Mother working hours	0.419**	0.177	0.782***	0.681*
	(0.194)	(0.274)	(0.198)	(0.373)
Mother works full-time	0.010**	0.009	0.023***	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
Paternal controls (N=192,792)	—	0.005	0.767***	0.757***	0.022***	0.011**
		(0.003)	(0.288)	(0.195)	(0.007)	(0.005)
Without first affected cohort (N=185,065)	—	0.005	0.806**	0.802***	0.024***	0.013**
		(0.003)	(0.316)	(0.209)	(0.008)	(0.005)
Without East Germany (N=169,535)	—	0.007**	0.818***	0.776***	0.021**	0.012**
		(0.003)	(0.305)	(0.233)	(0.008)	(0.006)
Without never changers (N=96,486)	—	0.007*	0.563	0.534*	0.015	0.005
		(0.004)	(0.399)	(0.292)	(0.010)	(0.006)
Including states with re-introduced fees (N=214,906)	—	0.005*	0.620**	0.625***	0.017**	0.011**
		(0.003)	(0.263)	(0.178)	(0.007)	(0.004)
Excluding states without fees for younger children (N=177,719)	—	0.004	0.680**	0.586***	0.017**	0.010*
		(0.003)	(0.280)	(0.192)	(0.007)	(0.005)
Excl. 5-year olds from main treatment group (N=192,792)	—	0.005	0.339	0.845***	0.030***	0.001
		(0.004)	(0.425)	(0.298)	(0.011)	(0.008)
Clustering at state-birth year level (N=192,792)	—	0.005	0.799**	0.782***	0.023**	0.011*
		(0.003)	(0.314)	(0.245)	(0.009)	(0.006)
Clustering at state level (N=192,792)	—	0.005*	0.799***	0.782***	0.023***	0.011***
		(0.002)	(0.233)	(0.171)	(0.007)	(0.003)
p-value for clustering at state level	—	0.077	0.005	0.001	0.004	0.003
p-value for wild cluster bootstrap (state level)	—	0.136	0.012	0.001	0.001	0.003

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
Paternal controls (N=328,299)	0.010	0.009	0.622***	0.488***	0.010**	0.011
	(0.006)	(0.007)	(0.169)	(0.183)	(0.004)	(0.007)
Without first affected cohort (N=314,393)	0.007	0.004	0.545***	0.406**	0.010**	0.007
	(0.007)	(0.007)	(0.186)	(0.193)	(0.004)	(0.007)
Without East Germany (N=293,295)	0.012*	0.015**	0.725***	0.615***	0.010**	0.016**
	(0.007)	(0.007)	(0.178)	(0.202)	(0.005)	(0.007)
Without never changer (N=174,446)	0.005	0.008	0.502**	0.370	0.006	0.008
	(0.008)	(0.008)	(0.213)	(0.244)	(0.006)	(0.008)
Including states with re-introduced fees (N=363,723)	0.006	0.005	0.443***	0.359**	0.008*	0.008
	(0.006)	(0.006)	(0.160)	(0.160)	(0.004)	(0.006)
Excluding states without fees for younger children (N=301,783)	0.011*	0.005	0.495***	0.323*	0.007	0.007
	(0.006)	(0.006)	(0.168)	(0.173)	(0.004)	(0.006)
Excl. 5-year olds from main treatment group (N=328,299)	-0.002	-0.004	0.178	0.297	0.014**	-0.007
	(0.010)	(0.011)	(0.298)	(0.299)	(0.007)	(0.009)
Clustering at state-birth year level (N=328,299)	0.009	0.007	0.608***	0.462**	0.010*	0.009
	(0.006)	(0.007)	(0.216)	(0.190)	(0.006)	(0.006)
Clustering at state level (N=328,299)	0.009	0.007	0.608***	0.462**	0.010**	0.009
	(0.006)	(0.008)	(0.139)	(0.158)	(0.005)	(0.007)
p-value for clustering at state level	0.157	0.398	0.001	0.013	0.046	0.207
p-value wild cluster bootstrap (state level)	0.181	0.5134	0.013	0.006	0.020	0.252

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).

Appendix C

Cost-benefit considerations

Are the day care fee abolitions we evaluate an efficient tool to increase parental labour supply? On the one hand, the reform increases public expenditures but, on the other hand, the increase in maternal employment also generates additional fiscal revenues. In the context of limited public resources, cost-benefit considerations allow for assessing the efficiency of the reform.

For this analysis, we employ SOEP data providing detailed information on gross and net earnings of mothers between 2005 and 2013. We focus on mothers with 5 to 9 year old children, the age range for which we find maternal labour supply responses in our analysis of the Micro Census. The Micro Census itself contains neither detailed net earnings information nor information on gross earnings. We focus on monetary dimensions, abstracting from potential reform effects on other non-monetary dimensions that could be affected by changes in maternal employment or changes in day care expenditures, such as family well-being (e.g. Baker et al., 2008; Barnett, 2011; Felfe & Zierow, 2014; Loeb, Bridges, Bassok, Fuller, & Rumberger, 2007) or child development (Busse & Gathmann, 2018).

The single steps of the calculation are reported in Table C.1. For mothers in the labour force, the share in full-time employment increases by 2.3 percentage points on average (column 1), i.e. 2.3 out of 100 women in the labour force enter full-time employment due to the treatment with day care fee abolitions. Working hours increase by 0.78 hours (column 2, main results as in Table 5). These effects could result from either (a) a large share of mothers adjusting their working hours rather marginally or (b) a few mothers adjusting their working hours substantially, while the larger share does not react much (or from a scenario in between). Based on our cross-sectional data, scenarios (a) and (b) are empirically equivalent.

However, these scenarios could affect cost-benefit consideration due to characteristics of the German tax and social security system that changes the fiscal share on gross earnings (defined

as [gross earnings - net earnings] / gross earnings).* If mothers adjust working hours only marginally, the fiscal share in gross earnings is unlikely to change. However, if few mothers adjust working hours substantially, their fiscal share in earnings could change as they could enter a higher tax bracket (fiscal share would increase) or e.g. reach the cap in public health insurance (fiscal share would decrease). Therefore, we work with two scenarios in our cost-benefit considerations (column 3).

In the **first scenario**, we assume a *minimum* reaction of mothers to generate the increase in the full-time share and the average increase in working hours. Based on the empirical distribution of working hours in our SOEP sample (see Appendix Figure C.1), the minimum increase of working hours would be two hours to shift about 2.3 percent of mothers over the full-time threshold at 30 hours per week (column 4). Based on the working hours increase and maternal gross hourly wages, we calculate the additional annual gross earnings (columns 5). We then calculate the fiscal share in maternal earnings based on gross and net earnings for mothers shifting into full-time employment due to a 2 hours increase (i.e. mothers working 28 to below 30 hours). For full-time shifters, we estimate a fiscal share of 0.31 (column 6). This results in an annual fiscal benefit of 510 euro (changes in annual gross earnings * fiscal share, see column 7).

The remaining 97.7 percent of mothers in the labour force (1-0.023) need to increase their working hours by 0.75 hours (column 8), such that the weighted average of 2.3 percent full time shifters and 97.7 percent of other mothers sums to an average increase of 0.78 working hours. We again calculate the additional annual gross earnings based on the working hours increase and maternal gross hourly wages (column 9). We calculate the fiscal share in maternal earnings for all mothers who do not cross the full-time threshold due to a 0.75 hour increase and estimate a fiscal share of 0.28 (column 10). The resulting annual fiscal benefit is 155 euro (fiscal share * increase in annual earnings, see column 11).

*The fiscal share captures all taxes and social security contributions. In the German context, these must all be considered for the calculation of fiscal benefits because the state, including, among others, its old age pension funds, benefits from resulting increases.

We calculate the average annual fiscal benefit as the weighted average between both groups of mothers (0.023 * 511 + 0.977 * 155, see column 12). Assuming that the effects last for up to four years (compare Figure 2), we sum the average annual fiscal benefit over four years and discount it by two percent per year (column 13). This fiscal benefit is only generated by women participating in the labour force, i.e. 66 percent (column 14). However, the day care fee abolitions amount to total fiscal costs of 774 euro (on average 65 euro/month for the last year of day care) for *all* children. Therefore, the fiscal benefit-cost ratio is 0.54 (total fiscal gain / total fiscal cost = 0.66*632/774, see column 15). Overall, the *minimum reaction scenario* suggests that the fee abolition reform is financed by 54 percent through the increased fiscal revenues that result from maternal employment responses.

In the **second scenario**, we assume a maximum reaction of the small share of full-time shifting mothers who adjust their working hours substantially. We define a maximum reaction as a shift from average part-time employment (17 hours/week) to average full-time employment (36 hours/week), i.e. working hours increase by 19 hours. In this scenario, 2.3 percent of mothers (our main full-time effect) cross the full-time threshold by working 19 hours more (column 4). The substantial increase in working hours would increase annual earnings by 13,452 euro (column 5). The fiscal share for shifters to full-time employment is now calculated based on the gross and net earnings of women working between 30 and 48 hours (the hourly range in which mothers with previous part-time employment below 30 hours could potentially end up in with an hours increase of 19 hours). The fiscal share is also 0.31 (column 6),[†] and result in a fiscal benefit of 4,170 euro (column 7).

For the other 97.7 percent of mothers, working hours would need to increase by 0.35 hours (column 8) to fulfill an average increase in working hours of 0.78 hours. The increase in annual gross earnings amounts to 252 euro (column 9). As the fiscal share remains at 0.28 (column 10), the annual fiscal gain is 71 euro (column 11). The average annual fiscal benefit is again

[†]It may be surprising that the fiscal share is identical for women working more hours despite a progressive tax system. The fiscal share covers taxes and social security contributions. One explanation could refer to caps in the public health insurance contributions. Another explanation could relate to German tax regulations on spousal earnings splitting (*Ehegattensplitting*).

calculated as the weighted average of fiscal benefits of full-time shifters and the other mothers. It amounts to 165 euro (column 12). Again assuming that the effects on maternal employment last for up to four years, the total fiscal benefit from mothers in the labour force is 640 euro (discounted at 2 percent per year; see column 13). As about 66 percent of mothers participate in the labour force (column 14) and as the total fiscal costs, at 774 euro, apply to all mothers (column 15), the fiscal benefit-cost ratio is 0.55 (column 16). In the *maximum reaction* scenario, we estimate a slightly higher financing share of 55 percent if compared to a minimum reaction scenario.

Our analysis reveals substantial heterogeneities in maternal labour supply responses by maternal cohabitation status, maternal education, and between more rural and more urbanised areas. Because of specificities of the tax and social security system, the fiscal share in maternal earnings could also vary for these groups with potential implications for the cost-benefit considerations. In the following, we repeat the analyses for mothers with these characteristics. For each group, we calculate the required hourly responses in a *minimum* and *maximum reaction* scenario and satisfy the point estimates on full-time effects and working hours increases. We calculate group-specific annual gross earnings and calculate group-specific fiscal shares in earnings. This accounts for group specific specificities in the tax and social security system. Thereby, we obtain the group-specific total fiscal benefits. We also consider group-specific reductions in day care expenses as day care fees also varied by, e.g., socio-economic characteristics before the reform. Considering group-specific labour force participation rates, our analysis results in group-specific fiscal benefit-cost ratios of the fee abolition reforms (column 16).

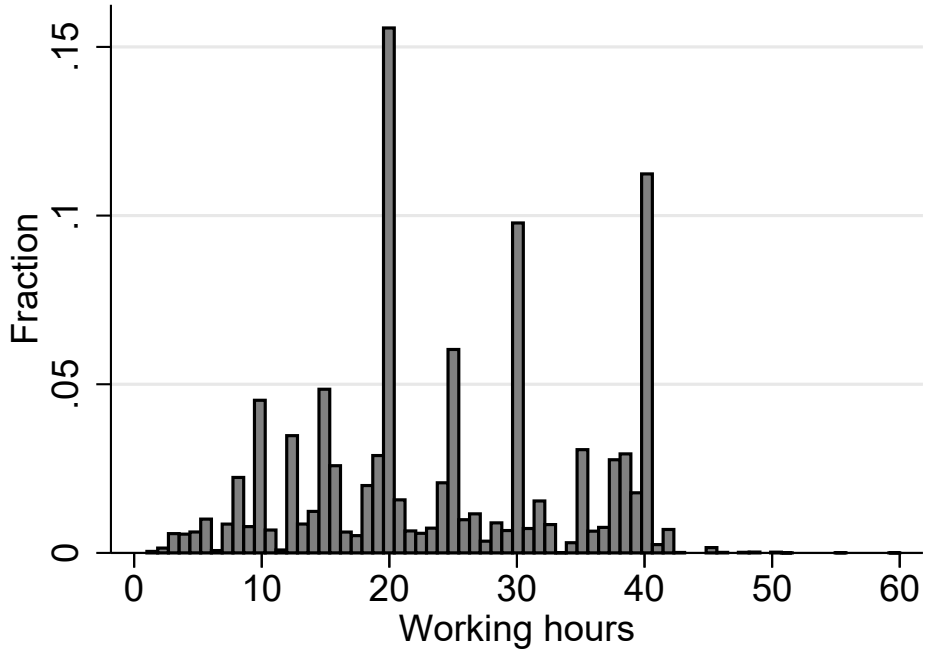
For the specific subgroups, we find that the benefit-cost-assessment varies substantially: For example, the employment reaction of single mothers and the related fiscal revenues completely cover the costs of day care fee abolitions, for highly educated mothers the benefit-cost ratio is over 0.80.

Overall, the cost-benefit considerations suggest that the government finances an average of 54 percent of its expenses through increased revenues from maternal employment. While the

estimates provide an approximate idea of the reform efficiency, they also have some limitations. For example, the estimation of the total fiscal costs is rather a lower bound estimate, as the increased use of all-day care increases public expenditures. Furthermore, we focus on a partial equilibrium effect, i.e. we assume that increases in maternal employment have no adverse effects on other workers. Furthermore, we abstract from fiscal multiplier effects, e.g. through higher earnings of mothers.[‡]

[‡]We also abstract from potential dynamic wage effects in the cost-benefit consideration. Our estimates suggest that full-time employment increases by only 2.3 percentage points and that the differences persist for approximately 4 years; i.e. the impact on dynamic wage growth is potentially small. Some evidence suggests that wage growth trajectories differ between part-time and full-time employed women (Fitzenberger & Wunderlich, 2002). However, other factors, like education and the initial position in the wage distribution, seem to be more decisive for wage dynamics in Germany.

Figure C.1. Distribution of maternal working hours



Notes: Figure based on mothers of children aged 5 to 9 in the labour force.
Source: Own calculations based on SOEP v33 (2005-2013).

Table C.1: Cost-benefit considerations of fee abolitions in the final year of day care

	Mothers in the labour force														
	Estimated effect on...			Shifting to full-time				Other mothers							
	full-time	working hours	Scenario	Δ working hours (p.w.)	Δ gross earnings (p.a.)	Fiscal share in earnings	Fiscal benefit (p.a.)	Δ working hours (p.w.)	Δ gross earnings (p.a.)	Fiscal share in earnings	Fiscal benefit (p.a.)	Average fiscal benefit (p.a.)	Total fiscal benefit	Share in labour force	Total fiscal costs	Fiscal benefit-cost ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Whole sample																
	0,023	0,78	Min.	2	1,644	0.31	510	0.75	552	0.28	155	163	632	0.66	774	0.54
			Max.	19	13,452	0.31	4,170	0.35	252	0.28	71	165	640	0.66		0.55
Partnership status: single (1) vs. cohabiting (2) mothers																
(1)	0.053	1.606	Min.	5	3,036	0.22	668	1.42	828	0.25	207	231	898	0.75	712	0.95
			Max.	19	11,340	0.28	3,175	0.64	372	0.24	89	253	981	0.75		1.03
(2)	0.02	0.68	Min.	1	912	0.26	237	0.68	516	0.30	155	156	607	0.65	779	0.51
			Max.	19	14,328	0.33	4728	0.31	240	0.30	72	165	641	0.65		0.53
Education: mothers with higher secondary schooling (1) vs. mothers with low secondary schooling (2)																
(1)	0.026	0,91	Min.	3	3,468	0.32	1,110	0.85	924	0.33	305	326	1,265	0.73	1,083	0.85
			Max.	19	2,014	0.35	7,052	0.42	468	0.33	154	334	1,296	0.73		0.87
(2)	0.017	0.36	Min.	2	1,404	0.33	463	0.33	216	0.27	58	65	253	0.51	657	0.20
			Max.	19	11,592	0.31	3,594	0.03	24	0.27	6	67	262	0.51		0.20
Urbanisation: living in more urban areas (1) vs. living in more rural areas (2)																
(1)	0.033	1.16	Min.	5	3792	0,34	1289	1.03	804	0.28	225	260	1,010	0.70	755	0.94
			Max.	19	15,336	0.33	5,061	0.55	432	0.29	125	288	1,118	0.70		1.04
(2)	0.006	0.07	Min.	1	948	0.23	218	0.07	48	0.28	13	15	57	0.64	725	0.05
			Max.	19	12,252	0.31	3,798	0	0	0.28	0	23	88	0.64		0.08

Notes: The analysis is based on mothers of children aged 5 to 9 in the labour force. *Scenario* refers to the allocation of the hours effect accross the distribution. *Min.* implies the minimal hours adjustment to generate the change in full-time employment, *Max.* implies the maximum hours adjustment (19 hours). The *fiscal share in earnings* is defined as (gross labour earnings - net labour earnings) / gross labour earnings. The *fiscal benefit (p.a.)* is defined as the fiscal share in earnings $*$ Δ gross earnings (p.a.). The *average fiscal benefit (p.a.)* is the weighted average of the fiscal benefit of full-time shifters and the other mothers. The *total fiscal benefit* is aggregated over 4 years and discounted to the year of fee abolition at 2 percent. The *fiscal benefit-cost ratio* is given as (total fiscal benefit * share in the labour force) / total fiscal costs.
Source: Own calculations based on SOEP v33 (2005-2013).