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DISCUSSION PAPER SERIES

IZA DP No. 14359

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ABSTRACT

Parents under Stress – Evaluating Emergency Childcare Policies during the First COVID-19 Lockdown in Germany

What are the effects of school and daycare facility closures during the COVID-19 pandemic on parental well-being and parenting behavior? Can emergency childcare policies during a pandemic mitigate increases in parental stress and negative parenting behavior? To answer these questions, this study leverages cross-state variation in emergency childcare eligibility rules during the first COVID-19 lockdown in Germany and draws on unique data from the 2019 and 2020 waves of the German AID:A family panel. Employing a DDD and IV approach we identify medium-term ITT and LATE effects and find that while emergency care policies did not considerably affect parents' life satisfaction, partnership satisfaction or mental health, they have been effective in diminishing harsh parenting behavior. We find partly gendered effects, specifically on paternal parenting behavior. Our results suggest that decreasing parental well-being likely constitutes a general effect of the pandemic, whereas the observed increase in negative and potentially harmful parenting behavior is largely directly caused by school and daycare facility closures.

JEL Classification: D04, D13, I18, I31, J13

Keywords: parental well-being, harsh parenting, COVID-19, policy evaluation, school and daycare closures, AID:A, difference-in-difference-in-differences, instrumental variable estimation

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1 Introduction

Confronted with nationwide closures of schools and daycare facilities due to the COVID-19 pandemic in spring 2020, many parents in Germany suddenly had to overhaul their work-care-arrangements. Only a very limited number of parents who between them had a specific constellation of systemically relevant occupations were granted access to emergency childcare (henceforth referred to as EC). In this paper, we exploit plausibly exogenous variation in eligibility rules across German federal states to evaluate the effect of emergency childcare policies on parental well-being and parenting behavior during the pandemic. Closed schools and childcare facilities, work from home arrangements, social distancing policies, and financial and health-related worries during the first lockdown created a stressful environment for families. These circumstances are likely to increase parenting stress, which in turn, might negatively influence parenting behavior (Abidin, 1992; Jackson and Choi 2018). Previous research indicates a positive association between negative parenting behavior and higher levels of children externalizing and internalizing problems, even if negative parenting behavior occurs infrequently (Pinquart, 2017). Moreover, harsh parenting has also been shown to be a risk factor for child abuse and neglect (Lee, Grogan-Kaylor, and Berger, 2014).

We employ a difference-in-difference-in-differences (DDD, or: triple differences) design combined with instrumental variable (IV) estimation to identify intention-to-treat (ITT) as well as local average treatment effects (LATE). We leverage cross-state variation in EC eligibility rules during the first COVID-19 lockdown in Germany, and can hence compare outcome changes of systemically relevant parents *with* EC access to outcome changes of equally systemically relevant parents in other states *without* EC access. This controls for the fact that systemically relevant parents (nurses, doctors and other key workers) are specifically affected in pandemic times, irrespective of EC. This cross-state comparison is based on the assumption that, were it not for EC policies, outcome changes for systemically relevant parents would have been similar across federal states. However, federal states were differently affected by the pandemic and hence reacted differently. To control for state-specific shocks that may have affected parents, we hence

additionally use outcome changes for parents with no systemically relevant occupation between them, resulting in a triple-difference design.

We draw on unique data from two waves of a German family panel "Growing up in Germany: Everyday Worlds" (AID:A) that surveyed families in 2019 and 4 to 5 months after the first COVID-19 lockdown in 2020. Based on a sample of 646 parents, we find that EC was not able to permanently shelter families from a considerable reduction in parental well-being. However, the provision of EC was effective in diminishing increases in harsh parenting in terms of 'becoming angry' among the EC-eligible parents. This effect of EC is evident several months post-lockdown, more pronounced in families with children of preschool-age or younger, and completely cancels out increases in harsh parenting among compliers. Furthermore, for fathers only, we find that EC prevented decreases in positive parenting behavior (child-centered communication), and increases in harsh parenting in terms of 'punishing harder than merited'. Evaluating medium-term effects, we likely measure lower bounds of immediate effects during the lockdown and identify the persistent component of the overall impact.

Our results disentangle effects caused directly by school and daycare closures from general effects of the pandemic since by studying families that use childcare, we are able to compare families that experienced a complete disruption of external childcare provision with those that, thanks to their access to EC, did not. This comparison is most meaningful for children in daycare where EC was quantitatively and qualitatively more equivalent to the pre-pandemic situation than EC for school-children. We find that, while decreasing parental well-being appears to be a general pandemic effect rather than a specific effect of the closures, the observed increase in negative and potentially harmful parenting behavior is largely directly caused by school and daycare closures.

These findings contribute to the growing body of empirical literature on how the COVID-19 pandemic affects families and family well-being[1]. We add to the existing literature by providing a first rigorous

[1] See e.g. Huebener *et al*. (2021) or Möhring *et al*. (2020) for empirical evidence on Germany and Prime, Wade, and Brown (2020) for a literature review on the possible consequences of the COVID-19 pandemic on well-being of families and children.

evaluation of EC policies, evaluating mid-term (by August, September 2020) rather than immediate effects, by exploiting intra-individual variation in a range of parental well-being and parenting behavior indicators. Furthermore our analysis relates to previous literature evaluating expansions in early childhood education and care (ECEC) on maternal labor market participation, parental well-being and child development (e.g. Bauernschuster and Schlotter, 2015; Schmitz, 2020; van Huizen and Plantenga, 2018). We contribute to this strand of literature by evaluating the effects of a temporary disruption in external childcare provision.

The paper is structured as follows: Section 2 describes the EC policies that were in place during the first COVID-19 lockdown in 2020 in the German federal states; Section 3 introduces the data used and describes sample selection; Section 4 presents the identification strategy; and Section 5 reports the main results, as well as complier analysis and robustness checks. The final section concludes.

2 Emergency Childcare Policies in the German Federal States in 2020

In Germany, childcare options and their take-up depend heavily on the child's age. Attendance rates are lowest for children under three. In 2019, only 34 percent of under-threes attended a childcare facility, with significant differences between East and West Germany, but also between urban and more rural regions. In contrast, daycare usage from the age of three is almost universal: daycare coverage for children aged three to five was over 90 percent in 2019 (BMFSFJ, 2019). Finally, by age six, 64 percent attend school, and reaching almost 100 percent by age seven (Statistisches Bundesamt, 2018).

During the nation-wide lockdown between mid-March and mid-April 2020, school and daycare closures were mandated in all German federal states. Moreover, to prevent the spread of COVID-19, and especially to protect the elderly, parents were encouraged not to rely on friends, neighbors, and grandparents for childcare support. Thus, many parents in Germany suddenly had to overhaul their work-care-arrangements and provide home-schooling on their own. However, a small number of parents with a specific constellation of systemically relevant occupations were granted access to EC. In the period between mid-March and mid-April 2020, all federal states provided "emergency childcare" based on parents' occupational systemic

relevance. Subsequently —according to a mutually agreed upon framework for the stepwise opening process (JFMK, 2020)—there was a phase of gradual re-opening where emergency childcare was steadily extended ("extended emergency childcare"). By June 2020, most federal states then switched to (restricted) normal operations of daycare facilities.

Figure 1 depicts the utilized childcare capacity during the first COVID-19 lockdown and the subsequent re-opening phase based on data from the "*Corona-KiTa-Studie*" (DJI and RKI, 2020), whereby weekly utilized childcare capacities represent the share of children that were in childcare compared to the total of children registered for daycare by March 2020.[2] In the initial phase of the lockdown with EC—between mid-March and mid-April—on average 3 percent of the childcare capacities were utilized while subsequently, during the phase of extended EC, on average 27 percent were utilized.

[Figure 1 about here]

In nearly all states (with exception of Hamburg and Saarland) parents' occupational systemic relevance was a crucial factor for EC eligibility during the "emergency care" phase of facility closures.[3] Systemically relevant occupations were defined as either occupations in the health and care sector (such as physicians, nursing stuff or laboratory assistants) or occupations needed to maintain the infrastructure (such as in the energy or water industries, transportation, alimentation or public safety). Note that there is some variation, since German federal states applied stricter or looser definitions of systemically relevant occupations (Deutscher Bundestag, 2020; Blum and Dobrotić, 2021). Moreover, since in Germany, federal state and county-level authorities are responsible for education and social services, regulations regarding EC eligibility varied across federal states. Table 1 provides information about these differences in EC eligibility rules (see Figure A.1 in the Appendix for a graphical display). While in some states both parents had to work in a systemically relevant occupation to be eligible for EC (*2-parent rule)*, in other states only one parent had to (*1-parent rule*). Additionally, in Bremen, Bavaria, Saxony and Schleswig-Holstein, a "*mixed rule"* was applied according to which, to gain access to EC parents had to be either both in a systemically

[2] For six federal states, these data also include after-school childcare for school children.
3 To be eligible for EC, employers had to confirm systemic relevance.

relevant occupation, or at least one parent had to work in an occupation in the health and care sector. In the federal states of Hamburg and Saarland, parents were encouraged to keep their children at home, but access to EC was not further regulated. Furthermore, while in some states the child's age was also a limitation factor in whether it could be placed in EC, in others child's grade determined the relevant upper limit.

[Table 1 about here]

Our analysis aims at identifying the impact of EC policies during the acute phase of school and daycare closures between mid-March and mid-April, exploiting the differences in eligibility rules across federal states as exogenous variation. Our empirical strategy abstracts from the effects of the subsequent provision of "extended emergency childcare" during the re-opening phase following the first COVID-19 lockdown, which is a key feature of our analysis. We feel it is important to focus on parents that had access to EC from the beginning of the lockdown, and hence experienced a significantly smaller disruption of daycare provision in comparison to all other parents that had children in daycare pre-pandemic (including those that utilized extended emergency childcare after two months of childcare at home). In view of the fact that in many cases emergency childcare did not provide full-time daycare, our estimates likely represent the lower bounds of the true effects.

3 Data and Sample Selection

Our analysis uses data from the 2019 wave of the AID:A family panel that surveyed about 6.000 households on living conditions of children, youth, young adults and parents. We combine this data with information from the "AID:A Corona Add-on" study, which re-interviewed about 780 households in August and September 2020 on their current living conditions, as well as their circumstances during the first COVID-19 lockdown in Germany in March and April 2020. Importantly for the purpose of our study, parents of children of pre-school age or younger were asked about their utilization of emergency childcare during the lockdown. Moreover, parents were also asked about their occupational "systemic relevance" and, crucially for our identification strategy, the type of systemic relevance (health-related or not). That is, respondents

were not directly asked about their occupational systemic relevance, but whether they "work in the health or care sector" and whether they "work in a sector that is prescribed systemic importance, such as, for example, energy and water supply, transportation, alimentation or public security". If partner information on systemic relevance is missing, we impute it via the partner's occupation stated in 2019, based on the classification of systemic relevance employed in Koebe et al. (2020), which links up with occupations at the 3-digit KldB 2010 level.[4] This information allows us to determine parents' eligibility for EC according to the official rules of the respective federal states of residence (see Section 2 and Table 1), irrespective of their reported utilization of EC. Note that the information on parents' occupational systemic relevance is crucial for our ability to identify effects of emergency childcare abstracting from effects of extended emergency childcare.

Note also, that our measure of EC utilization does not distinguish between utilization during the immediate "emergency childcare" phase or the subsequent phase of "extended emergency childcare". Only by instrumenting EC utilization with EC eligibility, can we tease out the local average treatment effects of EC use during the "emergency childcare" phase. We assume implicitly in our analysis that compliers in the IV analysis identify EC-eligible parents that actually utilized emergency childcare in the acute lockdown and not only extended emergency childcare in the subsequent phase of re-opening.

Estimating intention-to-treat effects on parental well-being and parenting behavior, we consider 636 parents from 482 two-parent families with at least one child below the age of 12[5] that was either in school or in external daycare when the pandemic hit Germany (Sample A). This sample is restricted to parents for whom we have full information on the main outcomes, and families for whom we have either information on occupational systemic relevance or occupational classification for both parents. Additionally, 16 families

[4] A robustness check presented in Table A.6 in the Appendix is based on an alternative regression-based imputation employing federal state and occupation fixed effects (KldB 2010, 3-digit level).

[5] Children below the age of 12 are defined as necessitous of childcare according to the Infection Protection Act (§56, Abs.1a). We exclude 24 single-parent households since some federal states applied particular EC eligibility rules to single parents, which we cannot examine based on low observation numbers.

from federal states without within-state variation of EC access (Hamburg and Saarland according to Table 1) have been excluded, since they do not contribute to our identifying variation.[6]

To estimate local average treatment effects, we consider a subset of 319 parents from 227 families with at least one child of pre-school age or younger (Sample B). We focus only on this group since unfortunately information on EC utilization was not collected for school children and is hence not available for the full sample (Sample A). Table 2 reports summary statistics for both samples.

[Table 2 about here]

About 54 percent of parents in our sample have no systemically relevant occupation in the parental couple and are thus not eligible for EC. Conversely, roughly 46 percent of parents[7] have a specific constellation of systemically relevant occupations between them. However, only 23.4 (24.1) percent of parents in Sample A (B) are eligible for EC, with their specific constellation of systemic relevance in the parental couple matching the EC eligibility rule applied in their federal state of residence. The EC utilization rate, at 26.3 percent in Sample B is significantly higher than the observed eligibility rate, since the information on utilization does not distinguish between utilization during the period of "emergency childcare" (mid-March to mid-April 2020) and utilization during the subsequent period of "extended emergency childcare" (mid-April to end-May 2020) during which utilization rates increased significantly (see Section 2, Figure 1).

The information displayed in Table 2 also shows that, on average, all employed measures of parental well-being and parenting behavior worsened from 2019 to 2020. Life satisfaction decreased by on average 0.22 (0.25) points, partnership satisfaction by 0.13 (0.08) points on a 6-point scale from 1 "*not at all*" to 6

[6] Our main results also hold without imposing this restriction (see Table A5 in the Appendix).
[7] About 32 percent of parents in Sample A work themselves in a systemically relevant occupation. 57 percent are employed in a health-related systemically relevant occupation, with "medical and health care occupations" as the dominant occupation category. 18 percent are employed in a non-health-related occupation of systemic relevance, with "occupations in business management and organization", "occupations in teaching and training" and "occupations in education and social work, housekeeping, and theology" as the most common occupational classifications.

"*very satisfied*" for Sample A (B). Observed decreases in the WHO-5 Well-Being Index[8] amount to on average 0.33 points on a 100-point scale for Sample A, and 0.55 for Sample B. Harsh parenting behavior increased in its frequency on average by 0.38 (0.36) points on a 6-point scale with respect to 'becoming angry', and less strongly in terms of 'punishing harder' with an average increase of 0.02 (0.06) points for Sample A (B). The latter measures of negative parenting behavior stem from the survey questions *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say"*, and *"I punish my child(ren) harder than what they merit,"* with answer categories ranging from 1 *"never"* to 6 *"(almost) always"*.

4 Empirical Strategy

The source of exogenous variation underlying our identification strategy mainly comes from the cross-state variation in EC eligibility rules during the first COVID-19 lockdown in Germany. Our identification strategy compares groups of parents with the same constellations of occupational systemic relevance, which differ in their EC eligibility due to variations in EC eligibility rules across federal states. We define EC-eligible parents as the "treatment group", and parents that are in some constellation of systemically relevant occupation but who are not eligible for EC as the "control group". For a more robust analysis, we add parents without any systemic relevant occupation in the parental couple as a further control group. Altogether, this leads to a difference-in-difference-in-differences design (Wooldridge, 2010, p.151). Subsequently, we employ an instrumental variable approach instrumenting EC utilization with EC eligibility to estimate local average treatment effects.

4.1 Regression Model of Intention-to-Treat Effects

To estimate the intention-to-treat effects of EC, that is, the effects of EC eligibility on parental well-being and negative parenting behavior, we use the following model in a triple differences setup:

[8] The World Health Organisation Well-Being Index (WHO-5) is a five-item measure of self-reported current mental well-being (WHO 1998). The resulting index ranges from 0 *"absence of well-being"* to 100 *"maximal well-being"* (see Topp et al., 2015).

(1) $$y_{irst} = \gamma_{st} + \alpha_{rt} + \theta_{rs} + \beta\, Eligibility_{rst} + \varepsilon_{irst},$$

where y_{irst} is the outcome of interest (parental well-being or parenting behavior, respectively) observed for parent *i* of systemic relevance (SR) constellation group *r* resident in state *s* in period *t* (with *t* = [2019; 2020]). The three dimensions of state (*s*), time *(t*) and SR constellation group (*r*) allow us to control non-parametrically for state-specific shocks (γ_{st}), interactions of SR constellation group and time effects (α_{rt}), as well as state-specific effects of SR constellation groups (θ_{rs}).

$Eligibility_{rst}$ is our treatment variable, and results from the entanglement of the four SR constellation groups, the three state-specific eligibility rule types, and a period effect. Specifically, in federal states with a "1-parent rule", all constellations except "no parent SR" are EC eligible; in federal states that apply a "2-parent rule" only the constellation of "both parents SR" is EC eligible; in federal states with a mixed rule, the constellations of "both parents SR", as well as the constellation "one parent SR, health related" are eligible for EC. Our coefficient of primary interest β is a difference-in-difference-in-differences type estimator. This parameter is identified through (1) cross-sectional variation across states with different EC eligibility rules (with EC-eligible parents as the treatment group and parents in similar SR constellations who are not EC-eligible due to different state rules as a control group), (2) temporal variation in parents' average outcome levels between the survey waves 2019 and 2020 (with the untreated year 2019 as control), and (3) temporal variation within states (with parents without a systemically relevant occupation as the "within-state" control group).

Throughout the analysis, all standard errors are clustered at the household level and are robust to heteroscedasticity. We show results from individual fixed effects regressions that additionally control for unobserved time-invariant factors, and improve precision with respect to pooled OLS.[9] Note that all within-individual time-invariant factors (including θ_{rs}) are controlled for by the individual fixed effects.

For completeness, we additionally report the difference-in-differences (or: double difference) results from the subsample of parents with some kind of SR constellation. That is, for the double-difference setup,

[9] Individual fixed effects help reduce the variance of ε_{irst} and hence the standard errors of the estimate of β.

we exclude the data for parents without any occupational SR in the parental couple, and we estimate equation (2), in which states are again subscripted with *s*, SR constellation groups with *r*, and time period with *t*:

$$(2) \qquad y_{irst} = \gamma_s + \alpha_t + \theta_r + \beta\ Eligibility_{rst} + \varepsilon_{irst}.$$

The double-difference estimator assumes that, were it not for differences in EC eligibility rules, outcome changes for parents of the same SR constellation group would have been similar across federal states. However, there might well be state-specific period effects in the context of the COVID-19 pandemic and associated state-level policy measures that may have affected parental stress irrespective of EC policies. A neat way to account for state-specific shocks is in fact to use other groups that are not directly affected by EC policies in either state (such as parents without any SR occupation in the parental couple) as an additional control group in a triple-difference setup, as outlined above. Outcome changes in this group, which is unaffected by the policy of interest, are then presumed to reflect region-specific period effects.

The causal interpretation of the intention-to-treat effects in this (and any DD or DDD) setting hinges on the common trend assumption. However, we cannot investigate pre-trends since the AID:A family panel only started in 2019. Nevertheless, it seems plausible to assume that EC eligibility, i.e., a combination of a certain occupational SR combination in the parental couple and the federal state of residence, was of no importance pre-pandemic. To assess the legitimacy of this assumption, i.e., the exogeneity of EC eligibility, we regress EC eligibility on a variety of family sociodemographic characteristics. As expected, none of them appears to be statistically significantly associated with EC eligibility status (see Table A.1 in the Appendix).

4.2 Regression Model of Local Average Treatment Effects

To directly examine the causal effects of EC utilization on parental well-being and negative parenting behavior, we exploit the fact that access to EC was only possible when parents met the state-specific eligibility rules. We employ a first-differences model in the first stage, which—in two period panel models—is numerically equivalent to an individual fixed effects model. With respect to the regression

model employed to estimate intention-to-treat effects, we exclude the second-level interactions (γ_{st}, α_{rt} and θ_{rs}) to yield a powerful first stage (Pischke, 2007, p.16). Overall, causal identification in this setup stems from the instrumental variable rather than the triple-difference approach. The analysis can be represented by the following system of equations:

(3) $\Delta y_{irst} = \vartheta + \sigma\, Utilization_{irst} + \Delta\, \varepsilon_{irst}$,

with the first stage given by:

(4) $Utilization_{irst} = \omega + \delta\, Eligibility_{irst} + \Delta\epsilon_{irst}$,

where $Utilization_{irst}$ is a dummy variable equal to 1 if parent *i* in federal state *s* reports having utilized EC during the first COVID-19 lockdown in Germany in spring 2020 (and 0 otherwise). The constant terms ϑ and ω represent first differences of the time effect. $Eligibility_{irst}$ serves as an instrument for parents' actual usage of EC. While exogeneity of the instrument (EC eligibility) is sufficient for a causal interpretation of the intention-to-treat effects from equations (1) and (2), IV estimation of equations (3) and (4) require the additional assumption that EC eligibility affects parental well-being and parenting behavior only through the actual utilization of EC, and not directly in any other way. In the context of the first COVID-19 lockdown, this assumption appears rather plausible: while systemic relevance *per se* might have been associated with factors that also influenced parental well-being and stress-levels (such as work in the health sector, potential exemptions from curfews and work-from-home-orders, or augmented infection risk exposure), the differences in EC eligibility regulations concerning the SR constellation in parental couples allow us to explicitly control for such differences.

5 Results and Discussion

5.1 Intention-to-Treat Effects

We use model (1) to estimate the intention-to-treat effects of the provision of EC on parental well-being and parenting behavior outcomes. These estimates inform about EC provision in its acute phase, when the

occupation-based eligibility rules described in Section 2 (Table 1) were in place, abstracting from the effects of subsequently extended emergency childcare.

Table 3 presents the main results with respect to parental well-being measures and indicators of negative parenting behavior. For each outcome, we present the estimated coefficient β—based on individual fixed effects regressions—as the double-difference estimator in Panel A, and the triple-difference estimator in Panel B of Table 3. Panel C reports a gender interaction of the triple-difference estimator to investigate treatment heterogeneity for mothers and fathers.

[Table 3 about here]

The double-difference results in Panel A indicate that among parents with at least one SR occupation among the parental couple, EC-eligible and non-eligible parents experienced similar decreases in parental well-being between 2019 and 2020. With respect to negative parenting behavior, it appears that while non-EC-eligible parents report strong increases in "harsh parenting" in terms of "quickly becoming angry if children don't do as I say", the EC-eligible are significantly less prone to such increases. These effects are marginally statistically significant. However, they cannot isolate the causal effect of EC eligibility, as there may have been other state-specific shocks to parental stress levels (e.g., due to other COVID-19 measures at the state-level or regional infection dynamics). We hence augment the double-difference model to examine this possibility by taking advantage of the fact that parents without any occupational systemic relevance were not granted access to EC in the acute lockdown period. This allows us to use outcome changes for this group to control for unobserved state-specific shocks via a triple-difference technique.

The triple-difference results presented in Panel B of Table 3 confirm that overall, access to EC did not considerably affect parental life satisfaction or partnership satisfaction. However, we estimate a positive effect on well-being according to the WHO-5 index of about 7.3 points on a 100-point scale, which is statistically significant at the 5-percent level. To classify the effect size, we compare it to the gender difference between mothers and fathers, which amounts to 3.0 points in the year 2019. Hence, our estimated effect is 2.4 times as large as the average gender difference in the WHO-5 index.

With respect to parenting behavior, it appears that EC was effective at preventing increases in "harsh parenting" in terms of 'becoming angry'. The estimated effects are statistically significant at the 5-percent level, and indicate that being EC-eligible decreases the frequency of 'becoming angry' by 0.407 points on a 6-point scale. To classify the effect size, we again compare it to the average gender difference between mothers and fathers (0.115 points in the year 2019). Our estimated effect is 3.5 times as large as the average gender difference for 'becoming angry'.

To compare effect sizes between WHO-5 well-being, which is an index from 0 to 100, and harsh parenting in terms of 'becoming angry', which is measured on a 6-point scale, we compute effects on standardized outcomes (see Table A.2 in the Appendix). It turns out that both effects are in fact rather similar in size: 0.429 and 0.423 of one standard deviation for WHO-5 well-being and 'becoming angry', respectively. Note again that these are mid-term effects, measured as of September/August 2020. Immediate effects during the acute lockdown in March/April 2020 are likely to have even been stronger.

Gender interactions presented in Panel C of Table 3 reveal statistically significant gender differences only for negative parenting behavior with respect to 'punishing harder'. Here, EC appears to have affected fathers only, in that it decreased the frequency of 'punishing children harder than merited' by about 0.316 points on a 6-point scale (or by 0.401 of one standard deviation, according to Table A.2 in the Appendix). Interestingly, we find a similar gender pattern when investigating positive parenting behavior in terms of child-centered communication (see Table A.3 in the Appendix).[10] It is only fathers that appear to respond to EC with increased frequencies of 'speaking with the child about his/her experiences' (by 0.336 points on a 6-point scale) and 'speaking with the child about things that annoy or burden him/her' (by 0.554 points on a 6-point scale), while control-group parents show significant decreases in the frequency of child-centered communication.

[10] Note that survey items on positive parenting behavior were child-specific in 2019 and collected only for children above the age of two. For this reason, we report results on this subsample and take the mean across children to make these observations comparable to the 2020 survey where these items where parent-specific. The survey questions read as follows: *"How frequently does the following occur? I speak with the child about his/her experiences" (1 "never" to 6 "(almost) always") , "How frequently does the following occur? I speak with the child about things that annoy or burden him/her" (1 "never" to 6 "(almost) always").*

Tables A.4 to A.6 in the Appendix provide three types of robustness checks for our main outcomes. First, we reproduce the triple-difference results of Panel B of Table 3 (and additionally the gender interaction for the outcome 'punish harder') based on weighted regressions employing a combination of AID:A design weights at the household level and "staying probability" weights at the individual level (Table A.4 in the Appendix). Second, we re-run the analyses on a sample that includes the two federal states that applied an "all access" EC eligibility rule in the acute lockdown (Hamburg and Saarland), which we exclude in our main analysis sample (see Table A.5 in the Appendix). Third, we show results based on an alternative imputation of partners' occupational systemic relevance, which is regression-based, employing federal state and occupation fixed effects (see Table A.6 in the Appendix). Overall, the results on harsh parenting behavior remain (sometimes marginally) statistically significant, and are qualitatively similar to our preferred estimates. This is also the case for estimates with respect to WHO-5 well-being, except for the weighted regressions, where the effect becomes statistically insignificant (see Table A.4 in the Appendix).

Given the categorical nature of the dependent variable 'becoming angry', we also investigate which frequency categories are most affected. That is, does the EC effect largely stem from changes in modest frequencies in 'becoming angry' or rather from frequency changes among already somewhat 'angry' parents? Table A.7 in the Appendix reports our key triple-difference results on 'becoming angry' from Panel B of Table 3 with respect to dichotomized outcome variables indicating different groupings of the frequency categories: the lowest frequency ("*never*"), the two lowest frequencies ("*never*" and "*seldom*"), the three highest frequencies ("*often*", "*very often*" and "(*almost) always*"), and the two highest frequencies ("*very often*" and *"(almost) always*").[11] The overall effect appears to largely originate in movements from the upper two frequency categories "*very often*" and *"(almost) always"* toward the category "*often*", as well as movements from "sometimes" toward the lowest two frequencies of "never" or "seldom" becoming angry.

[11] There are too few observations in the highest frequency category "(almost) always" to allow its separate investigation.

5.1 Local Average Treatment Effects

The intention-to-treat results show how EC availability during the first COVID-19 lockdown in Germany affected parental well-being and negative parenting behavior. To interpret these results, it is—as a first step—important to understand the pattern of EC take-up. First, we quantify the relationship between EC availability and EC utilization by estimating the first stage model (4) based on Sample B.[12] We estimate the coefficient on eligibility ($\hat{\delta}$) to be about .32 with a standard error of .08. This estimate implies a 10 percentage point increase in EC eligibility among parents induces (an additional) 3.2 percent of parents to take up EC. To roughly understand what type of parents utilize EC when they are eligible (compliers), we estimate equation (4) separately for different types of parents. We partition Sample B sequentially by regional population size, parental education, mothers' labor market involvement, and age of the youngest child—that is, we split the sample in two, with one part including values equal to or below the median, and another for values above the median.

Column (1) of Table A.8 in the Appendix displays the median value of each characteristic. Column (2) reports the proportion of the sample that falls above the respective median value. Columns (3) and (4) show the distribution of compliers across the two subgroups (below or equal to the median, and above median) for each characteristic. Following Akerman, Gaarder, and Mogstad (2015), the proportion of the compliers of a given type is calculated as the ratio of $\hat{\delta}$ for that subgroup to the $\hat{\delta}$ in the overall sample, multiplied by the proportion of the sample in the respective subgroup.

We see that EC is not randomly adopted within the group of eligible parents. Compliers are slightly underrepresented among families that live in relatively more urban areas (with more than 50,000 inhabitants), and strongly underrepresented among parents with a university degree, in parental couples where mothers work more than 20 hours a week, and in families with relatively older children (i.e., the youngest child is above the age of three). The underrepresentation of compliers among the high-educated might in part be explained by the fact that the feasibility of working-from-home strongly increases with an

[12] Note that we now restrict the sample to families with children of preschool age or younger, since there is no information on EC utilization for school children in the data.

academic degree (Alipour, Falck and Schüller, 2020).This conjecture is corroborated by the finding that compliers are also underrepresented in families where mothers did work more than 50 percent of their work time remotely during the lockdown in March/April 2020.

In the following, we focus on the outcomes of harsh parenting behavior, where the intention-to-treat regressions yield robustly statistically significant "reduced form" effects.[13] We deem the results with respect to WHO-5 well-being not entirely robust due to the lack of statistical significance in the double-difference as well as in the weighted regressions. In fact, also the LATE effects are not statistically significant for WHO-5 (see Table A.9 in the Appendix).

Invoking the exclusion restriction, we estimate how the utilization of EC affects the incidence of harsh parenting behavior among compliers. We approach the presentation of the LATE effects in a stepwise manner, taking the ITT effects presented in Table 3 as a starting point. Column (1) of Table 4 repeats the intention-to-treat effects reported in Table 3, as resulting from a first-differences regression and now without second-level interactions. In comparison with Table 3, the ITT effects without second-level interactions are about half the size, but are still statistically significant at the 5-percent level. Column (2) of Table 4 reports intention-to-treat effects estimated in the same way using Sample B instead of Sample A. ITT effects appear to increase in size and in statistical significance when families with relatively older children (i.e., with the youngest child being of school-age) are excluded from the sample. The specification without second-level interactions allows for a neat comparison of outcome changes between EC-eligible and non-eligible parents, since the estimated constant now indicates the average outcome changes for non-eligible parents. Hence, we can observe that the frequency of 'becoming angry' significantly increases by about 0.419 (0.426) points in the 6-point scale between 2019 and 2020 for non-EC-eligible parents, and that this increase is effectively reduced by about half (by 0.175 and by 0.270 points respectively for Sample A and B) for EC-eligible parents. Instead, with respect to 'punishing harder', we observe much smaller increases for the non-EC-eligible parents (0.035 and 0.095), which are not statistically significant for

[13] We report LATE estimates on the remaining outcomes in Table A.9 in the Appendix. They are statistically insignificant throughout. There are also no significant gender differences.

Sample A, and only marginally statistically significant at the 10-percent level for Sample B. There are also no considerable differences in outcome changes for EC-eligible parents.

Column (3) of Table 4 presents estimates of equation (2) based on OLS. The OLS estimate is informative regarding the correlation between EC utilization and our outcome of interest, without any distinction made between EC utilization during the acute lockdown and utilization of *extended* EC during the subsequent re-opening. Interestingly, in this case, there appears to be no statistically significant association between EC utilization (including during extended EC) and the frequencies of harsh parenting behavior.

[Table 4 about here]

Column (4) of Table 4 reports estimates based on IV estimation of equations (3) and (4). In contrast to the OLS estimate, with IV we estimate the effect of EC utilization on *compliers*, i.e., parents who utilize EC due to their SR-constellation based eligibility status. These compliers are most likely parents that had already taken up emergency care by the beginning of the lockdown. In turn, non-EC eligible parents that took up EC during the phase of extended EC are not compliers in this setup. The first stage is strong, with a Kleibergen-Paap rk Wald F-statistic of 16.15 on the excluded instrument, which means weak instrument bias is not a concern. The IV estimate with respect to 'becoming angry' is statistically significant at the 5-percent level, and suggests that EC utilization due to eligibility based on parents' occupational systemic relevance was effective at preventing increases in negative and potentially harmful parenting behavior that would have happened in the absence of EC. Specifically, EC utilization reduced the frequency of 'becoming angry' by almost one point on the 6-point scale (0.842). As expected, the effect size is considerably larger among compliers than among all EC-eligible parents, where eligibility is associated with a 0.270-point lower frequency of 'becoming angry' (see Column 2). It also becomes evident that EC utilization among the EC-eligible can entirely prevent the increase in the frequency of 'becoming angry' that non-EC eligible parents experienced (0.582 points on the 6-point scale). In contrast, with respect to 'punishing harder', there appears to be no significant effect of EC utilization on compliers.

Column (5) finally reports on an investigation of potential gender differences in the IV estimates. It turns out that while there is no significant difference in EC effects on 'becoming angry' between fathers and

mothers, the interaction with parental gender reveals that EC utilization significantly reduced the frequency of 'punish harder' for fathers, but not for mothers. The decrease in fathers' harsh parenting in terms of 'punishing harder' is statistically significant at the 5-percent level, and is of substantial size (0.795 points on the 6-point scale).

6 Concluding Remarks

Having carried out a first rigorous evaluation of emergency childcare (EC) policies during the first COVID-19 lockdown in Germany in early 2020, we find that EC was not able to permanently shelter families from a considerable reduction in parental well-being. However, the provision of EC was effective in diminishing increases in harsh parenting among EC-eligible parents. This effect of EC is more pronounced in families with children of preschool-age or younger, and completely cancels out increases in harsh parenting among compliers.

We evaluate effects 4 to 5 months after the first COVID-19 lockdown in Germany, and hence provide evidence on the medium-term consequences, rather than the immediate impact of emergency childcare. Further research is needed to assess the mechanisms behind the gendered impact of EC on 'punishing harder' and child-centered communication, where we find effects exclusively for fathers.

Overall, our results disentangle effects caused directly by school and daycare closures from general effects of the pandemic, since among families with childcare usage we compare those who experienced or—due to EC—did not experience a complete disruption of external childcare provision. Thus, we conclude that, while decreasing parental well-being is likely to be a general pandemic effect rather than a specific effect of the closures, the observed increase in negative and potentially harmful parenting behavior is largely directly caused by school and daycare closures.

An important limitation of our study is that—given the data at hand—we can only provide somewhat isolated effects on parental well-being and parenting behavior, and not a comprehensive view of the impacts of school and daycare closures. To draw meaningful policy conclusions, impacts on e.g. long-term child

development, health risks for parents and children, or the rate of new infections (see e.g.. Dehning *et al.*, 2020; Brauner *et al.,* 2021) must be additionally considered.

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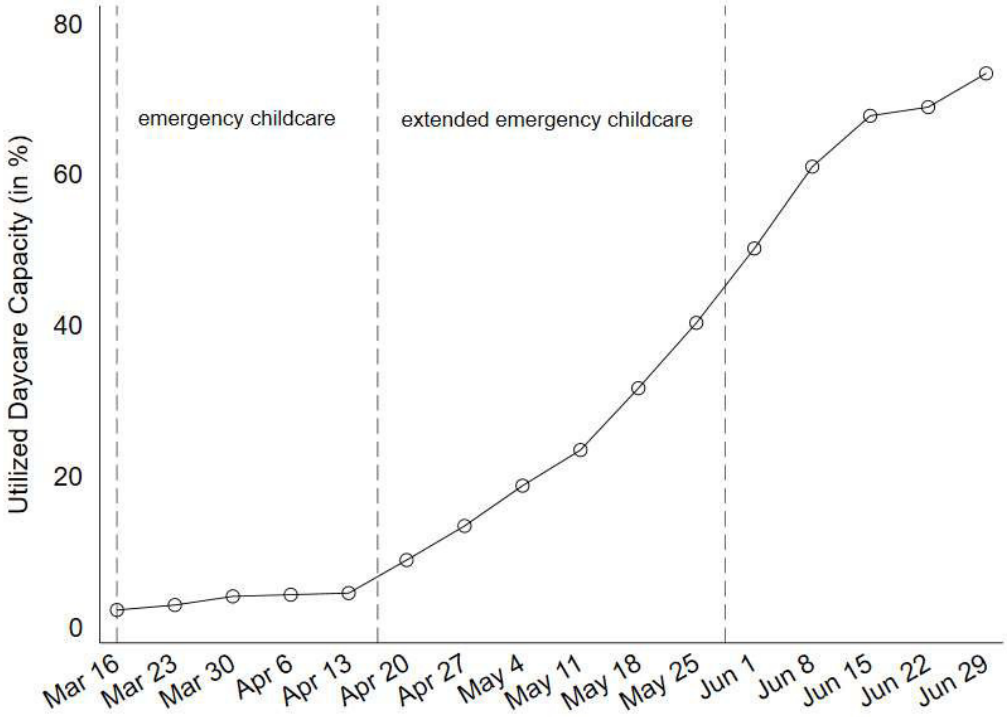
Figure 1. Utilized childcare capacity in Germany during the first COVID-19 lockdown in early 2020 and subsequent re-opening



Source: DJI-RKI (2020); own calculations.

Note: Utilized daycare capacity represents the share of children that are currently in daycare among those children that were registered in daycare by March 2020. DJI-RKI (2020) reports these shares weekly by federal state, based on communications from the respective federal state ministries; we subsequently aggregate those shares to the national level. Not all federal states report utilized capacities every week (week of Mar 16: *N*=9, Mar 23-30: *N*=13, Apr 6-13: N=14, Apr 20-June 1: *N*=15, June 8-15: *N*=12, June 22: *N*=11, June 29: *N*=7). There is no information available on the federal state of Baden-Württemberg throughout. For six federal states, these data also include after-school childcare for school children. We define the timing of transition from emergency childcare to extended emergency childcare and from extended emergency childcare to the phase of (restricted) normal operation as the week where more than five observed federal states switch status, based on information from DJI-RKI (2020, Table 1).

Table 1. Emergency Childcare Policies in the German Federal States during the first COVID-19 Lockdown

	Eligibility rules based on parents' occupations	Eligibility limit according to age or grade of children
Baden-Württemberg	2-parent rule	up to 6th grade
Bavaria	mixed rule	up to 6th grade
Berlin	1-parent rule	up to 6th grade
Brandenburg	2-parent rule	no restrictions
Bremen	mixed rule	up to 8th grade
Hamburg	all access	up to age 14
Hesse, link1, link2	1-parent rule	up to 6th grade
Lower Saxony	1-parent rule	up to 8th grade
Mecklenburg-Vorpommern	2-parent rule	up to 6th grade
North Rhine-Westphalia	2-parent rule	up to 6th grade
Rhineland-Palatinate	1-parent rule	up to 7th grade
Saarland	all access	up to age 12
Saxony	mixed rule	up to 4th grade
Saxony-Anhalt	2-parent rule	up to age 11
Schleswig-Holstein	mixed rule	up to 6th grade
Thuringia	2-parent rule	up to 6th grade

Sources: Decrees or corresponding press releases by the respective federal state (see hyperlinks in the first column).

Note: "2-parent rule": both parents have to work in a systemically relevant occupation to be eligible for EC. "1-parent rule": at least one parent has to work in a systemically relevant occupation to be EC eligible. "Mixed rule": parents have to either work both in a systemically relevant occupation or at least one parent works in an occupation in the health and care sector to gain access to EC. "All access": parents are encouraged to keep their children at home, but access to EC is not further regulated. Lower Saxony and Schleswig-Holstein adjusted their regulations after the first week of lockdown. We employ the adjusted regulations. The eligibility rules summarized here concern the first phase of emergency childcare. Eligibility expanded in the subsequent phase of extended emergency childcare, whereby all federal states except Thuringia (where the 2-parent rule remained in place) applied the 1-parent rule.

Table 2. Summary Statistics

	Sample A: Families with children below age 12		Sample B: Families with children of preschool-age or younger	
	Mean	SD	Mean	SD
Outcome variables – parental well-being (1 "not at all satisfied" to 6 "very satisfied")				
Life satisfaction 2019	5.074	0.775	5.107	0.778
Life satisfaction 2020	4.857	0.949	4.859	0.969
Δ life satisfaction	-0.217	0.949	-0.248	0.951
Partnership satisfaction 2019	5.072	1.004	5.103	0.974
Partnership satisfaction 2020	4.943	1.075	5.028	0.919
Δ Partnership satisfaction	-0.129	0.918	-0.075	0.908
WHO-5 2019 (index 0-100)	60.151	15.963	58.520	15.997
WHO-5 2020 (index 0-100)	59.818	18.129	57.969	17.888
Δ WHO-5	-0.333	18.022	-0.552	18.510
Outcome variables – harsh parenting (1 "never" to 6 "(almost) always")				
'Become Angry' 2019	2.525	0.941	2.586	0.977
'Become Angry' 2020	2.907	0.942	2.947	0.911
Δ 'Become Angry'	0.382	0.971	0.361	0.948
'Punish Harder' 2019	1.788	0.782	1.796	0.764
'Punish Harder' 2020	1.808	0.794	1.859	0.794
Δ 'Punish Harder'	0.020	0.816	0.063	0.787
Treatment variables				
Eligibility for emergency childcare	0.234	0.424	0.241	0.429
Usage of (extended) emergency childcare			0.263	0.441
Systemic relevance constellation				
No parent systemically relevant	0.538	0.499	0.539	0.499
One parent systemically relevant, not health-related	0.226	0.419	0.185	0.389
One parent systemically relevant, health-related	0.108	0.311	0.119	0.324
Both parents systemically relevant	0.127	0.334	0.157	0.364
Complier characteristics				
Up to 50,000 inhabitants	0.701	0.458	0.639	0.481
At least one parent holds university degree	0.411	0.492	0.436	0.497
Mother's weekly working hours (2019)	19.236	15.047	17.896	15.918
Age youngest child in household	5.127	3.213	3.242	1.577
N (Nr. of parents)	636		319	

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: The World Health Organization Well-Being Index (WHO-5) is a five-item measure of self-reported current mental well-being (WHO 1998). The resulting index ranges from 0 "absence of well-being" to 100 "maximal well-being" (see Topp *et al.*, 2015). 'Become Angry': "*How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say".* 'Punish Harder': "*How frequently does the following occur? I punish my child(ren) harder than what they merit.*"

Table 3. Intention-To-Treat Effects on Parental Well-Being and Negative Parenting Behavior

	Parental Well-Being			Negative Parenting Behavior	
	Life Satisfaction	Partnership Satisfaction	WHO-5	'Become Angry'	'Punish Harder'
	(1)	(2)	(3)	(4)	(5)
Panel A: Double Difference					
Year = 2020	-0.145*	-0.166**	-1.545	0.441***	0.090
	(0.084)	(0.079)	(1.697)	(0.080)	(0.076)
Eligibility × Year = 2020	-0.144	0.018	1.786	-0.180*	-0.117
	(0.116)	(0.114)	(2.226)	(0.107)	(0.097)
Constant	5.102***	5.133***	60.803***	2.503***	1.782***
	(0.029)	(0.029)	(0.555)	(0.027)	(0.024)
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*
N	588	588	588	588	588
Nr. of individuals	294	294	294	294	294
Nr. of households	217	217	217	217	217
Panel B: Triple Difference					
Eligibility × Year = 2020	0.141	-0.021	7.317**	-0.407**	-0.111
	(0.188)	(0.159)	(3.463)	(0.175)	(0.161)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,272	1,272	1,272	1,272	1,272
Nr. of individuals	636	636	636	636	636
Nr. of households	482	482	482	482	482
Panel C: Triple Difference with Gender Interaction					
Eligibility × Year = 2020	0.142	0.009	6.936*	-0.360*	0.002
	(0.196)	(0.165)	(3.584)	(0.187)	(0.156)
Father × Eligibility × Year = 2020	-0.004	-0.083	1.070	-0.130	-0.316**
	(0.159)	(0.131)	(2.751)	(0.137)	(0.127)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,272	1,272	1,272	1,272	1,272
Nr. of individuals	636	636	636	636	636
Nr. of households	482	482	482	482	482

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: Life satisfaction, as well as partnership satisfaction, are measured on a 6-point scale ranging from 1 "not at all satisfied" to 6 "very satisfied". The WHO-5 Well-being Index ranges from 0 "absence of well-being" to 100 "maximal well-being". 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say" (1 "never" to 6 "(almost) always").* 'Punish Harder': *"How frequently does the following occur? I punish my child(ren) harder than what they merit." (1 "never" to 6 "(almost) always").* Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents SR. Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4. Intention-To-Treat and LATE Effects on Negative Parenting Behavior for Parents with Children of Preschool Age or Younger. First-Differences Estimation.

Panel A	First Difference: 'Become Angry'				
	(1)	(2)	(3)	(4)	(5)
	Red. Form (ITT)	Red. Form (ITT)	OLS	IV(LATE)	IV (LATE)
Eligibility	-0.175*	-0.270**			
	(0.085)	(0.112)			
Usage			0.060	-0.842**	-0.816*
			(0.118)	(0.426)	(0.463)
Father × Usage					-0.071
					(0.410)
Constant	0.419***	0.426***	0.345***	0.582***	0.581***
	(0.046)	(0.062)	(0.062)	(0.128)	(0.129)
N	636	319	319	319	319
Rkf	.	.	.	16.15	8.40
Sample	A	B	B	B	B
Panel B	First Difference: 'Punish Harder'				
	(1)	(2)	(3)	(4)	(5)
	Red. Form (ITT)	Red. Form (ITT)	OLS	IV(LATE)	IV (LATE)
Eligibility	-0.062	-0.134			
	(0.072)	(0.090)			
Usage			0.044	-0.418	-0.132
			(0.101)	(0.313)	(0.289)
Father × Usage					-0.795**
					(0.343)
Constant	0.035	0.095*	0.051	0.173*	0.162
	(0.039)	(0.057)	(0.056)	(0.104)	(0.104)
N	636	319	319	319	319
Rkf	.	.	.	16.15	8.40
Sample	A	B	B	B	B

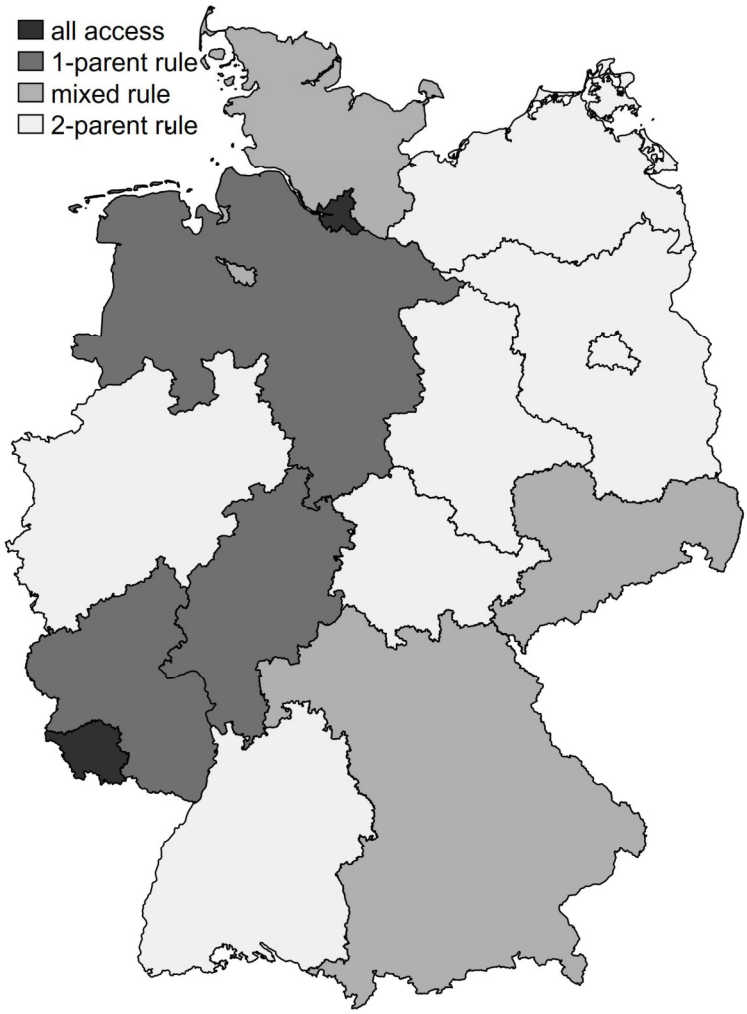
Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say" (1 "never" to 6 "(almost) always").* 'Punish Harder': *"How frequently does the following occur? I punish my child(ren) harder than what they merit." (1 "never" to 6 "(almost) always").* Columns 4 and 5: EC usage is instrumented by eligibility to EC during the acute lockdown based on parental SR constellation. Rkf: Kleibergen-Paap rk Wald F statistic. Sample A: families with children below age 12. Sample B: families with children of preschool-age or younger. Cluster-robust standard errors at household level.

*** p < 0.01, ** p < 0.05, * p < 0.1.

Appendix

Figure A.1. Emergency Childcare Policies in the German Federal States during the first COVID-19 Lockdown



Sources: Decrees or corresponding press releases by the respective federal state (see Table 1).

Table A.1. Exogeneity of EC Eligibility

	EC Eligibility
Q2 equivalence household income (ref.)	
Q2 equivalence household income	-0.061
	(0.053)
Q3 equivalence household income	0.019
	(0.059)
Q4 equivalence household income	-0.012
	(0.064)
Age mother	0.004
	(0.006)
Age father	-0.005
	(0.004)
Parent with university degree	0.052
	(0.045)
≤ 50.000 inhabitants	-0.031
	(0.046)
Migration background	-0.032
	(0.051)
More than 1 child in hh	-0.009
	(0.042)
Mean age children	0.000
	(0.009)
Share male children in hh	0.012
	(0.049)
At least one room per child	0.022
	(0.057)
Constant	0.290
	(0.177)
N	478

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2. Triple Difference. Intention-To-Treat Effects on Parental Well-Being and Negative Parenting Behavior. Standardized Outcomes.

	Parental Well-Being			Negative Parenting Behavior		
	Life Satisfaction	Partnership Satisfaction	WHO-5	'Become Angry'	'Punish Harder'	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility × Year = 2020	0.161	-0.020	0.429**	-0.423**	-0.141	0.002
	(0.215)	(0.153)	(0.203)	(0.182)	(0.205)	(0.198)
Father × Eligibility × Year = 2020						-0.401**
						(0.161)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,272	1,272	1,272	1,272	1,272	1,272
Nr. of individuals	636	636	636	636	636	636
Nr. of households	482	482	482	482	482	482

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: All outcomes are z-score rescaled to have a mean of zero and a standard deviation of one. Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents SR. Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3. Intention-To-Treat Effects on Positive Parenting Behavior (Child-Centered Communication)

	Double Difference		Triple Difference			
	‘Speak about Experiences,	‘Speak about Annoyances‘	‘Speak about Experiences,		‘Speak about Annoyances‘	
	(1)	(2)	(3)	(4)	(5)	(6)
Year = 2020	-0.161**	-0.320***				
	(0.072)	(0.090)				
Eligibility × Year = 2020	0.155	0.083	0.056	-0.056	0.117	-0.066
	(0.111)	(0.146)	(0.172)	(0.175)	(0.243)	(0.237)
Father × Eligibility × Year = 2020				0.336**		0.554**
				(0.156)		(0.246)
Constant	5.430***	5.258***	5.432***	5.430***	5.258***	5.257***
	(0.021)	(0.027)	(0.030)	(0.021)	(0.039)	(0.027)
SR constellation group × Year = 2020	*no*	*no*	yes	yes	yes	yes
State × Year = 2020	*no*	*no*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
N	552	552	1,186	1,186	1,185	1,185
Nr. of individuals	294	294	636	636	636	636
Nr. of households	217	217	482	482	482	482

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: ‘Speak about Experiences‘: *“How frequently does the following occur? I speak with the child about his/her experiences” (1 "never" to 6 "(almost) always").* ‘Speak about Annoyances‘: *“How frequently does the following occur? I speak with the child about things that annoy or burden him/her” (1 "never" to 6 "(almost) always").* AID:A 2019 surveyed both items child-specific for children above age two, whereas these items were surveyed parent-specific in the AID:A Corona Add-on 2020. We employ the mean across children for t = 2019. Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents SR. Cluster-robust standard errors at household level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.4. Triple Difference. Intention-To-Treat Effects on Parental Well-Being and Negative Parenting Behavior. Weighted Regressions

	Parental Well-Being			Negative Parenting Behavior		
	Life Satisfaction	Partnership Satisfaction	WHO-5	'Become Angry'	'Punish Harder'	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility × Year = 2020	0.310	0.096	3.710	-0.688***	-0.162	-0.084
	(0.210)	(0.173)	(3.633)	(0.223)	(0.179)	(0.181)
Father × Eligibility × Year = 2020						-0.254*
						(0.134)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,272	1,272	1,272	1,272	1,272	1,272
Nr. of individuals	636	636	636	636	636	636
Nr. of households	482	482	482	482	482	482

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: Results of weighted regressions based on a combination of AID:A design weights at the household level and "staying probability" weights at the individual level. Life satisfaction, as well as partnership satisfaction, are measured on a 6-point scale ranging from 1 "not at all satisfied" to 6 "very satisfied". The WHO-5 Well-being Index ranges from 0 "absence of well-being" to 100 "maximal well-being". 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say" (1 "never" to 6 "(almost) always").* 'Punish Harder': *"How frequently does the following occur? I punish my child(ren) harder than what they merit." (1 "never" to 6 "(almost) always").* Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents. Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.5. Triple Difference. Intention-To-Treat Effects on Parental Well-Being and Negative Parenting Behavior. Including "All-Access" States.

	Parental Well-Being			Negative Parenting Behavior		
	Life Satisfaction	Partnership Satisfaction	WHO-5	'Become Angry'	'Punish Harder'	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility × Year = 2020	0.171	-0.058	7.204**	-0.336*	-0.083	0.036
	(0.184)	(0.156)	(3.359)	(0.180)	(0.158)	(0.153)
Father × Eligibility × Year = 2020						-0.343***
						(0.123)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,318	1,318	1,318	1,318	1,318	1,318
Nr. of individuals	659	659	659	659	659	659
Nr. of households	498	498	498	498	498	498

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: This table displays results including observations from the federal states Hamburg and Saarland. Life satisfaction, as well as partnership satisfaction, are measured on a 6-point scale ranging from 1 "not at all satisfied" to 6 "very satisfied". The WHO-5 Well-being Index ranges from 0 "absence of well-being" to 100 "maximal well-being". 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say" (1 "never" to 6 "(almost) always").* 'Punish Harder': *"How frequently does the following occur? I punish my child(ren) harder than what they merit." (1 "never" to 6 "(almost) always").* Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents. Cluster-robust standard errors at household level.

*** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.6. Triple Difference. Intention-To-Treat Effects on Parental Well-Being and Negative Parenting Behavior. Regression-based Imputation.

	Parental Well-Being			Negative Parenting Behavior		
	Life Satisfaction	Partnership Satisfaction	WHO-5	'Become Angry'	'Punish Harder'	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility × Year = 2020	-0.035	0.082	6.078*	-0.351*	-0.068	0.064
	(0.193)	(0.181)	(3.586)	(0.179)	(0.171)	(0.161)
						-0.351**
						(0.138)
SR constellation group × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State × Year = 2020	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Individual FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
N	1,272	1,272	1,272	1,272	1,272	1,272
Nr. of individuals	636	636	636	636	636	636
Nr. of households	482	482	482	482	482	482

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: If missing, partner's SR status is predicted by regressing individual health-related SR (or non-health related SR, respectively) on occupation (KldB 2010, 3-digit) as stated in 2019 and state fixed effects. Life satisfaction, as well as partnership satisfaction, are measured on a 6-point scale ranging from 1 "not at all satisfied" to 6 "very satisfied". The WHO-5 Well-being Index ranges from 0 "absence of well-being" to 100 "maximal well-being". 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say" (1 "never" to 6 "(almost) always").* 'Punish Harder': *"How frequently does the following occur? I punish my child(ren) harder than what they merit." (1 "never" to 6 "(almost) always").* Systemic relevance (SR) constellation groups: (a) no parent SR, (b) one parent SR, not health-related, (c) one parent SR, health related, (d) both parents. Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.7. Triple Difference. Intention-To-Treat on 'Becoming Angry'. Dichotomized Outcome.

Panel A: 1= "never"; 0 otherwise	
Eligibility × Year = 2020	0.081
	(0.067)
N	1,272
Panel B: 1= "seldom" or "never"; 0 otherwise	
Eligibility × Year = 2020	0.161*
	(0.097)
N	1,272
Panel C: 1= "often", "very often" or "(almost) always"; 0 otherwise	
Eligibility × Year = 2020	-0.037
	(0.088)
N	1,272
Panel D: 1= "very often" or "(almost) always"; 0 otherwise	
Eligibility × Year = 2020	-0.092**
	(0.037)
N	1,272

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: Each panel represents results of separate regressions. All regressions include fixed effects as reported in Panel B of Table 3. 'Become Angry': *"How frequently does the following occur? I quickly become angry when my child(ren) don't do as I say."* Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.8. Complier Analysis

	Median	Sample share > median	Proportion of compliers: ≤ median	> median
	(1)	(2)	(3)	(4)
Population size of residence municipality (7 categories)	4 "20,000–50,000 inh."	0.36	0.66	0.34
At least one parent holds university degree (0/1)	0	0.44	0.72	0.31
Mother's weekly working hours (2019)	20	0.41	0.72	0.26
Age youngest child in household	3.17	0.48	0.65	0.37
Mother more than 50% remote work (2020)	0	0.30	0.75	0.24

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: We partition the IV sample (Sample B: families with a child of preschool-age or younger) sequentially by regional population size, parental education, mother's labor market involvement, and age of the youngest child (above and below median of each characteristic). Column (1) displays the median value of each characteristic. Column (2) reports the proportion of the sample that falls above the respective median value. Columns (3) and (4) show the distribution of compliers across the two subgroups (below or equal to the median and above median) for each characteristic. The proportion of compliers of a given type is calculated as the ratio of $\widehat{\delta}$ for that subgroup to the $\widehat{\delta}$ in the overall IV sample (Sample B), multiplied by the proportion of the sample in the respective subgroup.

Table A.9. LATE Effects on Parental Well-Being for Parents with Children of Preschool Age or Younger. First-Differences Estimation.

	First Differences:					
	Life Satisfaction		Partnership Satisfaction		WHO-5	
	(1)	(2)	(3)	(4)	(5)	(6)
Usage	0.110	0.049	0.309	0.355	0.133	3.578
	(0.402)	(0.456)	(0.446)	(0.478)	(7.656)	(7.897)
Father × Usage		0.171		-0.128		-9.593
		(0.407)		(0.360)		(7.055)
Constant	-0.277**	-0.275**	-0.157	-0.158	-0.587	-0.712
	(0.122)	(0.123)	(0.125)	(0.125)	(2.433)	(2.422)
N	319	319	319	319	319	319
Rkf	16.15	8.40	16.15	8.40	16.15	8.40
Sample	B	B	B	B	B	B

Source: AID:A 2019, AID:A Corona Add-on 2020; own calculations.

Notes: EC usage is instrumented by eligibility to EC during the acute lockdown based on parental SR constellation. Life satisfaction, as well as partnership satisfaction, are measured on a 6-point scale ranging from 1 "not at all satisfied" to 6 "very satisfied". The WHO-5 Well-being Index ranges from 0 “absence of well-being” to 100 “maximal well-being”. Cluster-robust standard errors at household level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.