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Abstract

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JEL Classification: D12, E21, E62, H24, H31

Keywords: N/A

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Spending effects of fiscal transfers in a pandemic*

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February 2023

Abstract

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Keywords: fiscal stimulus, household spending, marginal propensity to consume, pandemic, transfer.

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1 Introduction

Governments have successfully used direct transfers to households to stimulate consumption spending in economic downturns (Johnson et al., 2006; Parker et al., 2013). This policy intervention became particularly popular as a response to the Covid-19 pandemic (Gentilini, 2022). However, the estimated impact of transfers during ordinary recessions may be not useful for gauging the effectiveness of transfers during a pandemic. The Covid-19 recession differed markedly from earlier recessions because of the pandemic context surrounding it. Many measures taken to contain the spread of Covid-19 also inhibited spending possibilities and individuals voluntarily adjusted their consumption behavior to avoid infections (Goolsbee and Syverson, 2021).

In 2020, the German government enacted a fiscal stimulus package to counter the recessionary effects of the pandemic. Part of this package was the so-called child bonus, which consisted of three direct cash transfers to parents totaling €450 per child. In this paper, we estimate the impact of the child bonus on household spending and relate it to the macroeconomic and pandemic situation, exploiting both spatial and temporal variation. Our identification strategy exploits random variation in the payment dates, combined with scanner data on household consumption expenditure at the daily frequency. This allows us to control for other policy measures and macroeconomic conditions. We estimate the marginal propensity to consume for each of the three payments, which happened at different stages of the pandemic.

We use daily home scanner data from the "Gesellschaft für Konsumforschung" (GfK), which we link with a tailored survey conducted in January 2021. The GfK home scanner panel is comparable in structure to the Nielsen Consumer Panel but covers a wider range of goods as it also includes semi-durable products.[1] From the survey, we elicit the date

[1] Household scanner data has been used to study various questions, such as, for example, the measurement of inflation, the effect of government policies on consumption (Dubois et al., 2022), or the effect of monetary policy communication on household spending (Coibion et al., 2022).

of receipt of the regular child benefit in January 2021 and use this information to infer the dates of receipt of the child bonus in September 2020, October 2020, and May 2021. Since the payment dates are spread quasi-randomly within each month, we can compare the spending levels, on a given day, of two households that differ only in that one has received the child bonus, while the other has not. We estimate a highly significant effect of the first cash transfer on household spending, with a marginal propensity to consume (MPC) of about 12%. Extending the sample to up to three months after the transfer yields a higher MPC of 21%, but the estimate becomes less precise. We perform several robustness and placebo tests. When estimating daily spending effects before and after the receipt of the child bonus, we do not find differential trends before households receive the transfer. Moreover, we do not find an effect of the policy announcements on spending. Placebo estimations in 2019 also do not yield significant results.

Do pandemic-specific conditions influence the spending impact of the transfer? We find that the child bonus payments resulted in spending increases only in counties with a low Covid-19 incidence. Importantly, we neither find evidence for government-imposed restrictions impacting the spending effect nor can the differential effect be explained by local economic conditions. As a possible explanation for the sensitivity to the local infection rate, we find that the child bonus was mostly spent inperson on non-durable consumption goods. We do find evidence that households substitute in-person for online shopping when the local Covid-19 incidence is high. However, since the share of spending for online shopping is rather low, this substitution effect is limited in the aggregate.

Consistent with these results, we find no effect of the later two payments, which occurred in the context of considerably larger Covid-19 case rates, on household spending. This suggests that the effectiveness of cash transfers is limited when the risk of infection is high. Taking all these results together, we calculate an overall one-month MPC of 5% and a three-month MPC of 9%. One limitation of our home scanner data is that it does not contain information about spending on large durable items or services. If we were to assume that these spending categories were similarly affected, the MPC would rise to 14% in the short run and 25% in the medium run.

Last, we show that fiscal stimulus measures that increase economic activity also increase the number of contacts, which is a determinant of the infection rate. Specifically, since shopping is done mostly in person, the spending increase caused about 5% more shop visits, a proxy for contacts due to economic activity.

We contribute to the literature investigating the interaction between stabilization policies and the pandemic. Our results suggest that the marginal propensity to consume out of cash transfers may be muted due to high infection numbers. Furthermore, we show that spending resulting from the transfer itself can induce more contacts. This feedback effect, which features in integrated models of macroeconomic and epidemiological dynamics, limits the effectiveness of countercyclical fiscal policies during a pandemic (Eichenbaum et al., 2021; Kaplan et al., 2020). Related to our paper, Auerbach et al. (2022) find that defense spending by the US government during the Covid-19 pandemic had a smaller effect on employment in areas subject to stay-at-home orders, and failed to raise consumption. They consider a different policy, government contracts with private firms instead of cash transfers, which mainly works through an employment channel.

Furthermore, we contribute to the general literature on the spending effect of cash transfers during the Covid-19 pandemic. While most papers focus on the Economic Impact Payments (EIP) paid to US households, we provide evidence for cash transfers in Germany. An important distinction is the institutional context as Germany offers a relatively generous social safety net in comparison to the US. Indeed, across several data sets, only a small share of German households report being liquidity-constrained.[2] This

[2] In the Bundesbank Online Panel of Households, a representative German household survey, only 3% of households report in July 2020 that they could not borrow to cover their expenditures next month, and an additional 5% reported that they may have to borrow to cover their expenditures. This is also consistent with earlier evidence from the 2017 wave of the German Panel on Household Finances.

could be one of the reasons why our estimated overall MPC between 9% and 25% is on the lower end of the range of estimates reported in the literature. Our results are quite similar to Parker et al. (2022), who find a marginal propensity to consume of about 10% for non-durable goods and services in the first two rounds of the EIP, whereas the third round did not increase spending. In contrast, Baker et al. (2023) and Karger and Rajan (2021) estimate larger spending responses to the same stimulus payments in the US, with MPCs of 25% and 44%, respectively. Similar to our findings, many studies show that liquidity-constrained households react more strongly to cash transfers (Parker et al., 2022; Baker et al., 2023; Karger and Rajan, 2021; Chetty et al., 2020). Therefore, a possible explanation for these higher MPC estimates is the higher proportion of liquidity-constrained households in the latter two studies.

We also contribute to the literature that analyzes the spending responses to payments of different magnitudes. Most of the studies on cash transfers address the effect of much larger payments (Economic Impact Payments amounted to \$1300 per adult and \$500 per child), whereas the child bonus amounted to a more modest payment of €450 per child. The literature on the relationship between transfer size and MPC is quite mixed (Scholnick, 2013; Christelis et al., 2019; Fuster et al., 2020). We do not find a difference in the MPC between households receiving smaller or larger transfers (depending on the number of children).

Last, we contribute to the debate about the success of the German fiscal stimulus package.[3] As discussed above, we find a significant spending effect only for the first payment of the child bonus and no response for the second and third payments. In contrast, Bachmann et al. (2021) report that the VAT cut stimulated consumption by €34 billion, mainly by increasing purchases of durable goods, which can easily be bought

[3] There are two German policy papers looking at the child bonus. Behringer et al. (2021) rely on households self-reporting their counterfactual consumption, while Bachmann et al. (2022) compare households with more or fewer children in repeated cross-sectional survey data at the monthly level.

online. Moreover, their estimated spending response is not sensitive to the local Covid-19 incidence. This insight might explain our result that the later child bonus payments were less effective in stimulating consumption than the first one. The transfer was spent mainly on non-durable consumption goods that tend to be bought in person and, therefore, carry a higher risk of infection. Then, as infection rates rose over time, spending on such goods was reduced.

The rest of the paper is structured as follows. In Sections 2 and 3, we lay out the institutional context and describe the data set, paying particular attention to the characteristics of households with and without children. Section 4 explains our empirical strategy. Section 5 describes and discusses our findings. Finally, Section 6 concludes.

2 Institutional context

In the following, we first describe how the child bonus transfer works and how it is related to the other parts of the German fiscal stimulus package. Then, we explain why the Covid-19 pandemic affected families with children disproportionately.

Modalities of the transfer. The German child bonus was a direct transfer to families on top of the regular child benefit, amounting to €200 per child in September 2020, €100 per child in October 2020, and €150 per child in May 2021. Since the average household receiving the transfer has 1.5 children, the average total transfer amounts to 675€. The payment date of the regular child benefit and the child bonus is determined by the last digit of the child benefit number. The last digit is assigned countrywide on an ongoing basis to households that apply for the child benefit for the first time. Therefore, it is the same for all children living in the same household. Thus, the last digit is effectively randomly assigned and the payment date is determined purely by chance depending on the timing of the household's first-ever application for the child benefit.

The child bonus was paid by bank transfer to recipients, without the need for parents to apply for it. This is quite similar to the US Economic Impact Payments, most of which were also paid via bank transfer, and only 22% of the recipients were sent the payment via check (Parker et al., 2022). The announcement of the child bonus payments in early June 2020 received a lot of media attention. As Appendix Figure A.1 shows, there was also a spike in Google searches when the policy was announced. There is a second, somewhat smaller, spike in September, the month of the first payment. It is worth noting that the May 2021 tranche was only announced in early February 2021. The second announcement and the May 2021 payments show similar, but smaller spikes in interest.

The tax treatment of the child bonus implies that rich households ultimately do not benefit financially from receiving the child bonus. For each child, the tax authorities compare the financial benefit of the child benefit and bonus to the financial benefit of the child tax allowance.[4] If the latter exceeds the former, the household has to (partially) repay the child bonus in their tax declaration. About 80% of eligible households benefited in full from the child bonus and another 10% benefited at least in part.[5] In a robustness check, we drop households for whom, based on their income, marital status, and the number of eligible children, the child bonus does not raise their after-tax income.

Covid-19 fiscal policy package. The German policy response to the pandemic included several other initiatives in addition to the child bonus, such as an extension of short-time work, financial assistance to firms, and a temporary VAT cut. The total cost of the child bonus of about €6.4 billion is modest relative to the overall fiscal policy package.[6] The explicit aim of the child bonus was not only to support families and children

[4] The child tax allowance (*"Kinderfreibetrag"*) refers to an additional tax allowance based on the number of dependent children living in the household.

[5] More details on the tax treatment of the child bonus can be found at this link.

[6] The initial September and October payments were estimated to cost €4.3 billion. More details on the package can be found at this link.

but also to provide an impulse to private consumption.[7]

Impact of Covid-19 on families. There is evidence that families were particularly hard hit by the pandemic. Households with children experienced greater income losses and other forms of economic hardship than households without children (Armantier et al., 2020). We confirm this finding in the German context. In Appendix B, we show that households with dependent children are significantly more likely to report income losses at the start of the pandemic than households without dependent children. Furthermore, school closures had adverse consequences for families. First, the loss of schooling implies a substantial drop in lifetime earnings for affected children (Fuchs-Schündeln et al., 2022; Fuchs-Schündeln, 2022). Second, over a fifth of US parents reported losing a job or income due to a lack of child care (Muñoz-Rivera et al., 2021). Third, school closures constituted a major social shock, often leading to negative health outcomes for both parents and children (Kalil et al., 2020).

3 Data

To cleanly identify and measure the effect of the child bonus transfers on household spending, we commissioned a survey, which was run in January 2021, to almost 11000 households who participate in the longitudinal home scanner panel of the GfK (*Gesellschaft für Konsumforschung*). We then combine the information from the survey, which allows us to identify the timing of the transfer receipt, with the home scanner data on household spending patterns.

The GfK home scanner panel is a large panel of households that use a home scanner for product barcodes to document their spending behavior. It contains data not only on the items purchased and their prices but also about the shops that were visited or

[7] See, for example, the statement of the German finance minister at the press conference announcing the policy.

online purchases made. Importantly for our analysis, the data is available at a daily frequency. As households take part in the panel typically for long periods of time, the panel dimension of the data allows us to compare household spending both before and during the pandemic. More specifically, our sample runs from January to December 2019 and from July 2020 to June 2021. Additionally, we have data on semi-durable goods for the first half of 2020.

The data distinguish between spending on non-durable goods such as food items, and semi-durable goods, such as small household items, books, or electronics. Information on both types of spending is available for almost 90% of the households, which we use as our baseline sample. Among those, non-durable spending accounts for about two-thirds of overall recorded expenditures. We can also differentiate between in-person and online shopping. The latter only plays a minor role in the context of non-durable and semi-durable consumption goods, as it makes up only about 12% of those spending categories in our sample period. Furthermore, we calculate the number of shops visited per day and use it as a proxy for contacts due to economic activity. We also drop all households that report no spending in the months in which the child bonus was paid out (0.5% of the sample). In our baseline analysis, we exclude the bottom and top 1% of the spending distribution to account for outliers. The final sample for our analysis contains about 9200 households, around 17% of which have children eligible for the child bonus. Summary statistics on the spending data can be found in Appendix Table A.1.

The GfK regularly collects demographic information, such as household demographics, composition, and income. Information on households' wealth level and their financial situation, including borrowing constraints, was collected from the commissioned survey. Furthermore, we asked households for their self-assessed analytical skill and financial literacy. Importantly, using the survey, we determine the eligibility for the child bonus as well as the date of receipt. All survey questions, their exact wording, and their translation

Table 1: Identification using randomized payment dates

(1) last digit of child benefit number	(2) child benefit in January 2021	(3) child bonus in September 2020	(4) child bonus in October 2020	(5) child bonus in May 2021
0	05.01.2021	04.09.2020	05.10.2020	05.05.2021
1	08.01.2021	07.09.2020	07.10.2020	06.05.2021
2	11.01.2021	08.09.2020	08.10.2020	07.05.2021
3	12.01.2021	09.09.2020	08.10.2020	10.05.2021
4	13.01.2021	10.09.2020	12.10.2020	11.05.2021
5	14.01.2021	11.09.2020	14.10.2020	12.05.2021
6	15.01.2021	14.09.2020	15.10.2020	17.05.2021
7	18.01.2021	16.09.2020	16.10.2020	18.05.2021
8	19.01.2021	18.09.2020	19.10.2020	19.05.2021
9	21.01.2021	21.09.2020	21.10.2020	21.05.2021

Source: www.arbeitsagentur.de/familie-und-kinder/auszahlungstermine, and www.arbeitsagentur.de/familie-und-kinder/kinderbonus.

are listed in Appendix C.

Identification. To identify and isolate the effect of the transfer on household expenditure, we exploit the fact that payment dates are quasi-randomly assigned. Given that the spending data are at the daily frequency, we can then link the payment dates to the change in spending by the household. As explained in Section 2, the allocation of the last digit of the child benefit number, which determines the payment date, is an effectively random process. One exception from the rule is a subset of public sector employees who receive the child benefit with their salary in the middle or in the beginning of the month. We drop all public sector employees in a robustness check.

Due to strict data protection rules, we were not able to ask directly about the child benefit number. However, from our survey, we obtain the payment dates of the regular child benefit in January 2021, see column (2) of Table 1.[8] This information allows us to identify, for each eligible household, the last digit of the child benefit number displayed in column (1). We assume a two-day lag between the day the payment is made and when it is booked on a household's bank account. Then, we use the mapping in columns

[8] We exclude households that report implausible payment dates such as dates before the first payment date or more than four days after the last payment date (3% of the sample).

(3) to (5) of Table 1 to infer the payment date of the child bonus in September 2020, October 2020, and May 2021. There is a period of 16 to 17 days between the first and last payment. Generally, the higher the last digit of the child benefit number, the later the payment is issued.

In Appendix Table A.2, we test the randomness assumption of the payment dates by regressing the child benefit number on observable characteristics. This allows us to investigate whether certain household characteristics predict earlier or later payment. The results confirm that the child benefit number is not significantly related to any observable demographic or economic household characteristic. We also do not see a relation between the child benefit number and household spending in August 2020, one month before the treatment occurs.

Household and county characteristics. In Appendix Table A.3, we provide summary statistics of two sub-samples, households with and without children. On the one hand, households with children in our sample are on average younger, more often headed by a female, and have lower monthly net income per capita. On the other hand, they report a somewhat higher level of net wealth and are less likely to be single households. The proportion of households living in East Germany and of those with a college degree is similar between the two groups. Interestingly, we find that only a very small share of households in our data report they are financially constrained. It is remarkably similar among households with children (0.08%) and without children (0.07%). Also, financial literacy and analytical skills seem to be similar among these two groups. The average household that is eligible for the child bonus has 1.5 eligible children and is thus receiving €450 in 2020 and €225 in 2021. Since the average monthly net household income in our sample is about €2500, the child bonus represents about 18% and 9% of monthly net income in 2020 and 2021, respectively.

We observe the county of residence in our baseline sample, allowing us to add fine-grained fixed effects and match local economic and pandemic-related variables. First, we obtain information on the county unemployment rate and the share of the labor force that is in short-time work at the monthly frequency from the Federal Employment Agency. Second, we match daily Covid-19 case rates provided by the German public health authority. In particular, we calculate the Covid-19 case incidence, i.e. the number of newly reported infections in the last seven days at the county level per 100000 inhabitants. Third, we have a daily stringency index of the restrictions in place at the county level provided by the Federal Ministry for Economic Affairs and Energy. It includes information on 23 subcategories of potential restrictions, such as, for example, the closing of elementary and high schools, childcare facilities, retail shops, restaurants, mask mandates, nighttime curfews, and social distancing requirements, and is modeled after the Oxford stringency index (Hale et al., 2020). Within each subcategory, measures are ordered on an ordinal scale and summarized in a sub-index from 0 to 100. We make use of the aggregated stringency index, which is the average of all sub-indices.[9] Appendix Table A.4 shows summary statistics for the county-level variables.

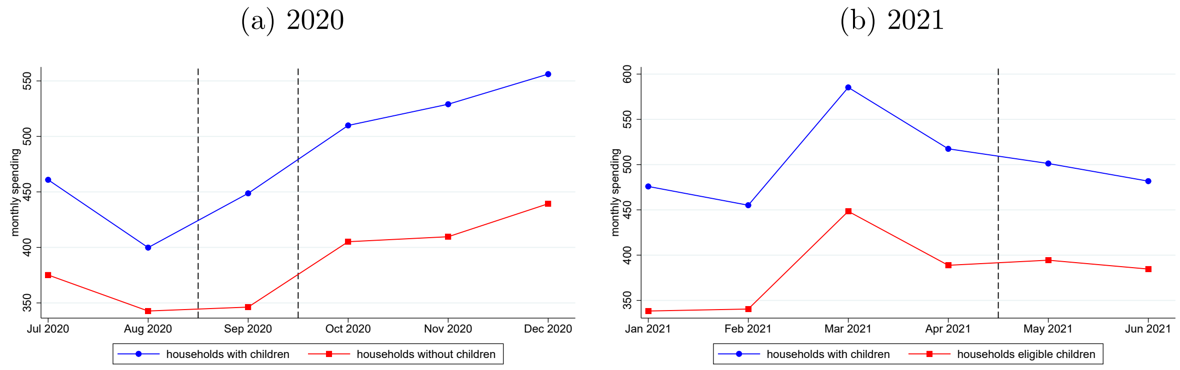
4 Empirical strategy

Aggregate spending over time. We start by showing descriptive patterns in the data before explaining our more rigorous empirical strategy. Figure 1 shows total household spending, for households with and without children, at the monthly frequency during the second half of 2020 and the first half of 2021.

While households with and without children are on similar spending trends before September 2020, we see an uptick in spending for households with eligible children in September (see Figure 1a). No such change is visible for households without eligible

[9] More information on the construction of the stringency index can be found here.

Figure 1: Monthly spending by households with and without children



Notes: This figure plots average monthly expenditures for German households with and without children eligible for the child bonus from July until December 2020 (Panel a) and January until June 2021 (Panel b). The dotted lines indicate the months in which the child bonus was paid out (September and October 2020, May 2021).

children, where the amount of spending is flat between August and September. The change in spending between September and October 2020, in contrast, is similar for the two types of households. In 2021, the two groups move in parallel for the whole period with no clear impact of the May 2021 payment (see Figure 1b). This is the first indication that, at the aggregate level, the September 2020 tranche of the child bonus had a noticeable impact on spending, while the October 2020 and May 2021 tranches did not.

While this monthly pattern is already suggestive, we use daily variation in spending to identify the impact of the child bonus on household spending. To do so, we use both an event study and a difference-in-difference regression approach. In the following, we describe how we estimate the spending effect of the child bonus more systematically.

Estimating the marginal propensity to consume. We use a difference-in-difference design to directly estimate the marginal propensity to consume out of the child bonus. We start by estimating the following empirical specification on our sample of households with and without children:

$$y_{it} = \alpha_i + \gamma_t + \beta Treat_i Post_{it} + \delta X_{ct} + \varepsilon_{it}, \qquad (1)$$

where y_{it} is normalized spending by household i on day t. Following Parker et al. (2022), we define normalized spending as the daily spending of household i on day t divided by the average daily spending of household i in the sample period. This allows us to interpret our estimates in percentage terms without using transformations that have been shown to be scale-dependent (Mullahy and Norton, 2022; Chen and Roth, 2023).[10] α_i is a household fixed effect, controlling for all time-constant characteristics of households, and γ_t are date fixed effects, which control for both aggregate economic and pandemic conditions. $Treat_i$ is a dummy that equals 1 if household i is eligible for the child bonus and $Post_{it}$ is a dummy that equals 1 if household i has already received the child bonus at date t. The coefficient β then identifies the average daily spending response of households after receiving the child bonus. We always cluster the error term ε_{it} at the household level. Next, we include additional fixed effects and time-varying control variables in equation (1). First, X_{ct} includes the 7-day Covid-19 incidence and the stringency index in county c at date t. Second, we include county-date fixed effects γ_{ct} that restrict our variation to households that live in the same county, effectively controlling for local economic and pandemic conditions. Our most comprehensive specification goes one step further by allowing both the local Covid-19 incidence and stringency index to affect households with and without children differently:

$$y_{it} = \alpha_i + \gamma_{ct} + \beta Treat_i Post_{it} + \delta Parent_i X_{ct} + \varepsilon_{it}. \tag{2}$$

We convert β into the marginal propensity to consume out of the child bonus in the following way. First, we calculate the cumulative percent effect by multiplying β, the average daily effect, by the average post-treatment duration in our estimation sample. Next, we multiply the cumulative effect by the mean spending level to get the spending response in terms of €. Last, we divide the spending response by the average child

[10] All results are robust to using the inverse hyperbolic sine transformation of y or just using y in levels.

bonus amount received, which is the average number of eligible children multiplied by the transfer amount per child. This calculation assumes that households only react to the new child bonus when they receive it, but do not react to the regular child benefit payments. These ongoing payments that households receive on a monthly basis are likely already anticipated and, therefore, households spend the money independently of the day of receipt. We test and verify this assumption by estimating equation (1) in 2019, one year before the introduction of the child bonus.

Daily spending responses. We also estimate the daily spending response to the child bonus to test for parallel trends between households that did not receive the child bonus (yet) and those who received it. We do so by estimating the following empirical specification on our sample of households with and without children:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=-\underline{k}, k \neq -1}^{\overline{k}} \beta_k D_{it}^k + \varepsilon_{it}, \tag{3}$$

where D_{it}^k is a dummy indicating that the payment of the child benefit for household i on day t occurred $k \in [-\underline{k}, ..., \overline{k}]$ days ago. We bin the endpoints of the effect window, $\underline{k} = -5$ and $\overline{k} = 13$, so as to capture the long-term effect before and after the effect window (Schmidheiny and Siegloch, 2023). This design enables us to test for flat pre-trends ($k \leq -1$) and estimates the adjustment paths of the post-treatment effect ($k \geq 0$). All other estimates are to be interpreted relative to the pre-treatment day $k = -1$, whose coefficient is normalized to zero.

Recent literature emphasizes that (static and dynamic) difference-in-difference designs with differential treatment timing estimated in a two-way fixed effects model can be biased in the presence of heterogeneous treatment effects (Sun and Abraham, 2021). Therefore, we use the estimator proposed by Sun and Abraham (2021), which yields an unbiased estimate even when treatment effects are not homogeneous.

Announcement effect. The September and October payments of the child bonus were announced by the German government on 3rd June 2020. The third tranche was announced on 2nd February 2021. Given that the announcement precedes the implementation by several months, some households may spend a portion of the child bonus in anticipation, i.e. before receiving it. We test for this announcement effect by comparing households with and without eligible children before and after both announcements. For this exercise, we estimate the following regression equation:

$$y_{it} = \alpha_i + \gamma_{ct} + \beta Treat_i Announcement_t + \varepsilon_{it}, \tag{4}$$

where $Treat_i$ is a treatment dummy that equals 1 if household i is eligible for the child bonus, $Announcement_t$ is a dummy that equals 1 if the announcement has already happened, and ε_{it} is an error term clustered at the household level.

5 Results

Table 2 shows that the marginal propensity to consume out of the child bonus, depending on the fixed effects and controls included, lies between 11.1% and 12.1%. While this effect is highly significant at the 1% level, it is moderate in size. Note that this is a short-term estimate which includes roughly one month of spending after receiving the transfer. We also estimate the marginal propensity to consume two and three months after receipt. As Appendix Table A.5 shows, the marginal propensity to consume increases somewhat in size, but estimates become less precise over time. The three-month MPC is about 21%, but only marginally significant.

Figure 2 plots the daily spending effects estimated with the estimator by Sun and Abraham (2021) before and after receipt of the transfer payment in September 2020, together with 95% confidence bands. While there is no significant difference in the trend before the child bonus after the receipt, total spending exhibits an increase of about 15%,

Table 2: Marginal propensity to consume: baseline

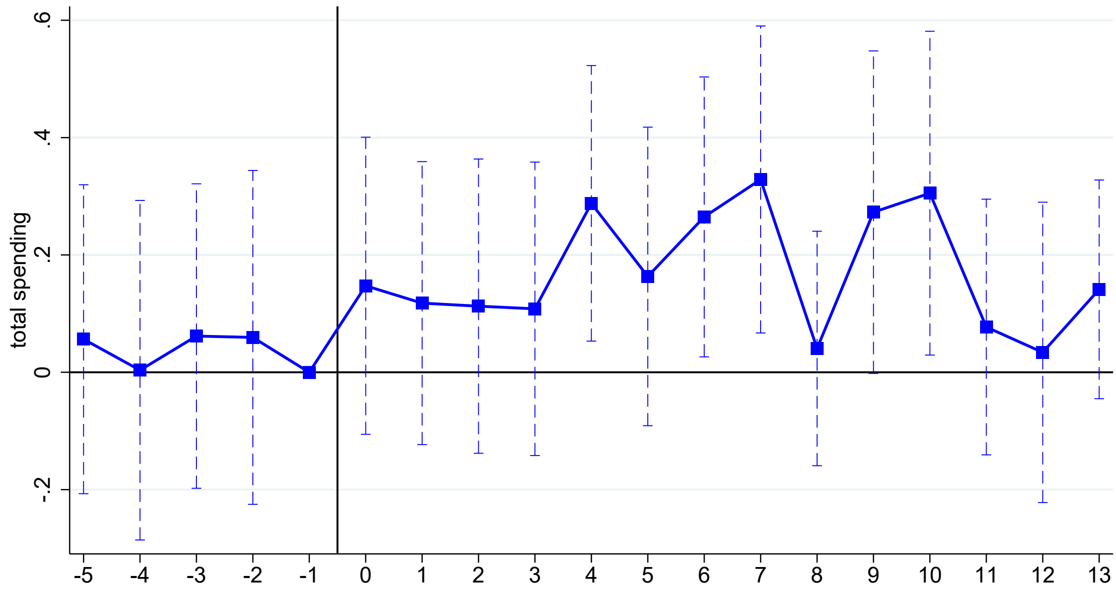
	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.105***	0.106***	0.109***	0.099***
	(0.028)	(0.028)	(0.029)	(0.031)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.117***	0.118***	0.122***	0.111***
	(0.031)	(0.031)	(0.032)	(0.034)
N	271530	271530	271500	271500
# cluster	9051	9051	9050	9050

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

which increases to 30%. Total spending remains higher for the post-treatment period and becomes significantly different from zero about four days after receipt. Results are very similar when we use the traditional two-way fixed effect estimator, suggesting that heterogeneous treatment effects do not play a major role in our setting (see Appendix Figure A.2). These results are consistent with a large literature documenting that the timing of income receipts matters for household spending decisions. Vellekoop (2018) examines data from the US Consumer Expenditure Survey and finds that spending on food and non-durables is linked to the timing of rent and mortgage payment dates. Other studies have found that spending is influenced by the dates of regular income payments, the so-called pay-day effect. Stephens (2003) reports that both the amount and probability of making expenditures increase immediately following the receipt of a Social Security payment.

An important question when interpreting our results is whether the estimates are picking up the response to regular child benefit payments. In other words, does the Covid-related cash transfer lead to different spending behavior than the income derived from the regular, predictable child benefit that is independent of the state of the business cycle? To answer this question, we estimate the same model for all months in 2019, where

Figure 2: Daily effects on total spending



Notes: This figure plots point estimates and 95% confidence bands from estimating equation (3) using the event study estimator by Sun and Abraham (2021) with normalized total spending as an outcome.

parents received the normal child benefit, but no child bonus. As Appendix Figure A.3 shows, household spending was not significantly higher after the receipt of the regular child benefit payment in any of the months in 2019. The average placebo marginal propensity to consume is very close to zero and insignificant. This suggests that the usual child benefit seems to be anticipated and already planned for by the households. In principle, this could also be the case for the extraordinary child bonus. Therefore, we test whether the announcement of the policy already had an effect by estimating equation (4) for both the July 2020 and the February 2021 announcements. Appendix Table A.6 shows that neither announcement had a significant effect on spending.[11]

Robustness checks. We subject our results to a number of robustness checks. First, we exclude households that, based on their income, marital status, and the number of kids, likely do not benefit financially from the child bonus since they have to repay it as

[11] We only have spending data on semi-durable goods for June 2020, but the results are similar for total spending in February 2021.

described in Section 2. As Table A.7 shows, this does not change our results much since they make up only about 9% of our sample. Next, we include both households that have extremely low and extremely high spending amounts by keeping the bottom and top 1% of the spending distribution in September. Again, the marginal propensity to consume is almost unchanged (see Appendix Table A.8). We also use alternative transformations of our outcome variable. When using spending in levels, or an inverse hyperbolic sine transformation, the MPC estimates are similar to our baseline (see Appendix Tables A.9 and A.10). To check whether measurement error in our treatment variable is playing a role, we drop households who stated to be unsure about the payment date of their regular child benefit (see Appendix Table A.11). This increases the size of the MPC to between 15% and 17%, consistent with slightly reduced measurement error. Furthermore, we cluster standard errors on the county level instead of the household level to allow for arbitrary correlation of the error terms within counties. This does not change inference (see Appendix Table A.12). We also test whether our effects are driven by any group that received the payment on a particular date. In Appendix Figure A.4, we show estimates of the MPC when dropping one payment group at a time. This does not change our results significantly for any of the groups. The same holds when we drop all public sector employees (see Appendix Table A.13). Last, we show in Appendix B.2 that the child bonus did not have an effect on the labor supply of households.

Treatment heterogeneity. When we disaggregate the data by spending categories, we see that the effect is entirely driven by non-durable consumption goods (see Table 3). Our results are consistent with Misra and Surico (2014), who find that most of the effect of the positive income shock due to a tax change is attributed to non-durable consumption. Interestingly, online spending increases disproportionately relative to in-person spending, but given its low share in total spending, it accounts only for a minor part of the overall

Table 3: Marginal propensity to consume: goods categories

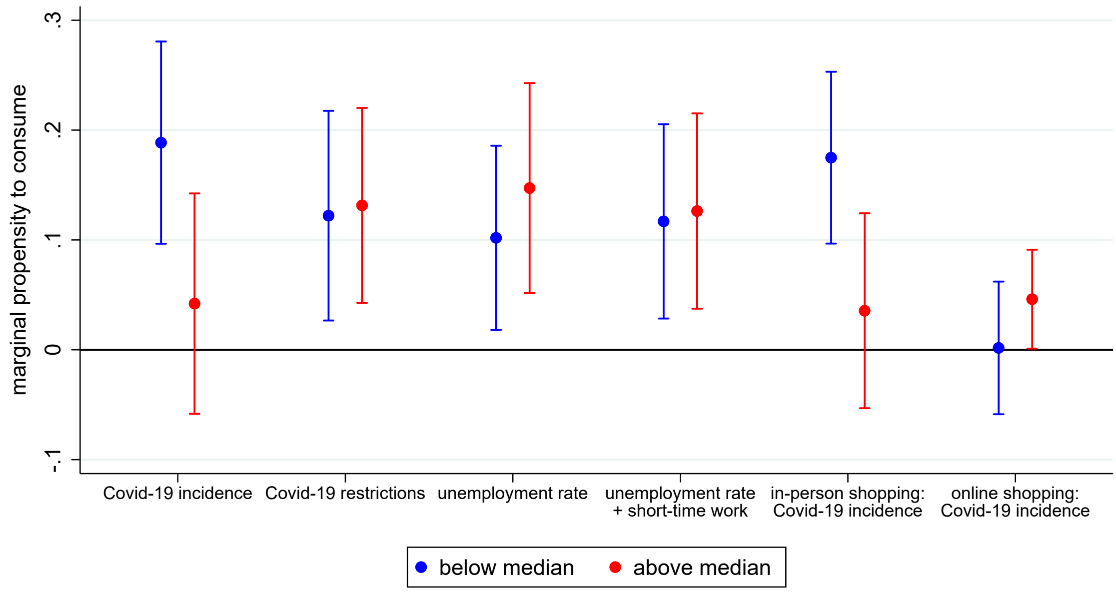
	(1) spending: semi-durables	(2) spending: non-durables	(3) spending: in-person	(4) spending: online
Treat x Post	0.010	0.106***	0.113***	0.212
	(0.066)	(0.027)	(0.028)	(0.142)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.004	0.081***	0.119***	0.028
	(0.028)	(0.021)	(0.030)	(0.019)
N	195120	270240	271260	55560
# cluster	6504	9008	9042	1852

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using normalized daily spending on semi-durables (column (1)), non-durables (column (2)), in-person shopping (column (3)) and online shopping (column (4)) as outcomes. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in the respective spending category divided by the average transfer amount (€200 times the mean number of children).

MPC (see Table 3).

Next, we estimate heterogeneous treatment effects for counties at different stages of the pandemic as well as in different macroeconomic conditions. As Figure 3 shows, when the Covid-19 incidence is above the median, the marginal propensity to consume out of the child bonus becomes very small and statistically insignificant. The difference in the MPC between counties with low and high case rates is statistically different at conventional confidence levels (see Appendix Table A.14). This is not driven by stricter restrictions as the MPC does not vary systematically with the Covid-19 restriction index. The difference between areas with high and low Covid-19 case rates is also not related to the broader local economic conditions as measured by the unemployment rate or the use of short-time work as we do not find heterogeneous effects along these dimensions (see Figure 3). In Appendix Table A.15, we include all interactions in the same model and only the Covid-19 incidence generates a statistically significant difference. This result is consistent with Goolsbee and Syverson (2021), who use cellphone data to show that only a small share of the drop in consumption was driven by policy measures and that individual choices play a larger role. Indeed, we find evidence for a substitution effect toward online

Figure 3: Heterogeneity of marginal propensity to consume: county characteristics

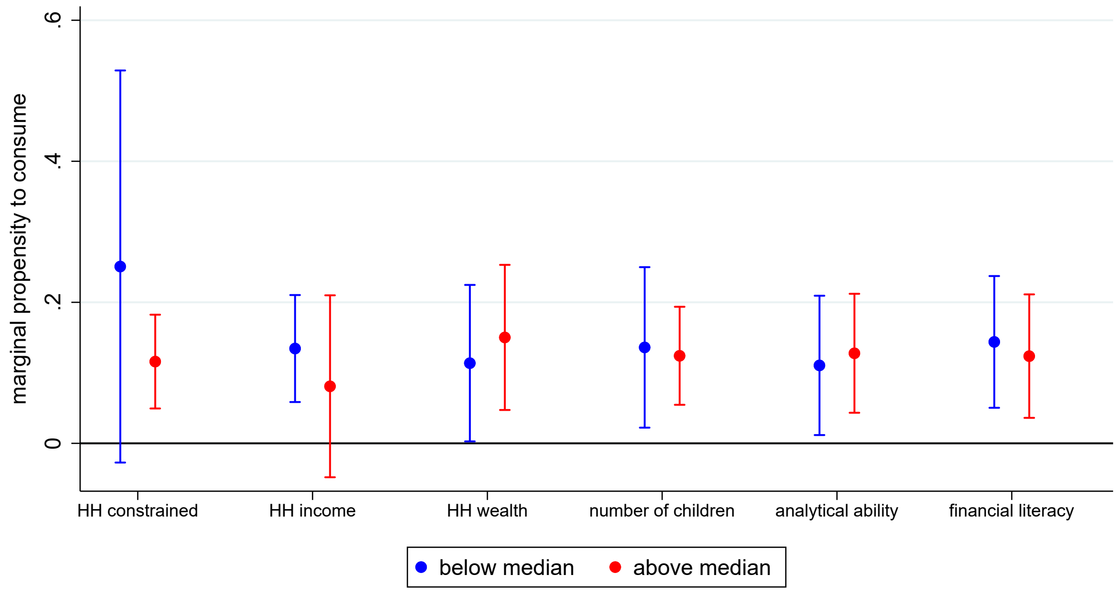


Notes: This figure plots heterogeneous effects and their 95% confidence bands for the marginal propensity to consume in different sample splits at the county level. Detailed regression results and p-values for the difference between the two samples can be found in Appendix Table A.14.

shopping in counties with high case numbers as the smaller MPC in counties with high case numbers is entirely driven by in-person shopping (see Figure 3). Conversely, we find the opposite result for online shopping, which increased only in counties with a high Covid-19 incidence. Taken together, these results suggest that individuals voluntarily restrict their economic activity when cases are high and, therefore, the impact of the child bonus is muted.

The literature has generally found that MPCs are higher for poorer and liquidity-constrained households (Parker et al., 2013; Jappelli and Pistaferri, 2014; Bounie et al., 2020). Therefore, we estimate the model separately for households with below-median wealth, below-median income, and self-reported liquidity constraints. The results are shown in Figure 4. We find that households with self-reported liquidity constraints have an MPC of 25%, which is more than twice our baseline estimate. However, one has to keep in mind that only 7% of the households in our sample are liquidity-constrained, which helps to explain the relatively small MPC estimate in Table 2. Low-income households

Figure 4: Heterogeneity of marginal propensity to consume: household characteristics



Notes: This figure plots heterogeneous effects and their 95% confidence bands for the marginal propensity to consume in different sample splits at the individual level. Detailed regression results and p-values for the difference between the two samples can be found in Appendix Table A.16.

exhibit a somewhat larger MPC than high-income households. Last, wealth does not appear to be a determinant of the marginal propensity to consume as the two estimates are very similar. None of the differences are statistically significant at conventional levels (see Appendix Figure A.16 for the p-values). Next, we investigate whether households with more than one child, which therefore receive a higher transfer, have a different MPC from households with only one child. The estimated MPCs are virtually the same, which implies that transfer size does not influence the MPC. Last, we also do not find heterogeneity in terms of households' self-assessed analytical skills and financial literacy (see Figure 4). Therefore, individual characteristics, even though acting as one would expect ex-ante, seem to have played a minor role in explaining our treatment effect.

October 2020 and May 2021 payments. Next, we evaluate the second and third payments of the child bonus in October 2020 and May 2021. Our earlier finding of significant spending increases after the first payment in September contrasts with the

Table 4: Marginal propensity to consume: October 2020 & May 2021

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Panel A: €100 per child payment in October 2020				
Treat x Post	0.000	0.001	0.006	-0.025
	(0.026)	(0.026)	(0.027)	(0.032)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.001	0.002	0.015	-0.061
	(0.064)	(0.064)	(0.066)	(0.079)
N	280612	280612	280581	280581
# cluster	9052	9052	9051	9051
	(5) total spending	(6) total spending	(7) total spending	(8) total spending
Panel B: €150 per child payment in May 2021				
Treat x Post	0.006	0.005	0.000	-0.004
	(0.026)	(0.026)	(0.026)	(0.033)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.010	0.008	0.001	-0.006
	(0.043)	(0.043)	(0.044)	(0.055)
N	261764	261764	261733	261733
# cluster	8444	8444	8443	8443

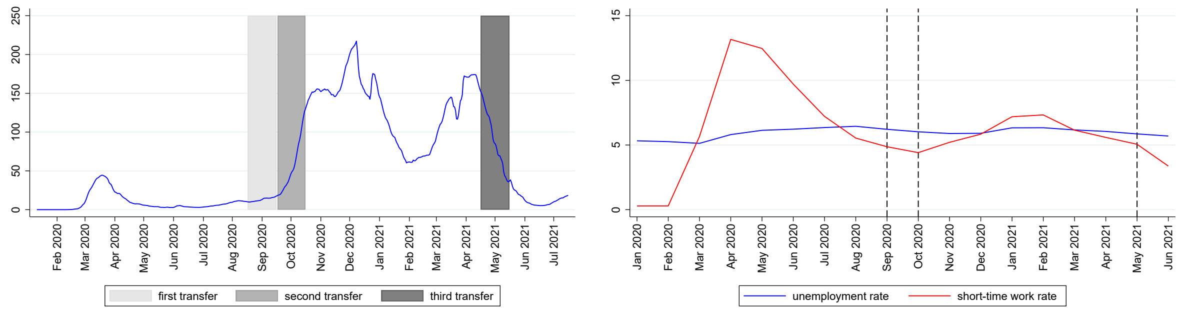
Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full October 2020 sample (Panel A) or the full May 2021 sample (Panel B) using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€100 in Panel A and €150 in Panel B times the mean number of children).

results obtained for the second and third payments of the child bonus. Table 4 shows that both the second and third transfers had no significant effect on spending. This implies that the overall spending effect of the child bonus was comparatively low. We calculate an overall marginal propensity to consume of 5.4% one month after the transfer receipt, which rises to 9.3% three months after the transfer receipt.[12] Note that we do not have data on services and durable goods and hence cannot estimate the effect on these spending categories. However, even if we were to assume that the effect was the same for

[12] This follows from the product of the cost share of the first payment and the MPC estimates from Appendix Table A.5.

Figure 5: Pandemic and macroeconomic trends over time

(a) Covid-19 incidence (b) Labor market



Notes: This figure plots the Covid-19 incidence per 100000 inhabitants (Panel a) as well as the unemployment rate and the share of the labor force in short-time work (Panel b). The dotted lines indicate the payment dates in September 2020, October 2020, and May 2021, respectively.

these goods, we arrive at an overall one-month (three-month) MPC of 14.2% (24.5%).[13] Given that the low overall MPC is mainly driven by the absence of a spending response in the latter two payments, we investigate the possible reasons for these null effects. A natural hypothesis is that these payments took place in a different pandemic and macroeconomic environment and therefore yielded different effects. We can relate these different hypotheses to our heterogeneity analysis of the September 2020 payment. Given that we find that the MPC is very small and statistically insignificant in counties with a high Covid-19 incidence, we would predict that the overall MPC should be smaller if case numbers are significantly higher. Figure 5a shows that the Covid-19 incidence in October 2020 and in May 2021 was up to an order of magnitude larger than in September 2020. Only 13% (0.5%) of respondents live in counties with a Covid-19 incidence in October (May) lower than the September median, i.e. the subset of counties for which we find a significant effect in September. Alternatively, the absence of a spending response could also be related to an improved macroeconomic situation. However, this is rather unlikely since we do not find significant differences between worse- and better-performing counties

[13] According to national accounts data non-durable consumption goods and semi-durables make up 38% of total spending (see Table 3.3.3 in Volkswirtschaftliche Gesamtrechnungen, Fachserie 18, Reihe 1.4, from the German Federal Statistical Agency). We map "kurzlebige Konsumgüter" to semi-durables and "Verbrauchsgüter" to non-durables.

in September 2020 (see Figure 3). Furthermore, the economic situation, as measured by the unemployment rate or the share of the labor force in short-time work, was almost unchanged in October 2020 and May 2021 compared to September 2020 (see Figure 5b). Last, the differential effects could be due to the different sizes of the transfer payments since the latter two payments were smaller than the first one. This explanation seems unlikely since we do not find a difference between households with one child and those with more children in September. Despite households with more than one child getting more than twice as high a transfer, their MPC is very similar to that of households with only one child (see Figure 4). Taken together, these results imply that the child bonus payments were only effective in stimulating consumption when households felt secure to spend it. This has important policy implications for the design of stimulus payments in the context of a pandemic.

During a pandemic, stimulating economic activity has an undesirable effect on health outcomes if the associated increase in contacts raises infection rates, leading to a higher death toll and to agents voluntarily reducing economic activities to protect themselves against infections. An agent's decision to work and consume gives rise to an externality since the individual does not internalize the effect of her actions on the probability of infection (Eichenbaum et al., 2021). In fact, the Covid-19 pandemic triggered containment measures that have the purpose of reducing consumption, the exact opposite of what countercyclical fiscal policy aims to achieve in normal times.

Fiscal stimulus and infection rates. Given our finding that most of the increase in spending caused by the child bonus was connected to in-person shopping, there is a potential trade-off between stabilizing economic activity and increasing contact rates. We can shed some light on this trade-off by examining the effect of the child bonus on a proxy for contacts, the number of shops visited per day. In principle, the recipients have two

Table 5: Effect on the number of shop visits

	(1) number of shop visits	(2) number of shop visits	(3) number of shop visits	(4) number of shop visits
Treat x Post	0.030***	0.030***	0.031***	0.023**
	(0.009)	(0.009)	(0.009)	(0.010)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
Additional shop visits	0.665***	0.666***	0.706***	0.524**
	(0.202)	(0.202)	(0.208)	(0.222)
N	274620	274620	274590	274590
# cluster	9154	9154	9153	9153

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using the number of shop visits as an outcome. The additional visits are calculated by multiplying the estimate by the mean number of post-treatment days.

ways to spend the transfer. They could spend more on each trip to the shop or they could increase the number of trips. Table 5 shows that the child bonus indeed had a positive effect on the number of shop visits. According to our estimates, the child benefit caused between 0.52 and 0.71 additional shop visits per recipient, which translates to a roughly 5% increase in overall shop visits in September 2020. This result might be connected to the fact that our results are driven by non-durable consumption goods which mostly require visiting a shop, whereas durable goods can be purchased more easily online.

The potential trade-off between the economic benefits and the health costs of cash transfers during a pandemic suggests that the potential of such transfers to stabilize the macroeconomy is limited. Instead, such fiscal measures should be viewed as instruments to mitigate the adverse distributional consequences of the pandemic. Models integrating macroeconomic and epidemiological dynamics have begun to address issues of heterogeneity (see, for example, Eichenbaum et al., 2021; Kaplan et al., 2020).

6 Conclusion

This study estimates the spending effects of the child bonus, a cash transfer to German parents that was part of the policy response to the Covid-19 pandemic in 2020. We are able to cleanly identify the marginal propensity to consume, given that the treatment dates are quasi-randomly distributed. We combine this setup with high-quality household scanner data at a daily frequency. Thus, we observe actual spending behavior by households and do not have to rely on survey responses. The marginal propensity to consume out of the first transfer was about 12% one month after receipt and rises up to 21% three months after receipt. Contrary to that, we do not find any significant effect of the latter two payments. The absence of an effect of the second and third payment can be explained by a muted response in the presence of higher Covid-19 infection numbers. Consistent with that, we find that the first payment only increased spending in counties with low infection rates. Taken together, the overall one-month (three-month) MPC for the goods that we can observe amounts to 5.4% (9.3%). A qualifying remark is that we do not observe spending on services or large durable goods. Therefore, our estimates should be viewed as a lower bound. Still, even if we were to assume that the effect was the same for services and durable goods, we arrive at a rather small overall MPC of 14.2% (24.5%). Our results have important implications for the design of stimulus measures during a pandemic. Given the failure of the later two payments to raise spending, the cost of the policy could have been 44.4% (€200/€450) of the original cost without reducing its effect on household spending. This would have lowered the overall cost from €6.4 billion to only €2.8 billion. Therefore, our results indicate that during a pandemic governments should consider targeting stimulus measures during times when infection rates are low.

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A Additional Figures and Tables

Table A.1: Summary statistics: spending data

	mean	sd	min	max	N
total spending	399.76	459.89	1.49	18787.43	26844
non-durable spending	259.28	156.08	0.00	1955.33	26844
semi-durable spending	140.48	407.32	0.00	18333.33	26844
in-person spending	352.72	403.64	0.00	18787.43	26844
online spending	47.05	181.05	0.00	5799.00	26844
number of shop visits	14.90	10.24	0.00	102.00	26844

Notes: Summary statistics at the household level for September 2020, October 2020 and May 2021. All variables are measured at the monthly level.

Table A.2: Correlation of child benefit number and observable variables

	(1) child benefit number	(2) child benefit number
female	0.065	0.001
	(0.228)	(0.242)
age	-0.012	-0.009
	(0.009)	(0.010)
East Germany	0.067	-0.009
	(0.166)	(0.184)
number of kids	-0.134	-0.067
	(0.175)	(0.191)
hhsize	0.019	-0.046
	(0.139)	(0.151)
single	-0.325	-0.430
	(0.286)	(0.313)
college or more	0.124	0.007
	(0.178)	(0.199)
log total spending in August	0.003	0.058
	(0.077)	(0.085)
household constrained	-0.329	-0.488
	(0.274)	(0.297)
low income	0.032	0.074
	(0.171)	(0.191)
low wealth		0.006
		(0.179)
high analytical skill		0.026
		(0.174)
high financial literacy		0.264
		(0.170)
N	1547	1234

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parenthesis. The coefficients are based on a linear regression of the child benefit number on observable characteristics. In column (1), we include all characteristics that we observe for the full sample. In column (2), we add dummies for below-median wealth, above-median analytical skill, and above-median financial literacy.

Table A.3: Summary statistics: household level

	Households with children					Households without children				
	mean	sd	min	max	N	mean	sd	min	max	N
female	0.86	0.34	0	1	1547	0.67	0.47	0	1	7607
age	43.99	8.84	19	77	1547	61.00	12.05	19	77	7607
East Germany	0.27	0.45	0	1	1547	0.28	0.45	0	1	7607
household size	3.46	1.04	1	10	1547	1.62	0.62	1	6	7607
single	0.14	0.35	0	1	1547	0.44	0.50	0	1	7607
college or more	0.25	0.43	0	1	1547	0.27	0.45	0	1	7607
income per capita	1359.24	536.56	250	2500	1547	1688.50	565.64	250	2500	7607
net wealth (in 1000€)	85.40	138.06	0	500	1235	79.43	133.78	0	500	5859
household constrained	0.08	0.27	0	1	1547	0.07	0.25	0	1	7593
analytical skill	5.35	2.52	0	10	1546	5.30	2.63	0	10	7587
financial literacy	4.41	2.62	0	10	1546	4.35	2.71	0	10	7597
number of eligible children	1.51	0.70	1	6	1547	0.00	0.00	0	0	7607

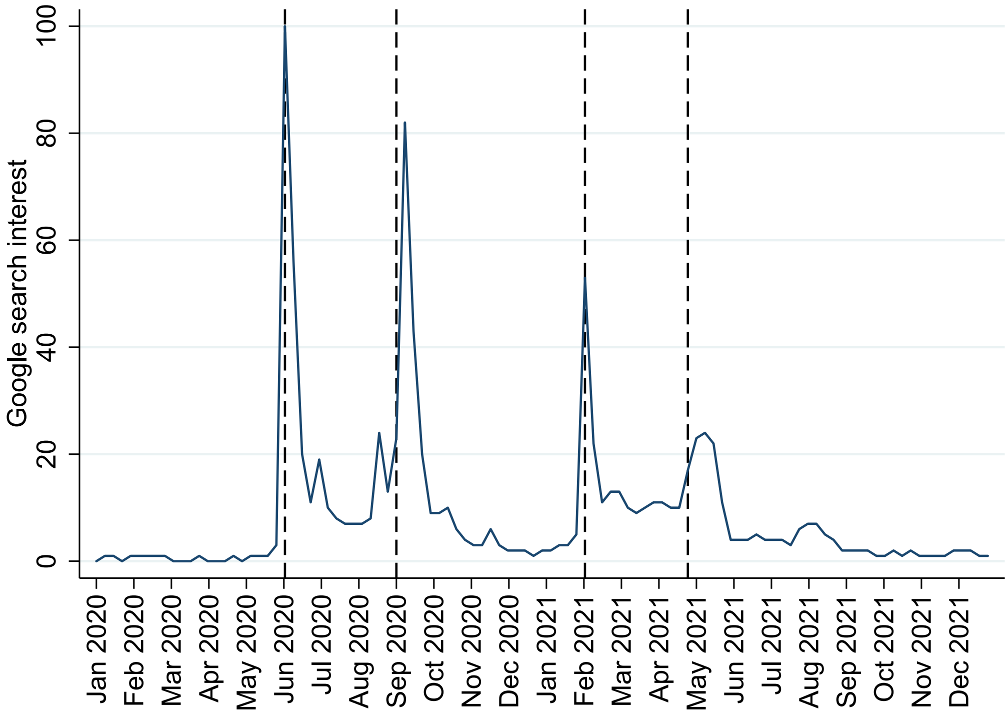
Notes: Summary statistics for the baseline sample split between households with and without children that are eligible for the child bonus. Both income and wealth are elicited in intervals. We assign the mid-point for each interval but the last open-ended category, where we assign the lower bound.

Table A.4: Summary statistics: county level

	mean	sd	min	max	N
September 2020					
unemployment rate	5.71	2.27	2.10	16.00	401
share of labor force in short-time work	4.60	2.43	1.03	20.87	401
Covid-19 incidence	10.92	9.73	0.00	112.27	12030
stringency index	32.32	5.04	20.72	42.74	12030
October 2020					
unemployment rate	5.51	2.24	1.90	15.60	401
share of labor force in short-time work	4.13	2.26	0.95	20.13	401
Covid-19 incidence	48.04	46.13	0.00	322.34	12431
stringency index	30.92	4.57	21.20	57.07	12431
May 2021					
unemployment rate	5.37	2.25	1.90	14.80	401
share of labor force in short-time work	4.73	2.10	0.00	18.88	401
Covid-19 incidence	90.29	57.24	2.34	541.64	12431
stringency index	52.81	14.92	6.06	66.28	12431

Notes: Summary statistics at the county level for September 2020, October 2020 and May 2021. The labor market variables are measured at monthly level and the Covid-19 variables are measured at the daily level.

Figure A.1: Google search interest for "Kinderbonus"



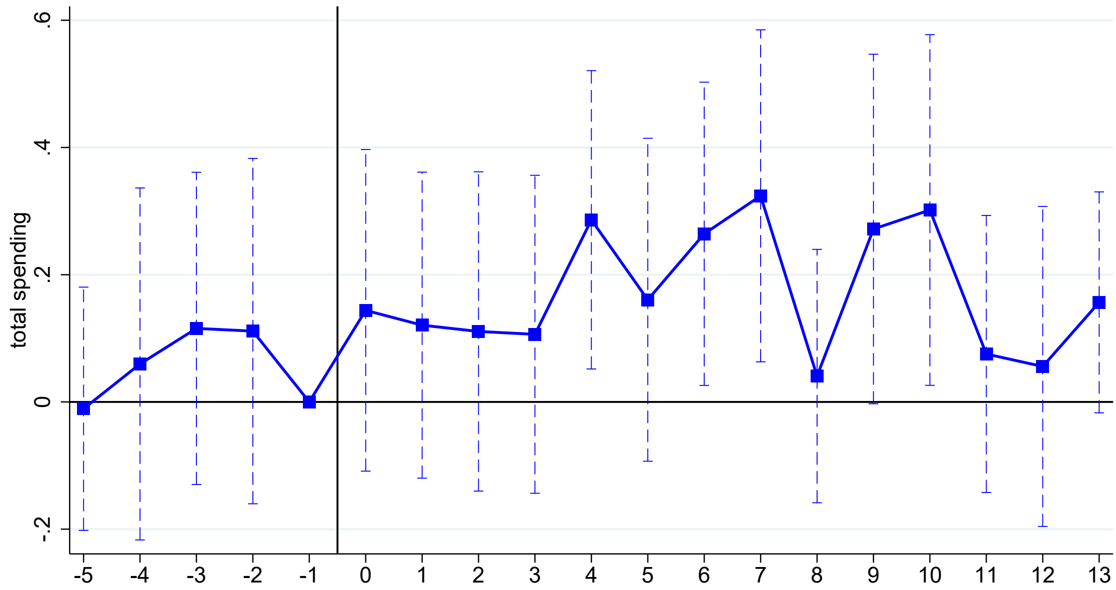
Source: Google Trends. Notes: This figure plots the intensity of Google searches for the term "Kinderbonus" which is normalized to 100 at the point of highest interest. The first and second dashed lines refer to the announcement and start of the payment of the first two tranches of the child bonus. The third and fourth dashed lines refer to the announcement and start of the payment of the third tranche of the child bonus.

Table A.5: Estimation of marginal propensity to consume: long-term MPC

	(1) 1-month estimation sample	(2) 2-month estimation sample	(3) 3-month estimation sample
Treat x Post	0.109***	0.058**	0.046*
	(0.029)	(0.026)	(0.027)
Household FE	yes	yes	yes
Date x county FE	yes	yes	yes
MPC	0.122***	0.161**	0.210*
	(0.032)	(0.072)	(0.121)
N	271500	552111	823732
# cluster	9050	9051	9052

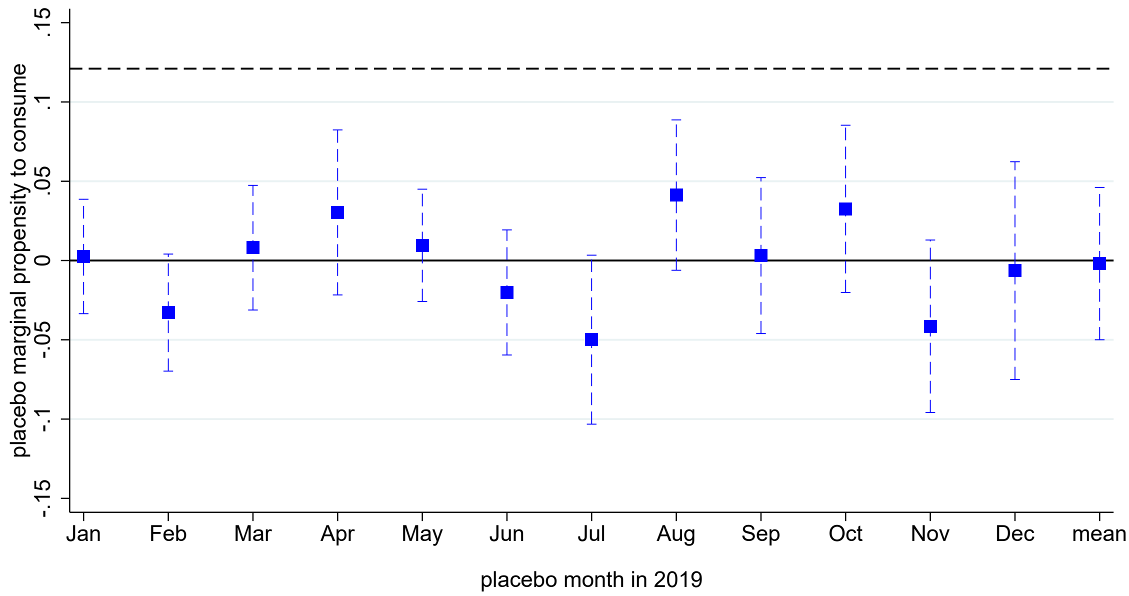
Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) one, two and three months after payment receipt using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Figure A.2: Daily effects on total spending: standard two-way fixed effects estimator



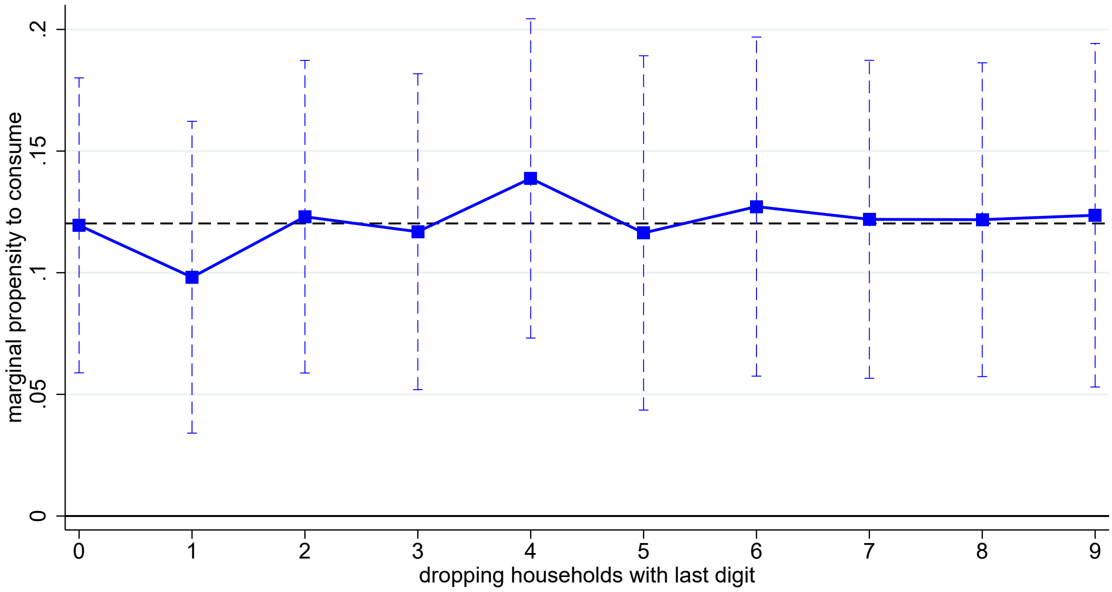
Notes: This figure plots point estimates and 95% confidence bands from estimating equation (3) using the traditional two-way fixed effects estimator with normalized total spending as an outcome.

Figure A.3: Placebo marginal propensity to consume in 2019



Notes: This figure plots point MPC estimates and 95% confidence bands of regression results based on equation (1) on the respective 2019 monthly sample using normalized total spending as an outcome. The placebo MPCs are calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average child benefit amount. The dashed line indicates our baseline MPC.

Figure A.4: Marginal propensity to consume: dropping treatment groups



Notes: This figure plots point MPC estimates and 95% confidence bands while dropping households with different last digits in their child benefit number one at the time. The baseline estimate is represented by a dotted line.

Table A.6: Announcement effect

	(1) spending: semi-durables	(2) spending: semi-durables	(3) spending: semi-durables	(4) spending: semi-durables
Panel A: Announcement of the September & October 2020 payments				
Treatment x Announcement	-0.033	-0.033	0.023	0.012
	(0.097)	(0.097)	(0.102)	(0.108)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	-0.006	-0.007	0.005	0.002
	(0.019)	(0.019)	(0.020)	(0.021)
N	112115	112115	112081	112081
# cluster	6595	6595	6593	6593
	(5) total spending	(6) total spending	(7) total spending	(8) total spending
Panel B: Announcement of the May 2021 payment				
Treatment x Announcement	0.061	0.061	0.062	0.035
	(0.039)	(0.039)	(0.040)	(0.045)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.067	0.067	0.069	0.039
	(0.043)	(0.043)	(0.044)	(0.049)
N	151578	151578	151560	151560
# cluster	8421	8421	8420	8420

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (4). The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€300 in Panel A or €150 in Panel B times the mean number of children).

Table A.7: Marginal propensity to consume: exclude households above tax threshold

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.100***	0.100***	0.102***	0.093***
	(0.030)	(0.030)	(0.031)	(0.033)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.111***	0.112***	0.113***	0.103***
	(0.033)	(0.033)	(0.034)	(0.037)
N	249330	249330	249300	249300
# cluster	8311	8311	8310	8310

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample excluding households that, based on the income and marital status, likely did not get the child bonus using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Table A.8: Marginal propensity to consume: including bottom and top 1% of spending distribution

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.108***	0.108***	0.111***	0.103***
	(0.029)	(0.029)	(0.029)	(0.031)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.120***	0.121***	0.124***	0.115***
	(0.032)	(0.032)	(0.033)	(0.035)
N	274620	274620	274590	274590
# cluster	9154	9154	9153	9153

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample including households in the bottom and top 1% of the September spending distribution using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Table A.9: Marginal propensity to consume: outcome in levels (in €)

	(1) total spending in levels	(2) total spending in levels	(3) total spending in levels	(4) total spending in levels
Treat x Post	1.303***	1.310***	1.479***	1.310**
	(0.477)	(0.477)	(0.486)	(0.514)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.097***	0.098***	0.110***	0.098**
	(0.036)	(0.036)	(0.036)	(0.038)
N	271530	271530	271500	271500
# cluster	9051	9051	9050	9050

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using daily spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days divided by the average transfer amount (€200 times the mean number of children).

Table A.10: Marginal propensity to consume: inverse hyperbolic sine

	(1) IHS total spending	(2) IHS total spending	(3) IHS total spending	(4) IHS total spending
Treat x Post	0.076***	0.076***	0.088***	0.073***
	(0.021)	(0.021)	(0.021)	(0.023)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.084***	0.085***	0.098***	0.081***
	(0.023)	(0.023)	(0.024)	(0.025)
N	271530	271530	271500	271500
# cluster	9051	9051	9050	9050

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using the hyperbolic sine transformation of daily spending as an outcome. The MPC is calculated by multiplying the estimate with the mean number of post-treatment days divided by the average transfer amount (€200 times the mean number of children).

Table A.11: Marginal propensity to consume: exclude households unsure about exact date

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.141***	0.142***	0.156***	0.141***
	(0.038)	(0.038)	(0.040)	(0.043)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.156***	0.156***	0.173***	0.155***
	(0.042)	(0.042)	(0.044)	(0.048)
N	247260	247260	247230	247230
# cluster	8242	8242	8241	8241

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the September 2020 sample without households that indicated being unsure about the exact date of their payment using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Table A.12: Marginal propensity to consume: cluster standard errors at county level

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.105***	0.106***	0.109***	0.099***
	(0.026)	(0.027)	(0.027)	(0.029)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.117***	0.118***	0.122***	0.111***
	(0.030)	(0.030)	(0.030)	(0.033)
N	271530	271530	271500	271500
# cluster	401	401	400	400

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the county level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using normalized total spending as an outcome with standard errors clustered at the county level. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Table A.13: Marginal propensity to consume: drop public sector employees

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.112***	0.112***	0.111***	0.094**
	(0.033)	(0.033)	(0.034)	(0.037)
Household FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.118***	0.118***	0.118***	0.100**
	(0.035)	(0.035)	(0.036)	(0.039)
N	221610	221610	221610	221610
# cluster	7387	7387	7387	7387

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample without civil sector employees using normalized total spending as an outcome. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

Table A.14: Heterogeneity of marginal propensity to consume: county characteristics

	(1) low Covid-19 incidence	(2) high Covid-19 incidence	(3) lax Covid-19 restrictions	(4) strict Covid-19 restrictions
Treat x Post	0.165***	0.039	0.113**	0.115***
	(0.041)	(0.047)	(0.045)	(0.040)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.189***	0.042	0.122**	0.132***
	(0.047)	(0.051)	(0.049)	(0.045)
p-value of difference	0.036		0.888	
N	135651	135336	141620	129880
# cluster	6649	7129	5446	5149
	(5) low unemployment rate	(6) high unemployment rate	(7) low share of labor force either unemployed or in short-time work	(8) high share of labor force either unemployed or in short-time work
Treat x Post	0.091**	0.134***	0.100***	0.121***
	(0.038)	(0.044)	(0.039)	(0.043)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.102**	0.147***	0.117***	0.126***
	(0.043)	(0.049)	(0.045)	(0.045)
p-value of difference	0.485		0.883	
N	135390	136110	135630	135870
# cluster	4513	4537	4521	4529
	(9) in-person spending: low Covid-19 incidence	(10) in-person spending: high Covid-19 incidence	(11) online spending: low Covid-19 incidence	(12) online spending: high Covid-19 incidence
Treat x Post	0.174***	0.037	0.012	0.429**
	(0.040)	(0.046)	(0.218)	(0.214)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.175***	0.036	0.002	0.046**
	(0.040)	(0.045)	(0.031)	(0.023)
p-value of difference	0.022		0.248	
N	135561	135185	27220	28234
# cluster	6645	7124	1364	1480

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) splitting the sample by the Covid-19 incidence, strictness index, unemployment rate, and the share of the labor force that is either unemployed or in short-time work. In columns (1) to (6), the outcome variable is total spending, in columns (9) and (10) it is in-person spending, and in columns (11) and (12) it is online spending. The p-value refers to the difference in the MPC for the different samples. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean of the outcome variable divided by the average transfer amount (€200 times the mean number of children).

Table A.15: Heterogeneity of marginal propensity to consume: county characteristics (joint estimation)

	(1) total spending	(2) total spending	(3) total spending	(4) total spending	(5) total spending
Treat x Post	0.039	0.113**	0.091**	0.100***	-0.041
	(0.047)	(0.045)	(0.038)	(0.039)	(0.087)
Treat x Post x low Covid-19 incidence	0.126**				0.139**
	(0.063)				(0.067)
Treat x Post x strict Covid-19 restrictions		0.002			0.066
		(0.060)			(0.069)
Treat x Post x high unemployment rate			0.043		0.047
			(0.058)		(0.076)
Treat x Post x high unemployment and short-time work rate				0.020	0.037
				(0.058)	(0.076)
Household FE	yes	yes	yes	yes	yes
Date x county x group FE	yes	yes	yes	yes	yes
N	270987	271500	271500	271500	270987
# cluster	9050	9050	9050	9050	9050

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) fully interacted with dummies for below-median Covid-19 incidence, above-median Covid-19 strictness index, unemployment rate, or unemployment and short-time rate using normalized total spending as an outcome.

Table A.16: Heterogeneity of marginal propensity to consume: household characteristics

	(1) HH constrained	(2) HH unconstrained	(3) low income	(4) high income
Treat x Post	0.271*	0.103***	0.130***	0.061
	(0.153)	(0.030)	(0.037)	(0.050)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.251*	0.116***	0.134***	0.081
	(0.142)	(0.034)	(0.039)	(0.066)
p-value of difference	0.355		0.483	
N	14640	252390	125730	145230
# cluster	488	8413	4191	4841
	(5) low wealth	(6) high wealth	(7) one child/small transfer	(8) two or more children/large transfer
Treat x Post	0.107**	0.127***	0.087**	0.151***
	(0.053)	(0.044)	(0.037)	(0.043)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.114**	0.150***	0.136**	0.124***
	(0.057)	(0.053)	(0.058)	(0.035)
p-value of difference	0.637		0.861	
N	93390	116040	253020	244350
# cluster	3113	3868	8434	8145
	(9) high analytical	(10) low analytical	(11) high financial literacy	(12) low financial literacy
Treat x Post	0.099**	0.115***	0.124***	0.116***
	(0.045)	(0.039)	(0.041)	(0.042)
Household FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.111**	0.128***	0.144***	0.124***
	(0.050)	(0.043)	(0.048)	(0.045)
p-value of difference	0.795		0.757	
N	118950	151440	136530	134070
# cluster	3965	5048	4551	4469

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) splitting the sample by household wealth, income, whether the household is constrained, analytical ability, financial literacy, or the number of children. The p-value refers to the difference in the MPC for the different samples. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in spending divided by the average transfer amount (€200 times the mean number of children).

B Additional evidence on incomes and labor supply

We use a monthly survey of German households, the IAB's high-frequency online personal panel (HOPP), to document the income losses of households with and without dependent children following the pandemic as well as the potential labor supply responses of households in response to the child bonus. The survey participants are sampled randomly from social security data to be representative of the German labor market (Haas et al., 2021).

B.1 Differential impact of Covid-19 on German households with and without children

We use the first wave of the data set, conducted in May 2020, which contains a question of whether the household's net income (strongly) decreased, stayed the same, or (strongly) increased since February 2020. We define three dummy variables: one for income loss, which takes the value one if the respondent states that their income either decreased or strongly decreased, one for constant income, and one for income gain, which takes the value one if the respondent states that their income either increased or strongly increased. The data set also includes information on the households' composition, which we use to identify households with children under 18 years of age. We regress the dummies for income loss, constant income, and income gain on the dummy for having children in the household.

As Appendix Table B.1 shows, households with children are 3.6 percentage points more likely to report income losses than households without children. This difference is highly significant and economically meaningful when compared to the sample average of 29.5% of respondents in the HOPP data set that report an income loss.

Table B.1: Differential impact of Covid-19 on households with and without children

	(1) income drop since February 2020	(2) same income as in February 2020	(3) income increase since February 2020
household with eligible children	0.036***	-0.034***	-0.002
	(0.009)	(0.010)	(0.005)
mean	0.295	0.648	0.057
N	10831	10831	10831
# cluster	10831	10831	10831

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parenthesis. The results are based on a regression using a dummy for reporting an income drop, the same income, or an income increase since February 2020 as the outcome and a dummy for having at least one eligible child in the household.

B.2 Labor supply effects of the child bonus

In principle, the child bonus could have induced an income effect, raising the demand for leisure and thereby reducing labor supply. Given that the child bonus was a one-off transfer, we do not expect substantial adjustments of the recipients' labor supply. Nevertheless, we test this hypothesis by using monthly data on hours worked in the panel component of the HOPP data set. More specifically, the data include hours worked by both the respondent and a potential partner from July 2020 to October 2020. We use this data to estimate a difference-in-difference model with the following regression equation:

$$h_{it} = \alpha_i + \gamma_t + \beta Treat_i Childbonus_t + \varepsilon_{it}, \quad (5)$$

where h_{it} refers to hours worked by either the respondent or the partner of household i in wave t, $Treat_i$ is a treatment dummy that equals 1 if household i is eligible for the child bonus, $Childbonus_t$ is a dummy that equals 1 for the months in which the child bonus was paid out, September and October 2020, and ε_{it} is an error term clustered at the household level. α_i are household fixed effects, which control for all time invariant characteristics of the household, while γ_t are wave fixed effects, which absorb all aggregate trends in labor supply. Last, β represents the effect of the child bonus on labor supply. As Appendix Table B.2 shows, there is no evidence for the child bonus inducing any change in the labor supply of households. The effect on hours worked is small and statistically

Table B.2: Effect of the child bonus on labor supply

	(1) hours worked: respondent	(2) hours worked: partner	(3) hours worked: total
Treat x Child Bonus	0.134	0.496	0.228
	(0.338)	(0.446)	(0.531)
Household FE	yes	yes	yes
Month FE	yes	yes	yes
mean	34.882	35.224	54.974
N	7282	4611	7282
# cluster	2885	1824	2885

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the household level are in parenthesis. The regression results are based on equation (5) estimated on waves three to five of the IAB HOPP data.

insignificant for the respondent (see column 1), his or her partner (see column 2), and the households' total hours worked (see column 3). Therefore, we conclude that the child bonus did not affect labor supply decisions in a measurable way.

C GfK Homescanner Panel Survey – January 2021

The GfK Homescanner Panel Survey survey, January 2021 wave, is used in our analysis to identify the timing of the child bonus receipt, as well as to gather information about the households.

Q1 **Child benefit eligibility**: Bekommt Ihr Haushalt Kindergeld? (*Does your household receive child benefit payments?*)

- Ja, für ein Kind. (*Yes, for one child.*)
- Ja, für zwei Kinder. (*Yes, for two children.*)
- Ja, für drei Kinder. (*Yes, for three children.*)
- Ja, für vier Kinder. (*Yes, for four children.*)
- Ja, für mehr als vier Kinder. (*Yes, for more than four children.*)
- Nein. (*No.*)

Q2 **Knowledge exact payment date of children benefit**: Wissen Sie genau, an welchem Tag im Januar 2021 Sie das Kindergeld bekommen haben? (*Do you know on which exact day in January 2021 you received the child benefit payment?*)

- Ja. (*Yes.*)
- Nein. (*No.*)

Q3 **Exact date of child benefit**: An welchem Datum haben Sie im Januar 2021 das Kindergeld bekommen? (*On which exact day in January 2021 did you receive the child benefit payment?*)

Hinweis: Bitte überprüfen Sie gegebenenfalls Ihren Kontoauszug. (*Please check your account statement if necessary.*)

- Am _____ _____ Januar, 2021 (*On _____ _____ January, 2021*)

Q4 **Date of child benefit.**: Bitte geben Sie dennoch das Datum an, an dem Sie im Januar 2021 das Kindergeld bekommen haben. (*Nevertheless, please state on which day in January 2021 you received the child benefit payment.*)

Hinweis: Bitte überprüfen Sie gegebenenfalls Ihren Kontoauszug oder schätzen Sie das ungefähre Datum. (*Please check your account statement or estimate the date.*)

- Am _____ _____ Januar, 2021 (*On _____ _____ January, 2021*)

To study potential heterogeneity patterns, we use the responses to the following survey questions:

Q5 **Financial constraint**: Inwieweit hatten Sie in den letzten sechs Monaten Schwierigkeiten, Ihre laufenden Ausgaben zu bezahlen? (*Did have you had any difficulties in the past six months to pay your expenses?*)

Hinweis: Bitte nur eine Angabe. Bitte wählen Sie die Antwort, die am ehesten auf die Situation in Ihrem Haushalt passt. (*Only one answer required. Please choose the answer that most closely matches the situation in your household.*)

- Ich / Wir hatte(n) keine Schwierigkeiten, da das Einkommen des Haushalts ausreichte. (*I/We had no problem since my/our household income sufficed.*)
- Ich / Wir hatte(n) keine Schwierigkeiten, da ich / wir auf Ersparnisse zurückgreifen konnte(n). (*I/We had no problem since I/we could make use of my/our savings.*)
- Ich / Wir hatte(n) Schwierigkeiten, aber ich / wir konnte(n) Geld leihen oder einen Kredit aufnehmen. (*I/We had problems but could take out a loan to pay our expenses.*)
- Ich / Wir hatte(n) Schwierigkeiten und ich / wir konnte(n) kein Geld leihen oder Kredit aufnehmen. (*I/We had problems and could not take out a loan to pay our expenses.*)

Q7 **Skills**: Im Folgenden sehen Sie einige Aussagen als Gegensatzpaare. Bitte geben Sie pro Zeile jeweils an, ob Sie eher der linken Aussage oder eher der rechten Aussage zustimmen. Verwenden Sie dazu bitte die Zahlen von „0“ bis „10“: „0“ bedeutet, dass Sie der linken Aussage voll und ganz zustimmen, und „10“ bedeutet, dass Sie der rechten Aussage voll und ganz zustimmen. (*In the following, you are going to see several statements as pairs of opposites. Please state whether you agree with the statement on the left or right side using numbers from 0 to 10. 0 means full agreement with the left statement and 10 means full agreement with the right statement.*)

- **Analytical:**

  Ich bin ein analytischer Mensch. (*I am an analytical person.*) 0_____ 1_____ 2_____ 3_____ 4_____ 5_____ 6_____ 7_____ 8_____ 9_____ 10_____ Ich handle eher intuitiv. (*I am an intuitive person.*)

- **Financial literacy:**

  Ich kenne mich mit Finanzen / Finanzmathematik sehr gut aus. (*I am very familiar with financial topics.*) 0_____ 1_____ 2_____ 3_____ 4_____ 5_____ 6_____ 7_____ 8_____ 9_____ 10_____ Ich kenne mich mit Finanzen / Finanzmathematik überhaupt nicht aus. (*I am not familiar with financial topics at all.*)

Q9 **Net wealth**: Wie hoch schätzen Sie das gesamte Vermögen (netto) Ihres Haushalts ein? Das Gesamtvermögen (netto) ist der Wert all dessen, was den Haushaltsmitgliedern gehört abzüglich aller Schulden und Verbindlichkeiten. (*What is the net wealth of your household? The net wealth is sum of all assets and liabilities of all members of your household.*)

- Unter 0€ (*Less than €0*)

- 0 bis unter 2500€ (*Between €0 and €2500*)
- 2500 bis unter 5000€ (*Between €2500 and €5000*)
- 5000 bis unter 10000€ (*Between €5000 and €10000*)
- 10000 bis unter 25000€ (*Between €10000 and €25000*)
- 25000 bis unter 50000€ (*Between €25000 and €50000*)
- 50000 bis unter 75000€ (*Between €50000 and €75000*)
- 75000 bis unter 100000€ (*Between €75000 and €100000*)
- 100000 bis unter 250000€ (*Between €100000 and €250000*)
- 250000 bis unter 500000€ (*Between €250000 and €500000*)
- Mehr als 500000€ (*More than €500000*)

Q10 **Monthly household net income**: Wie hoch ist das monatliche Nettoeinkommen Ihres Haushaltes insgesamt? Hinweis: Damit ist die Summe gemeint, die sich ergibt aus Lohn, Gehalt, Einkommen aus selbständiger Tätigkeit, Rente oder Pension, jeweils nach Abzug der Steuern und Sozialversicherungsbeiträge. Rechnen Sie bitte auch die Einkünfte aus öffentlichen Beihilfen, Einkommen aus Vermietung, Verpachtung, Wohngeld, Kindergeld und sonstige Einkünfte hinzu. (*What is the monthly net income of your household? This refers to the sum of wages, salaries, income from self-employed, annuities or pensions after taxes and social security payments are deducted. Please also add income from public transfers, such as child benefits or housing benefits, as well as rental income.*)

- unter 500€ (*Less than €500*)
- 500 bis 749€ (*Between €500 and €749*)
- 750 bis 999€ (*Between €750 and €999*)
- 1000 bis 1249€ (*Between €1000 and €1249*)

- 1500 bis 1749€ (*Between €1500 and €1749*)
- 1750 bis 1999€ (*Between €1750 and €1999*)
- 2000 bis 2249€ (*Between €2000 and €2449*)
- 2250 bis 2499€ (*Between €2250 and €2499*)
- 2500 bis 2749€ (*Between €2500 and €2749*)
- 2750 bis 2999€ (*Between €2750 and €2999*)
- 3000 bis 3249€ (*Between €3000 and €3249*)
- 3250 bis 3499€ (*Between €3250 and €3499*)
- 3500 bis 3749€ (*Between €3500 and €3749*)
- 3750 bis 3999€ (*Between €3750 and €3999*)
- 4000 bis 4999€ (*Between €4000 and €4999*)
- Mehr als 5000€ (*More than €5000*)