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Spending effects of child-related fiscal transfers

Olga Goldfayn-Frank
Vivien Lewis
Nils Wehrhöfer

Deutsche Bundesbank, Wilhelm-Epstein-Straße 14, 60431 Frankfurt am Main,
Postfach 10 06 02, 60006 Frankfurt am Main

Tel +49 69 9566-0

Please address all orders in writing to: Deutsche Bundesbank,
Press and Public Relations Division, at the above address or via fax +49 69 9566-3077

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Non-technical summary

Research Question

The so-called child bonus was a cash transfer of in total €450 per child paid to German parents in response to the Covid-19 pandemic. How effective was this fiscal stimulus measure? This question is of great importance in a macroeconomic environment where monetary policy is constrained by the zero lower bound and public debt levels are already high. Furthermore, the effectiveness of countercyclical fiscal policy might be impaired by the pandemic itself. At the microeconomic level, the purpose of the child bonus was to alleviate the hardship that the Covid-19 restrictions placed on families. Moreover, given the evidence that the adverse consequences of the pandemic were unevenly distributed, it is important to understand the effects of the transfer on households with different characteristics.

Contribution

This paper estimates the spending effects of the child bonus and relates them to the state of the economy and to the pandemic situation. Our unique contribution is two-fold. First, we are able to identify the marginal propensity to consume in a clean way, given that the treatment dates are randomly distributed over a two-week interval. Second, we have household scanner data at the daily frequency. Thus, we observe actual spending behavior by households and, therefore, do not have to rely on survey responses.

Results

Our estimation results show that the child bonus had a comparatively low transfer multiplier. The transfer was paid out in three payments. The marginal propensity to consume out of the first payment of €200 per child is about 12%. The effect is concentrated in the non-durable consumption goods category, and it is higher in areas with lower infection rates. It is stronger for households with low income or liquidity constraints, but only a small fraction of the population reports such constraints. We find no significant effect of the latter two payments, yielding an aggregate marginal propensity to consume of 5.4%. We conclude that the child bonus should rather be seen as a redistributive policy instrument, and not so much as a fiscal stimulus measure.

Nichttechnische Zusammenfassung

Fragestellung

Eltern in Deutschland erhielten als Reaktion auf die Coronapandemie mehrere Transferzahlungen in Form des sogenannten Kinderbonus, in Höhe von insgesamt 450€ pro Kind. Wie effektiv war dieser Fiskalimpuls? In einem gesamtwirtschaftlichen Umfeld, in dem die Geldpolitik durch die Nullzinsgrenze eingeschränkt ist und die öffentlichen Schuldenstände bereits hoch sind, ist diese Frage sehr relevant. Des Weiteren könnte die Wirksamkeit antizyklischer fiskalpolitischer Maßnahmen durch die Pandemie selbst beeinträchtigt sein. Auf der Mikroebene zielte die Maßnahme darauf ab, die Belastungen abzufedern, denen die Familien durch die Covid-19-Beschränkungen ausgesetzt waren. Es gibt Belege, dass die negativen Folgen der Pandemie ungleichmäßig verteilt waren. Deshalb ist es wichtig, ein Verständnis davon zu erlangen, welche Effekte die Transferzahlungen auf Haushalte mit verschiedenen Merkmalen hatten.

Beitrag

In diesem Forschungspapier wird der Ausgabeneffekt des Kinderbonus geschätzt und mit der Wirtschafts- sowie der Pandemielage in Beziehung gesetzt. Wir liefern zwei wesentliche Beiträge. Erstens gelingt es, die marginale Konsumneigung sauber zu identifizieren, da die Auszahlungstermine über ein Zwei-Wochen-Intervall zufallsverteilt sind. Zweitens liegen tägliche Scannerdaten zu den Ausgaben der privaten Haushalte vor. Dadurch lässt sich das tatsächliche Ausgabeverhalten der Haushalte beobachten, und die Untersuchung muss sich nicht auf Umfrageergebnisse verlassen.

Ergebnisse

Die Ergebnisse unserer Schätzungen zeigen, dass der Kinderbonus einen relativ niedrigen Transfermultiplikator hatte. Die Transferzahlung erfolgte in drei Raten. Bei der ersten Teilzahlung von 200€ belief sich die marginale Konsumneigung auf etwa 12%. Der Effekt konzentrierte sich auf die Verbrauchsgüter und fiel in Gegenden mit niedrigeren Infektionszahlen stärker aus. Er war bei Haushalten mit niedrigem Einkommen oder Liquiditätsbeschränkungen stärker ausgeprägt, wobei allerdings nur ein geringer Bevölkerungsanteil solche Beschränkungen angab. Für die zweite und dritte Teilzahlung wurde kein signifikanter Effekt festgestellt. Insgesamt lag die aggregierte marginale Konsumneigung bei 5,4%. Ingesamt gesehen wird festgestellt, dass der Kinderbonus weniger als fiskalische Stimulierungsmaßnahme, sondern eher als Umverteilungsinstrument gesehen werden sollte.

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Olga Goldfayn-Frank
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Abstract

As part of Germany's fiscal response to the Covid-19 pandemic, parents received three payments totalling €450 per child. Randomization in the payment dates and daily scanner data allow us to identify the effects of these transfers on household spending. We find a significant but small spending effect of the first transfer, with an estimated marginal propensity to consume of about 12%. The effect is higher for low-income and liquidity-constrained households, and in areas with lower infection rates. The second and third payment failed to increase spending. Our results indicate that the child bonus was redistributive rather than stimulative.

Keywords: child bonus, Covid-19, fiscal stimulus, household spending, marginal propensity to consume, pandemic, transfer.

JEL classification: D12, E21, E62, H24, H31.

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1 Introduction

In 2020 and 2021, German parents received three transfers totalling €450 per child, on top of the regular child benefit. The so-called child bonus was part of a larger policy package enacted with the explicit aim to counter the recessionary and distributional effects of the Covid-19 pandemic.[1] This paper estimates the impact of the child bonus on household spending. Our identification strategy exploits random variation in the payment dates, combined with scanner data on household consumption expenditure at the daily frequency. We estimate the marginal propensity to consume for all three payments and relate it to both the state of the macroeconomy and the pandemic situation.

At the aggregate level, gauging the effectiveness of fiscal stimulus measures in a recession is of great importance. This is especially true in a macroeconomic environment where monetary policy is constrained by the zero lower bound on nominal interest rates and public debt levels are already high. Moreover, the fiscal multiplier in a pandemic could be lower than that observed in an ordinary recession (Auerbach et al., 2021).

At the micro-level, the purpose of Covid-related policies was to alleviate the hardship caused by the pandemic. Families with children were hit hard by widespread school closures (Fuchs-Schündeln et al., 2021). The child bonus was meant to support families, which were particularly affected by the Covid-19 restrictions. Indeed, we confirm that households with dependent children were significantly more likely to report a drop in their net income than households without dependent children during the first months of the pandemic.[2] More generally, there is overwhelming evidence that the adverse effects of the pandemic were very heterogeneous across the economy and tended to worsen inequality (Bounie et al., 2020; Alon et al., 2020; Eichenbaum et al., 2022). Therefore, it is particularly important to understand the effects of the cash transfer on households with different characteristics. To this end, we exploit detailed survey data on household characteristics.

It is a challenging task to identify the effect of the child bonus on household spending due to the co-existence of different policy measures as well as pandemic conditions. We use home scanner data from the "Gesellschaft für Konsumforschung" (GfK) in combination with a GfK survey that was conducted in January 2021. From the survey, we elicit the date of receipt of the regular child benefit in January 2021, and use this information to infer the date of receipt of the child bonus in September and October 2020, and in May 2021. Since the payment dates are spread randomly over a two-week time period 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 the marginal propensity to consume (MPC) out of the cash transfer and find a small value of 11.6% to 12.1% for the September 2020 tranche of the child bonus, which at €200 per child was the largest of the three. When estimating daily spending effects before and after the receipt of the child bonus, we do not find differential trends before the payment. We find no significant effect on spending for the second and third tranches, which were paid out in October 2020 and May 2021, amounting to €100 and €150, respectively. In a separate exercise, we find no effect of the policy announcements on spending. This implies that the child bonus, which cost about €6.4 billion in total,

[1] The German policy response to the pandemic included several other initiatives, such as an extension of short-time work, financial assistance to firms and a temporary VAT cut.

[2] See Appendix B for details on the data and estimation.

induced only €0.344 billion in additional spending, with an overall MPC of about 5.4%.[3]

Further investigation shows that the significant spending effect of the September payment was entirely driven by spending on non-durable consumer goods. There was no effect on semi-durable goods. The spending response to the child bonus payments was larger for households with low income levels. Households that reported facing liquidity constraints showed a considerably higher MPC of about 25%.

In a pandemic, there are important additional aspects to consider regarding the effect of fiscal transfers. On the one hand, government-imposed restrictions as well as fear of infection can lead to lower economic activity and thus a weaker response to the stimulus. Indeed, we find that in response to child bonus payments, spending increased in September only in counties with low Covid-19 case numbers. Furthermore, restrictions on certain industries, such as e.g. the closing of restaurants, caused an increase in household savings. We find that households with a higher increase in savings before the payments respond less to the transfer. Consistent with these two results, we find no effect of the later payments on household spending as they occurred in the context of considerably larger Covid-19 case numbers and high saving rates. On the other hand, an increase in economic activity also increases the number of contacts, which is a critical determinant of the infection rate. We find that the child bonus was mostly spent in-person and caused about 5% more shop visits, a proxy for contacts due to economic activity.

Our main contribution to the literature is to cleanly identify the spending effects of the transfer. Moreover, we gauge the effectiveness of the transfer by looking at actual spending data and do not rely solely on survey responses. There is no consensus in the literature on the size of the MPC out of fiscal transfers, both in normal times and during Covid-19. The overall low spending response we find is not inconsistent with existing evidence on the consumption response to fiscal stimulus during the pandemic. Most closely related, Parker et al. (2022) find a 10% MPC out of the Economic Impact Payments (EIP) in the USA, for non-durable and service spending, a result similar to our estimates. In contrast, Karger and Rajan (2021) and Baker et al. (2020) estimate much larger spending responses to the EIP, between 25% and 50%. One possible explanation of this higher MPC estimate is the higher proportion of liquidity-constrained households in the latter two studies. Indeed, using data at the zip code level, Chetty et al. (2020) document a heterogeneous spending response to the EIP between 8% in the highest-income areas, and 25% in the lower quarter distribution of income. Our estimates also show much higher MPC for liquidity-constrained households. However, only 7% of the respondents in our survey report to be liquidity- or borrowing-constrained. This is consistent with other data sources, showing that only a very small share of German households are liquidity-constrained.[4]

Another possible explanation of the low spending response to the fiscal stimulus could be the size of the payment. The aforementioned studies address the effect of much larger payments of $1300 per adult and $500 per child, whereas the child bonus amounted to only €450 per child. We find very similar marginal propensities to consume for households with one child that received a small transfer, and households with more than one child

[3] Our data do not contain information about spending on large durable items or services. Even if were to assume that the spending categories our data do not cover were similarly affected, the MPC would rise to only 14.2%.

[4] 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% report 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 (PHF).

that received a transfer at least twice as large. There are ambiguous results on the effect of transfer size on the MPC. Scholnick (2013) shows, in the case of mortgage payments, that the consumption response is smaller for larger income changes. Using survey evidence, Christelis et al. (2019) find that larger unexpected positive income shocks lead to a higher MPC, especially in the presence of liquidity constraints. Fuster et al. (2020) find mixed evidence, with more households adjusting their consumption for larger income changes, but a smaller change in consumption conditional on adjusting.

We also contribute to the literature investigating the interaction between an ongoing pandemic and stabilization policies. Our results suggest that the MPC out of transfers may be muted due to high infection numbers or high levels of savings accumulated due to government restrictions. Furthermore, we show that 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). Consistent with this result, Auerbach et al. (2021) find that defence 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.

Our study also contributes to the debate about the success of the German fiscal stimulus package.[5] As discussed above, we find only a small MPC for the first payment of the child bonus and no response for the second and third payments, suggesting a rather small stabilization effect. 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 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 Section 2, we describe the data set, paying particular attention to the characteristics of households with and without children. Section 3 explains our empirical strategy. Section 4 describes and discusses our findings. Finally, Section 5 concludes.

2 Data

To measure the effect of the child bonus payment on household spending, we use data from the GfK (*Gesellschaft für Konsumforschung*) home scanner panel. The data set covers non-durable and semi-durable consumption goods. It contains detailed information not only about the items purchased, but also about the shops that were visited, as well as certain household characteristics, such as income and household composition. We broadly distinguish between spending on non-durable goods such as food items, and semi-durable goods, e.g. clothing. In our sample period, non-durable spending accounts for about two

[5] There are two German policy papers about the child bonus. Behringer et al. (2021) asks survey respondents to self-assess their spending response to the child bonus and found it to be more effective in stimulating spending than the VAT cut. This highlights the challenge of asking households to estimate their own counterfactual spending path. Bachmann et al. (2022) compare households with more or fewer children in repeated cross-sectional survey data at the monthly frequency. They find an aggregate null effect with substantial uncertainty, with liquidity-constrained households reacting more strongly.

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`.

thirds of total spending. We can also differentiate between in-person and online shopping. The latter only plays a minor role for the consumption goods in our data, making up about 12% of total spending in our sample period, most of which is concentrated in semi-durable goods. This is already an increase from 8% in 2019. Furthermore, we calculate the number of shops visited at the daily level as a proxy for contacts due to economic activity. The data cover January to December 2019 and July 2020 to June 2021, which allows us to compare household spending before and during the pandemic. Scanner data on semi-durable goods are provided by a sub-sample of those households that participate in the home scanner panel. As our baseline sample, we use households for whom we observe both non-durable and semi-durable goods. We exclude the bottom and top 1% of the spending distribution to account for outliers.[6] Additionally, we have data on semi-durable goods for the first half of 2020. Importantly for our analysis, the data are available at the daily frequency. Summary statistics on the spending data can be found in Appendix Table A.1. To determine the eligibility for the child bonus, the date of receipt, as well as additional household characteristics, we administered a survey to almost 10000 participants of the GfK home scanner panel in January 2021. The relevant survey questions 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 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. More specifically, the payment date of the regular child benefit and the child bonus is determined by the last digit of the child benefit number, which is assigned countrywide on an ongoing basis to households that apply for child benefit for the first time. The number is the same for all children living in the same household. This makes the allocation of the number an effectively random process. One exception from the rule are 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.

[6] We include these observations in a robustness check.

Table 2: Correlation of child benefit number and observables

	(1) child benefit number	(2) child benefit number
female	0.196	0.207
	(0.226)	(0.241)
age	0.000	0.003
	(0.009)	(0.010)
East Germany	-0.010	-0.036
	(0.167)	(0.186)
number of eligible kids	-0.047	0.023
	(0.176)	(0.194)
household size	0.019	-0.021
	(0.139)	(0.152)
single	-0.198	-0.216
	(0.287)	(0.315)
college or more	0.178	0.121
	(0.181)	(0.204)
total spending in August	0.000	0.000
	(0.000)	(0.000)
household constrained	-0.328	-0.376
	(0.278)	(0.304)
low income	-0.124	-0.145
	(0.173)	(0.196)
checks account weekly		0.023
		(0.198)
low wealth		-0.083
		(0.181)
high analytical skill		-0.121
		(0.177)
high financial literacy		0.192
		(0.174)
N	1846	1474

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 a dummy indicating whether the respondent checks her bank account at least on a weekly basis and dummies for below-median wealth, high analytical skill and high financial literacy, respectively.

Due to strict data protection rules, we were not able to ask directly about the child benefit number. However, from the GfK household survey, we obtain the payment dates of the regular child benefit in January 2021 (see column (2) of Table 1).[7] 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 (3) to (5) of Table 1 to infer the payment date of the child bonus in September 2020, October 2020, and May 2021. As the table shows, there is a period of 16 to 17 days between the first and last payment. Generally, the higher the last digit of the child

[7] 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.

benefit number, the later the payment is issued.

We test the randomness assumption of the payment dates by regressing the child benefit number on observable characteristics in Table 2. 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. We drop households which do not answer questions on having children or on the date of receipt of their child benefit payment. We restrict our sample to households that report both non-durable and semi-durable spending and drop all households that report no spending in September 2020, October 2020, or May 2021. The final sample for our analysis contains 9154 households, around 17 percent of which have children eligible for the child bonus.

Household and county characteristics. Table 3 provides 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.[8] The proportion of households living in East Germany and of those with a college degree are quite similar between the two groups. We also have information on how frequently households check their bank account, which we measure via a dummy taking the value one if households report to check their account at least weekly. Furthermore, we ask households for their self-assessed analytical skill and financial literacy on a scale from zero to ten. Using the answers, we create dummies for high analytical skill and financial literacy, which take the value one if the respondent is above the respective sample median. 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. For the average household, the child bonus represents about 18% of the mean monthly net income in 2020 and 9% of the mean monthly net income in 2021.

We observe the county of residence for all respondents 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 seven-day 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 extensive daily information on the local restrictions in place provided by the Federal Ministry for Economic Affairs and Energy. These data include restrictions for elementary and high schools, child care facilities, retail shops, restaurants, mask mandates, night time curfews and social distance requirements. For each category, we have a dummy indicating whether a restriction is in place at the daily level. From that data set, we also create a strictness index by calculating the share of restrictions in place out of all possible restrictions. Appendix Table A.2 shows summary statistics for the county-level variables.

[8] Single households are households containing only one adult.

Table 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	1846	0.67	0.47	0	1	7607
age	44.26	8.95	19	77	1846	61.00	12.05	19	77	7607
East Germany	0.26	0.44	0	1	1846	0.28	0.45	0	1	7607
household size	3.45	1.04	1	10	1846	1.62	0.62	1	6	7607
single	0.13	0.34	0	1	1846	0.44	0.50	0	1	7607
college or more	0.27	0.44	0	1	1846	0.27	0.45	0	1	7607
income per capita	1394.29	544.68	250	2500	1846	1688.50	565.64	250	2500	7607
net wealth (in €1000)	90.56	141.67	0	500	1476	79.43	133.78	0	500	5859
household constrained	0.07	0.26	0	1	1846	0.07	0.25	0	1	7593
checks account frequently	0.77	0.42	0	1	1844	0.70	0.46	0	1	7603
analytical skill	5.39	2.52	0	10	1845	5.30	2.63	0	10	7587
financial literacy	4.39	2.62	0	10	1845	4.35	2.71	0	10	7597
number of eligible children	1.50	0.70	1	6	1846					

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.

3 Empirical strategy

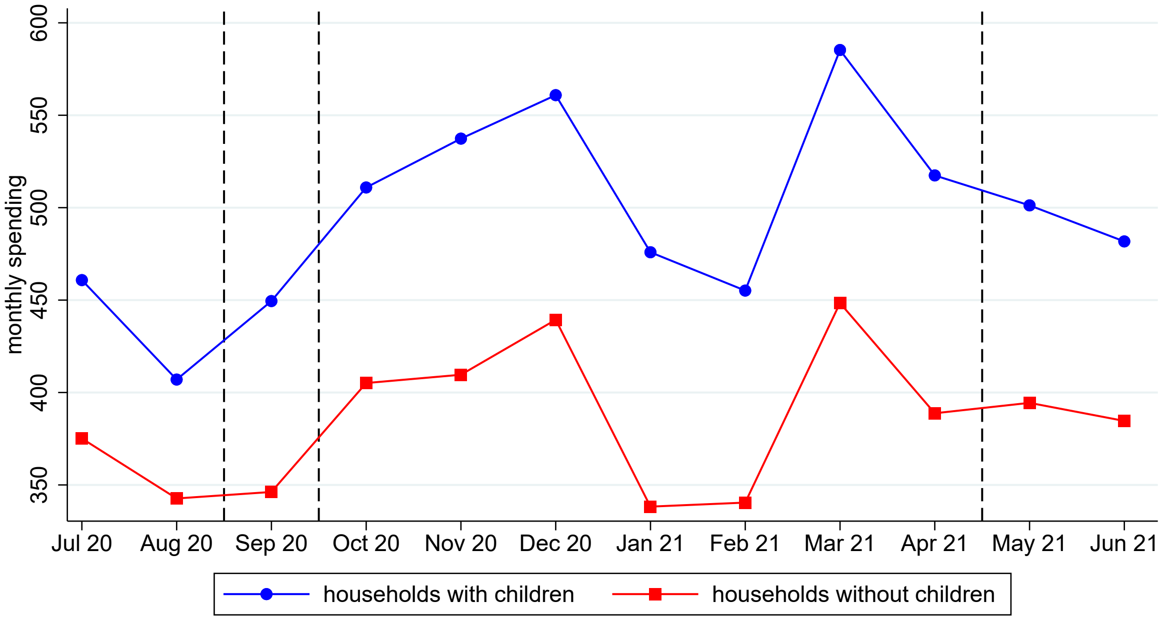
Two preliminary remarks are in order. First, the child bonus in Germany was paid by bank transfer. Therefore, it is possible that recipients who do not regularly check their account balance were unaware of having received the transfer. However, since the child bonus received a lot of media attention after its announcement on 3rd June 2020, we believe that most households were aware of it. Second, the tax treatment implies that rich households do not benefit from the transfer.[9] Roughly 80% of households benefited in full from the child bonus and another 10% benefited at least in part.[10] In a robustness check, we drop households for whom, based on their income, marital status and number of children, the child bonus does not raise their after-tax income.

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 1). No such change is visible for households without eligible 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 1). This is a first indication that, at the aggregate level, the September 2020 tranche of

[9] The sum of child benefit and child bonus, summed over all children, is compared with the tax benefit of the child allowance. If the latter exceeds the former, a household does not benefit from the child bonus. The exact income threshold depends on the marital status and number of children. For example, a married couple with three children benefits fully up to an income of €67816 and in part up to an income of €105912.

[10] For details on the tax treatment of the child bonus are provided under this link.

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 2020 until June 2021. The dotted lines indicate the months in which the child bonus was paid out (September and October 2020, May 2021).

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:

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

where y_{it} is normalized spending by household i on day t. Following Parker et al. (2022), we define normalized spending as daily spending of household i on day t divided by average daily spending of household i in the sample period. This allows us to interpret our estimates in percentage terms.[11] 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

[11] All results are robust to using the inverse hyperbolic sine transformation of y instead (Browning and Crossley, 2009). Results are also very similar when using the outcome in levels or a $log(x + 1)$ transformation.

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 separate dummies for all restrictions in place 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 Covid-19 incidence and local restrictions 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 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 which 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. Similar to the event study by Baker et al. (2020), 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, 2020). 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.

A 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 first and second tranche 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.

4 Results

Table 4 shows that the marginal propensity to consume out of the child bonus, depending on the fixed effects and controls included, lies between 11.6% and 12.1%. While this effect is highly significant at the 1% level, it is quite moderate in size. We note that spending on services and durable goods is not included in our data set, and we therefore regard these estimates as a lower bound. According to national accounts data, non-durable and semi-durable goods made up about 38% of aggregate spending in 2020, whereas durable goods and services made up 12% and 50%, respectively.[12] Our result is very similar to Parker et al. (2022), who find a 10% MPC for the US Economic Impact Payments. Moreover, Bunn et al. (2018) also find a similarly small spending response of 12-13% to positive income shocks.

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. We can see that, while there is no significant difference in the trend before the child bonus, after the receipt total spending exhibits an increase of about 15%, which increases up 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.1). 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. Dahan and Nisan (2020) find that benefit recipients in Jerusalem with pay days after their water bill due date are more likely to make late payments. 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

[12] 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.

Table 4: Estimation of marginal propensity to consume: baseline

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.104***	0.104***	0.108***	0.108***
	(0.028)	(0.028)	(0.029)	(0.030)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.116***	0.116***	0.120***	0.121***
	(0.031)	(0.031)	(0.032)	(0.034)
N	271500	271500	271470	271470

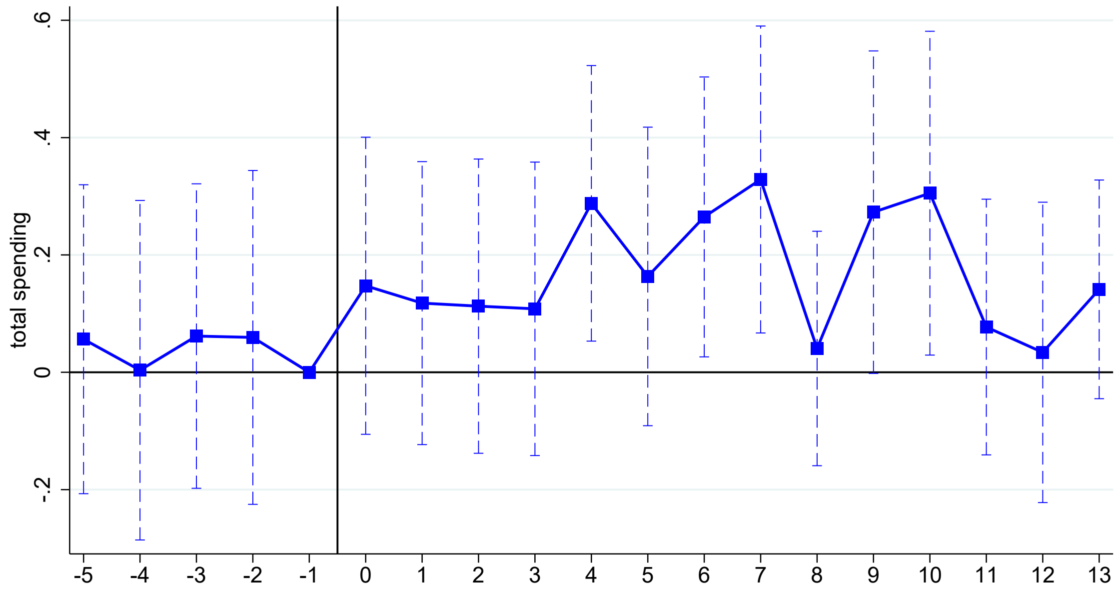
Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) on the full September 2020 sample using normalized total spending as an outcome. The MPC is calculated 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).

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 parents received the normal child benefit, but no child bonus. As Appendix Figure A.2 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.3 shows that neither announcement had a significant effect on spending.[13]

Robustness checks. We subject our results to a number of robustness checks. First, we exclude households that, based on their income, marital status and number of kids, likely do not benefit from the child bonus. As Table A.4 shows, this does not change our results much since they make up only about 9% of our sample. Next, we include both households that report 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.5). We also use different transformations of our outcome variable. When using spending in levels, a inverse hyperbolic sine transformation or a $log(x+1)$ transformation, the MPC estimates are similar to our baseline (see Appendix Tables A.6, A.7 and A.8). 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.9). This increases the size of the MPC to between 15% and 17%, consistent with slightly reduced measurement error. Furthermore, we cluster standard errors on

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

Figure 2: Estimates of daily effects on total spending: September 2020



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.

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.10). Additionally, we vary the end date of our estimation sample. While, in the baseline, we end our sample on 30th September, we vary the end by up to one week forward and backward in Appendix Figure A.3. The marginal propensity of consume does not vary strongly by the choice of end date. 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.11). 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 by spending categories, we see that the effect is entirely driven by non-durable consumption goods (see Table 5). 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 MPC (see Table 5).

The literature has generally found that MPCs are higher for poorer households (Parker et al., 2013; Jappelli and Pistaferri, 2014; Bounie et al., 2020). Therefore, we estimate the model separately for low-wealth, low-income and self-reported liquidity-constrained households. More specifically, we split the sample using a median split for income and wealth. The median net income (net wealth) in the sample is €1625 (€7500). The results are shown in Table 6. On the one hand, wealth does not appear to be a determinant of the marginal propensity to consume as the estimates are very similar. On the other hand,

Table 5: Estimation of 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)
HH 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	270270	271260	55560

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).

low-income households exhibit a larger MPC than rich households, who do not react significantly. Households with self-reported liquidity constraints have an even higher MPC of 25%, more than twice our baseline estimate. However, one has to keep in mind that only 7% of the households in our sample report that they are liquidity-constrained, which helps to explain our low aggregate MPC estimate in Table 4. Another dimension along which households differ is the frequency with which they check their bank account balance. Those households that check their account balance at least once a week are characterized by a higher spending response than other households. With respect to self-assessed analytical skill and financial literacy, we do not find differential effects (see Appendix Table A.12). We also estimate heterogeneous treatment effects for counties in different macroeconomic conditions as well as different stages of the pandemic. As Table 7 shows, when the Covid-19 case rates are high, the marginal propensity to consume out of the child bonus becomes small and insignificant. This is not driven by stricter restrictions since, if anything, stricter rules are associated with a larger MPC. There is some evidence for a substitution effect towards online shopping in areas with high case numbers (see Appendix Table A.13). However, it does not play a quantitatively important role since online shopping accounts for a low share of overall spending. 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. When looking at the unemployment rate or the share of the labor force that is either unemployed or in short-time work, the effects are slightly higher in counties which are doing worse, but these differences are not statistically different. 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. Another explanation for the relatively low spending response are the high saving rates caused by the Covid-19 pandemic and related restrictions which made it very hard to spend on, for example, restaurants or vacations. At the aggregate level the savings rate jumped from a pre-pandemic level of about 11% up to 20% in the second quarter of 2020 and stayed at a higher level afterwards (see Appendix Figure A.5). We measure

Table 6: Estimation of marginal propensity to consume: household characteristics

	(1) low wealth	(2) high wealth	(3) low income	(4) high income
Treat x Post	0.103*	0.127***	0.128***	0.061
	(0.053)	(0.044)	(0.037)	(0.050)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.110*	0.150***	0.132***	0.081
	(0.057)	(0.053)	(0.039)	(0.066)
N	93360	116040	125700	145230
	(5) HH constrained	(6) HH unconstrained	(7) checks account frequently	(8) checks account rarely
Treat x Post	0.271*	0.101***	0.126***	0.077
	(0.153)	(0.030)	(0.033)	(0.064)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.251*	0.114***	0.141***	0.085
	(0.142)	(0.034)	(0.037)	(0.071)
N	14640	252360	192180	78240

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 or whether the household checks their account at least weekly. 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).

excess savings at the micro level in two complementary strategies. First, we calculate mean savings at the county-month level from the Bundesbank Online Panel Households (BOP-HH). Second, we take the difference between monthly income and spending in the GfK data at the individual level.[14] In both cases, we take the difference between monthly savings during the pandemic and the baseline pre-pandemic average and cumulate these excess savings until August 2020. As Appendix Table A.14 shows, the MPC is larger both for individuals and in areas with lower excess savings.

October 2020 and May 2021 payments. Next, we evaluate the second and third tranche of the child bonus in October 2020 and May 2021. Our earlier finding of significant spending increases after a one-off cash transfer of €200 per child in September contrasts with the results obtained for the second and third tranche of the child bonus. Table 8 shows that both the second and third transfer had no significant effect on spending. This implies that the overall spending effect of the child bonus was rather low. Given an overall cost of €6.4 billion, it led to a €0.344 billion increase in spending, which implies an aggregate MPC of about 5.4%.[15] Note that we do not have data on services and durable

[14] Both data sets have different strengths and shortcomings. The BOP-HH data are at the county level, but have a comprehensive measure of savings, whereas the GfK data exclude services and durables, but are at the individual level.

[15] This follows from the product of the overall cost with the share of costs for the first payment and the 12.1% MPC estimate from Table 4: €6.4 billion x (200/450) x 0.121 ≈ €0.344 billion.

Table 7: Estimation of marginal propensity to consume: county characteristics

	(1) low case rates	(2) high case rates	(3) lax rules	(4) strict rules
Treat x Post	0.166***	0.038	0.129***	0.094**
	(0.041)	(0.047)	(0.041)	(0.042)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.190***	0.041	0.145***	0.104**
	(0.047)	(0.051)	(0.046)	(0.047)
N	135585	135372	144680	126790
	(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	0.088**	0.134***	0.098**	0.121***
	(0.038)	(0.044)	(0.039)	(0.043)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.099**	0.147***	0.114**	0.126***
	(0.043)	(0.049)	(0.045)	(0.045)
N	135360	136110	135600	135870

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 seven day Covid incidence, the strictness index, the unemployment rate and the share of the labor force that either unemployed or in short-time work. 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).

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 these goods, we arrive at an overall MPC of 14.2%.[16] The null results for the latter payments are likely the result of a change in the macroeconomic state of the economy, the financial situation of households and/or the pandemic-related context between September 2020 and May 2021. We can relate these hypotheses to our heterogeneity results from the September 2020 payment. Given that our MPC estimate is significant only in areas with low case numbers, we would expect the overall MPC to be lower when case numbers are significantly higher than they were in September. Appendix Figure A.6 shows that this was the case both in October 2020 and in May 2021. Only 13% of respondents live in counties with Covid-19 cases per 100000 inhabitants in October lower than the September median, i.e. the subset of counties for which we find a significant effect in September. For May 2021, the fraction is even lower at 0.5%. We also observe that the savings rate stays abnormally high after the first payment (see Appendix Figure A.5). Therefore, households might be even less liquidity-constrained for the latter two payments causing a lower MPC. 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 in September 2020 and the economic situation only changed marginally in October 2020 and May 2021 (see Appendix Figure A.7). 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. We test for this explanation by splitting the recipients in September in two groups: households with one

[16] Non-durable consumption goods and semi-durables make up 38% of total spending. Therefore, if the effect was proportional, the total effect would be 5.4% / 0.38 ≈ 14.2%.

Table 8: Estimation of 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 (0.026)	-0.000 (0.026)	0.006 (0.027)	-0.020 (0.033)
HH 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 (0.064)	-0.001 (0.064)	0.015 (0.066)	-0.050 (0.079)
N	280612	280612	280581	280581
	(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 (0.026)	0.005 (0.026)	0.000 (0.026)	-0.007 (0.034)
HH 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 (0.043)	0.008 (0.044)	0.001 (0.044)	-0.012 (0.056)
N	261764	261764	261733	261733

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).

eligible child, who get a small transfer of €200 in September and households with more than one child, who get a larger transfer of on average €450. We estimate the MPC for both groups separately in Appendix Table A.15. 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. 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

Table 9: 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***	0.028***
	(0.009)	(0.009)	(0.009)	(0.010)
HH 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***	0.664***	0.706***	0.626***
	(0.202)	(0.202)	(0.208)	(0.218)
N	274620	274620	274590	274590

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.

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 ways to spend the transfer. They could spend more on each trip to the shop or they could increase the number of trips. Table 9 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 0.63 and 0.71 additional shop visits per recipient, which translates to a roughly 5% increase in overall shop visits in September 2020.[17] This result might by 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. When we split our sample by case rates and the stringency index, we find, similar to the spending results, that shop visits mainly increased in areas with low case numbers and that restrictions did not seem to play a major role in reducing shop visits (see Appendix Table A.17). This suggests that households mainly increased their contacts due to the payments in areas where the infection risk was lower, which reduces the overall stabilization effect of the payments.

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).

[17] We obtain similar results when estimating a Poisson model (see Appendix Table A.16).

5 Conclusion

This paper 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. Our unique contribution is two-fold. First, we are able to identify the marginal propensity to consume in a clean way, given that the treatment dates are randomly distributed over a two-week interval. Second, we have at our disposal household spending data at the daily frequency. Thus, we observe actual spending behavior by households and do not have to rely on survey responses. Our estimates show that the child bonus had a comparatively low transfer multiplier. The marginal propensity to consume out of the first transfer is about 12% and we do not find any significant effect of the latter two payments yielding an aggregate marginal propensity to consume of 5.4%. 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 and high savings rates throughout the pandemic. Consistent with that, we find that households increased the number of visits to shops due to the child bonus only if the overall infection risk was low. Moreover, the effect is concentrated in the non-durable consumption goods category. It is stronger for households with low income or liquidity constraints, but only a small fraction of the population reports such constraints. 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 small overall MPC of 14.2%. We conclude that the child bonus should rather be seen as a redistributive policy instrument, and not so much as a fiscal stimulus measure.

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A Additional Figures and Tables

Table A.1: Summary statistics: spending data

	mean	sd	min	max	N
total spending	399.93	460.44	1.49	18787.43	26752
non-durable spending	259.25	156.14	0.00	1955.33	26752
semi-durable spending	140.67	407.85	0.00	18333.33	26752
in-person spending	352.84	404.17	0.00	18787.43	26752
online spending	47.09	181.12	0.00	5799.00	26752
number of shop visits	14.91	10.24	0.00	102.00	26752

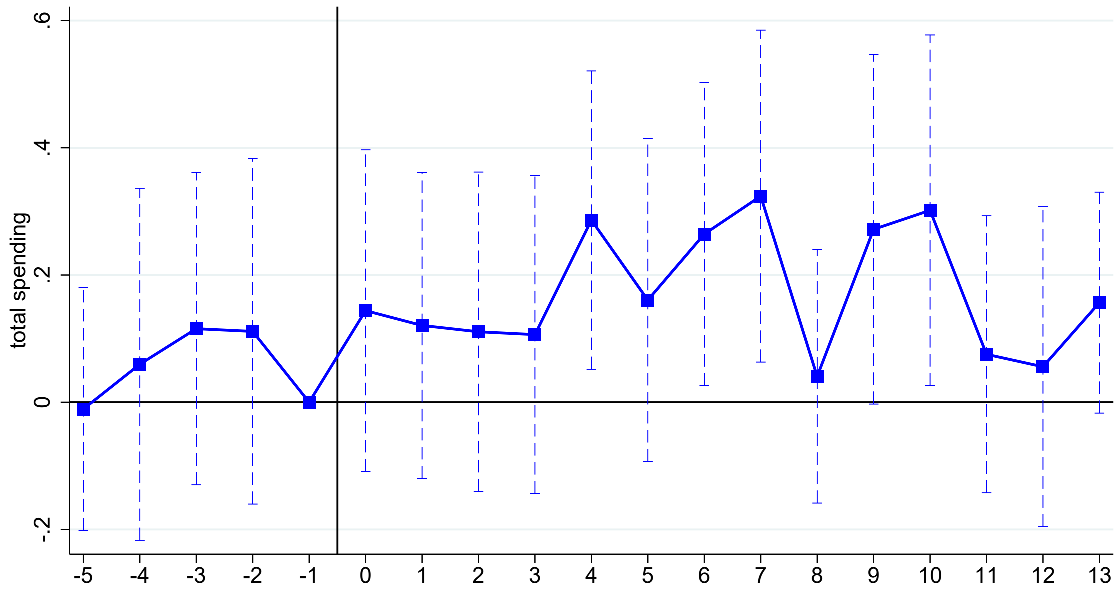
Notes: Summary statistics at the household level for September 2020, October 2020 and May 2021. All variables are measured at monthly level.

Table A.2: 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
7-day case incidence	10.92	9.73	0.00	112.27	12030
stringency index	0.78	0.14	0.38	1.00	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
7-day case incidence	48.04	46.13	0.00	322.34	12431
stringency index	0.80	0.14	0.38	1.00	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
7-day case incidence	90.29	57.24	2.34	541.64	12431
stringency index	0.84	0.17	0.38	1.00	12431

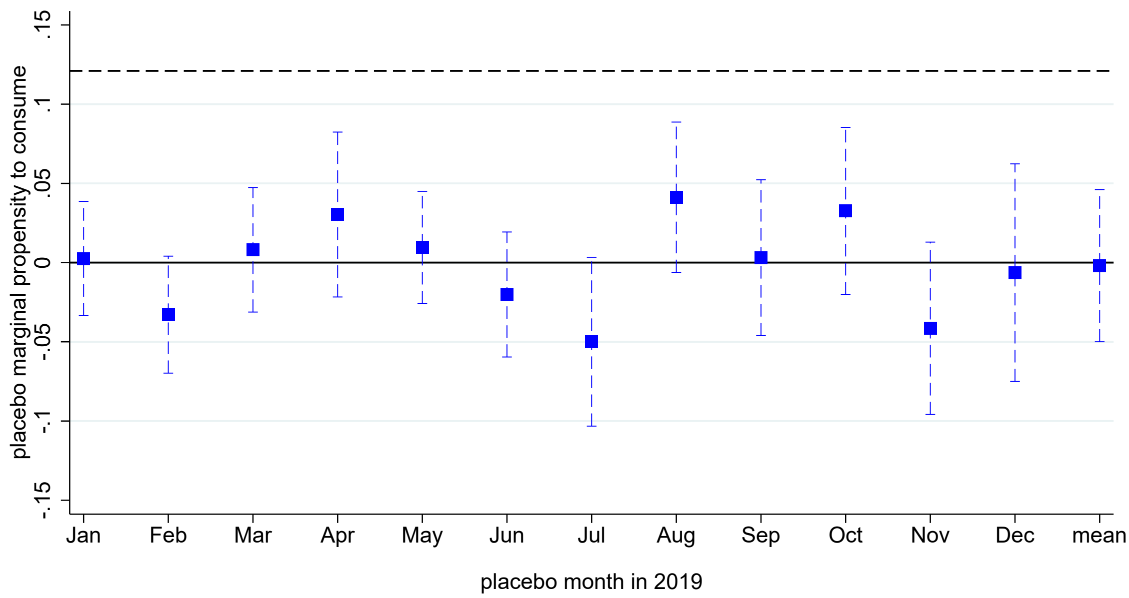
Notes: Summary statistics at the county level for September 2020, October 2020 and May 2021. The labor market variables are measured at monthly level and the Covid-19 variables are measured at the daily level.

Figure A.1: Estimates of daily effects on spending



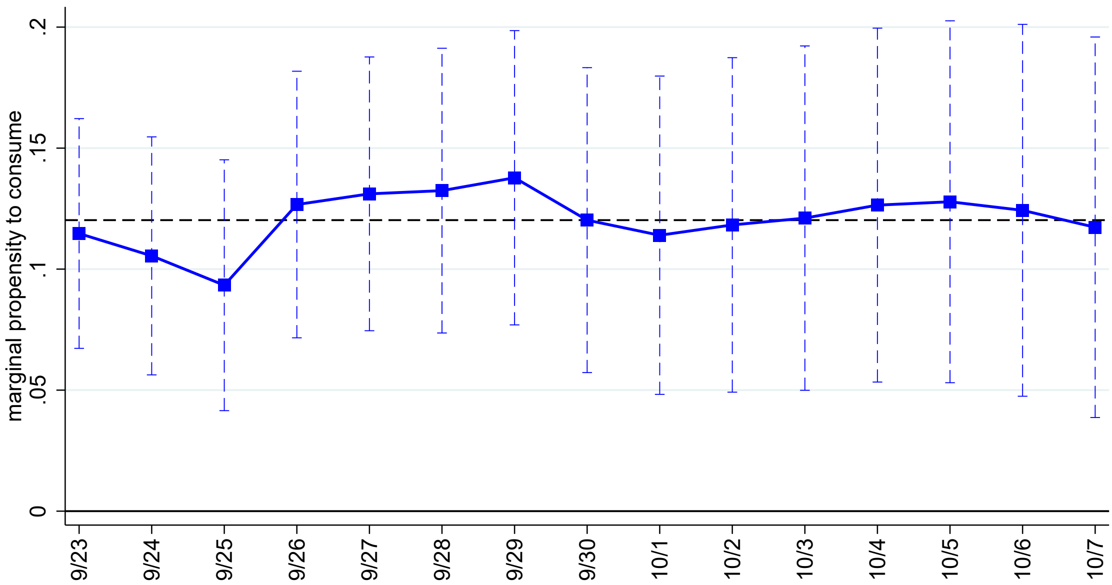
Notes: This figure plots point estimates and 95% confidence bands from estimating equation (3) using the traditional two-way fixed effect estimator with normalized total spending as an outcome.

Figure A.2: Effect of the regular child benefit: Placebo marginal propensity to consume



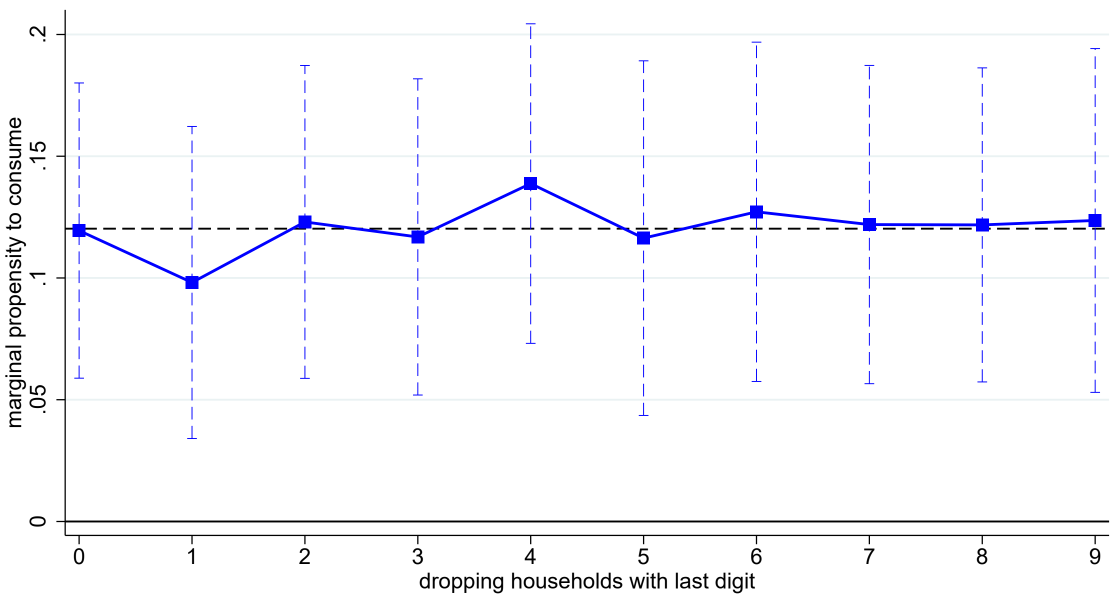
Notes: This figure plots point MPC estimates and 95% confidence bands of regression results based on equation (1) on the respective 2019 monthly sample using normalized total spending as an outcome. The placebo MPCs are calculated by multiplying the estimate by the mean number of post-treatment days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

Figure A.3: Marginal propensity to consume by sample end date



Notes: This figure plots point MPC estimates and 95% confidence bands while varying the end date of the sample. The baseline estimate, which is obtained by ending the sample on September 30^{th}, is represented by a dotted line.

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.3: 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	-0.034	0.023	0.029
	(0.097)	(0.097)	(0.102)	(0.103)
HH 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	0.006
	(0.019)	(0.019)	(0.020)	(0.020)
N	112115	112115	112081	112081
	(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	0.060	0.062	0.039
	(0.039)	(0.039)	(0.040)	(0.045)
HH 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	0.042
	(0.043)	(0.043)	(0.044)	(0.049)
N	151578	151578	151560	151560

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.4: Estimation of marginal propensity to consume: exclude households above the tax threshold

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.098***	0.098***	0.100***	0.103***
	(0.030)	(0.030)	(0.031)	(0.032)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.109***	0.109***	0.111***	0.114***
	(0.033)	(0.033)	(0.034)	(0.036)
N	249300	249300	249270	249270

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.5: Estimation of marginal propensity to consume: including the 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***	0.113***
	(0.029)	(0.029)	(0.029)	(0.031)
HH 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***	0.120***	0.124***	0.126***
	(0.032)	(0.032)	(0.033)	(0.034)
N	274620	274620	274590	274590

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.6: Estimation of 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	1.385***	1.384***	1.532***	1.534***
	(0.489)	(0.490)	(0.498)	(0.526)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.103***	0.103***	0.114***	0.114***
	(0.036)	(0.036)	(0.037)	(0.039)
N	269130	269130	269100	269100

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.7: Estimation of 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	0.078***	0.078***	0.091***	0.085***
	(0.021)	(0.021)	(0.021)	(0.023)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.091***	0.091***	0.106***	0.099***
	(0.025)	(0.025)	(0.026)	(0.027)
N	269130	269130	269100	269100

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 taking the percentage effect and multiplying it by the mean number of post-treatment days divided by the average transfer amount (€200 times the mean number of children).

Table A.8: Estimation of marginal propensity to consume: log(x+1)

	(1) log total spending + 1	(2) log total spending + 1	(3) log total spending + 1	(4) log total spending + 1
Treat x Post	0.066***	0.066***	0.077***	0.072***
	(0.018)	(0.018)	(0.018)	(0.019)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.076***	0.076***	0.089***	0.083***
	(0.021)	(0.021)	(0.022)	(0.023)
N	269130	269130	269100	269100

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 log (total spending + 1) as an outcome. The MPC is calculated by taking the percentage effect and multiplying it 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: Estimation of 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	0.139***	0.138***	0.154***	0.158***
	(0.038)	(0.038)	(0.040)	(0.042)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.153***	0.152***	0.170***	0.174***
	(0.042)	(0.042)	(0.044)	(0.047)
N	247230	247230	247200	247200

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.10: Estimation of marginal propensity to consume: cluster standard errors at the county level

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.104***	0.104***	0.108***	0.108***
	(0.027)	(0.027)	(0.027)	(0.028)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.116***	0.116***	0.120***	0.121***
	(0.030)	(0.030)	(0.030)	(0.032)
N	271500	271500	271470	271470

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.11: Estimation of marginal propensity to consume: drop civil sector employees

	(1) total spending	(2) total spending	(3) total spending	(4) total spending
Treat x Post	0.110***	0.110***	0.110***	0.106***
	(0.033)	(0.033)	(0.034)	(0.036)
HH FE	yes	yes	yes	yes
Date FE	yes	yes		
Covid controls		yes		
Date x county FE			yes	yes
Covid controls x parent				yes
MPC	0.116***	0.117***	0.116***	0.112***
	(0.035)	(0.035)	(0.036)	(0.038)
N	221580	221580	221580	221580

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.12: Estimation of marginal propensity to consume: analytical skill & financial literacy

	(1) high analytical	(2) low analytical	(3) high financial literacy	(4) low financial literacy
Treat x Post	0.095**	0.115***	0.124***	0.113***
	(0.045)	(0.039)	(0.041)	(0.042)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.106**	0.128***	0.144***	0.120***
	(0.050)	(0.043)	(0.048)	(0.045)
N	118920	151440	136530	134040

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 median analytical skill and median financial literacy. 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: Estimation of marginal propensity to consume: Covid-19 cases & online shopping

	(1) in-person spending: low cases	(2) in-person spending: high cases	(3) online spending: low cases	(4) online spending: high cases
Treat x Post	0.175***	0.036	0.012	0.429**
	(0.040)	(0.046)	(0.218)	(0.214)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.176***	0.035	0.002	0.046**
	(0.040)	(0.045)	(0.031)	(0.023)
N	135497	135249	27220	28234

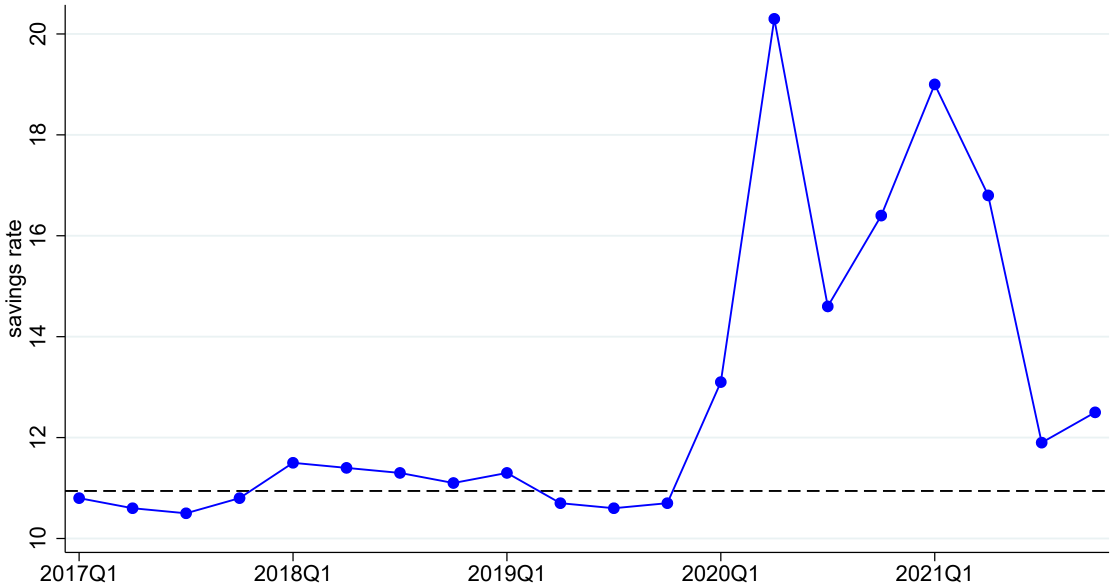
Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) splitting the sample by the seven-day Covid-19 incidence using normalized in-person and online spending as outcomes. The MPC is calculated by multiplying the estimate by the mean number of post-treatment days and the sample mean in in-person or online spending divided by the average transfer amount (€200 times the mean number of children).

Table A.14: Estimation of marginal propensity to consume: excess savings

	(1) low excess savings: BOP HH	(2) high excess savings: BOP HH	(3) low excess savings: GfK	(4) high excess savings: GfK
Treat x Post	0.173***	0.090**	0.132***	0.096**
	(0.050)	(0.042)	(0.042)	(0.042)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
MPC	0.195***	0.103**	0.153***	0.103**
	(0.057)	(0.048)	(0.048)	(0.045)
N	113820	110130	127770	128070

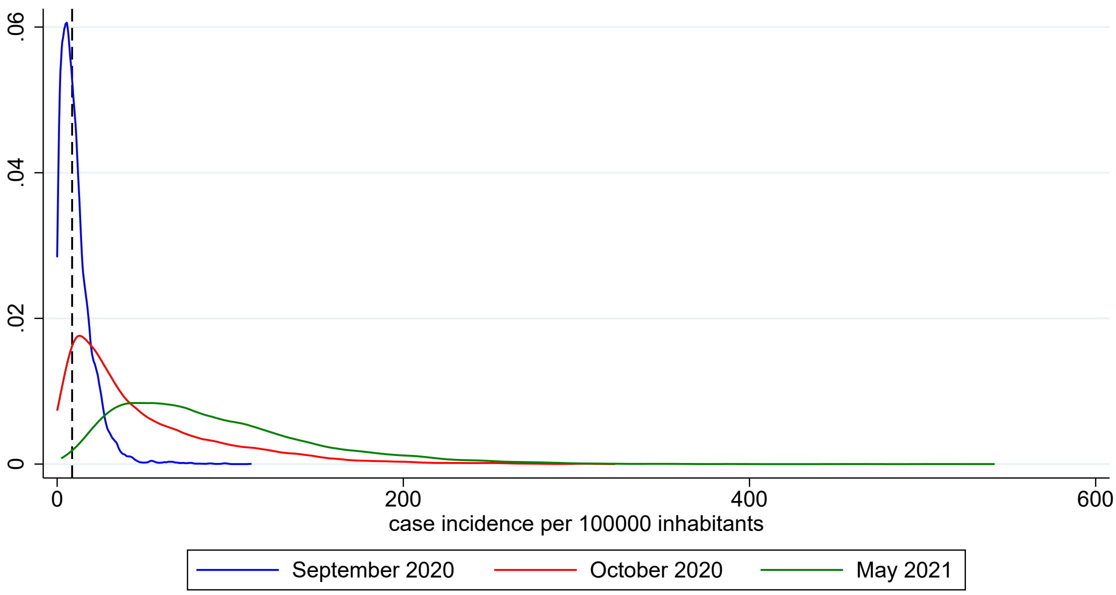
Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on the household level are in parenthesis. The regression results are based on equation (2) splitting the sample by median excess savings at the county level from the Bundesbank Online Panel Household Survey and median excess savings at the individual level from the Gfk data set. 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.5: Quarterly savings rate at the national level



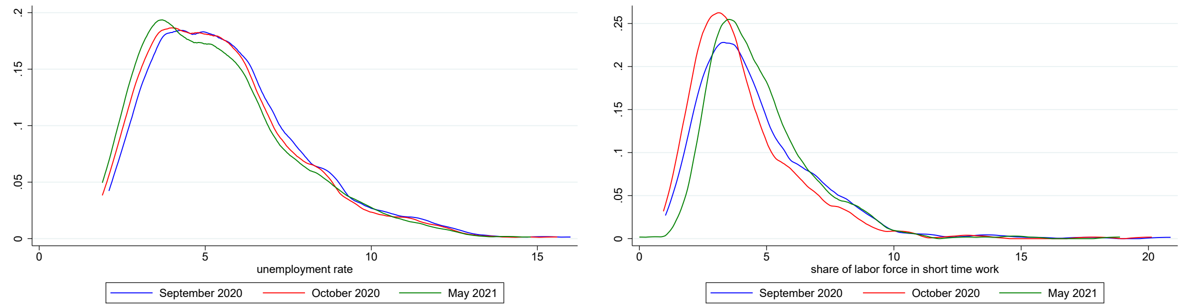
Notes: This figure plots the seasonally-adjusted quarterly savings rate from the national accounts between 2017 and 2021. The dotted line indicates the pre-2020 average.

Figure A.6: Distribution of Covid-19 case incidence in September 2020, October 2020 and May 2021



Notes: This figure plots the kernel distribution of the number of Covid-19 cases per 100000 inhabitants across the 401 German counties in September 2020, October 2020 and May 2021 using a Epanechnikov kernel. The dotted line indicates the median value for September 2020.

Figure A.7: Distribution of the unemployment rate and share of labor force in short-time work in September 2020, October 2020 and May 2021



(a) unemployment rate | (b) share of labor force in short-time work

Notes: This figure plots the kernel distribution of the unemployment rate (Panel A) and the share of the labor force in short-time work (Panel B) across the 401 German counties in September 2020, October 2020 and May 2021 using a Epanechnikov kernel.

Table A.15: Estimation of marginal propensity to consume: transfer size

	(1) small transfer (one child)	(2) large transfer (more than one child)
Treat x Post	0.087**	0.148***
	(0.037)	(0.043)
HH FE	yes	yes
Date x county FE	yes	yes
MPC	0.136**	0.121***
	(0.058)	(0.035)
N	253020	244320

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) including only treatment households with one eligible child or only treatment households with more than one eligible child. 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.16: Effect on the number of shop visits: Poisson regression

	(1) number of shop visits	(2) number of shop visits	(3) number of shop visits	(4) number of shop visits
Treat x Post	0.058***	0.058***	0.055***	0.047**
	(0.020)	(0.020)	(0.020)	(0.021)
HH 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	0.583***	0.579***	0.547***	0.473**
	(0.209)	(0.209)	(0.210)	(0.219)
N	274440	274440	258520	258520

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 taking the percentage effect and multiplying it by the sample mean and the mean number of post-treatment days.

Table A.17: Effect on the number of shop visits: Covid-19 cases and rules

	(1) low cases	(2) high cases	(3) strict rules	(4) lax rules
Treat x Post	0.044***	0.020	0.032**	0.028**
	(0.013)	(0.015)	(0.013)	(0.014)
HH FE	yes	yes	yes	yes
Date x county FE	yes	yes	yes	yes
Additional shop visits	0.993***	0.443	0.724**	0.628**
	(0.290)	(0.348)	(0.292)	(0.310)
N	137120	136954	128300	146290

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 both a low and high case incidence and lax and strict rules as well as urban and rule areas. The additional visits are calculated by multiplying the estimate by the sample mean and the mean number of post-treatment days.

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.[18] 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 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.

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 to see any substantial adjustments of 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

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 children	0.0358*** (0.0095)	-0.0018 (0.0043)	-0.0340*** (0.0098)
N	10831	10831	10831

Notes: Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parenthesis.

[18] The IAB (Institut für Arbeitsmarkt- und Berufsforschung) is the Research Institute of the Federal Employment Agency.

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	0.0685 (0.3439)	0.5411 (0.4479)	0.1592 (0.5350)
N	7283	4612	7283

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.

data to estimate a difference-in-difference model with the following regression equation:

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

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 insignificant for the respondent (see column 1), his or her partner (see column 2), and total hours worked of the household (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 children 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 in the ex-ante analysis, 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*)
- Ich möchte diese Frage nicht beantworten (*I do not want to answer this question*)

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*)

Q11 **Attention to account**: Wie oft überprüfen Sie die Kontoumsätze / den Kontostand Ihres Girokontos? Bitte nur eine Angabe. (*How often do you check your statement of account? Please give only one answer.*)

- seltener als einmal pro Woche. (*Less than once per week.*)
- etwa einmal pro Woche. (*Once per week.*)
- mehrmals pro Woche. (*More than once per week.*)