The Economic Impact of Advance Child Support: Financial Well-being, Labor Supply, and Consumption of Single-Parent Households*

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Abstract

About one in five children across OECD countries lives in single-parent households, many of which receive no financial support from the non-resident parent. To address this, several countries have introduced public advance child support schemes. This paper investigates the impact of such payments on the financial well-being, labor supply decisions, and household spending patterns of single-parent families, drawing on a major reform to Germany's advance child support program that substantially expanded both benefit duration and eligibility in 2017. Using representative data from the German Microcensus and the Income and Expenditure Survey, we find these payments to improve families' financial situations without crowding out private child support. Since eligibility was tied to economic independence of single-parent households, the reform also led to a decrease in the probability of receiving welfare benefits, which appears to be driven by exits from welfare due to increases in labor supply at the intensive rather than the extensive margin. We also find changes in expenditure patterns of affected families, with increased spending

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on food and beverages as well as goods related to the human capital development and well-being of children.

1 Introduction

Single-parent households represent a substantial share of families in many developed countries. Across OECD nations, around 17 % of children live in single-parent households – a proportion that has remained high or even increased in some countries in recent decades (e.g., Nieuwenhuis and Maldonado, 2018; OECD, 2024*b*). These families face elevated risks of poverty, social and material deprivation, stress and family conflict, with serious implications for children's development and well-being (e.g., Moullin and Harkness, 2021; Nieuwenhuis and Maldonado, 2018; OECD, 2024*a*). Child support payments[1] may reduce the economic disadvantages connected to single parenthood. However, many resident parents – mostly mothers – do not receive consistent child support. Only about 25 % of single mothers in the US and UK, and 47 % in Germany, receive full or regular payments (Beblo et al., 2025; Hakovirta and Mesiäislehto, 2022; Pilarz and Cuesta, 2025). In response, many European countries have introduced public advance child support programs to ensure that children receive financial support when the non-resident parent fails to pay. These programs aim to stabilize household income, reduce child poverty, and encourage parental responsibility, with states typically seeking reimbursement from the liable parent.

Despite their widespread adoption – especially across Europe – very little is known about the economic impacts of public child support schemes. An evaluation of advance child support, therefore, requires answers to three questions. First, do public child support payments genuinely improve the financial well-being of single-parent families, or do they merely substitute for private support payments from non-resident parents? Second, how does the design of such programs – particularly when they are tied to other welfare benefits or are means-tested – influence labor supply decisions of the resident parent? Third, how do changes in child support payments, along with any resulting shifts in labor market behavior, affect family spending patterns, especially on goods and services that promote children's human capital and overall well-being?

A priori, the answer to these question is not obvious. Public advance child support

[1] Child support is a legal obligation requiring non-resident parents – typically post-divorce or separation – to contribute financially to their children until adulthood or another legally defined endpoint. They are mandated across all OECD countries, although varying in design and enforcement (OECD, 2025*a*).

payments may improve financial well-being by providing income stability in the absence of reliable private transfers, though they risk crowding out private support if not carefully designed. When such programs interact with other welfare benefits, they can create either work incentives (by conditioning support on employment), or disincentives (by acting as a partial income replacement), thus influencing labor supply decisions. Changes in income and employment resulting from these programs may, in turn, affect household consumption patterns, particularly in relation to expenditures on children, given that the payments are earmarked as child-specific support.

One challenge in studying the causal effects of child support is that selection into single parenthood – and into receipt of public advance child support (often due to the absence of private payments from the non-resident parent) – is not random. In this paper, we overcome these endogeneity concerns by exploiting a major reform to Germany's advance child support system in 2017, thereby providing first quasi-experimental evidence on how public child support affects single-parent families. The reform expanded eligibility for guaranteed and substantial advance child support payments to include children over the age of 11 and eliminated the previous 72-month payment cap. As a result, the number of single-parent families receiving advance child support more than doubled. Using representative microdata from the German Microcensus (MZ) and the Income and Expenditure Survey (EVS), we estimate difference-in-differences models that compare outcomes for single mothers with children newly eligible for advance child support to those whose eligibility was unchanged. Our results show that the reform improved families' financial situations without crowding out private support payments. Since eligibility was tied to economic independence of single-parent households, the reform also led to an increase in working hours among mothers – particularly those previously in the middle of the working hours distribution – and reduced welfare dependency. Evidence based on the Income and Expenditure Survey demonstrates that the reform also significantly altered expenditure patterns of affected families, with increased expenditures on food, beverages, and goods related to the human capital development and well-being of children. These findings are robust to a wide range of sensitivity checks, including accounting for regional policy changes, using a placebo treatment group, alternative sample definitions, and sample selection around reform implementation.

The findings add to several strands of the literature. First, our findings contribute to the literature on post-separation finances of single parents, particularly the role of child support and alimony payments. Most research on child support has examined how stronger enforcement and higher obligations affect fertility and relationship dynamics.[2] and several studies also examine parental behavior.[3] With respect to family finances and mother's labor supply, structural models predict that higher child support smooths consumption around divorce and modestly reduces maternal labor supply (Rangel, 2006), yet experimental evidence from Wisconsin finds no such reduction (Cuesta and Cancian, 2015). A review of US studies shows that stronger enforcement is linked to lower welfare use (Huang and Han, 2012). Research on alimony policies shows that reducing post-marital support increases female labor force participation in the US, particularly among educated women (Verma and Iyer, 2021). Consistent with this, introducing post-separation alimony for cohabiting couples in Canada and Brazil led to declines in female labor force participation (Chiappori et al., 2017; Rangel, 2006).[4] Similar effects are observed in Germany, where women at risk of alimony loss – especially younger cohorts and those with career interruptions – responded with higher participation and working hours (Schaubert, 2023; Usman, 2024), while other studies find no significant average effects (Bredtmann and Vonnahme, 2019). None of these studies considers the general impact of public advance child support or the interaction of it with private payments – despite its widespread adoption. We fill this gap by providing first causal evidence on the effects of public child support payments on single-mother households, complementing and extending the insights from the previously private-focused perspective.

Second, we add to research on the impacts of transfers on the financial situation and

[2]Stronger enforcement is associated with lower nonmarital fertility (Huang and Han, 2012), and was shown to reduce conflict and divorce rates (Nixon, 1997; Zhylyevskyy, 2012), as well as decrease abortion and increase marriage rates among unmarried pregnant women (Tannenbaum, 2020). Higher child support obligations are shown to reduce separation and divorce (Brown et al., 2025; Walker and Zhu, 2006), decrease cohabitation with non-biological fathers (Cancian and Meyer, 2014), and increase fertility with new partners (Kim et al., 2017; Rossin-Slater and Wüst, 2018).

[3]Fathers are more likely to pay child support when they contribute a larger share of income in their new household (Ermisch and Pronzato, 2008). While higher child support obligations increase transfers to mothers, they also reduce father-child co-residence (Rossin-Slater and Wüst, 2018). At the same time, stronger enforcement is associated with greater paternal involvement (Huang and Han, 2012). Moreover, reduced child support disregards have been shown to increase in-kind transfers, indicating strategic responses by non-resident parents (Gunter, 2013).

[4]Additional findings suggest that reduced alimony also lowers marriage and in-wedlock fertility (Fahn et al., 2016), increases divorce rates, and decreases women's expected lifetime income (Verma and Iyer, 2021).

consumption patterns of single-mother households. Hoynes and Patel (2018) show that Earned Income Tax Credit expansions reduce poverty among single mothers, though effects are small at the very lowest income levels. Similarly, Gruber (2004) finds that Aid to Families with Dependent Children cash welfare helped divorced mothers smooth short-term consumption, boosting spending on essentials like food and housing with little crowding-out of other support. Evidence from programs not explicitly targeted at single mothers in the UK, Canada, Germany and Mexico also show that higher transfers to families tend to increase everyday and child-related expenditures, including transport, food, housing, clothing, and education (Angelucci and Attanasio, 2009; Gregg et al., 2006; Hoddinott and Skoufias, 2004; Jones et al., 2019; Najjarrezaparast and Pendakur, 2021; Raschke, 2016; Stichnoth et al., 2018).[5] Experimental evidence from Macedonia further suggests that transfers to mothers increase food spending more than transfers to fathers (Armand et al., 2020).

Third, this paper contributes to the literature on the labor supply behavior of single parents. Numerous studies show that employment-focused policies – such as the Working Families Tax Credit and the New Deal for Lone Parents in the UK, the Earned Income Tax Credit in the US, the Canada Child Tax Benefit, and welfare in Norway – have increased employment and working hours among single mothers while reducing welfare reliance (Bastian and Lochner, 2022; Blundell et al., 2008; Gregg et al., 2003; Løken et al., 2018; Milligan and Stabile, 2007; Mogstad and Pronzato, 2012). Similarly, the introduction of the US Food Stamp program in the 1960s-70s had the opposite effect, reducing employment and hours worked among single mothers (Hoynes et al., 2016). Ortigueira and Siassi's (2023) structural model further highlights that optimal support should vary by child age, advocating greater income support for mothers with preschoolers and stronger work incentives for those with school-aged children. We extend this literature by examining how transfers targeted at children over the age of 11, which are linked to mothers' economic independence, affect maternal labor market behavior. Specifically, we investigate both employment decisions and the number of hours worked, assessing how mothers at various points in the working hours distribution – such as those working few, moderate, or many hours – adjust their labor supply in response to the policy.

In sum, we believe that this paper makes two key contributions to the literature. First,

[5]Stichnoth et al. (2018) also find some substitution toward tobacco among male smokers.

it provides first causal evidence on the impact of advance child support payments on single-parent families – a policy instrument widely implemented across European countries but rarely examined empirically. Our findings show that, when carefully designed to account for interactions with the broader welfare system as well as private child support enforced through legal mechanisms, such payments can effectively mitigate the economic disadvantages associated with single parenthood. We thereby complement existing studies on post-separation finances which are primarily focused on private child support and alimony payments. Second, we contribute more broadly to the literature on labor supply and consumption in single-parent families, showing that additional benefits to mothers are primarily allocated toward enhancing children's well-being, while the associated additional income does not appear to exert a significant disincentive on labor supply.

The paper proceeds as follows: Section 2 outlines Germany's advance child support scheme; Section 3 describes the data; Section 4 explains the empirical strategy; Section 5 presents results, mechanisms, and robustness checks; Section 6 concludes.

2 Advance Child Support Payments in Germany

In Germany, approximately 20 % of children live in single-parent households and around 82 % of single-parent families are led by mothers (Beblo et al., 2025). These households face significant financial challenges: many single mothers struggle to cover basic expenses (see Appendix Figure A.1 and A.2), despite the country's generous social welfare system (OECD, 2025*b*). The risk of poverty among single parents is estimated to be nearly twice that of the average for all individuals with children (Beblo et al., 2025).

To reduce the incidence of child poverty in single-parent households, German law constitutes that both parents are financially responsible for minors who have not yet completed their education or if they are under the age of 21, living in their parents' household and are still in education. If parents live apart and the child primarily resides with one parent, the resident parent fulfills their obligation by providing day-to-day care and the non-resident parent typically bears the full financial responsibility by providing child support (§1606 (3) BGB).[6] In practice, however, it is estimated that 28 % of eligible single mothers in

[6]The amount of child support depends on the income of the parent obliged to pay, the child's age, and the number of children entitled to support. It is derived from the child's tax-exempt subsistence minimum and is recalibrated biennially by the Federal Ministry of Justice and varies by age group as

Germany receive no child support, and 25 % receive less than the full amount (Beblo et al., 2025).[7] Rates are even lower in the US and UK, where only about a quarter of single mothers receive regular or full payments (Hakovirta and Mesiäislehto, 2022; Pilarz and Cuesta, 2025).

To address this, Germany implemented a guaranteed[8] advance child support scheme (*Unterhaltsvorschuss*) which provides state-funded, substantial monthly payments to single[9] parents upon application when the non-resident parent fails to make regular payments. This system is similar to programs found in other OECD countries, such as Sweden's maintenance support (*underhållsstöd*) and France's family support allowance (*Allocation de soutien familial*).[10] The payment amount is not means-tested and as of 2025, corresponds to a maximum amount of 227€ for children under the age of 6, 299€ for children aged 6-11, and 394€ for children aged 12-17.[11] Upon receiving advance payments, the resident parent transfer their legal claim for child support to the state, which then seeks reimbursement from the non-resident parent through instruments such as wage garnishment or debt collection.[12] Recovery attempts by the German state only have limited success – only around 20 % of the amount is actually reclaimed (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2023). In practice, this effectively turns the

follows: 87 % of the subsistence minimum for children up to the age of 5, 100 % for children aged 6 to 11 and 117 % for children aged 12 to 17 (§1612a BGB).

[7]Collecting child support payments from non-resident parents can be logistically and emotionally challenging for resident parents: In a German study, more than one-third of single mothers (35 %) not receiving child support reported forgoing child support to avoid straining their relationship with the non-resident parent (Hubert et al., 2020).

[8]In advance schemes, the state covers payments and later seeks reimbursement from the debtor parent. Guaranteed schemes provide support regardless of repayment success (OECD, 2025*a*).

[9]To be eligible, the resident parent must be unmarried – specifically single, widowed, or divorced – and not have remarried.

[10]These schemes are widespread across Europe (see Appendix Figure A.3) but less common in North-America or Australia, where unpaid support typically leaves children reliant on general social welfare. In the UK and some US states, agencies assist resident parents in collecting child support from non-resident parents, but no public advance payments are provided.

[11]Payments correspond to the statutory minimum child support determined under §1612a (1) BGB and are reduced by the amount of child benefit payable for a first child (§2 UhVorschG), the amount of child support payments from the non-resident parent, and orphan's benefits. Thus, advance child support payments are lower on average than regular child support payments, which are also based on the non-resident parent's income. Advance child support payments are also deducted from other benefits such as Social Allowance (Sozialgeld), Welfare Benefits (ALG II), Social Assistance (Sozialhilfe), or Housing Benefit (Wohngeld), except for the Child Supplement (Kinderzuschlag), where only 45 % is offset (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2025).

[12]During the application process, the parent applying for advance child support must disclose information on the non-resident parent's contact details, employment situation, income, assets and number of children. Once accepted, the non-resident parent is contacted for the repayment. At this stage, the non-resident parent is required to demonstrate his inability to pay the advance child support payments back to the state.

scheme into a tax-funded transfer to single parents.

Germany's advance child support scheme was substantially altered by a reform which was announced on August 17, 2017 and made effective retrospectively as of July 1, 2017. The reform – which is used to identify causal effects – expanded benefit duration and eligibility to older children. Prior to 2017, advance child support was limited to children under age 12 and payed for a maximum duration of 72 months (Deutscher Bundestag, 2018). The 2017 reform extended eligibility to all children below age 18 and removed the time limit on these payments. However, children aged 12 to 17 – who became newly eligible under the reform – must meet additional conditions: they qualify only if neither they nor their resident parent receive welfare benefits (SGB II), or if the resident parent receiving such benefits has a gross monthly income of at least €600 (§1 UhVorschG).

This reform resulted in a substantial increase in the number of beneficiaries of advance child support payments, as illustrated in Figure 1. While the number of benefiting children under the age of 6 remained relatively stable – and even declined slightly – over time, the number of beneficiaries aged 6-11 increased following the reform. Most striking, the reform added approximately 200,000 to 300,000 beneficiaries aged 12 to 17 each year. This resulted in an increase in yearly public spending of about 150 Billion (see Appendix Figure A.4). As a result, it ranks as the third-largest line item in the family policy budget, following spending on parental leave benefits.[13]

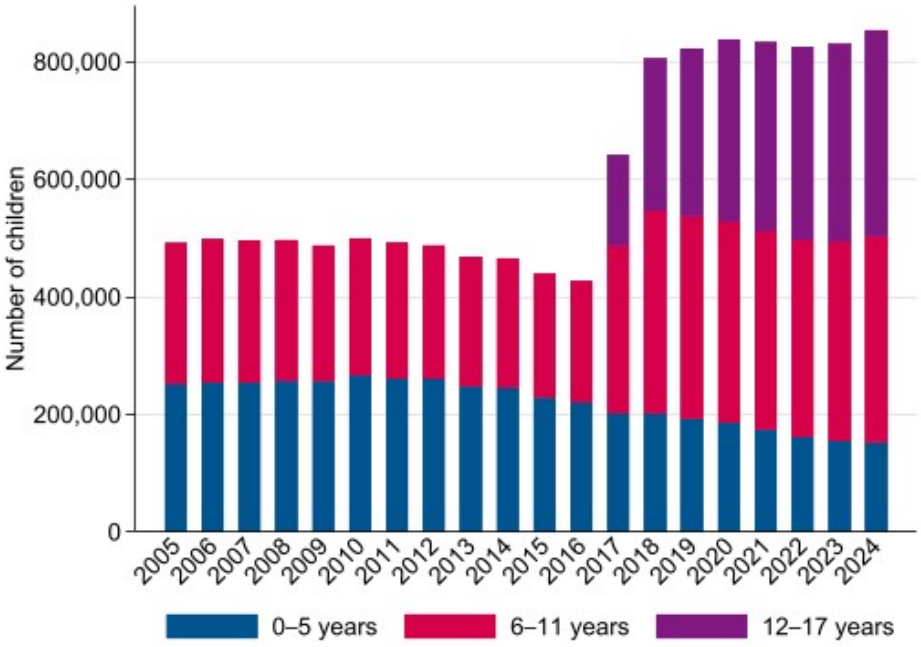
3 Data

3.1 Microcensus

We use German Microcensus data from 2012 to 2021 to examine the impact of the reform of advance child support payments on the family's financial situation and mother's labor supply. The Microcensus is a representative, annual survey of 1 % of all German households residing in private households or communal accommodations (Statistisches Bundesamt, GESIS, 2022). Its large sample size and mandatory participation help reduce selection and compositional bias, which might be particularly relevant when studying

[13] A comparison of total government expenditures (Appendix Figure A.4) with the maximum payment levels (Appendix Figure A.5) and the number of beneficiaries (Figure 1) indicates that nearly all children receive the full amount of advance child support each month.

Figure 1: Number of beneficiaries of advance child support payments



Notes: The figure shows the number of beneficiaries of advance child support payments over time and by age group.
Source: UVG Geschäftsstatistik (2025), own calculations.

single-parent families in repeated cross-sectional data.

Our main sample consists of single mothers – defined as women living with at least one child under the age of 18 whose father is not part of the household – who have not remarried, thus potentially meeting the eligibility criteria for advance support payments. Mothers cohabiting with a new partner without being married remain eligible for advance child support and are therefore included in our sample.[14] The treatment group comprises mothers with children aged 12–17, while the control group includes those with children aged 0–5. Both groups may also have children aged 6–11, though we show that our results remain robust when these families are excluded (see Appendix Table A.3). Our final sample size corresponds to 60,813 observations (38,727 in the treatment group and

[14]We focus on single mothers, who account for 82 % of all single parents in Germany (Beblo et al., 2025), as analyzing mothers and fathers jointly would mask important differences due to mothers' significantly lower average (full-time) employment rates. Unfortunately, the Microcensus does not provide information on custody arrangements or the frequency of contact between children and non-resident parents. Consequently, we are unable to identify the small subset of families in which parenting responsibilities are equally shared (about 5 - 8 %, Beblo et al., 2025; Walper et al., 2020) – circumstances that would typically render the mother ineligible for advance child support payments. We exclude the small subset of mothers who live with both a child whose father is present in the household and another child whose father is not. Similarly, we exclude widowed mothers as any orphan's allowance received by the child substantially reduces the amount of advance child support.

22,086 in the control group).

We consider two main sets of outcome variables: First, we examine the reform's impact on outcomes reflecting the family's *financial situation* using two main measures: (i) mother's total income, comprising income from employment as well as public transfers, such as advance child support payments,[15] and (ii) private support payments – including child support, alimony from former partners, and other forms of private financial assistance. Since 2020, private support payments are only surveyed in the EU-SILC Microcensus subsample (12 %). They refer to the previous calendar year[16] and explicitly only ask for child support payments from the non-custodial parent. Reassuringly, the shares are very similar over time, indicating that our measure primarily captures child support payments in all years.[17] Unfortunately, the Microcensus only contains information on receiving advance child support within the 12 % EU-SILC subsample since 2020. We use data from the Income and Expenditure Survey (described in the following) to assess this.

Second, we assess maternal *labor supply responses* and *welfare reliance*, using four outcome measures: (i) a binary indicator for being employed, and (ii) a binary indicator for receipt of welfare benefits (Arbeitslosengeld II).[18] To study labor supply responses at the intensive margin, we look at (iii) a continuous measure of weekly working hours, set to zero for non-employed mothers; and (iv) a binary variable for near full-time employment, defined as working 30 or more hours per week.

Table 1 presents descriptive statistics for our full estimation sample, as well as separately for the treatment (households with children above age 11) and control groups (households with only children below age 6). 39 % of single mothers report receiving private support payments. Average (log) income is 7.2, corresponding to 1,339€. Overall, 73 % of mothers

[15] In the Microcensus, personal income is reported in categorical ranges. In our analysis, we take the logarithm of the median of each income range to reduce skewness and the influence of outliers, better meet the assumptions of linear regression, and allow for a more intuitive interpretation of coefficients as percentage changes.

[16] We use the 2020 and 2021 surveys to capture payments in 2019 and 2020, respectively. This means our 2019 sample combines two waves, while the 2020 sample – based solely on 2021 data – is smaller. Robustness checks (Table A.3 in the Appendix) confirm that our findings hold without the EU-SILC waves.

[17] The share of parents obtaining child support also aligns with findings in Hartmann (2014)

[18] As with private support, after 2020, benefit receipt is only captured in the EU-SILC subsample. Results are robust to excluding these waves (Appendix Table A.3). As an additional check, we use a variable indicating whether welfare benefits constitute the mother's primary income source – which is available for the full sample for the whole study period – and find similar results (see Appendix Table A.3).

are employed, with substantial variation between groups: 83 % in the treatment group and 56 % in the control group. Accordingly, 25 % receive welfare benefits. Regarding employment intensity, 47 % of mothers are employed for 30 hours or more per week, with an average of 23 hours overall – again showing clear group differences (26 hours in the treatment group vs. 16 hours in the control group). These differences largely stem from our treatment definition, which is based on the child's age. This is also reflected in maternal age with mothers in the treatment group being substantially older than those in the control group (44 vs. 31 years). Relatedly, treatment group mothers are more likely to have completed post-secondary education. The share of mothers holding tertiary degrees as well as the immigration share and the number of children is however remarkably similar across the two groups.

Table 1: Descriptive statistics

	Overall Sample	Control Group	Treatment Group
Outcome variables			
Private support payments	0.38 (0.49)	0.40 (0.49)	0.37 (0.48)
Log(income)	7.21 (0.58)	7.01 (0.61)	7.33 (0.53)
Employed	0.73 (0.44)	0.56 (0.50)	0.83 (0.38)
Receives unemployment benefits	0.25 (0.43)	0.38 (0.49)	0.18 (0.38)
Working hours	22.55 (16.46)	16.45 (16.66)	26.02 (15.29)
Near full-time employment	0.47 (0.50)	0.34 (0.47)	0.55 (0.50)
Control variables			
Age (in years)	39.34 (8.85)	30.90 (6.59)	44.14 (5.87)
Post-secondary degree			
No	0.25 (0.43)	0.34 (0.47)	0.19 (0.39)
Vocational	0.62 (0.49)	0.54 (0.50)	0.67 (0.47)
Tertiary	0.13 (0.34)	0.12 (0.32)	0.14 (0.35)
Immigrated to Germany	0.19 (0.39)	0.19 (0.39)	0.19 (0.39)
Number of children in the household			
1 child	0.66 (0.47)	0.66 (0.47)	0.66 (0.47)
2 children	0.28 (0.45)	0.27 (0.44)	0.28 (0.45)
3+ children	0.06 (0.24)	0.07 (0.25)	0.06 (0.24)
Observations	60,813	22,086	38,727

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The numbers represent shares if no unit is given. Standard deviations in parentheses. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5.

Source: German Microcensus (2012-2021).

3.2 Income and Expenditure Survey (EVS)

We additionally use data from the Income and Expenditure Survey (EVS) in Germany to investigate the impact on single-parent household spending (Federal Statistical Office of Germany, 2025). This administrative survey is collected every five years (we use the 2008, 2013 and 2018 wave), with around 60,000 private households participating, making it the largest survey of its kind within the European Union. Given the high social policy relevance of the EVS – particularly as a statistical basis for determining standard needs for benefits such as unemployment support, child benefits, and advance child support payments – the survey is considered to be of very high quality. In addition, the EVS is a quota sample, meaning all households are selected and surveyed according to a predefined quota plan. This allows us to obtain a sufficiently large sample of single mothers. We apply similar sample restrictions as in the Microcensus data, leaving us with 3,469 observations (2,673 the in treatment and 796 in the control group).

For our outcomes, we mainly rely on information collected in detailed household diaries, in which participating households record all income and expenditures over a three-month period.The EVS groups spending on individual items into 14 broader categories according to COICOP (Classification of Individual Consumption by Purpose).[19] To simplify our analysis, we combine a number of these broad categories into eight groups to assess the reform's impact on family spending composition: food and beverages; clothing, shoes, health and care products; rent (including heating); traffic; education; leisure, entertainment and culture; household appliances and home repairs; other goods and services. We also present results for a set of more narrowly defined goods and services that are particularly relevant to children's well-being or their learning and development: (i) kids clothes and shoes, (ii) tutoring, (iii) toys, (iv) books and stationery and (v) extracurricular activities. Most importantly, the EVS contains information on the amount of advance child support payments received by each household member, which we use to document the substantial post-reform increase in these payments. Descriptive statistics are presented

[19]These categories are: expenditures for housing and energy; transportation; post and telecommunications; health and personal care; clothing and shoes; interior furnishings, household appliances and items; ongoing household management; leisure, entertainment, and culture; restaurants, canteens, hotels, inns; food, beverages, tobacco products; education and childcare; other goods and services; insurance contributions; formation of financial assets; final payments, installment payments, debit and overdraft interest; new loan acquisition and future outstanding payments.

in Table A.1.

4 Empirical Strategy

To identify the causal effect of advance child support payments on families' financial well-being, mothers' labor supply and welfare dependency as well as household spending, we exploit the expansion of eligibility to children aged 12 and older, along with the extension of benefit duration in 2017. In our main specification, we estimate a difference-in-differences (DiD) model, comparing single mothers with children aged 12 to 17 ($Treat = 1$) to those with children aged 0 to 5, before and after the reform:[20]

$$Y_{it} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + X'_{it}\beta_4 + \delta_{rt} + \epsilon_{it} \tag{1}$$

Here, Y_{it} denotes the outcome variables capturing family finances, maternal labor supply, welfare dependency, and household spending for family i in year t. The indicator variable $Post_t$ equals 1 for periods after July 1, 2017, and 0 otherwise. The vector of control variables X'_{it} includes the mother's age (linear and squared), migration background, education level, and fixed effects for the number of children in the household. Additionally, the specification includes a full set of year-by-federal state fixed effects (δ_{rt}) to fully capture state-specific heterogeneity due to e.g. education or family reforms at the state level.[21] Under the assumption of common trends in absence of the reform and no-anticipatory effects, β_3 identifies the causal reform impact on the outcomes considered. We discuss and empirically examine threats to identification in Section 5.4.

We also explore distributional reform effects on mothers' working hours by assessing heterogeneity across the outcome distribution using quantile treatment effect models proposed by Havnes and Mogstad (2015) and Firpo et al. (2009). This method assesses how mothers below a given percentile respond to the reform, relative to the control group. The RIF-DiD estimator relies on the identifying assumption that, in the absence of the

[20] We compare within single-mother families rather than against couple households, since single mothers with children of different ages still face similar institutional environments whereas couple families are subject to fundamentally different support and constraints, such as joint taxation and intra-household insurance. This makes comparable trends and responses to other policies and societal changes far more plausible within single mothers.

[21] Including state-by-year fixed effects improves the precision of our estimates, though results are very similar when using only state fixed effects (see Appendix Table A.3).

reform, changes in the proportion of mothers at specific percentiles would have been the same across treatment and control groups.

To examine dynamic treatment effects and assess the validity of the parallel trends assumption, we estimate event-study specifications based on Equation 1, interacting the treatment indicator with year-specific dummy variables t:

$$Y_{it} = \gamma_0 + \gamma_1 Treat_i + \sum_{t \neq 2017} \gamma_2^t (Treat_i \times Year_t) + X'_{it}\gamma_3 + \lambda_t + \mu_r + \delta rt + \epsilon_{it} \quad (2)$$

In this specification, the year 2017 is omitted and serves as the reference period, as the reform took effect on July 1, 2017, and advance child support payments were primarily made starting in 2018 due to administrative staff shortages (Rehwald, 2018).[22] The coefficients γ_2^t capture the differential outcomes for the treatment group in each year relative to 2017, thereby allowing us to assess both pre-trends and post-reform dynamics in mothers' labor supply, financial situation, and household spending. The treatment indicator, control variables, and fixed effects are defined as in Equation 1.

5 Results

5.1 Effects on the Families Financial Situation

We begin by estimating the reform's impact on the likelihood of receiving advance child support and on the average payment amount (Column 1 and 2 in Table 2). Consistent with aggregate numbers in Figure 1, we find that the reform significantly increased the likelihood of receiving advance child support payments of single-parent households with children above the age of 11: estimates amount to about 27 percentage points (with a pre-reform mean of 5 %[23]). The average monthly amount received increased by about 70 € in 2008 prices. Combining theses numbers suggests that families who receive advance child support payments post-reform almost all get the full amount. Appendix Figure A.6 confirms that the share and the amount obtained by treatment and control group move in parallel before reform implementation and only increased sharply in 2018 for our

[22] Figure A.4 also demonstrates that public spending only began to increase substantially in 2018.

[23] The pre-reform mean is above zero because families in the treatment group are allowed to have children aged 6 to 11 as well who might have been eligible before the reform.

treatment group while the control group exhibits no change in trends.

We next examine potential spillovers on private support payments. Understanding how private transfers respond to publicly funded child support is critical, as such programs may alter fathers' incentives to contribute. On the one hand, the reform could discourage payments by reducing the perceived urgency if mothers are already receiving public support. On the other hand, it may encourage compliance as the state – unlike individuals – may have stronger enforcement mechanisms to recover owed amounts.[24] Moreover, fathers may become aware of mothers' application for advance support, increasing the likelihood of voluntary payment from the outset to avoid legal disputes. Finally, some fathers may face moral or social pressure to contribute once the state assumes their obligations, perceiving public involvement as a negative signal of non-compliance.

Results in Table 2 indicate no crowding-out of private child support payment. On the contrary, the estimate in Column 3 shows an increase in private support payments by about 5 percentage points for mothers with children aged 12 to 17, relative to those with children under the age of 6 (14 % of the pre-reform mean, 14 % of a standard deviation).

Table 2: Effects on public and private support payments and mother's income

	(1)	(2)	(3)	(4)
	Advance child support payments		Private support	Log(Income)
	yes	amount	payments	
Treat × Post	0.271***	67.9***	0.052***	0.033***
	(0.052)	(12.2)	(0.009)	(0.009)
Observations	3,469	3,469	61,798	74,198
Pre-reform mean	0.05	10.15	0.37	7.28

Notes: Difference-in-differences coefficients from Equation 1 using data from the Income and Expenditure Survey in Columns 1 and 2 and data from the Microcensus in Columns 3 and 4. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Amount of advance support payments in EUR and 2008 prices. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: Income and Expenditure Survey (2008, 2013, 2018), German Microcensus (2012-2021).

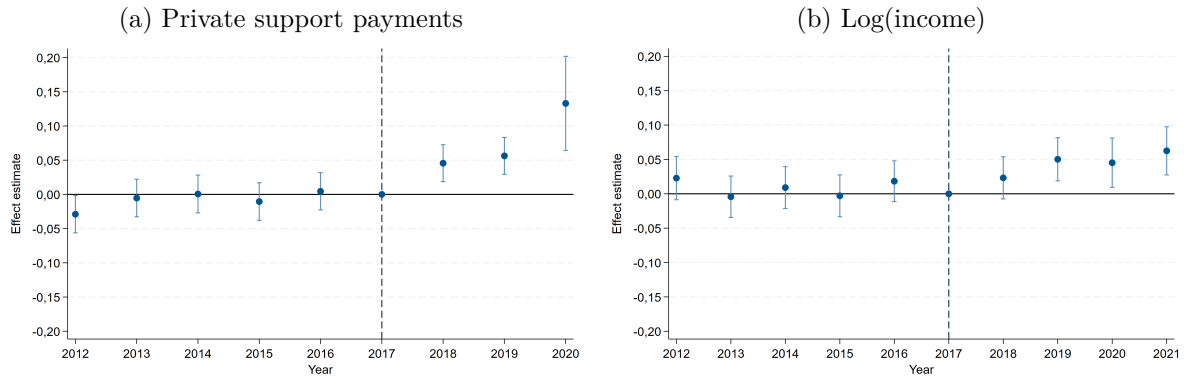
As a second aspect of evaluating whether the additional cash transfer improves the financial situation of single-parent families, we examine mothers' income, which includes public payments such as advance child support. Column 4 of Table 2 indicates that the increase in both public and private support resulted in a 3% rise in mothers' net income. This relatively small effect size may arise from opposing influences: while advance child

[24] When mothers receive advance payments, they transfer their child support claim to the state, which then seeks reimbursement from the non-resident parent through wage garnishment or debt collection.

support provides additional funds to families, the requirement for parents to be employed to receive these payments may reduce welfare dependence, thereby diminishing the overall impact on household income (see Chapter 5.2) .

Figure 2 illustrates the dynamic treatment effects on private support payments and income. It plots the interaction between the treatment group and each year relative to 2017, the final pre-reform year. For private support payments, the pre-reform coefficients (2012–2016) show no statistically significant differences, supporting the parallel trends assumption and thus a causal interpretation. Post-reform, all interaction coefficients are positive and statistically significant, indicating increases in private support payments of 5 to 14 percentage points.[25] Event study graph B likewise shows no statistically significant pre-trends for income. Treatment effects become statistically significant from 2019 onward, ranging between 5 and 6 percentage points annually – nearly twice the magnitude of the overall treatment effect reported in Table 2. The delayed response in income aligns with the notion that labor supply adjustments are subject to short-term rigidities in working hours.

Figure 2: Event Study Analysis: Support Payments and Income



Notes: This figure presents event study estimates of the reform's impact on private support payments and log(income), relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021).

Heterogeneity analyses in Appendix Table A.2 suggest that effects are driven by families with only one child – specifically, families where the eligibility for advance child support

[25] As noted in Section 3, the 2020 sample for this variable is considerably smaller, resulting in wider confidence intervals. This also explains why the overall DiD estimate (5.2 percentage points) aligns more closely with the 2018 and 2019 effects and is not strongly influenced by the 2020 estimate (see Appendix Table A.3).

depends solely on this child – compared to families that may additionally have a younger child (aged 6–11) who could have been eligible for advance payments prior to the reform. The effects are also slightly larger in East Germany and among mothers without a university degree. We note, however, that coefficients are not significantly different across subgroups due to smaller sample sizes, implying that the analysis should be interpreted as suggestive evidence. However, the larger effects observed for mothers without a university degree indicate that the reform particularly benefited lower-income mothers (the average pre-reform income for those without a university degree was 1,360€ compared to 2,230€ for those with a degree) and may have contributed to reducing income inequality among single-parent households.

To sum up, the reform not only lead to an increase in public advance child support payments but also increases in private payments to single-mothers, improving their overall financial situation.

5.2 Effects on Maternal Labor Supply and Welfare Dependency

We next turn to the effects of the reform on resident parents' labor supply, with particular attention to the extent to which the reform's design shaped both labor supply behavior and welfare dependency. A condition for advance child support payments is that children qualify only if neither they nor their resident parent receives welfare benefits (SGB II), or if the resident parent receiving such benefits has a gross monthly income of at least €600 (§1 UhVorschG). This income threshold may incentivize parents to seek employment or increase their working hours to maintain access to the child support benefit.[26] Conversely, these payments may serve as a partial income replacement, potentially reducing the urgency to work or making part-time employment more financially viable.

The DiD estimates in Table 3 suggest no significant impact on the extensive margin of labor supply: coefficients on mothers' employment probability are small in magnitude and precisely estimated. However, we find that mothers with children aged 12–17 are 2 percentage points less likely to receive welfare benefits after the reform compared to those with children under 5 (13 % compared to the pre-reform mean, 6 % of sd, Column

[26]Eligibility for welfare benefits depends on whether household income is sufficient to meet subsistence needs, which varies with household circumstances (e.g., number of children). Consequently, individuals can maintain eligibility while employed, provided their income remains insufficient.

2). This decrease in welfare dependency seems to be primarily driven by an increase in working hours, and thus labor income. While the DiD coefficient in Column 3 of Table 3 suggests a modest, and quite imprecisely estimated, average impact on weekly working hours of about half an hour (1.7 % relative to pre-reform mean, 3 % of sd), we find much stronger effects for the probability to work 30 hours or more post-reform (3 percentage point increase, corresponding to 5 % of the pre-reform mean and 5 % of a standard deviation).

Table 3: Effects on Maternal Labor Supply and Welfare Dependency

	(1)	(2)	(3)	(4)
	Employed	Welfare Benefits	Working Hours	Near full-time Employment
Treat × Post	0.010	-0.024***	0.440*	0.027***
	(0.007)	(0.008)	(0.233)	(0.007)
Observations	75,277	61,798	75,233	75,233
Pre-reform mean	0.82	0.19	25.61	0.53

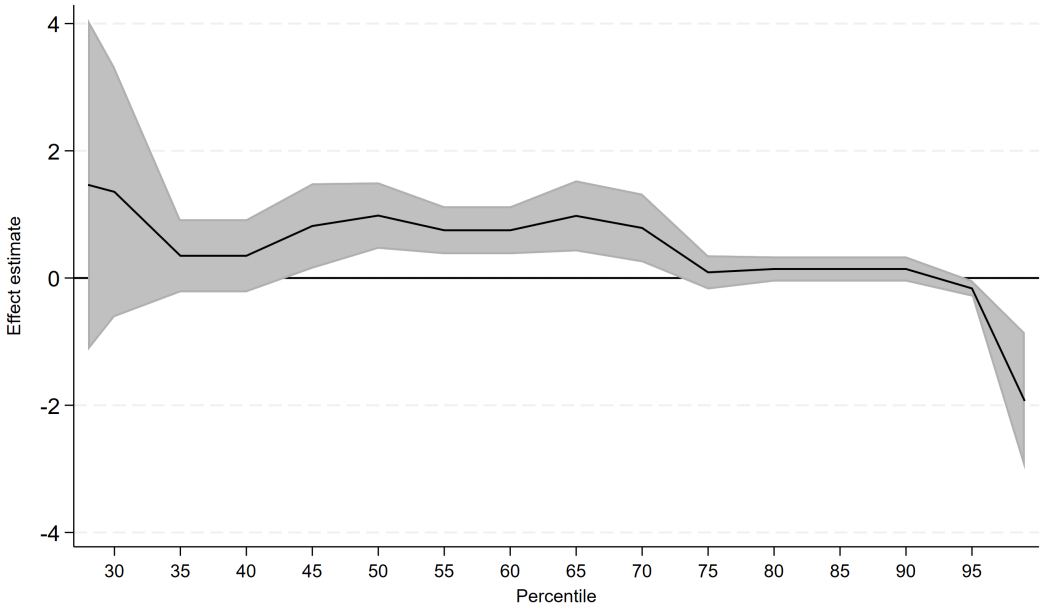
Notes: Difference-in-differences coefficients from Equation 1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: German Microcensus (2012-2021).

To more formally assess potential non-linearities in the impact of advance child support payments on working hours, we present quantile treatment effects in Figure 3 using the RIF-DiD estimator (see e.g., Havnes and Mogstad, 2015; Huebener et al., 2017). The reform seems to have the largest impact on the 45th and 70th percentiles of the working hour distribution – corresponding to 25 to 37 weekly hours – where mothers increased their weekly hours by 0.75 to 1 hour. This suggests that the reform primarily encouraged mothers who were already moderately attached to the labor market to extend their working hours, likely because the greater financial stability provided by advance payments allowed them to take on slightly longer schedules. By contrast, at the upper end of the distribution, i.e. mothers working 41 hours or more, we observe a reduction in hours, with those at the 95th percentile (around 54 hours) cutting back by 2 hours per week. This implies that some mothers with very high labor supply may have used the additional income to reduce extreme workloads, potentially substituting it for more time with their children. Meanwhile, we observe a precise null effect for mothers at the 75th to 90th percentiles (38 to 42 hours), indicating no change in this group's working hours,

which may suggest contractual or time constraints limiting further increases.[27]

Figure 3: Quantile Treatment Effects: Mother's Working Hours



Notes: This figure displays quantile treatment effect (QTE) estimates from the RIF-DiD estimator, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021).

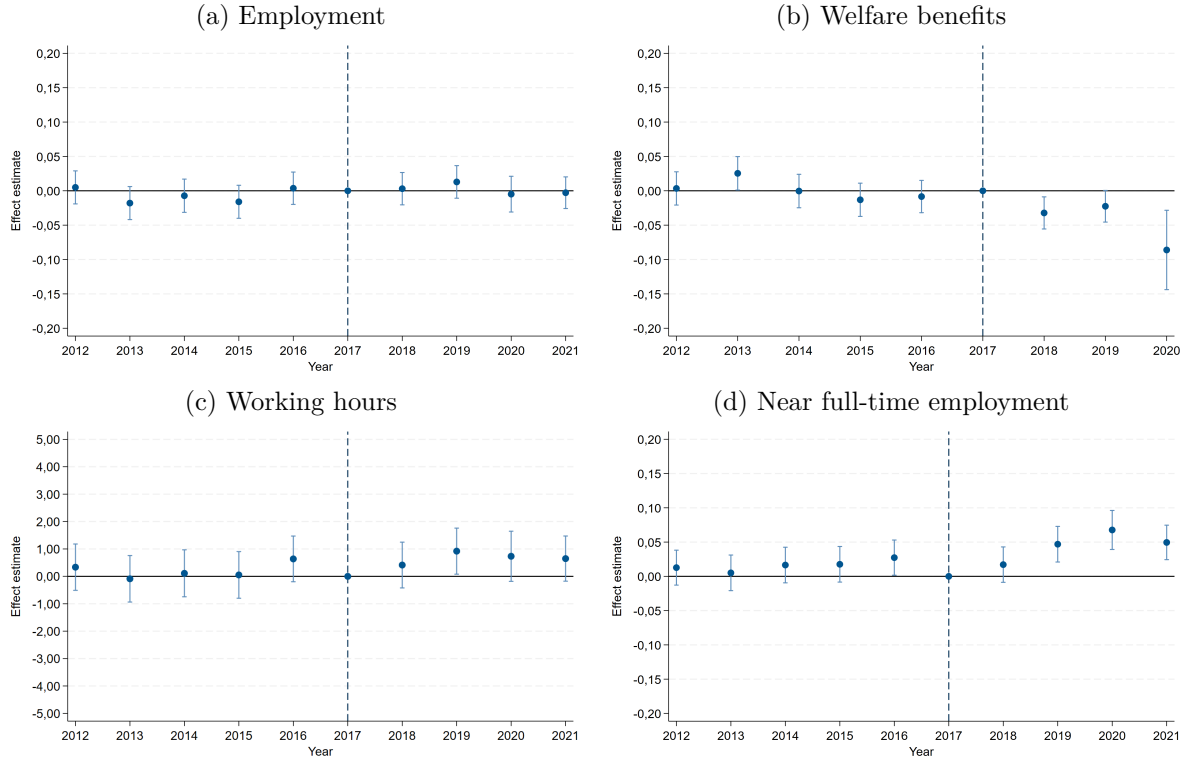
Figure 4 presents the event study estimates for labor market outcomes, examining both pre-trends and dynamic post-reform effects. In line with the DiD results, we observe no significant effect on mothers' employment rates. For welfare benefits, the pre-trends are flat, supporting the parallel trends assumption. Following the reform, we see a decline in benefit receipt, which is strongest in 2020.[28] Conclusions remain unchanged when using a variable indicating whether welfare benefits constitute the mother's primary income source – which is available for the full sample (see Appendix Table A.3). For working hours, there is no consistent pre-trend in either total hours worked or the likelihood of near full-time employment, aside from a positive outlier in 2016. In 2019, we observe a statistically significant increase in total working hours of about one hour per week, while

[27] There is also some indication of labor market entry, as coefficients at the lower end of the distribution are positive, though imprecisely estimated and not statistically significant, which is in line with our small and insignificant DiD estimate.

[28] As noted in Section 3, in 2020 this variable was only captured in the EU-SILC subsample, resulting in a smaller sample size and a wider confidence interval in this year. This also explains why the overall DiD estimate (2.4 percentage points) aligns more closely with the 2018 and 2019 effects and is not strongly influenced by the 2020 estimate.

other post-reform coefficients are not statistically significant. The likelihood of near full-time employment starts to increase in 2019 – corresponding to a 5 to 6 percentage point increase each year. The slight delay in the response of working hours can be attributed to the requirement that individuals must formally request adjustments to their work schedules. Consequently, an immediate adjustment would have been implausible.

Figure 4: Event Study Analysis: Maternal Labor Supply and Welfare Dependency



Notes: This figure presents event study estimates of the reform's impact on employment, receipt of social benefits, and working hours, relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021).

Consistent with the outcomes reported in Section 5.1, we find that the effects of the reform are more pronounced for single-mothers with lower education (Appendix Table A.2). Mothers without university education – those with vocational training or no post-secondary degree – experience gains on both the extensive and intensive margins of labor supply: they are more likely to be employed, work more hours and are less likely to receive welfare benefits (Panel C of Table A.2). For single mothers with only one child, we additionally see an increase at the extensive margin of employment. When differentiating by East and West Germany, we find increases in labor supply in both regions, with larger effect sizes in the East. Notably, pre-reform labor market outcomes among single mothers

are very similar in both regions, although these regions differ markedly in norms regarding maternal labor supply in the general population (e.g., Gambaro et al., 2023; Jessen et al., 2024). It should be noted that coefficients differ significantly across subgroups only for welfare benefits when comparing East and West Germany or by educational attainment. For all other outcomes, subgroup differences are not statistically significant and therefore remain merely suggestive.

In sum, the reform reduced mothers' reliance on welfare benefits, driven by increases in working hours at the intensive rather than at the extensive margin: mothers previously working 25–37 hours per week expanded their hours, while average employment rates remained unchanged.

5.3 Effects on Family Spending and Child-Related Investments

Finally, we examine how changes in child support payments, along with any resulting shifts in labor market behavior, affect family spending patterns, especially on goods and services that promote children's human capital and overall well-being. Previous research documents both tighter budget constraints of single-parent families (see also Appendix Figure A.1 and A.2) and systematic differences in spending between single- and two-parent households (e.g. Ziol-Guest et al., 2006), suggesting that additional transfers might play a major role in shaping expenditure patterns.

Table 4 reports estimates for family's spending composition and spending on child-specific goods. Each row shows the results for a different dependent variable. We find the reform to significantly increase monthly spending on food and beverages by about 32€ relative to a pre-reform mean of 342€ (9 %).[29] The estimate on rent is of similar size, but much less precisely estimated and thus insignificant. For all other spending categories, we fail to detect any significant changes, though we note the limited data size and the fact that in the EVS only one post-reform period (2018) is available yet. Thus, we might fail to detect changes in consumption that require an adjustment period, such as shifts in rent expenditures when moving to a new flat.[30] An alternative to examining the total spending

[29]Examining single goods within this categories shows that a small part of this increase is also driven by an increase in expenditures for cigarettes. This increase in temptation goods is in line with findings from the child benefit literature in Germany (Stichnoth et al., 2018).

[30]We are also not able to show meaningful event study results when using EVS data as we only have 3 survey periods in total. Note that the EVS weighting and sampling procedure follows mandatory MZ

amount is to look at the share of expenditures for each spending category. Results are reported in Appendix Table A.4 and show that budget shares were unaffected by the reform, suggesting that it did not alter consumption preferences in relative terms (i.e., the marginal utility derived from each consumption bundle).

Table 4: Effects on Family spending composition and child specific goods

Dep. variable	(1) Pre-reform mean	(2) Reform effect	(3) N
Food and beverages	342.4	32.0*** (11.5)	3,469
Clothes, shoes, health, care	194.6	5.5 (12.8)	3,469
Kids clothes and shoes	50.1	2.0 (2.55)	13,273
Rent (incl. heating, etc.)	726.4	25.1 (20.6)	3,469
Traffic	244.7	-80.0 (86.7)	3,469
Education	29.8	-10.8 (7.8)	3,469
Tutoring	16.6	12.3*** (2.8)	13,273
Leisure, entertainment, culture	305.6	4.5 (22.7)	3,469
Toys	9.2	3.5** (1.5)	13,273
Books and stationery	16.1	2.2* (1.3)	13,273
Extracurricular activities	14.2	0.8 (1.9)	13,273
Household appliances, home repairs, etc.	105.2	-15.8 (19.0)	3,469
Other goods and services	169.5	6.9 (10.8)	3,469

Notes: Difference-in-differences coefficients from Equation 1. For broader consumption categories, the treatment group consists of single mothers with children aged 12–17, while the control group comprises single mothers with children aged 0–5. For child-specific goods, the control group instead consists of two-parent families with children aged 12–17, since these goods are not relevant for younger children and thus. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: Income and Expenditure Survey (EVS) 2008, 2013, 2018.

For expenditures related more directly to children's human capital and overall well-being, results in Table 4 suggest that the reform increased spending on tutoring as wells as books/stationery and toys. Post-reform treatment families spend about 12€ more on tutoring and 3€ more on toys (40 % relative to pre-reform mean).[31]

Overall, these findings indicate that additional transfer payments to mothers are primarily allocated toward everyday and child-related expenditures, consistent with previous evidence (e.g., Armand et al., 2020; Gruber, 2004).

sampling, implying that assumptions valid for MZ are also likely to apply to the EVS data.

[31]Note that we have to base our estimation results for these child-specific goods on a different control group, namely two-parent families with children aged 12-17. The reason is that almost all child-specific goods are not relevant for children below age 6.

5.4 Robustness of the Results

In this section we conduct a number of robustness checks to support the validity of our empirical findings. Event-study results in Figures 2 and 4 already confirmed that outcomes for single-mothers with children above the age of 11 relative to single-mothers with children below age 6 followed parallel trends before reform implementation, and only started deviating post-reform.

Although we do not observe statistically significant pre-trends, one potential concern is that treatment and control groups may have been affected differently by broader societal or political developments that roughly coincide with reform-implementation. This concern might be particularly relevant given their differing age structures (mean age of mothers of 39 vs. 31 years; see Table 1). To address this, we first note that our estimates are not sensitive to including state-by-year fixed effects, i.e. when flexibly controlling for any family or educational reforms implemented at the state level such as changes in state parental allowance (Gathmann and Sass, 2018), abolishment of daycare fees (Huebener et al., 2020) or changes in the child benefit supplement[32] which are likely to impact treatment and control groups differently (see Panel a in Appendix Table A.3). Controlling for this regional heterogeneity also considers broader economic shocks to local labor markets. Second, we use mothers with children aged 12 to 17 who live with the child's father as a placebo treatment group. These couple-families are more similar in terms of age but are not affected by the reform (see Panel b in Appendix Table A.3). These estimates are significantly different (smaller in magnitude in absolute terms) than our main estimates, except for overall working hours and labor supply at the extensive margin where we find no significant overall effect in our main estimation. For income, we also observe a positive effect in the placebo test, even substantially larger than our main estimate. When examining event-study version of the effect on income using this placebo group, it is evident that the significant DiD coefficient is driven by a non-parallel trend which is smooth around reform implementation (see Appendix Figure A.7).[33] Third, we allow

[32]Since 2005, Germany has provided a child benefit supplement (Kinderzuschlag) to families whose income is sufficient to cover their own needs but not those of their children. Starting in 2016, the benefit was gradually increased to account for inflation and rising living costs by about 10€ yearly. It is estimated that around 2.5 % of households with children receive it. This gradual and small increase should not impose any threat to our identification, though we note that the share of receiving households is likely to be higher in our control group due to lower average working hours and income.

[33]The placebo specification on private support payments yields a positive effect, but the estimate is

the correlation with our set of control variables to be different pre- and post reform by interacting each control variable with a post dummy (Panel c in Table A.3). Overall, these checks indicate that our results are not driven by time-varying confounders, such as policy changes, compositional shifts or other factors that differentially affect outcomes of the treatment or control group around reform implementation.

Another important possible violation of our identifying assumption is that changes in public child advances may affect the likelihood of parental divorce or separation, thus leading to selection into our primary analysis sample. However, in our data we find no such evidence: marriage probabilities of mothers with children above age 11 vs. mothers with children below age 6 do not change discontinuously around the date of reform implementation (DiD coefficient: -0.0017 with se of 0.0020, 0.2% relative to pre-reform mean of 78%).

Next, using data from Google searches[34] in Germany, we investigate population awareness and provide evidence against anticipatory effects. Appendix Figure A.8 displays two peaks in Google searches for the term *Unterhaltsvorschuss*: one in January, when the reform was discussed and the draft law was proposed, and another in July 2017, when the law was adopted and became effective. As we consider the entire year of 2017 as the pre-reform period in our event studies, the first peak in January 2017 should not pose a threat to our identification strategy. The graph also indicates that prior to 2017, the level of search interest was very low and stable, and following the peaks in 2017, the level remained elevated, demonstrating that overall awareness of advance child support substantially increased with reform implementation.

We also test wether our estimates are sensitive to the exclusion of families with children above age 5 and below age 12.[35] Children in this middle age group were already eligible for advance child support before the reform, as eligibility previously extended to age

substantially smaller than in our main analysis and statistically different (χ^2-test p-value = 0.001). The estimated effect on the receipt of social benefits is positive in this specification – opposite in direction from our main estimate. The effect on overall working hours in this placebo test is similar to our main result, which raises some doubts about the robustness of this particular outcome. However, for the probability of near full-time employment, the placebo effect is considerably smaller, statistically insignificant, and significantly different from our main estimate.

[34]Google Trends data have been shown to be reliable proxies for individual behavior in various contexts, including job searches (Baker and Fradkin, 2017) and domestic violence (Anderberg et al., 2022).

[35]Remember that our treatment and control groups are defined as mothers whose children fall into a specific age range (12–17 vs. 0–5 years), excluding those with children in both age groups. However, both groups may include mothers who also have children aged 6–11.

11 and was limited to a maximum of six years. Thus, if parents separated later in the child's life, the child likely received support pre-reform. However, the reform eliminated the duration limit, potentially expanding eligibility for children whose parents separated shortly after birth. As a result, the expected reform effect for this group is ambiguous, and the inclusion of these families may introduce some bias into our main estimates. To assess this, we conduct a robustness check excluding families with children aged 6–11. As shown in Appendix Table A.3 (Panel d), the estimated effects become even larger under this stricter definition, supporting the validity of our main results.

Finally, we assess whether the exclusion of the year 2020 affects our estimates. The rationale for this exclusion is that, since 2020, private support payments and welfare receipt are only surveyed in the 12 % EU-SILC Microcensus subsample and refer to the previous calendar year. Consequently, we utilize the 2020 and 2021 surveys to capture these outcomes for the years 2019 and 2020, respectively, which results in our 2019 sample combining two waves, whereas the 2020 sample – based solely on 2021 EU-SILC data – is smaller. The robustness check presented in Panel e in Appendix Table A.3 confirms that our findings hold without the EU-SILC waves. For welfare benefits, we test whether using an alternative outcome variable, which indicates whether welfare benefits constitute the mother's primary income source and is available for the full sample throughout the study period, changes our results. The estimation in Appendix Table A.3 (Panel f) is very similar to our main estimate. Additionally, results in Panel g of Table A.3 also indicate that results remain stable when not applying the provided survey weights.

6 Conclusion

The question on how to effectively mitigate economic disadvantages to single parenthood is of perennial importance given the high and increasing share of single-parent households across many industrialized countries and the disadvantages these families face along various dimensions (e.g. Boll and Schüller, 2023; Harkness, 2022). We provide first causal evidence on the economic impact of public advance child support payments, a policy instrument widely implemented across (European) countries but scarcely examined in the previous literature. For identification, we exploit a reform to Germany's advance child support system that extended the duration of benefits and expanded eligibility to

older children in 2017. Our findings show that the reform improved the financial situations of single mothers without crowding-out private child support contributions. On the contrary: private support payments increase post-reform, likely reflecting greater awareness and stronger enforcement threats faced by fathers due to the transfer of legal child support claims from the mother to the state. We also find that the design of advance child support programs plays a major role: since eligibility for public payments was tied to the economic independence of single-parent households, we observe a decrease in the probability of receiving welfare benefits, which appears to be driven by exits from welfare due to increases in labor supply at the intensive rather than the extensive margin. We also find that changes in child support payments, along with the resulting shifts in labor market behavior, affect family spending patterns. The reform led to higher household spending on food, beverages, and goods closely tied to child development and well-being.

Taken together, this study highlights a number of novel findings on the role of advance child support schemes. First, we establish their economic importance for single-parent households. Second, we demonstrate that program design is pivotal, particularly in terms of (i) the interaction between child support schemes and welfare systems, and (ii) enforcement arrangements. These findings carry significant implications for child support policy design more broadly, including in countries such as the US and UK, where the current focus is on enforcement rather than on pre-financing mechanisms. Descriptive evidence from Huang and Han (2012) shows that improvements in US child support enforcement led to a 12 % increase in support orders and a 14 % increase in payments. Our results underscore the central role of enforcement and suggest that state-led collection is more effective than leaving the responsibility to resident parents.

When interpreting the results of our study, several limitations have to be kept in mind. First, because the extension of payment duration and the expansion to older children were implemented simultaneously, we cannot disentangle these two effects, although such differentiation would likely be of substantial interest to policymakers. Second, we estimate effects for single-parent families with children above age 11. Results might differ when policies target younger children or low income single-parent households, such as schemes in Belgium or Spain (Hakovirta et al., 2022). Third, a comprehensive policy evaluation should assess the impacts on children's outcomes, as mother's employment and job stabil-

ity are linked to less harsh parenting and fewer behavioral problems in children (Dunifon et al., 2003; Johnson et al., 2012). Therefore, increases in mother's working hours and reductions in welfare dependency may have significant implications for children, beyond the more indirect effect associated with changes in households consumption that we find. Fourth, regular public child support payments provide income stability to single-parent households, which may substantially reduce family stress – a key pathway through which the effects of poverty and single parenthood are transmitted to children (Heintz-Martin et al., 2022). Future evaluations should also consider related parental outcomes. Fifth, while we find that higher public child support payments also raise private payments, we cannot explain fathers' motives. Given low recovery rates in Germany and elsewhere (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2023), future research could examine fathers' responses. This will likely require collection of additional data, as sample sizes of non-resident fathers are too small in the existing datasets.

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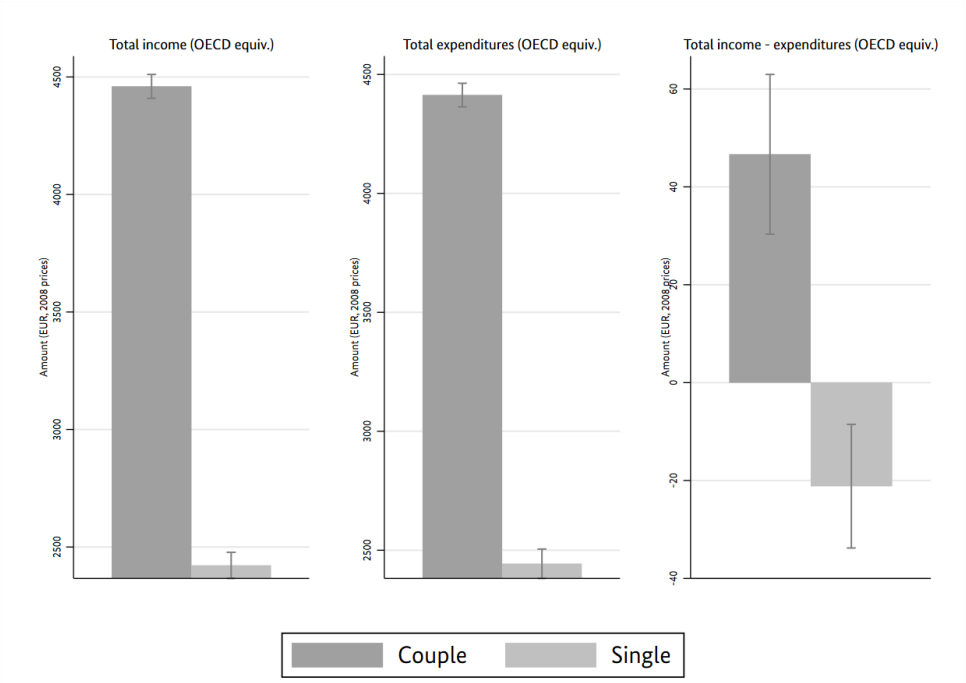
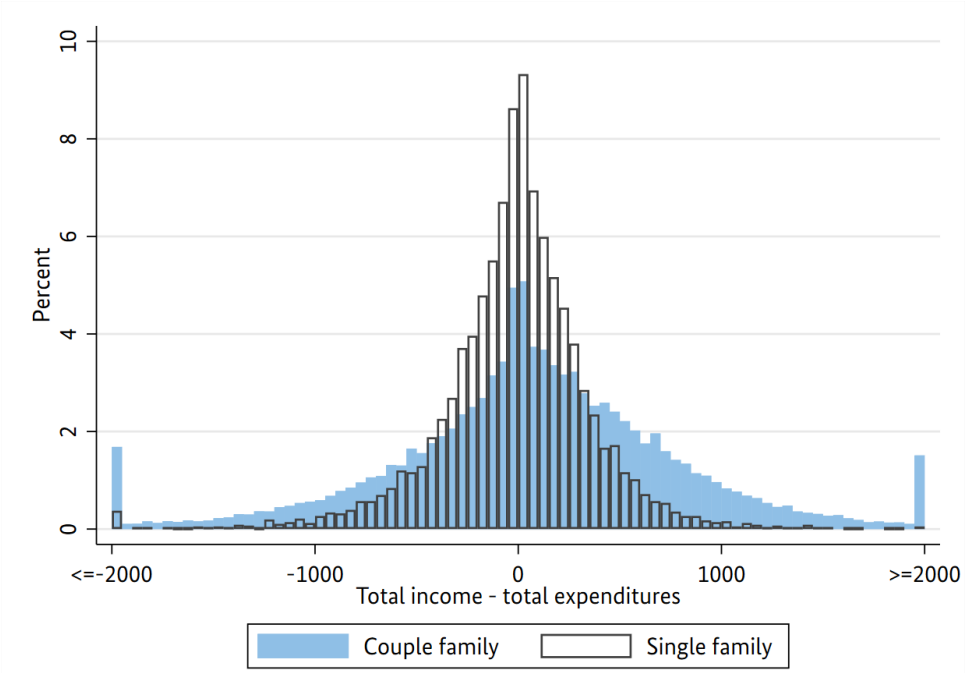
Appendices

Figure A.1: Income and Expenditures by household type



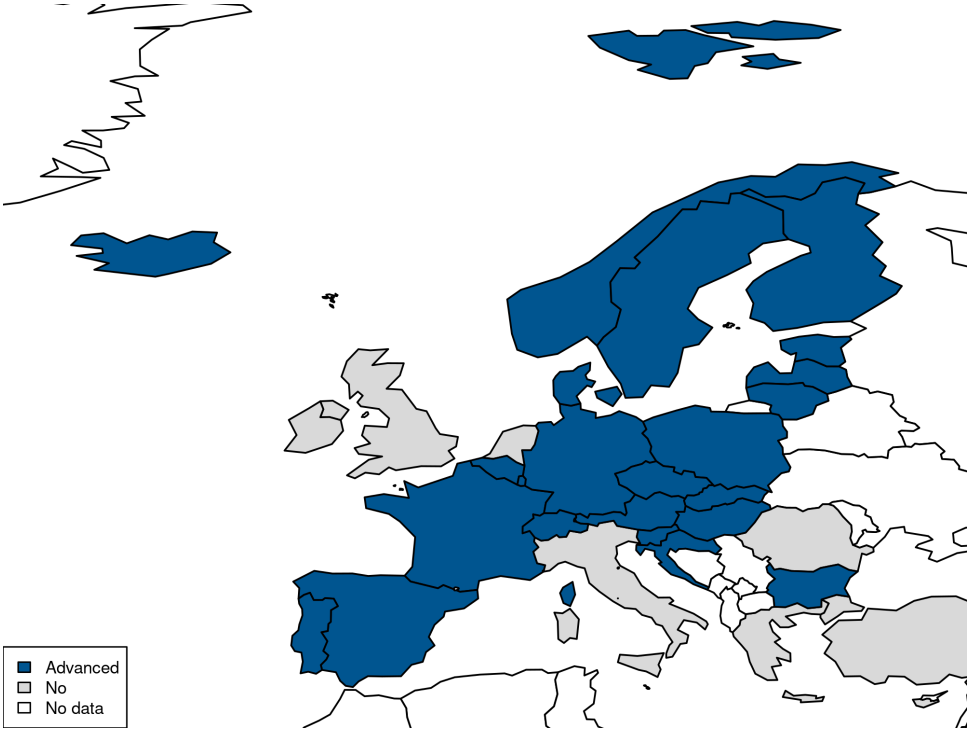
Notes: The figure shows average total incomes and expenditures (deflated in 2008 prices and OECD equ. adjusted) for single and couple households with children below age 18. Total income is composed of income from employment, both dependent and self-employed, income from assets (e.g., interest, rents), public transfer payments (e.g., pensions, social assistance, housing allowance, etc.) as well as private transfer payments and income from subletting. Total expenditures contain all consumption outlays together with certain recurring obligations. This measure constitutes the basis for poverty and distributional analyses, as well as for constructing the consumer price index basket. This data is collected in detailed household diaries (see Section 3.2).
Source: Income and Expenditure Survey 2008, 2013, 2018.

Figure A.2: Distribution of total income - expenditures by household type



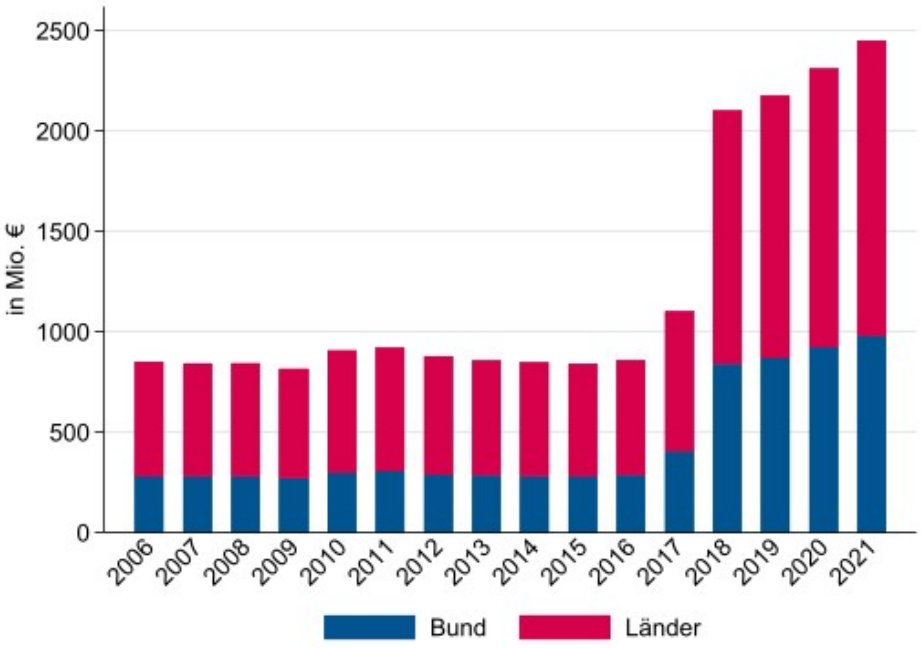
Notes: The figure shows the distribution of total incomes minus expenditures (deflated in 2008 prices) for single and couple households with children below age 18. For a detailed explanation on what these measures compose see Appendix Figure A.1.
Source: Income and Expenditure Survey 2008, 2013, 2018.

Figure A.3: Advance Child Support in Europe 2023/2024



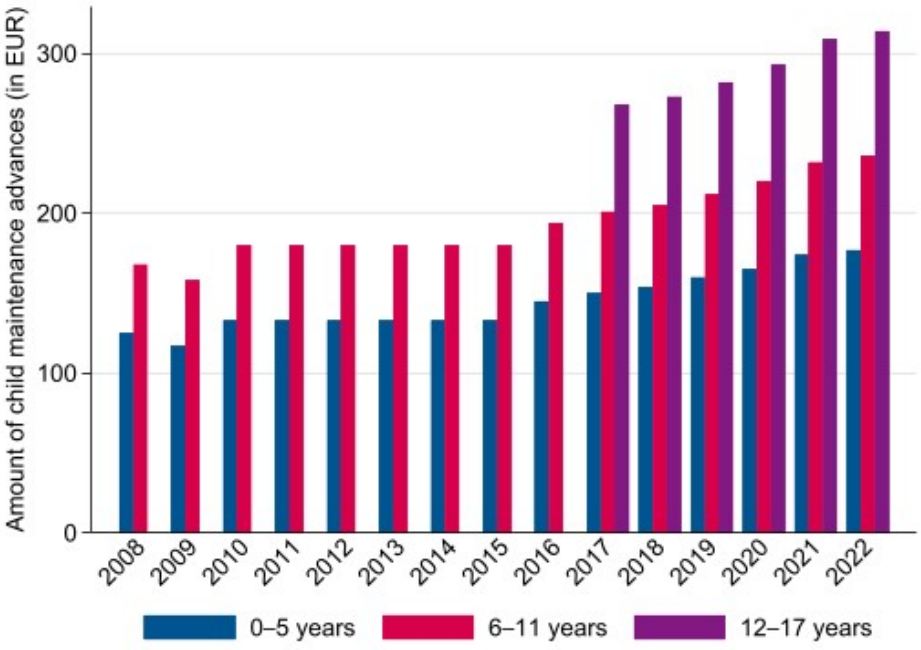
Notes: The figure shows which countries have an advance child support system in place.
Source: OECD (2025*a*) based on National governments and National Legislative Framework.

Figure A.4: Public spending on advance child support over time



Notes: The figure shows the amount spend by the federal government (*Bund*) and by the federal states (*Länder*) on public child maintenance over time (in Mio. €).
Source: UVG Geschäftsstatistik (2025), own calculation.

Figure A.5: The evolution of the amount of advance child support over time



Notes: The figure shows the maximum amount of advance child support by age group and over time.
Source: UVG Geschäftsstatistik (2025), own calculation.

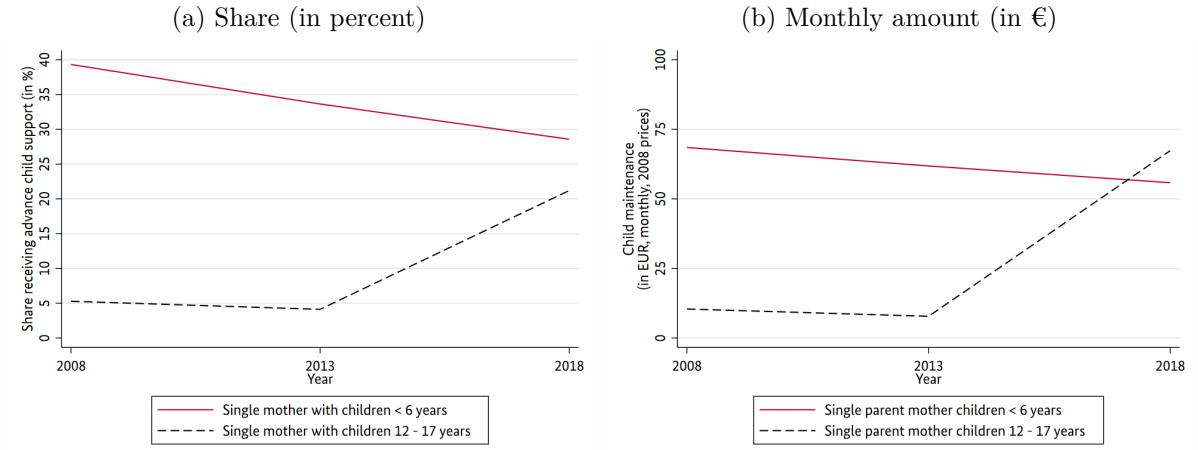
Table A.1: Descriptive statistics EVS

	Overall Sample	Treatment Group	Control Group
Outcome variables			
Advance child support payments			
Advance child support	0.16 (0.37)	0.10 (0.30)	0.37 (0.48)
Monthly amount of advance child support (EUR)	35.63 (99.52)	26.44 (94.98)	66.49 (107.94)
Monthly expenditures by category (in EUR)			
Food and beverages	329.4 (135.9)	347.8 (136.3)	267.9 (115.1)
Clothes, shoes, health, care	180.2 (180.6)	192.5 (187.3)	139.0 (149.0)
Kids clothes and shoes	43.8 (58.4)	43.9 (63.3)	43.5 (37.8)
Rent (incl. heating, etc.)	695.7 (266.4)	727.2 (273.7)	589.8 (208.0)
Traffic	234.2 (585.3)	241.4 (494.3)	210.0 (820.1)
Education	35.0 (71.6)	29.4 (69.4)	53.7 (75.8)
Tutoring	11.7 (61.9)	13.7 (65.5)	4.7 (47.0)
Education and Care	24.8 (77.5)	11.5 (52.3)	69.7 (120.2)
Leisure, entertainment, culture	290.0 (260.9)	316.2 (275.3)	202.1 (179.7)
Toys	11.6 (27.0)	10.1 (26.7)	16.6 (27.5)
Books and stationery	15.3 (23.2)	16.4 (24.3)	11.6 (18.4)
Extracurricular activities	12.9 (30.7)	14.8 (33.6)	6.4 (16.4)
Household appliances, home repairs, etc.	102.1 (222.0)	107.6 (241.1)	83.4 (138.2)
Other goods and services	162.0 (129.6)	170.6 (137.9)	133.2 (90.8)
Control variables			
Age (in years)	38.38 (7.91)	40.93 (6.09)	29.80 (7.23)
Post-secondary degree			
No	0.22 (0.41)	0.21 (0.41)	0.26 (0.44)
Vocational	0.61 (0.49)	0.61 (0.49)	0.59 (0.49)
Tertiary	0.17 (0.38)	0.18 (0.38)	0.15 (0.36)
German Citizenship	0.99 (0.12)	0.99 (0.11)	0.98 (0.15)
Number of children in the household			
One child	0.55 (0.50)	0.51 (0.50)	0.66 (0.47)
2 children	0.36 (0.48)	0.39 (0.49)	0.27 (0.44)
3+ children	0.09 (0.28)	0.09 (0.29)	0.07 (0.26)
Observations	3,469	2,673	796

Notes: The numbers represent shares if no unit is given. Standard deviations in parentheses. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All monetary variables in EUR and expressed in 2008 prices.

Source: Income and Expenditure Survey (2008, 2013, 2018).

Figure A.6: Advance child support payments by treatment status



Notes: This figure displays (a) the share of single-mother households with at least one member receiving advance child support payments, and (b) the average monthly amount of advance child support received per household.
Source: Income and Expenditure Survey (2008, 2013, 2018).

Table A.2: Heterogeneous treatment effects on private support, income, labor supply, and welfare dependency

	(1) Private support	(2) Log(Income)	(3) Employed	(4) Welfare Benefits	(5) Working Hours	(6) Near full-time Employment
Panel A: By number of children						
One child	0.062***	0.047***	0.016**	-0.026***	0.558*	0.025***
	(0.011)	(0.011)	(0.008)	(0.009)	(0.295)	(0.009)
Observations	40,805	48,891	49,545	40,805	49,515	49,515
Pre-reform mean	0.34	7.26	0.84	0.17	27.21	0.59
Two or more children	0.030**	0.010	-0.007	-0.020	0.171	0.029**
	(0.015)	(0.016)	(0.012)	(0.014)	(0.385)	(0.012)
Observations	20,993	25,307	25,732	20,993	25,718	25,718
Pre-reform mean	0.42	7.33	0.78	0.25	22.49	0.43
Panel B: East vs. West Germany						
East Germany	0.069***	0.059***	0.020	-0.069***	0.875*	0.038***
	(0.017)	(0.015)	(0.013)	(0.015)	(0.473)	(0.014)
Observations	15,921	19,447	19,596	15,921	19,586	19,586
Pre-reform mean	0.32	7.21	0.78	0.29	25.88	0.60
West Germany	0.046***	0.023**	0.006	-0.008	0.262	0.021**
	(0.010)	(0.011)	(0.008)	(0.009)	(0.267)	(0.008)
Observations	45,877	54,751	55,681	45,877	55,647	55,647
Pre-reform mean	0.38	7.30	0.83	0.17	25.54	0.51
Panel C: By university degree						
No university degree	0.053***	0.035***	0.015*	-0.034***	0.645**	0.030***
	(0.009)	(0.010)	(0.008)	(0.009)	(0.260)	(0.008)
Observations	53,545	63,606	64,567	53,545	64,530	64,530
Pre-reform mean	0.36	7.22	0.80	0.22	24.55	0.51
University degree	0.037	0.011	0.011	0.011	0.058	0.0285
	(0.024)	(0.027)	(0.015)	(0.013)	(0.621)	(0.021)
Observations	8,253	10,592	10,710	8,253	10,703	10,703
Pre-reform mean	0.40	7.71	0.93	0.05	32.44	0.69

Notes: Difference-in-differences coefficients from Equation 1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: German Microcensus (2012-2021).

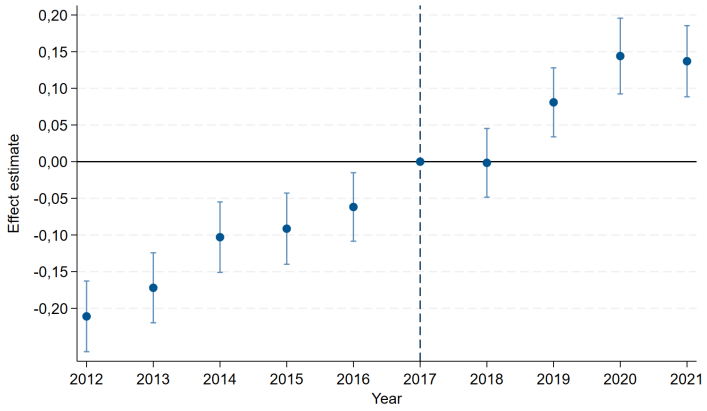
Table A.3: Robustness of reform effects on private support, income, labor supply, and welfare dependency

	(1) Private support	(2) Log(Income)	(3) Employed	(4) Welfare Benefits	(5) Working Hours	(6) Near full-time Employment
Main estimate	0.052***	0.033***	0.010	-0.024***	0.440*	0.027***
	(0.009)	(0.009)	(0.007)	(0.008)	(0.233)	(0.007)
Observations	61,798	74,198	75,277	61,798	75,233	75,233
a) Excluding state-by-year FE	0.0507***	0.0305***	0.00703	-0.0194***	0.296	0.0220***
	(0.00870)	(0.00905)	(0.00660)	(0.00752)	(0.233)	(0.00718)
Observations	61798	74198	75277	61798	75233	75233
b) Placebo treatment	0.035***	0.204***	0.007	0.025***	0.415**	0.009
	(0.007)	(0.014)	(0.006)	(0.007)	(0.203)	(0.006)
Observations	181,551	219,173	222,623	181,551	222,500	222,500
Chi-test (p)	0.001	0.000	0.457	0.000	0.877	0.001
c) Interacting controls with post indicator	0.0421***	0.0267**	0.00791	-0.0376***	0.593*	0.0366***
	(0.0132)	(0.0132)	(0.0100)	(0.0113)	(0.358)	(0.0112)
Observations	61798	74198	75277	61798	75233	75233
d) Excluding families with 6-11 year olds	0.055***	0.047***	0.017**	-0.033***	0.668**	0.031***
	(0.010)	(0.011)	(0.007)	(0.008)	(0.268)	(0.008)
Observations	49,055	58,852	59,682	49,055	59,645	59,645
e) Excluding 2020	0.0492***			-0.0222***		
	(0.00883)			(0.00764)		
Observations	61,078			61,078		
f) Primary income source: WB				-0.0228***		
				(0.00631)		
Observations				75,277		
g) Not weighted	0.051***	0.031***	0.006	-0.023***	0.336	0.023***
	(0.009)	(0.009)	(0.006)	(0.007)	(0.226)	(0.007)
Observations	61,798	74,198	75,277	61,798	75,233	75,233

Notes: Difference-in-differences coefficients from Equation 1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

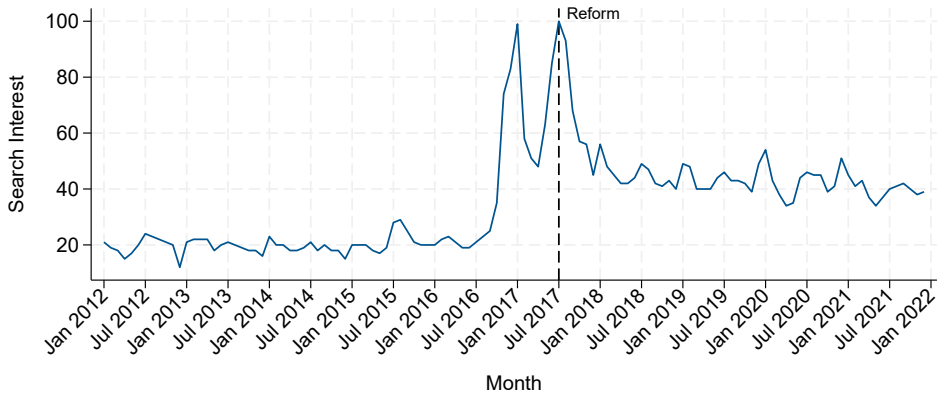
Source: German Microcensus (2012-2021).

Figure A.7: Placebo Event Study: Income



Notes: This figure presents event study estimates of the reform's impact on mother's income, relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of couple families with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021).

Figure A.8: Google Search Trends



Notes: The figure shows monthly Google search interest for the term "Unterhaltsvorschuss". The values represent search interest relative to the peak popularity of the term (100 = maximum interest).
Source: Google analytics (retrieved 08/2025).

Table A.4: Effects on budget shares: family spending and child specific goods

Dep. variable: shares relative to total expenditures)	(1) Pre-reform mean	(2) Reform effect	(3) N
Food and beverages	0.11	0.01 (0.01)	3469
Clothes, shoes, health, care	0.06	-0.00 (0.00)	3469
Rent (incl. heating, etc.)	0.25	0.01 (0.01)	3469
Traffic	0.07	-0.00 (0.01)	3469
Education	0.01	-0.01 (0.00)	3469
Leisure, entertainment, culture	0.09	-0.01 (0.01)	3469
Household appliances, home repairs, etc.	0.03	-0.00 (0.00)	3469
Other goods and services	0.06	0.01** (0.00)	3469

Notes: Difference-in-differences coefficients from Equation 1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: German Microcensus (2012-2021).