Families of Austerity: Benefit Cutbacks and Family Stress in the UK

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Abstract

Benefit cutbacks have been prominent after the Great Recession. The Family Economic Stress Model theorises how financial losses such as those brought by cutbacks might adversely affect parental and child well-being, but linkages to policy effects have been few. Effects on children, in particular, might manifest during adolescence, yet scholarly and policy emphasis rests on early childhood. We extend current knowledge with a comprehensive assessment of how benefits cutbacks affect parents and their adolescent children.

We rely on the UK Household Longitudinal Study and event studies to examine the effects of cutbacks in the UK, which have been particularly severe in recent years. We find that lower-income (single) mothers accumulated benefit income losses equal to around half (a third) of their benefit income before cutbacks. Despite increased workforce participation, mothers could not fully compensate for these losses via labour income. Financial worries and mental health worsened among lower-income and single mothers, despite little evidence of change in material hardships. Adolescent socio-emotional difficulties increased, especially among girls in lower-income families. We find little evidence, though, that cutbacks disrupted parenting. We suggest that benefit cutbacks might deserve re-thinking given the inequitable consequences they can impose across generations.

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Previous research has shown that expansions of state-benefit programs can boost family incomes and thereby improve parental well-being and children's life chances ([Milligan & Stabile, 2011](#); [Dahl & Lochner, 2012](#); [Hoynes et al., 2016](#); [Barr et al., 2022](#)). Few have looked into the consequences of benefit cutbacks, despite their prominence following the recession of 2008/9 ([Alesina et al., 2020](#)). Whether families might be affected by cutbacks just as much as they are by expansions, or even more and why, are open questions, all the more urgent now that living costs have soared and economic relief measures tied to the pandemic have expired.

Benefit cutbacks may have detrimental effects on parents and children via the paths outlined by the Family Economic Stress Model (FESM, e.g. [Elder, 1974](#); [McLoyd, 1990](#); [Conger et al., 1994](#); [Mistry et al., 2008](#)). The FESM underscores how lack of income may exert economic pressure on families, heightening perceptions of financial inadequacy and worry. The latter could adversely affect parental mental health, increase inter-parental conflict, and lead to harsher or more neglectful parenting – ultimately hurting children's behavioural and socio-emotional adjustment. Most tests of the FESM have been correlational, though, as studies defined “lack of income” based on measures of family income at one point in time or averaged across multiple periods. The income dynamics that might lead to such a lack of income have been relatively overlooked (but see [Gennetian et al., 2015](#); [Hill, 2021](#)). In addition, income dynamics can be causally linked to policy changes more readily than (average) income levels at one point in time. The investigation of such policy effects has also remained beyond the remit of studies informed by the FESM. Our study provides one of the first causal assessments of how benefit cutbacks can spur income dynamics and affect parents and children in line with the FESM.

Among child outcomes, in particular, the FESM has been linked most often to socio-emotional and behavioural adjustment ([Conger & Donnellan, 2007](#); [Masarik & Conger, 2017](#)), a well-established predictor of future life chances ([Heckman et al., 2006](#); [Currie, 2009](#)). Adolescence is a critical developmental stage in this respect (e.g. [Blakemore, 2008](#); [Goodman & Goodman, 2009](#); [Patalay & Fitzsimons, 2018](#)). We link adolescent socio-emotional and behavioural adjustment to benefit cutbacks. Our study thus builds

and expands on previous literature on adolescent outcomes and retrenchment of state-benefit programs, so far limited to the study of policy experiments and welfare reform in the 1990s in the US ([Gennetian et al., 2002](#)).

In short, we address three gaps, namely the study of *benefit cutbacks*, how these may causally affect *components of the FESM over time*, and whether and how cutbacks may also influence *adolescent socio-emotional and behavioural adjustment*. We focus on the UK for its government’s “austerity” cutbacks have stood out in the last decade ([Cribb et al., 2018](#); [De Agostini et al., 2018](#); [Fetzer, 2019](#)). In doing so, our findings bear relevance for countries who have implemented similar cutbacks in the period (e.g., Ireland; [Alesina et al., 2020](#)) and for similar examples of cutbacks to state benefits targeting families with children¹. Rich panel data from the UK Household Longitudinal Study (UKHLS, [University of Essex \[ISER\], 2020](#)) for the period 2009-2020 allows us to implement an event-study design and capture how changes in parental and children’s well-being have evolved in the aftermath of austerity.

1. Background

1.1. Policy changes, households, and the FESM

This study is closely related to previous research on benefit changes and parental and children’s well-being (for reviews, [Gennetian et al., 2002](#); [Cooper & Stewart, 2021](#); [Simpson, Albani, et al., 2021](#)). For example, studies have found improvements in mental health among mothers receiving income boosts via the Earned Income Tax Credit in the US ([Evans & Garthwaite, 2014](#); [Boyd-Swan et al., 2016](#)). Research on the expansions of a child-benefit program in Canada linked benefit receipt to lower self-reported depression among mothers and less behavioural maladjustment among their children aged 4 to 10 ([Milligan & Stabile, 2011](#)). Whilst evidence about benefit expansions abounds, comparable studies on cutbacks are lacking.

The consequences of cutbacks may be symmetric to those of benefit expansions, but previous research suggests otherwise. For adult mental health, studies have found stronger negative associations with income loss compared to positive associations with

¹For one, a host of European countries including Germany, the Netherlands, Ireland, as well as the UK itself, have clawed back income support to families with more than one child, starting in the 1990s and more recently in the aftermath of the Great Recession (e.g. [Riphahn & Wiynck, 2017](#)).

income gains ([Benzeval & Judge, 2001](#); [Kromydas et al., 2021](#)). By clawing back income support to households, cutbacks can thus deliver income losses and elicit more significant effects on well-being than benefit expansions.

As per household production models, by lowering the budget line and reservation wages, cutbacks may push household members to increase their work hours or find new employment (e.g. [Simpson, Bambra, & Brown, 2021](#) for a review in the context of welfare reform). Changes in labour supply may avert income loss and its consequences thanks to the extra earnings. Alternatively, earnings may not compensate for lost benefit income or employment itself may prove stressful and reduce the time allocated to leisure and childcare, with adverse effects on well-being. The overall consequences of cutbacks on well-being within households are thus theoretically ambiguous. Effects may be zero-sum or self-reinforcing when jointly considering income and employment dynamics spurred by policy changes ([Heckman & Mosso, 2014](#)).

If any, adverse changes in parental and children's well-being would accord with the FESM. Much like the economic downturns that spurred the FESM literature ([Elder, 1974](#); [Conger & Conger, 2002](#)), cutbacks may affect components of the FESM by triggering economic pressure. The FESM underscores the primacy of *perceived* pressure, a state of heightened worry over household financial circumstances ([Conger & Donnellan, 2007](#); [Mistry et al., 2008](#)). Pressure may also manifest in material hardships such as the inability to keep up with essential expenses like bills, rent or food. Perceived and more objective economic pressure may lead to worsened parental mental health, disrupt inter-parental relationships, and shift parenting towards less warm and harsher styles. The effects of economic pressure may then “cross over” to affect children's well-being, especially their socio-emotional and behavioural adjustment (e.g., [McLoyd, 1990](#); [Yeung et al., 2002](#); [Kiernan & Huerta, 2008](#); [Washbrook et al., 2014](#); [Schenck-Fontaine & Panico, 2019](#)). However, studies have stopped short of causal claims. Calls to causally link components of the FESM to policy changes ([Gennetian et al., 2008](#)), in particular, have not stimulated enough research.

One exception is research on US welfare reform in the 1990s. These studies examined child outcomes situated more causally downstream than socio-emotional and be-

havioural adjustment, focusing instead on educational outcomes during adolescence ([Gennetian et al., 2002](#)). Nevertheless, evidence of some detrimental effects of welfare reform on adolescent school performance and grade repetition may suggest that US welfare reform triggered pathways coherent with the FESM. Although less extensive than for child outcomes, evidence was also found for a deterioration in parent-adolescent interactions (*ibidem*: 43-44; see also [Kalil et al., 2022](#) for a recent reappraisal). A more comprehensive analysis of parental environments informed by the FESM is lacking. Besides, US welfare reform mainly imposed tighter work requirements and shorter time limits for welfare receipt. It might not be straightforward to generalise findings from the US literature to other policy changes, particularly more recent examples of cutbacks to benefit levels.

1.2. Benefit cutbacks after the Great Recession in the UK

Culminating with the *Welfare Reform Act* of 2012 and the *Welfare Reform and Work Act* of 2016, the UK government passed a raft of reforms to state-benefit programs, citing the need to lower the budget deficit and foster gainful employment after the Great Recession. Seven programs were subjected to repeated reforms in the period: 1) Child Benefit; 2) Working Tax Credit and Child Tax Credit; 3) Child Tax Credit; 4) Jobseeker's Allowance; 5) Income Support; 6) Employment and Support Allowance; 7) Housing Benefit. The last six are all means-tested programs, with the poorest half of households having 85% of all of such so-called "legacy" entitlements. Households with children make up to 51% of working-age households on legacy benefits (≈ 7 million in the period) and are the only ones eligible to receive Child Benefit (see, e.g., [Browne et al., 2016](#); [Brewer et al., 2019](#)). In addition, the *Welfare Reform Act* introduced two new programs, namely 8) Universal Credit and 9) Personal Independence Payment, which would respectively replace legacy benefits and Disability Living Allowance over time. By and large, cutbacks restricted eligibility, froze transfer levels or their up-rating, and curtailed entitlements ([Beatty & Fothergill, 2018](#); [Fetzer, 2019](#)). Only a handful of changes, in particular to Working Tax Credit in 2012-13², tightened work mandates.

²During the same period, work requirements were tightened for single parents receiving out-of-work benefits, under the so-called Lone Parent Obligations (LPO, [Katikireddi et al., 2018](#); [Avram et al., 2018](#)). These changes applied to single parents with children aged between 5 and 10 (reforms in the

Overall, reforms directly clawed back income support from people claiming benefits. Hence, we consider these reforms as benefit cutbacks, affecting, first, household (benefit) income dynamics. Table 1 provides a synthesis of the reforms, whilst Appendix 1 presents each reform in more detail.

Incomes in low- and middle-income households were disproportionately affected by the reform process. At the bottom of the income distribution, state benefits cushioned earning losses in the immediate aftermath of the Great Recession (Cribb et al., 2017; see also Cribb et al., 2018; Avram et al., 2019). Yet, income losses followed policy reforms after 2011-12. Falls in benefit receipt, in particular, lead to an estimated 4% net household income loss for households in the poorest fifth of the family income distribution (Cribb et al., 2018). Benefit income fell more sharply among families with no earners (*ibidem*) and, among those with children, in single-parent households (De Agostini et al., 2018). Therefore, we expect that the impact of cutbacks as per the FESM was also most prominent in lower-income and single-parent families.

In a burgeoning field, research examining separate austerity reforms has already found adverse effects on adult mental health (Wickham et al., 2020; Reeves et al., 2020; Brewer et al., 2022), one of the outcomes of interest in the FESM. These studies have identified clear policy effects, yet often focused on selected groups of claimants (esp. job-seekers, lower-income households) to minimise the influence of simultaneous changes across welfare programs. More evidence is also needed on the intergenerational effects of cutbacks across generations and related inter-household disparities. We treat simultaneous changes across welfare programs as the unitary policy agenda they were (see Fetzner, 2019 for a similar approach) and conduct our analyses within the multi-generational framework provided by the FESM.

2. Data

We use data from Waves 1 to 10 of the UK Household Longitudinal Study (UKHLS, University of Essex [ISER], 2020) for the period 2009-2020 (pre-pandemic). UKHLS

period October 2009-May 2012), thus outside the remit of our study. Similarly, the *Welfare Reform and Work Act* of 2016 increased conditionalities for those on Universal Credit who are responsible carers of children under five. Our focus lies, differently, on parents of adolescent children.

Table 1: Benefit programs, reforms, and reform exposure definitions.

Program	Reform	Year	Main changes	Exposure: definition using UKHLS
Working Tax Credit, Child Tax Credit	Tax Credits reform (1)	2011	Stricter working hours requirement and changes to taper rates, indexation, supplements, childcare element	On Tax Credits continuously prior to April 2011
Housing Benefit	Local Housing Allowance (LHA)	2011	Housing Benefit not paid if private rent above new LHA threshold set at 30th percentile of local rents	On Housing Benefit and private renter continuously prior to April 2011
Child Benefit	High Income Child Benefit Charge	2013	Child Benefit progressively withdrawn from higher-income households	On Child Benefit continuously prior to January 2013; at least one earner averaging $\geq 50,000$ GBP prior to January 2013
Various	1% up-rating (1)	2013	Flat up-rating of certain benefits instead of rising with inflation	On any legacy benefit, except for Housing Benefit and Child Benefit, continuously prior to April 2013
Housing Benefit	Bedroom Tax	2013	Limit to Housing Benefit payments for social renters based on property/household features	On Housing Benefit and social renter continuously prior to April 2013 + excess bedroom vis-à-vis household structure (Fetzer, 2019); did not apply in Scotland
Various	Benefit Cap (1)	2013	Benefits capped if total benefit income above certain thresholds	On benefits exposed to cap prior to September 2013 - exemptions (Kennedy et al., 2016) + “likely capped” (Reeves et al., 2020): single, or two-parent family with 3+ children, rented accommodation, on Housing Benefit; did not apply in Northern Ireland until 2016
Universal Credit	Universal Credit	2013	New program encompassing old “legacy system” for new claimants, gradual roll-out	Ever received Universal Credit after April 2013
Disability Living Allowance	Personal Independence Payment	2013	New program substituting Disability Living Allowance (DLA), gradual roll-out	Ever received PIP after April 2013
Various	1% up-rating (2)	2014	Flat up-rating of certain benefits instead of rising with inflation	On Housing Benefit and Child Benefit continuously prior to April 2014
Working Tax Credit, Child Tax Credit	Tax Credits reform (2)	2016	Reduction in income thresholds and increase in taper rates	On Tax Credits continuously prior to April 2016
Various	Benefit Cap (2)	2016	Benefits capped if total benefit income above certain thresholds	See Benefit Cap (1) (see also, Kennedy et al., 2016; Reeves et al., 2020)
Various	Benefit Freeze	2016	Freeze in the value of most working-age benefits	On legacy benefits continuously prior to April 2016

is a household panel survey of the members of around 40,000 households (Wave 1) in the UK. The “Parent sample” comes from the core questionnaire administered to adults aged 16 or over. This is matched with a “Youth sample” derived from the Youth

questionnaire, a separate survey of household members aged 10 to 15. Our analysis thus encompasses parents and their teenage children.

Measures for adolescent outcomes are only available in odd waves. Once limited to those waves and dropping 258 observations (1.3%) with missing information, our Youth sample consists of 11,952 adolescents and 19,173 person-wave observations. We match these adolescents to their biological, step- or adoptive parents for an initial total of 7,830 mothers (87,650 person-wave observations) and 5,531 fathers (36,343 person-wave observations). We apply five restrictions to the Parent sample. First, we drop 2,053 person-wave observations for parents observed past age 59 to limit analyses to working-age adults not affected by pension reforms in the period. Second, given that our approach will rely on households' staggered exposure to benefit cutbacks across study waves, we drop 1,574 observations belonging to cohorts that are too small to lend themselves to credible statistical inference (i.e. cohorts exposed already in Wave 1, or only in Wave 8, 9 or 10). Third, to capture the dynamic effects of exposure to benefit cutbacks, we restrict analyses to three waves prior and eight waves after first exposure, thus excluding a further 2,803 person-wave observations. Fourth, we drop 14,853 person-wave observations from the Parent Sample due to listwise deletion. Finally, we drop 666 singleton observations, i.e. parents observed only once in the resulting dataset, as they would not contribute to estimation in our setting (e.g., [Correia, 2014](#)). The final sample comprises 5,912 mothers (40,186 person-wave observations), 4,010 fathers (25,515 person-wave observations), and 10,131 adolescents (17,083 person-wave observations). All sub-samples are unbalanced panels.

Listwise deletion is thus the most consequential sample restriction. The unbalanced nature of the panel, further, may raise concerns over selective attrition (see, e.g., [Lynn & Borkowska, 2018](#)). All analyses are therefore weighted to take into account both listwise deletion and panel attrition ([Seaman & White, 2013](#); [Kromydas et al., 2021](#)). The procedures we followed to construct weights are reported in Appendix 2.

We carry out analyses on subgroups based on household income. Benefit cutbacks disproportionately affected lower-income households, yet higher-income families might have been affected by some of the austerity reforms too (e.g. via the High Income Child

Benefit Charge). As such, we could not categorise well-off families as the “unexposed” comparison group. Comparison groups will be identified *within* each income group, enhancing the plausibility of key identifying assumptions in our analyses. Besides, by examining reform effects for each income group, we can assess whether reforms deepened socioeconomic disparities in family well-being. Specifically, we split households based on their permanent disposable household income (averaged across all available waves). We distinguish three income groups based on anchored tertile values (33th percentile $\approx$ 30,700 GBP, 66th percentile $\approx$ 45,300 GBP). Tertile values are anchored to the household income distribution in Wave 6 (2014-16, weighted by survey weights) to circumvent the shift in the distribution caused by the recession in earlier waves. Other than by family income, we will also present separate analyses by family structure, distinguishing partnered and single mothers based on their status at the start of the panel. As per our Background, reforms disparately affected single parents, the overwhelming majority of which are mothers.

Table 2 reports descriptive statistics on sample composition across key demographics. Parents are typically in their 40s, have at least one child in the household, and their youngest child is aged 9-11. Parents in the top income group are older and tend to have older and fewer children. When divided by household structure, samples of partnered and single mothers have comparable demographic composition, except for their position in the income distribution. Around 3/4 (1/4) of single (partnered) mothers have incomes below the bottom tertile.

2.1. Outcomes

2.1.1. Income dynamics

We first consider income dynamics in the aftermath of cutbacks. We examine individual benefit income, as computed in UKHLS from self-reported benefit receipt ([Fisher et al., 2019](#)). When respondents report joint receipt of a given benefit, benefit amounts are split among recipients in a UKHLS household (*ibidem*). Benefits that are most often reported jointly among parents in our sample are Child Tax Credit (37%), Working Tax Credit (34%), Housing Benefit (32%), and Child Benefit (22%). Hence, benefit income losses estimated separately for each parent in two-parent families provide a lower

Table 2: Sample composition and sample averages for main outcomes (weighted, UKHLS Wave 1-10).

Parent Sample	By income-tertile group						By household structure			
	Bottom third		Middle third		Top Third		Partnered mother	Single mother		
	Fathers	Mothers	Fathers	Mothers	Fathers	Mothers				
Age	43.3	40.6	44.2	42.3	46.9	45.5	43.1	41.1		
Age of the youngest child in the household	8.8	9.3	8.3	9.5	10.8	11	9.8	10.3		
No. children in the household	1.7	1.5	1.7	1.6	1.4	1.3	1.5	1.3		
Single parent	.07	.37	.01	.08	.004	.03				
Bottom third household income							.24	.76		
Middle third household income							.37	.18		
Top third household income							.39	.06		
Benefit income (1,000 GBP)	4.5	8.5	2.4	5	0.8	2.1	4.3	9.8		
Labour income (1,000 GBP)	12.1	6.2	24.4	11.7	46.3	23.1	14.3	11.6		
Currently employed	.74	.54	.90	.74	.96	.85	.72	.67		
Working hours	37	24.9	38.9	26.7	40.5	29.4	27.3	28.1		
Financial worry	2.9	2.9	2.5	2.4	1.9	1.9	2.3	2.9		
Housing arrears	.22	.25	.10	.10	.02	.03	.10	.26		
Monthly food expenditure (GBP)	172	171	197	197	232	234	206	173		
GHQ-12 (z-score)	− 0.1	− 0.07	− 0.01	0.02	0.03	0.06	0	− 0.03		
N	5,295	12,863	9,305	13,960	10,915	13,363	32,303	7,883		
Youth Sample										
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Age	12.4	12.4	12.5	12.5	12.6	12.6	12.5	12.6	12.5	12.5
Internalising problems (z-score)	0.03	0.02	0	0.04	− 0.04	− 0.04	− .01	0	0.07	0.05
Externalising problems (z-score)	0.01	0.01	− 0.02	0.01	− 0.03	− 0.03	− 0.04	− 0.03	0.13	0.12
N	2,477	2,569	3,164	3,089	2,907	2,877	6,740	6,683	1,398	1,485

bound of total household losses.

We complement these analyses by also examining labour income dynamics (and related changes in labour supply in terms of workforce participation and working hours, Appendix 4). Incomes are divided by 1,000, indexed at 2013 prices, and examined in levels to keep the 0s in.

2.1.2. Economic pressure

In line with the FESM (Conger & Conger, 2002; Conger & Donnellan, 2007), we assess whether households have experienced economic pressure as a result of austerity reforms. We consider, first, a subjective measure of financial distress (see also Schenck-Fontaine & Panico, 2019). We examine 5-fold responses to the question “How well would you say you yourself are managing financially these days?”. Responses range from “living comfortably” (1) to “finding it very difficult” (5).

We also investigate more objective measures of economic pressure by focussing on essential needs. Failure to accommodate household needs may not fully capture the ex-

tent of material deprivation leading to financial worry (see, e.g., [Mistry et al., 2008](#)), yet it is a clear sign that families might be confronting economic hardship, which may impinge on family functioning ([Yeung et al., 2002](#); [Gershoff et al., 2007](#); [Schenck-Fontaine & Panico, 2019](#)). We define such hardship first in terms of housing arrears, with a dummy coded 1 when households are behind some or all payments for bills or rent, and 0 otherwise. Findings are hardly changed when rent and bills arrears are analysed separately. We will thus analyse arrears together for the sake of brevity.

Beyond housing, we consider another essential, food, and analyse food expenditures. In each wave, parents are asked about the amount of money spent on food and groceries from supermarkets, other food shops or markets. We consider equivalised expenditures, i.e. divided by the square root of household size, and further deflate this outcome to 2013 prices.

2.1.3. Parental mental health

We evaluate self-reported parental mental health as measured by the 12-item version of the General Health Questionnaire (GHQ-12). GHQ-12 is a widely used screening instrument for depression and depressive symptoms, validated internationally (for a recent review, [Griffith & Jones, 2019](#)). The twelve items tap into multiple aspects of psychological well-being, from concentration and sleep to confidence and feelings of self-worth. The sum of scores (1 to 4) for each item provides us with a Likert measure of mental health, ranging from 0 to 36, the latter score indicating the maximum level of distress. We reverse the coding of the GHQ-12 measure, i.e. higher scores indicate better health. We further standardise the GHQ-12 score by taking the subgroup-specific means in each subsample as a reference for ease of interpretation. Additional analyses reported in Appendix 10 will focus on indicators of mental ill health and sub-dimensions within GHQ ([Griffith & Jones, 2019](#)).

2.1.4. Parenting

As for parenting, we relied on the Parenting Style and Dimensions Questionnaire (PSDQ, [Robinson et al., 1995](#); for a review, [Olivari et al., 2013](#)), a 32-item version of which was administered from Wave 3 onwards. Only parents of 10-year-olds completed this questionnaire module. Appendix 11 provides details on sample size and the seven par-

enting constructs we derive, namely: parental warmth, respectfulness, consistency in setting rules, lack of reasoning with the child, verbal hostility, physical punishment, and directiveness. Scores on each dimension are standardised for each sub-sample of parents in different income groups.

2.1.5. Adolescent socio-emotional and behavioural adjustment

Finally, outcomes in the Youth sample tap into adolescent socio-emotional and behavioural adjustment. We rely on 20 items from the Strengths and Difficulties Questionnaire (SDQ). This is an extensively validated screening tool for psychopathology in children and adolescents ([Goodman & Goodman, 2009](#)), largely used in the literature on the FESM. Comparable measures have also been adopted in studies on the intergenerational consequences of state-benefits expansion ([Milligan & Stabile, 2011](#)). Adolescents take the questionnaire themselves, thus minimising the risk of shared-method bias occurring when parents report both on themselves and their children. As recommended for population-based non-clinical samples ([Goodman et al., 2010](#)), we construct two summary scores. The first captures internalising problems, i.e. emotional symptoms and peer problems. The second encompasses externalising problems, i.e. conduct problems and hyperactivity/inattention. The two scales are standardised with respect to the subgroup- and age-specific means.

3. Empirical approach

3.1. Exposure to benefit cutbacks

As per the last column of Table 1, we consider a parent exposed to a given benefit cutback if they continuously met eligibility rules for the reform in question before its implementation ([Fetzer, 2019](#); for a more in-depth overview of each reform, see Appendix 1³). Only for Universal Credit and Personal Independence Payment, due to their gradual roll-out, parents are considered exposed if and when they reported receiving such benefit themselves (from Wave 5, i.e. 2013-2015, onwards). Whenever possible, we keep coding consistent with previous studies examining some of the same reforms

³In addition, estimated losses for selected reforms can be found here ([Beatty & Fothergill, 2016](#)), averaged across all working-age adults and by local authority, and here ([Portes & Reed, 2018](#)) by income decile/quintile and by household structure.

(Fetzer, 2019; Reeves et al., 2020). Similar to previous research using UKHLS in particular (Fetzer, 2019), we combine information from the core questionnaire and the benefit income module (Fisher et al., 2019) to reflect eligibility rules for each reform. For example, the High Income Child Benefit Charge came into effect in January 2013, affecting households with a high-earning member. The exposed group in our study consists of parents that, in all study waves prior to January 2013, reported receiving Child Benefit and living in a household in which either partner (or the single parent) reported gross earnings above the threshold set by the reform, namely 50,000 GBP.

Figure 1: Timeline of benefit cutbacks (panel a.) and sample shares (panel b.) of parents affected by each reform (Parent sample, weighted; UKHLS waves 1-10).

After defining exposure to each reform separately, we assign parents to “event cohorts” based on the wave of *first individual* exposure to benefit cutbacks. For a single mother exposed to both Tax Credits reform at Wave 3 and the Benefit Freeze by the time of her survey interview at Wave 7, for instance, the event clock will be set to 0 in Wave 3. We will then track her outcome trajectories relative to exposure in Wave 3. As we are examining multiple reforms, many with overlapping eligibility, repeated exposure to benefit cutbacks is the norm, occurring in over half of the Parent sample. Yet, we refrain from

assessing repeated exposure in our analyses to avoid selectivity and statistical power issues. Estimates will thus refer to the total effects of benefit cutbacks, starting from the time of first exposure. Further, we choose to focus on individual parental exposure. Arguably, both parents in two-parent households will be affected by a given reform, regardless of which one is the recipient of tax-benefit programs subject to policy change. At the same time, defining exposure at the household rather than individual level would over-complicate the interpretation of our findings, mixing up individual exposure with cross-over effects between partners and adding the risk of miscategorising exposure for parents splitting in the period. Having defined parental exposure in this fashion, finally, we assigned children in the Youth sample to their event cohort. We matched children to the event cohort of their mother, as around 22% of person-wave observations in the Youth sample do not report a paternal ID that can be matched to fathers in the Parent sample. For 777 observations (4.5% of the Youth Sample), matching is only possible with respect to paternal information.

Figure 1 provides an overview of the timeline of the reforms examined in this study (bottom x -axis). The reform process was staggered across multiple years, with reforms becoming effective in April 2011, January, April and September 2013, April 2014, April, and November 2016. Figure 1 also displays the shares of parents (person-wave observations) exposed to each reform in the whole sample and the shares among them that have the lowest incomes or are single mothers. Overall, data for the Parent sample supports the notion that lower income and single mothers were disproportionately subject to benefit cutbacks.

3.2. Event studies: estimation and identification

We use event-study specifications to track the effects of exposure to benefit cutbacks. More than 90% of parents are observed for four waves or more (up to ten), allowing for medium-run evaluations of within-individual outcome changes. We rely on the estimator proposed by Sun and Abraham (2021), i.e. an ‘interacted’ specification which overcomes biases in model specifications including individual and time fixed effects (or two-way fixed effects, TWFE henceforth; for more details and alternative estimators, see Appendix 9). Our main analyses follow the equation:

$$Y_{i,t} = \sum_{k \neq -1} \sum_{c \neq 0} \gamma_{c,k} (\mathbb{1}\{C_i = c\} \cdot D_{i,t}^k) + \theta_i + \lambda_t + v_{i,t} \quad (1)$$

where $Y_{i,t}$ is one of the study outcomes. We include dummies for event time, i.e. waves $k = \{-3, -2, 0, 1, \dots, 7\}$ relative to exposure to benefit cutbacks ($D_{i,t}^k$), and interact these with a full set of indicators for event cohorts c . With $k = -1$, we indicate the omitted reference period in event time, one wave prior to exposure at $k = 0$. As k grows, we will capture the long-term effects of cumulative exposure to benefit cutbacks, possibly including multiple reforms. As per Table 1, respondents have not been directly asked if they were subjected to a specific reform. Rather, we consider parents exposed whenever they are observed as continuous recipients of a program subjected to reform (Fetzer, 2019). Dynamic treatment effects over event time k are thus best interpreted as intention-to-treat (ITT) estimates.

We have six treated cohorts numbered by the wave of first exposure to benefit cutbacks. The comparison group ($c = 0$) consists of parents “unexposed” to benefit cutbacks in the period (see Appendix 9 for sensitivity analyses with alternative comparison groups). Parents are unexposed in our setting either because they were ineligible for benefits, would have been eligible but never claimed them (take-up for all programs in this study is incomplete; see, e.g., Ko & Moffitt, 2022), or have only received benefits intermittently prior to policy reform. Panel a. of Figure 1A in the Appendix provides an overview of sample shares in each cohort in the Parent sample. Most parents were first exposed to reforms between Waves 2 and 4, and around 25% of parents belong to the comparison group. While the statistical interaction between event time and event cohorts will provide separate estimates for each exposed cohort, we will obtain a single estimate for each period k in event time by summing together cohort-specific effects, each weighted by the sample share of the corresponding event cohort in period k (see Panel b of Figure 1A for reference).

Individual fixed effects θ_i and wave fixed effects λ_t complete the specification. Identification in this setting requires parallel counterfactual trends, i.e. that outcomes for treated and comparison groups would have evolved in parallel, counterfactually, in the absence of any exposure to benefit cutbacks. The estimator we have chosen is par-

ticularly appropriate when researchers have no strong priors suggesting that parallel trends will only hold in the presence of covariates (Sun & Abraham, 2021; De Chaisemartin & d’Haultfoeuille, 2021). Tests for the plausibility of parallel trends are presented and discussed in more detail in Appendix 8. They suggest, in short, little evidence of divergent pre-trends, especially for the subgroups and outcomes for which we register the largest changes after exposure to austerity. A bounding exercise (Ramachan & Roth, 2022), also in Appendix 8, further contextualises our findings vis-à-vis possible violations of parallel trends.

As for analyses based on the Youth sample, we can rely on a large N but limited time periods. Roughly 71% of observations in the Youth sample belong to adolescents observed for at least two waves and up to a maximum of four. This hinders credible estimation in an event-study setting when adding individual fixed effects. Therefore, models simplify to a more common event-study specification (see, e.g., Kleven et al., 2019):

$$Y_{i,t} = \alpha + \sum_{k \neq -1} \gamma_k D_{i,t}^k + \lambda_t + \epsilon_{i,t} \quad (2)$$

where $D_{i,t}^k$ expresses the relevant event-time coefficient. Without individual fixed effects, estimates reflect mean outcome differences in each period k , among the exposed as opposed to the never-exposed and not-yet exposed within the same wave. With this in mind, credible identification requires parallel counterfactual trends for adolescent outcomes, as was the case for parents. Appendix 8 provides supporting evidence for the plausibility of parallel trends among the Youth sample. Models are weighted and estimated separately by income group, using information from the study mother (except for the 777 observations mentioned above, for which we can only retrieve paternal information). Further, we follow standard practices in the field and report results separately for boys and girls.

Finally, when analysing parenting measures on the sub-sample of parents of 10-year-olds, event-study analyses would rely on relatively small cell sizes for each dummy before and after exposure to benefit cutbacks. Having repeated observations for some parents, but only if referring to different children, adding individual fixed effects in event studies (as per Equation 1) would equate to a sibling design. As a result, we

could be focusing on residual variance due to how parents have behaved differently after austerity reforms or on variation across siblings, as different children evoke different parenting behaviours. We focused on the former and abandoned event-study specifications. Instead, we built a dummy contrasting parents after exposure to austerity ($D_{it} = 1$, 71% of the sample) with parents never exposed or not-yet-exposed ($D_{it} = 0$, 29% of the sample). We use recently developed methods to estimate the average treatment effect on the treated (ATT, [Słoczyński, 2022](#)), net of pre-exposure or likely exogenous covariates: whether single parent at the baseline, level of completed education, ethnicity, region of residence, age of the respondent, number of children in the household, age of the youngest child in the household. Estimates for this part of the analysis rely on a selection-on-observables assumption.

Standard errors are bootstrapped with 50 replications (100 for youth outcomes) to account for the use of weights ([Seaman & White, 2013](#)). Unweighted estimates support the main conclusions of this paper and are statistically indistinguishable from their weighted counterparts.

4. Findings

4.1. Parents across the income distribution

Figure 2 presents our findings for the main parental outcomes in our study, focussing on mothers in three income groups. Panel a. on the left of Figure 2 shows that lower-income mothers experienced by far the largest losses in terms of annual benefit income. At the end of the observation period, such losses add up to around 4,289 GBP ($p < .001$), equal to around half of their pre-reform average benefit income. Losses stop at around 1,127 GBP for mothers in the intermediate income group ($p = .007$) and 1,351 GBP for those with higher incomes ($p < .001$).

Panel b. at the top right-hand corner of Figure 2 provides evidence that heightened economic pressure also followed exposure to cutbacks. We find that mothers in low-income households experienced an increase in financial worry following austerity reforms. On a five-point scale where five indicates finding one's current financial situation "very difficult", estimates suggest that financial worry grew by up to around .34-.36 points from period 4 onwards (for period 7, e.g., $p = .016$). Estimates for the other

income groups are much smaller and often have a negative sign, especially among higher-income mothers. In the Appendix, we find no clear-cut evidence, on the other hand, of effects on housing arrears or the amounts of food expenditure across income groups (Figure 6A). Overall, effects on perceived economic pressure seem decoupled from effects on selected indicators of hardship in our sample.

In the bottom-right corner of Figure 2, we investigate changes in maternal mental health. For women with lower incomes, we find a sizeable mental health deterioration as measured by GHQ scores. A decline of around 21-23% of a SD is already visible in periods 1 and 2 after exposure to cutbacks ($p = .043$ and $p = .031$) and it is detected most clearly in periods 4 ($p = .011$), 5 ($p = .003$), 6 ($p = .001$), and 7 ($p = .015$), whilst reaching a maximum of around 39% of a SD. We find a comparable worsening of mental health among women in the intermediate income group but only limited to the last two periods in our study (18% of a SD and $p = .099$, 32% of a SD and $p = .004$ in periods 6 and 7, respectively). By contrast, we cannot detect a post-exposure change in GHQ scores among higher-income mothers, for whom estimates are smaller and typically have a positive sign. The clearest signal of worsening mental health among lower-income mothers is found for the GHQ sub-dimension capturing feelings of self-worth, as shown by Figure 20A in the Appendix. Further, estimates suggest an increase of around 16-20 percentage points in the chances of experiencing mental ill-health symptoms above clinical cutoffs for GHQ measures (3/4 symptoms).

Figure 2 thus portrays evidence of benefit income losses, financial worry, and worsened mental health among women with lower incomes in the Parent sample. As displayed in Figure 2A in Appendix 4, benefit income losses are accompanied by extra labour earnings due to increased workforce participation, especially among mothers in both the bottom and intermediate income groups. It follows that benefit income losses might have stemmed not just from cutbacks *per se*, but also from renewed participation in the labour market - which might have affected benefit eligibility, at least for some programs. For mothers with lower incomes, however, the extra earnings do not fully compensate for benefit income losses. Despite substantial increases in both workforce participation (up to around 11 percentage points) and work hours (up 5 hours per week), the

Figure 2: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among mothers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

earnings gains of mothers in low-income households only reach at best around 1,928 GBP (period 4, $p = .004$). Our conclusion, therefore, is that cutbacks resulted in a new composition of income for mothers, although the extra labour income did not match benefit income losses. Income loss coupled with renewed participation in paid work and insufficient earnings might have led low-income mothers to heightened feelings of financial worry and adverse impacts on their mental health.

We report results for fathers in the Appendix. Figure 5A shows little evidence of substantial benefit income dynamics or of changes in financial worry and general mental health. The portion of benefit income accruing to lower-income fathers in our estimates only decreases by a maximum of around 1,177 GBP in period 1 ($p = .009$) and hovers around 500-1,000 GBP in the remaining periods. Estimates for fathers in other income groups are comparable in size and by and large detected with $p < .001$. Father-reported hardships concerning arrears or food expenditure also do not exhibit clear-cut trends over event time nor differences across income groups (Figure 6A). As for labour sup-

ply, we find some evidence of a parallel increase in labour income which more than offsets benefit income losses among lower-income fathers, reaching up to 1,816 GBP in period 5 ($p = .032$) and 2,317 GBP in period 6 ($p = .048$). For this group, labour supply adjustments are more evident for work hours, although largely undetected at conventional levels of statistical significance. In sum, cutbacks had few overall effects on men in our Parent sample.

4.2. Single mothers

We now compare mothers who are partnered with those who are single at the baseline. Panel a. of Figure 3 presents benefit income dynamics in relation to cutbacks. Although estimates are noisier due to the smaller sample size, single mothers seem to have experienced larger benefit income losses than partnered mothers. By period 7, for example, losses for mothers who were in a couple at baseline stop at around 1,179 GBP ($p < .001$). Estimates for single mothers indicate losses of around 3,743 GBP ($p = .003$), around a third of their pre-reform average.

We then find strong evidence of increasing economic pressure among single mothers. In panel b. in the top-right corner of Figure 3, single mothers' outlook on current finances is shown to deteriorate steadily in the aftermath of cutbacks. Estimated increases in financial worry reach around 74% of a point by the last period after cutbacks ($p = .001$). No evidence of changes emerges when examining the same outcome for all mothers in a couple. Differently, but in line with results in the previous section, we find little evidence of changes in either housing arrears and limited evidence, if anything, of increases in food expenditure, regardless of family structure (Figure 7A in the Appendix). Single mothers, nonetheless, report a relatively higher likelihood of arrears and lower food spending already before the reforms.

As for mental health in panel c. of Figure 3, single mothers seem particularly affected in the medium run. Their GHQ scores decline by up to around 52-57% of a SD at the end of the post-reform observation period ($p = .002$ and $p = .021$ in period 6 and 7, respectively). When examining in-depth measures based on the GHQ (Figure 19A), somewhat larger effects are found for sub-dimensions such as feelings of self-worth and stress. For single mothers, estimates suggest an increase of up to 15-24 percentage

points in the chances of self-reported mental ill health, based on GHQ diagnostics (see Figure 21A for more details).

Figure 3: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among mothers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

Hence, single mothers seem to have borne the brunt of austerity cutbacks. In Appendix 4, we ask if our findings reflect both benefit income losses and changes to labour income and workforce participation (Figure 4A). We find little evidence of increases in labour income among single mothers, suggesting little compensation across income components for this group, although standard errors are large. Workforce participation seems to have increased among single mothers, particularly work hours, for which estimates reach a maximum of 7 extra hours per week in period 6 ($p = .016$). Our conclusions for single mothers thus mirror the post-austerity income and stress dynamics found for lower-income mothers, perhaps not surprisingly considering the over-representation of single mothers at the bottom distribution (Table 2).

4.3. Adolescents

Figure 4 portrays socio-emotional and behavioural adjustment in the Youth sample relative to parental exposure to austerity reforms. We find evidence of a gradual and substantial increase in self-reported internalising problems for girls in lower-income households. The latter are estimated to have grown by around 15% of a SD in period 1 (albeit $p = .103$), and up to 54% of a SD by period 6 ($p < .001$) and 57% of a SD by period 7 ($p = .001$). Especially in later periods⁴, the contrast is starker with peers in the middle- and top-income groups, mirroring the picture for maternal mental health in Figure 3. For externalising problems, we also find evidence of growing difficulties among lower-income girls. We observe increases by around 31% ($p = .007$) in period 1 and up to a maximum of 34% of a SD in period 5 ($p = .008$). Estimates are less clear-cut but of comparable size in the last periods (e.g., period 7, 28% of a SD, $p = .121$). Differences across income groups are stark, albeit less pronounced for internalising problems compared to externalising problems.

Estimates are somewhat smaller for boys, as displayed at the bottom of Figure 3. Increases in internalising problems and differences among children from different income groups are more evident in later periods. In periods 6 and 7, estimates climb up to 49-50% of a SD ($p < .001$ and $p = .004$, respectively) for boys in the low-income group, whereas estimates are smaller and even negative for boys in the two other income groups in the same period. Findings for boys are more similar across income groups when it comes to externalising problems, especially when comparing families at the bottom and top of the income distribution. In most post-exposure periods, for example, we find evidence of growing externalising problems among boys in top-income families, by as much as 22% of a SD in period 3 ($p = .013$).

Other than across income groups, we investigate heterogeneity by family structure (Figure 8A in the Appendix), in line with previous analyses for mothers. We lose 777 observations that we could not match to maternal IDs in the Youth sample. Sample

⁴Our sub-samples by household income (household structure) range, in each period around exposure to austerity, from a minimum of 100 (73) to a maximum of 403 (870) observations among the treated. Estimates for period 7 are based on fewer treated observations, approximately 40 in most sub-samples, and should thus be interpreted with added caution.

Cutbacks → Adolescent outcomes

Figure 4: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among adolescents (Youth sample, UKHLS Waves 1, 3, 5, 7, and 9). Event-study estimates are derived from a linear regression as per Equation 2. Standard errors are bootstrapped with 100 replications. Study weights are applied.

sizes for children of single mothers are also relatively small ($< 1,500$ observations). Point estimates are typically larger for children of single mothers, especially for externalising problems among girls and internalising problems among boys. Post-exposure trends are generally in line with their counterparts among lower-income households. Yet statistical differences with children of partnered mothers, by and large, cannot be detected. In addition, the growth in socio-emotional difficulties appears by no means limited to children of single mothers. When averaged among children of all partnered mothers, we still find evidence of an increase in internalising problems among girls in most post-exposure periods, of up to around 16% of a SD (in periods 4 and 6; $p = .009$ and $p = .046$, respectively).

4.4. Linking parents and children: an analysis of parenting dimensions

Socio-emotional difficulties in children might stem from less effective parenting, as parents struggle with the financial and psychological consequences of cutbacks. We could

not examine parenting at length in this study, as detailed measures are only available for the cross-section of parents of 10-year-olds. However, we present analyses on this sub-sample in Appendix 11.

We find only little evidence that exposure to austerity reforms affected parenting across income groups. Looking at panel a. of Figure 20A, the largest increases among mothers are observed for the z-scores of verbal hostility, physical punishment, but also warmth. None of these estimates, however, is statistically significant at conventional levels. At best, estimates suggest conflicting/counteracting changes in maternal parenting, both warmer and more prone to harsh discipline. For fathers in panel b. of Figure 20A, we find the clearest signal concerning the ability to enforce consistent rules. The z-scores for consistency decrease by around 25% of a SD among lower-income fathers ($p = .05$), whilst stopping at 3% of a SD ($p = .664$) among fathers in the middle group and -2% of a SD ($p = .793$) among higher-income fathers.

When comparing single mothers to partnered mothers in Figure 21A, we find the starkest difference when it comes to (lack of) reasoning with the child, i.e. if parents resort to taking away privileges or leaving the child alone with no explanation. The z-score for this measure increases by 37% of a SD among single mothers ($p = .018$) and, conversely, by only around 1% of a SD among partnered mothers ($p = .902$). As for the other dimensions, we find little evidence of heterogeneous responses between single and partnered mothers.

Overall, there are only a few indications of changes in parenting among those exposed to cutbacks as compared to the unexposed. We find somewhat more clear-cut effects for consistency among lower-income fathers and reasoning among single mothers. These estimates might suggest that parents might have found it harder to spend time and energy enforcing discipline by reasoning with the child rather than unilaterally.

5. Discussion

Our study examined the consequences of the last decade of benefit cutbacks on families with adolescent children in the UK. We find that lower-income and single mothers faced the largest benefit income losses in the aftermath of cutbacks. Despite contemporaneous and substantial increases in workforce participation, we find that the

extra earnings did not fully compensate benefit income losses among mothers. Lower-income and single mothers' outlook on their current financial circumstances deteriorated in the same period, as did their mental health. Socio-emotional and behavioural adjustment also worsened among adolescents in lower-income households, particularly among girls. We find little evidence of differences in parenting among parents exposed and unexposed to austerity.

Our findings are relevant for the evaluation of benefit changes, research into the FESM, and studies on adolescence and policy change. Concerning benefit changes, we show that cutbacks may have opposite and more sizeable effects than expansions of state-benefit programs. For example, although interval estimates are largely compatible with previous research, point estimates in this study are a multiple of changes in maternal mental health assessed in studies on benefit expansions ([Milligan & Stabile, 2011](#); for a review, [Simpson, Albani, et al., 2021](#)). These findings are in line with observational studies highlighting larger mental health effects of income losses as opposed to gains ([Benzeval & Judge, 2001](#); [Kromydas et al., 2021](#)). Evidence in this study should spur more research on the asymmetric effects of welfare expansions delivering benefit income boosts and cutbacks resulting in benefit income losses.

Our findings also show that the effects of cutbacks can be well understood within the theoretical framework offered by the FESM⁵. Expanding on prior research, we highlight effects that mature over time and in the aftermath of policy changes. Future studies could examine similar “exposure histories” triggered by policy to more fully grasp the “E” in the FESM. At the same time, some of our findings contrast with the FESM and motivate further theoretical and empirical scrutiny. For one, we find that cutbacks affected subjective but not objective aspects of economic pressure (i.e. essential needs) in our sample. It could be that parental financial worries arose with respect to “wants” and modest extras (children’s birthdays, holidays, eating out, and so forth; [Mistry et al., 2008](#)) as opposed to essential needs. Besides, our sample consists of relatively older parents with their adolescent children. Relative to parents of young children, parents

⁵A full-fledged mediation analysis was beyond the remit of this paper. Future studies using causal mediation analyses could deploy a policy-induced exposure like ours and quantify the strength of each pathway predicted by the FESM.

in our sample might have been better equipped to stave off hardships by adjusting their labour supply, as we observed. They could have also relied more on savings, borrowing or other channels (e.g. [McLoyd, 1990](#)) which we could not observe⁶. These mechanisms deserve renewed attention in the FESM literature. Our findings support, nonetheless, a key tenet of FESM in that *perceived* economic pressure may have detrimental consequences for parental and child well-being even when households manage not to fall into hardship ([McLoyd, 1990](#); [Conger & Donnellan, 2007](#); [Mistry et al., 2008](#); [Schenck-Fontaine & Panico, 2019](#)).

We also did not find clear-cut evidence of changes in parenting as a result of exposure to cutbacks. The FESM predicts that economic pressure will affect children's socio-emotional difficulties by leading to less effective or even detrimental parenting styles. It could that changes in parenting might have been more evident if we had access to parenting measures for the full sample rather than just for parents of 10-year-olds. Challenges to parenting may have also built up as benefit income losses accumulated, but we lacked repeated measures to assess that. Alternatively, it is also well established that children may perceive their family's financial and psychological difficulties more acutely during adolescence ([Mistry & Elenbaas, 2021](#)). For example, adolescents might become more exposed to current struggles in the household by taking on a more adult role as they age (e.g. caregiving, running errands, etc.; [Ridge, 2011](#); [Chzhen et al., 2022](#)). Other mechanisms affecting socio-emotional difficulties could have been conflict between parents (e.g. [McLoyd, 1990](#); [Conger et al., 1994](#)) or the increased opportunity for peer comparison and stigmatisation at school ([Treanor, 2020](#)). We leave these as directions for future research.

As for adolescent outcomes and policy change, we find sizeable changes in adolescent well-being in response to cutbacks. Similar to changes in maternal mental health, effects on adolescents seem to develop over time and are often a multiple of those found in studies on benefit expansions, often focused on early and middle childhood ([Cooper &](#)

⁶Looking at previous studies on the UK, in addition, families in our sample might have avoided falling into arrears by "doubling up" ([Clair, 2021](#)) and food insecurity might have emerged, but more as a result of reforms occurring (too) late in our observation period (Universal Credit, [Reeves & Loopstra, 2021](#)). Other forms of hardships (e.g. evictions, see [Fetzer et al., 2022](#)) could not be tracked with our data.

[Stewart, 2021](#)). Building on US research on welfare retrenchment, on the other hand, we show how adolescents can respond to policy changes affecting (maternal) income and employment in the household ([P. A. Morris et al., 2005](#)). Due to the well-established link between socio-emotional and behavioural adjustment and school outcomes, adolescents exposed to UK austerity may have been further penalised in the education domain, similar to children affected by US welfare reform in the 1990s ([Gennetian et al., 2002](#)). It is therefore warranted to devote future studies to the long-run life chances of children affected by austerity reforms.

Together with its contributions, our study has limitations. The first is that we could not disentangle the role of income and employment, and both margins may contribute to the overall effect of welfare reform ([Heckman & Mosso, 2014](#)). The consequences of cutbacks should thus be interpreted as the combined result of lost benefit income which, despite pushing mothers to increase workforce participation, was left uncompensated by labour earnings. Further research is needed to single out welfare reform effects that operate via income vis-à-vis employment channels (e.g. [Katikireddi et al., 2018](#); [Brewer et al., 2022](#)).

In addition, a causal interpretation of our findings rests on the parallel-trend assumption. A second limitation of our study is that the plausibility of parallel trends and related causal claims can be called into question if shocks other than austerity reforms differentially affected the exposed and unexposed in our study. Appendix 8 provides evidence in support of parallel trends, together with robustness tests for violations of parallel trends due to contemporaneous shocks.

For one, austerity reforms were passed during the economic reprise that followed the Great Recession in the UK. We have little reason to believe this may have biased our conclusions. The economic reprise may have provided disparate opportunities for households across income groups and by family structure, but we contrast households *within* each group based on differences in their benefit-claiming history. Besides, whilst some of the outcome trajectories we observed could have occurred due to the economic reprise and regardless of austerity (e.g. parental labour supply), others are more counter-intuitive. For example, an increase in adult financial worries or children's socio-emotional

difficulties would be more typical of a recession than a period of economic growth (e.g. [Kalil, 2013](#)). Rather than macroeconomic trends, a more relevant contemporaneous shock⁷ is the dramatic rise in benefit sanctions in the period 2010-2016, which disproportionately affected some of the exposed in our study (those claiming out-of-work benefits, [Webster, 2019](#)). Our estimates may incorporate the effects of sanctions on outcomes such as workforce participation, financial worries, and mental health ([Pattaro et al., 2022](#); see also Appendix 8).

Our study tested for the association between components of the FESM and exposure to cutbacks. A third limitation of our study is that we are thus bound to intention-to-treat estimates. As long as households actually affected by reforms are a fraction of the exposed in our sample, our estimates of reform effects could be conservative. On the other hand, measurement error might have biased our conclusions if we misclassified unaffected households among the exposed and assigned those affected by reforms to the comparison group. Future studies could complement our evidence with more refined measures of the affected population. Further, intention-to-treat estimates of reform exposure do not capture dose-response effects, i.e. how outcomes vary depending on the intensity of income losses spurred by reforms. We find that parental and child responses to austerity matured over time in parallel with the accumulation of substantial benefit income losses. Dose-response effects are thus likely and constitute an important avenue for further research.

A final limitation of our study is that our estimates have external validity for families with adolescent children (in the UK). Further generalisations or the examination of heterogeneous effects could be the subject of future research. In particular, whether and how our findings generalise to families with younger children is an open question. Women in these families might face more obstacles (e.g. affording and arranging childcare) if pushed to increase their earnings via labour supply. As a result, they could face larger income losses as support from state benefits wanes. If the (benefit) income

⁷Violations of parallel trends could have also stemmed from simultaneous reforms of multiple benefit programs, but this may bias single-reform studies more than comprehensive evaluations of the austerity era like ours. On the flip side, our study could not disentangle the effects of the nuts and bolts of specific reforms (e.g. eligibility thresholds, waiting periods, etc.), for which single-reform studies remain best suited (e.g. [Brewer et al., 2022](#)).

channel dominates the employment one, the financial worries, hardships, and psychological distress faced by households with younger children might be larger than the ones we assessed in our sample.

Conclusions

We have examined the intergenerational consequences of benefit cutbacks for families with children. We found evidence that parents and children have been impacted by cutbacks, often unequally so along the income distribution and by family structure. Lower-income and single mothers have been particularly affected in terms of their mental health, whilst their adolescent children reported worse socio-emotional and behavioural adjustment. Adults and children in lower-income and single-parent households are thus at risk of “diverging destinies” compared to their more affluent counterparts (for the UK, see also [Bradbury et al., 2019](#); [Harkness et al., 2020](#)). We showed to what extent and how these “destinies” unfold due to policy design.

In this regard, it is worth noting that several countries have introduced or expanded cash benefits to families with children during the pandemic (albeit sometimes only temporarily, e.g. the US). The UK opted to introduce a temporary uplift to Universal Credit, for which claimant numbers surged to around 6 million families during the pandemic ([Emmerson et al., 2021](#)). The uplift equalled around 1,040 GBP per year and was removed in October 2021, potentially delivering income losses similar to the ones we studied. Households receiving so-called legacy benefits, the focus of this study, did not receive the temporary uplift altogether and are still subjected to some of the reforms analysed here (e.g. [Reeves et al., 2020](#)). Evidence from this study suggests that such cutbacks may disrupt parental well-being and children’s development.

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Families of Austerity: Benefit Cutbacks and Family Stress in the UK

Appendix

1. Benefit cutbacks in more detail

Families with children typically receive a bundle of benefits. Child Benefit, Working Tax Credit and Child Tax Credit (Tax Credits, hereafter) are key components of such bundle. Child Benefit is an unconditional transfer paid to families for each child under 19 still in school or further education. In January 2013, the so-called High Income Child Benefit Charge came into effect. Transfers were progressively withdrawn¹ from more affluent households, i.e. those including at least one member earning more than 50,000 GBP in annual pre-tax income.

Tax Credits are in-work earning supplements that mainly accrue to low- and middle-income families and to single parents in particular (similar to the EITC in the US, e.g. [Brewer & Hoynes, 2019](#)). Among other changes, coverage of childcare costs was lowered from April 2011. Work mandates were tightened for members of couples with children from April 2012. To qualify for Working Tax Credit, partners were required to be employed for at least 24 hours between them, with one person working at least 16 hours, up from the 16-hour requirement that was sufficient before the reform. Together with changes to Child Benefit, these measures are estimated to have affected between 15 to 25% of UK households ([Fetzer, 2019](#)). Tax Credits were further amended in 2016-2017. For our population of interest, families with adolescent children, changes mainly involved a reduction in income thresholds and an increase in taper rates. Beyond the remit of this paper, families with new children born on or after 6 April 2017 were also affected by the so-called “two-child limit”, restricting the child element of the Child Tax Credit and other programs to the first two children in each household.

Other than Child Benefit and Tax Credits, Housing Benefit was also subjected to cuts from April 2011. Housing Benefit is a housing subsidy paid to low-income renters in the private- and social-rented sectors in the UK. So-called Local Housing Allowance (LHA) rates determine eligibility as well as the size of payments allotted to tenants in the private sector. With LHA reform (2011), the eligibility threshold for payments was lowered from the median to the 30th percentile of the area-specific rent, to the detriment of more than 770,000 households ([Fetzer et al., 2022](#)). Whilst this change applied to private renters, social renters were hit by the so-called “bedroom tax”, a downward

¹Affected households included at least one earner above 50,000 GBP, with a taper rate of 1% for each additional 100 pounds over the threshold. Child Benefit was thus completely withdrawn from households with at least one member earning more than 60,000 GBP ([HM Revenue and Customs \[HMRC\], 2013](#)).

adjustment of Housing Benefit targeting tenants with spare bedrooms “not justified” by the size and age composition of the household (see, e.g. [Fetzer, 2019](#); [Bogue, 2019](#)). The bedroom tax affected an estimated 460-660,000 households across the country ([Beatty & Fothergill, 2018](#); [Fetzer, 2019](#)).

Further, Housing Benefit, the rest of legacy benefits, and Child Benefit were all affected by a sequence of transitory cuts (e.g. [Beatty & Fothergill, 2018](#)). 1% up-rating substituted adjustment by inflation for most benefits, for three years starting in 2013-2014 (two years, starting in 2014-2015, for Housing Benefit and Child Benefit). This was paired together with a cap to benefit levels for the same period². The Benefit Cap, still in place to this day, limits the total benefit income each household can claim per year ([Kennedy et al., 2016](#)). Eligibility depends on which benefits are currently claimed by household members, total benefit income out of benefits affected by the cap, composition of the household (whether a single individual or other), and area of residence (inside or outside London). In 2016-17, the benefit cap was renewed and expanded to affect more claimants. The government also introduced a further four-year benefit freeze in the same period.

Finally, the implementation of two more radical reforms slowly progressed in the years after the *Welfare Reform Act* of 2012. A new single-payment system called Universal Credit was designed to replace legacy benefits over time³, introducing a five-week waiting period before the first payment, taper rates based on earned income, etc. - with complex distributional consequences ([Brewer et al., 2013](#); [Joyce, 2016](#); [Brewer et al., 2019, 2020](#)). Up until around 2016, new claimants for out-of-work benefits such as Jobseeker’s Allowance were progressively assigned to Universal Credit. Later, more complex cases, including families with children, started to be processed under the new system (see, e.g., [Reeves & Loopstra, 2021](#)). Similarly, the new system of Personal Independence Payment was gradually rolled out to replace Disability Living Allowance, imposing significantly harsher health assessments and conditionalities for people living with a disability (see, e.g., [Fetzer, 2019](#)).

2. Probability weights for listwise deletion and panel attrition

Our analyses are weighted to account for listwise deletion and selective attrition. By far, GHQ-12 is the main variable responsible for missingness (12 out of 18% missingness among eligible parents in our study), as respondents did not fill in or return the

²The Benefit Cap applied to recipients of Child Benefit, a string of out-of-work benefits, and legacy benefits. Among other exemptions, however, the cap did not apply to households with at least one member qualifying for Working Tax Credit (for more details, see [Kennedy et al., 2016](#)).

³As of 2021, Universal Credit has only applied to new claims. People receiving legacy benefits have not yet been transferred to the new system. Long delayed, a pilot for such “managed migration” was launched in early 2020, but the process was soon halted due to the COVID-19 pandemic. Managed migration resumed in May 2022.

questionnaire’s self-completion module. As for attrition, previous studies have found that the same groups most often targeted by austerity reforms in our study, such as lower-income households and private tenants, are more likely to drop out of UKHLS (Lynn & Borkowska, 2018; Fetzer et al., 2022).

We devise probability weights in two steps (see, e.g., Seaman & White, 2013; Hernán & Robins, 2020; for a similar application using UKHLS, see Kromydas et al., 2021). First, we select parents eligible for the Parent sample that are either complete cases with no missing information on the study variables or have some missing information and will thus not be part of the final sample. Separately for each wave, and for women and men, we predict the probability of being a complete case based on age (in three age bands, 39 or younger, 40-49, 50 or older), whether the respondent has reported any benefit income in that wave, household income tertile group in that wave, and UKHLS sub-sample (UKHLS original members, former members of the British Household Panel [BHPS], and members of the UKHLS ethnic minority booster sample). For women only, we add a dummy for single motherhood⁴ too. We derive predicted probabilities and compute stabilised weights, with the baseline probability of belonging to the different UKHLS sub-samples at the numerator. As opposed to using inverse-probability weights (with one as the numerator), stabilising weights this way may yield less extreme weights (Hernán & Robins, 2020: 159). Wave-specific stabilised weights are combined via multiplication to obtain the final complete-case probability weights.

We then construct weights for attrition among complete cases. We compute a series of logistic regressions predicting the wave- and sex-specific probability of being observed in the current wave w but not in the subsequent wave $w + 1$. We include all predictors from our complete-case analyses, plus a dummy for whether the respondent is currently a private renter, their benefit income, labour income, whether they are employed or not, their GHQ-12 score and a dummy for depressive symptoms (GHQ-12 “caseness” at or above 4), subjective financial worry, a dummy for housing arrears, their equivalised food expenditure, and event-time dummies for time to/since exposure to benefit cutbacks. Once again, we derive predicted probabilities and compute stabilised weights, with the baseline probability of belonging to the different UKHLS sub-samples at the numerator. We combine wave-specific attrition weights via multiplication. We then combine complete-case and attrition weights to compute final analysis weights. After winsorising at the 99th percentile, the analysis weight has a mean of 1.06 and a standard deviation of 0.32 (Parent sample).

⁴Due to the small cell size, adding this variable for men may lead to undesirably large weights. Considering that we will conduct subgroup analyses on partnered and single mothers, we include single motherhood as a predictor for women, nonetheless.

3. Cohort shares

Figure 1A: Cohort shares a) in the whole sample and b) by wave of first exposure to benefit cutbacks (Parent sample, UKHLS Waves 1-10; pooled, study weights are applied).

4. Earnings and labour supply

Figure 2A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among mothers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

Cutbacks → Earnings & labour supply

Figure 3A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among fathers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

Cutbacks → Earnings & labour supply

Figure 4A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among mothers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

5. Fathers

Figure 5A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among fathers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

6. Economic pressure: essential needs

Cutbacks → Essential needs

Figure 6A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

Cutbacks → Essential needs

Figure 7A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

7. Adolescent outcomes by family structure

Cutbacks → Adolescent outcomes

Figure 8A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among adolescents (Youth sample, UKHLS Waves 1, 3, 5, 7, and 9). Event-study estimates are derived from a linear regression as per Equation 2. Standard errors are bootstrapped with 100 replications. Study weights are applied.

8. Parallel trends

We provide here formal tests for the plausibility of parallel trends in our analyses. When evidence points to diverging pre-trends, further, we consider what may explain pre-exposure differences and how these compare to post-exposure “effects” (Kahn-Lang & Lang, 2020).

When using TWFE (Parent Sample), we use both a simple Wald test for the joint significance of pre-exposure dummies - based on the interacted TWFE estimator (Sun & Abraham, 2021) - and a novel method based on imputing the missing counterfactual outcomes for the exposed in the pre-exposure period (see Borusyak et al., 2021 for more details; see also Roth, 2021 for a discussion of different tests and the merits of combining them). Results are displayed in Figures 9A-12A, where p -values below the conventional threshold of .05 indicate that the observed data is compatible with diver-

gent pre-trends⁵. Overall, we observe few statistical differences in pre-trends and almost none among the groups where we previously detected the bulk of post-exposure changes, namely lower-income mothers, single mothers, girls in lower-income families, and boys in higher-income families.

The only notable exception shown in Figure 9A concerns individual benefit income among mothers with lower incomes at the baseline. Going back to Figure 2, benefit income appears to be *growing* relative to one wave prior to exposure, by around 1,349 GBP three waves prior ($p = .145$) and 1,178 GBP two waves prior ($p = .101$). Pre-exposure changes are also observed among fathers in the lower-income group (Figure 5A). These changes likely reflect income adjustments through state benefits during the (pre-exposure) recession, as documented by previous studies (e.g. [Cribb et al., 2017](#)). In addition, the magnitude of such changes is only a fraction of post-exposure losses, particularly for mothers, lending confidence to the assertion that benefit income losses would not have been observed to the same degree if not for austerity reforms.

We used a Wald test for the joint significance of pre-exposure dummies in the Youth sample. p -values for these tests are reported in Figure 12A. Overall, none of the tests we performed suggest that parallel outcomes trends may be implausible among respondents in the Youth sample.

⁵Standard errors are clustered at the individual level rather than bootstrapped to carry out such tests in Stata. Clustered standard errors might be more conservative than their bootstrapped counterpart. In practice, differences are too small in our setting to affect our conclusions.

Mothers

Figure 9A: p -values for a series of pre-trends tests, by study outcome, among mothers in different income groups (Parent sample, UKHLS Waves 1-10). Study weights are applied.

Fathers

Figure 10A: p -values for a series of pre-trends tests by study outcome, among fathers in different income groups (Parent sample, UKHLS Waves 1-10). Study weights are applied.

Mothers

Figure 11A: *p*-values for a series of pre-trends tests by study outcome, among mothers by family structure (Parent sample, UKHLS Waves 1-10). Study weights are applied.

Figure 12A: *p*-values for a series of pre-trends tests (Wald), by study outcome and among adolescents by income group (left) and family structure (right) (Youth sample, UKHLS Waves 1, 3, 5, 7, and 9). Study weights are applied.

To buttress our causal claims, we have also performed a bounding exercise⁶ (Rambachan & Roth, 2022). The test bounds event-study estimates to possible violations of parallel trends. Specifically, we use a version of the test in which the largest pre-trend difference (i.e. $\gamma_{k=-2}$ or $\gamma_{k=-3}$) is the benchmark. The test then proceeds to contrast event-study estimates to multiples ($\bar{M}$) of the pre-event difference (i.e. $\bar{M} \times \gamma_k$). When $\bar{M}$ takes value 0, we are evaluating a scenario in which parallel trends are met. The first value of $\bar{M}$ for which the confidence interval includes 0 is the “breakdown” value, i.e. the value of a deviation of parallel trends so big that would lead to failing to detect the effect of interest at the chosen alpha level (here, $\alpha = .05$). In the following, we chose to inspect the smallest and largest event-study estimates (with their post-event timing signalled by k once more) for key outcomes and key groups.

Figure 13A displays results for lower-income mothers and three outcomes: benefit income, mental health, and financial worry. For all outcomes and estimates in Figure 13A, we find that our effects of interest would be detected if parallel trends are exactly met ($\bar{M} = 0$). For benefit income, we find that the smallest event-study effect would cease to be detected if parallel trends deviated by as much as the maximal pre-exposure difference ($\approx 1,349$ GBP), whereas the largest effect would cease to be detected for a violation of parallel trends 2.5 times as big as the maximal pre-exposure difference ($2.5 \times 1,349 \approx 3,372$ GBP). To the best of our knowledge, no shock other than austerity reforms could have delivered such losses to the exposed in our study. Benefit sanctions applied to out-of-work benefits whose amounts are typically meagre in the UK (Pattaro et al., 2022). It is unlikely that sanctions alone produced the kind of benefit income losses observed in this study.

⁶For code and examples, see <https://github.com/asheshrambachan/HonestDiD>.

Figure 13A: Bounding tests for parallel trends violations ([Rambachan & Roth, 2022](#)).

For mental health, the breakdown values displayed in Figure 13A are 1 for the smallest and 1.5 for the largest event-study effect. Hence, contemporaneous shocks would have had to lead to a deviation of parallel trends by around 15-23% of a SD. This difference is compatible with the mental health effects of work requirements among single-mothers in the UK ([Katikireddi et al., 2018](#)) and, by extension, could reflect the effects of sanctions among the exposed in our study. For financial worry, we would detect the smallest effect of interest only in the case of exact parallel trends, while the largest effect in later periods is not detected if violations of parallel trends amounted to the maximal pre-exposure difference. That difference is small, i.e. 0.2 on a scale from 1 to 5. We lack estimates from previous studies on other plausible shocks and a similar outcome. We note that our conclusions for financial worry among lower-income mothers are sensible to small violations of parallel trends.

For each of the three outcomes, similar conclusions apply to the effects of austerity reforms among single mothers (Figure 14A). As for adolescents, Figure 15A examines the case of girls in lower-income households. For both measures of socio-emotional and behavioural adjustments, all estimates are identified if parallel trends are exactly met ($\bar{M} = 0$). For internalising problems, results are sensible to violations of parallel trends as large as 2.5 times (half of) the largest pre-exposure difference for the largest (smallest) effect detected. Hence, a violation of parallel trends of around 17% of a standard deviation ($2.5 \times .07$) would invalidate the largest effect estimate. For externalising

problems, the breakdown value for the largest effect detected stops at 1, i.e. equal to the largest pre-exposure difference which is around 6% of a SD. Especially for such small deviations, estimates might be compatible with the effects of sanctions rather than cuts (Lohman et al., 2004), but overall evidence on the effect of sanctions on children is largely inconclusive in the literature (except for outcomes capturing child maltreatment, see, e.g., Pattaro et al., 2022).

Figure 14A: Bounding tests for parallel trends violations (Rambachan & Roth, 2022).

Figure 15A: Bounding tests for parallel trends violations ([Rambachan & Roth, 2022](#)).

9. A comparison of modern event-study estimators

We compare our preferred specification to other “modern” event-study designs that also overcome possible biases of conventional TWFE designs. The estimator proposed by Callaway and Sant’Anna (2021; CS, henceforth) uses semiparametric estimation rather than just outcome regression as in our preferred specification (Sun & Abraham, 2021; SA, henceforth). What is more, CS allows us to use the “not-yet-treated” at each point in event time as the comparison group: These are cohorts that are not yet exposed to austerity at a given point in event time, either because they will never be exposed during the observation period or because they will be exposed only later on. The CS estimator can thus provide evidence that our results do not depend on a particular definition of the comparison group.

Borusyak and colleagues (2021) have proposed an imputation-based method, from which we borrowed the test for pre-trends in the previous section. The imputation-based estimator fits the outcome regression on individual and time fixed effects among the not-yet-treated to predict the counterfactual “never-treated” outcome among the treated. Importantly, estimates using this imputation-based estimator do not correspond to shifts from a universal reference period (the wave prior to the event in our study) as in the SA estimator and in our application of the CS estimator. Estimates

correspond to the difference between the actual outcome and the average counterfactual pre-treatment outcome. This property of the imputation-based estimator helps us check that our estimates do not depend on the choice of a particular estimator and also on the choice of a universal reference period.

Finally⁷, we compare results from these three estimators to conventional TWFE regression, fitted without interactions between event-time dummies and event cohorts. Standard errors are clustered at the individual level to ensure comparability across estimators; differences are minimal with bootstrapped standard errors presented in the main text. For the sake of brevity, we provide figures only for mothers in low-income households and single mothers. Results for key outcomes – benefit income, financial worry, and mental health – are displayed in Figure 16A for mothers with lower incomes and Figure 17A for single mothers. We find little evidence of heterogeneity across estimators. In Figure 18A, we report results for labour supply and earnings for both lower-income (left) and single mothers (right). We find substantial overlap across “modern” estimators. If anything, using “traditional” TWFE would have led us to underestimate labour supply responses, particularly on the chances of being employed or not.

Figure 16A: Sensitivity analyses for benefit income, financial worry, and mental health outcomes across different event-study specifications for lower-income mothers.

⁷Current implementations of another estimator (De Chaisemartin & d’Haultfoeuille, 2020) do not allow for weights, differently from all the solutions mentioned in the main text. Applications of this fourth estimator to unbalanced panels like ours are also described as tentative in the supporting material for the corresponding software. We have chosen not to include results from this fourth estimator in the Appendix, but estimates are substantially similar to those of the other checks and support our main conclusions.

Figure 17A: Sensitivity analyses for benefit income, financial worry, and mental health outcomes across different event-study specifications for single mothers.

Figure 18A: Sensitivity analyses for labour supply and earnings across different event-study specifications for lower-income mothers (panel a.) and single mothers (panel b.).

10. In-depth analyses of mental health

We consider additional measures of mental ill health to help with the interpretation of our main findings. First, we investigate via factor analyses whether GHQ-12 can be decomposed in separate sub-dimensions. We find three factors that align with those highlighted in recent studies ([Griffith & Jones, 2019](#)). We name them accordingly as: (1) *lowered self-worth*, mainly predicted by items such as “losing confidence” or “think of self as worthless”; (2) *social dysfunction*, predicted by items asking if respondents, e.g., feel like they are “playing a useful part” or are “capable of decisions” and “able to face problems”; and (3) *stress*, predicted by items registering “loss of sleep”, whether respondents feel “constantly under strain” or are “having problems overcoming difficulties”, among others. In addition, we consider measures based on GHQ “caseness”, whereby responses to individual GHQ items are dichotomised, with 1 indicating a “symptom” of common disorders such as depression or anxiety, and then added up together. Following previous studies, we derive indicators of mental ill health, using the typical cutoffs at three and four symptoms (e.g., [S. Morris et al., 2017](#); [Kromydas et al., 2021](#)).

For mothers with lower incomes in the top-left panel of Figure 19A, lowered self-worth appears to be the largest contributor to the mental health deterioration observed in Figure 2. The z-score for this sub-dimension increases by up to 40% of a SD in period 7 ($p = .003$). Whilst we find the clearest signal for lowered self-worth, estimates are statistically indistinguishable for the other sub-dimensions. The figure for single mothers in the panel below is largely similar, with increases across all dimensions evident in periods 6 and 7, somewhat more so for lowered self-worth and stress, similar to the pattern for the summary measure in Figure 3.

On the right-hand side, we examine what these qualitative changes mean when using GHQ as a diagnostic tool for mental ill health. For mothers on lower incomes, evidence suggests an increase in mental ill health by an upper bound of 16-20 points depending on the cutoff, i.e. up to 57% of the pre-reform baseline. For single mothers, similar to GHQ z-scores, the chances of GHQ signalling mental ill health increase by up to 15-24 percentage points at the end of the observation period (e.g., for three or more symptoms, 24 pp, $p = .015$ in period 7) – up to 69% of the pre-reform baseline.

Figure 19A: Point estimates and 95% confidence intervals for the effects of exposure to benefit cuts among mothers (Parent sample, UKHLS Waves 1-10). Event-study estimates are derived from an interacted TWFE estimator as per Equation 1. Standard errors are bootstrapped with 50 replications. Study weights are applied.

11. Parenting dimensions

We relied on the Parenting Style and Dimensions Questionnaire (PSDQ, [Robinson et al., 1995](#); for a review, [Olivari et al., 2013](#)). A 32-item version of the questionnaire was administered from Wave 3 onwards. Only parents of 10-year-olds were asked to fill in this questionnaire module. A portion of those parents filled the questionnaire multiple times, one per each 10-year-old child. After listwise deletion, we are left with 3,814 observations for mothers (2,855 unique IDs) and 2,408 observations for fathers (1,788 unique IDs).

Using exploratory factor analysis, we find six dimensions of parenting that largely map back to the constructs first identified by Robinson and colleagues ([1995](#)) when developing the questionnaire. The six dimensions are: a) *warmth*, capturing parental affection and responsiveness towards the child; b) *respect*, i.e. how often parents respect and encourage children's desires and opinions; c) *consistency*, measuring how often parents enforce discipline by reasoning with the child, giving and explaining rules; d) *lack of reasoning*, when children are, conversely, disciplined with no explanation; e) *verbal hostility*, tapping on parental tendencies to yell or direct anger towards the child; f) *physical punishment*, asking parents how often they, e.g., spank or slap the child; g) *directiveness*, i.e. whether and how much parents scold and criticise the child.

Figure 20A: Point estimates and 95% confidence intervals for the average treatment effect on the treated of exposure to benefit cuts (parents of 10-year-olds; UKHLS Waves 3-10). Estimates are derived using [Słoczyński's method](#) ([Słoczyński, 2022](#)) and net of the following covariates: whether single parent at the baseline, level of completed education, ethnicity, region of residence, age of the respondent, number of children in the household, age of the youngest child in the household. Standard errors are bootstrapped with 50 replications. Study weights are applied.

Figure 21A: Point estimates and 95% confidence intervals for the average treatment effect on the treated of exposure to benefit cuts (mothers of 10-year-olds; UKHLS Waves 3-10). Estimates are derived using [Słoczyński's method](#) ([Słoczyński, 2022](#)) and net of the following covariates: level of completed education, ethnicity, region of residence, age of the respondent, number of children in the household, age of the youngest child in the household. Standard errors are bootstrapped with 50 replications. Study weights are applied.