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Income Volatility and Parenting Styles during Hard Times

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Non-technical summary

Instability in incomes is a point of concern in high-income countries with weakening safety nets, and the ripple effects of such income volatility on families are the subject of growing scientific interest. This paper examines if and how parenting styles change when households have unstable incomes. Parenting styles are increasingly understood as a key driver of child development, and capture the typical ways in which parents set rules and respond to their children's needs. Most previous studies have relied on income measures at one point in time, defined parenting using only a handful of indicators, and focused only on mothers. To complement previous evidence, I use high-quality UK survey data on income changes and family life collected during a period of great macroeconomic and policy turmoil, from the Great Recession to austerity and the COVID-19 pandemic (2009-2022).

I find that parenting styles respond differently to income volatility in households with higher and lower incomes at the start of the survey. Mothers with higher but more unstable incomes score lower on a measure of warmth and higher on harshness and permissive-ness. On the contrary, lower-income mothers score lower in harshness and permissiveness when their incomes are unstable due to repeated benefit income gains. As for fathers, those with higher incomes score lower on warmth when facing more earnings instability, whereas lower-income fathers seem to compensate for such instability with higher warmth. Results hold when taking into account factors that may explain parenting differences in households facing more or less volatility.

Overall, findings challenge commonly-held assumptions in scholarly and public debates around parenting in lower- and higher-income households, whilst highlighting the importance of safety-net protections against volatility.

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Abstract

Parenting styles are often intervened upon to mitigate disparities in children's well-being, with the focus of scholarly and public debates resting on lower-income mothers. Although differences in parenting across the income distribution are well-established, the extent to which income itself might be one of the motors of such differences is disputed. Little attention has been paid to income volatility, in particular, despite its secular rise, recent salience, and the links between volatility and parenting drawn by theories across the social and developmental sciences.

I thus investigate if and how income volatility affects parenting styles, relying on data from the UK Household Longitudinal Study (UKHLS, 2009-2022). Based on a variety of strategies to address measured and unmeasured confounding, I find that parenting styles are especially responsive to volatility among parents with household incomes above the median at baseline. For higher-income mothers, household and labour income instability are associated with lower warmth scores, whereas instability driven by household (benefit) income gains is associated with higher harshness and permissiveness scores. Differently, mothers with lower incomes score lower on harsh and permissive scales when experiencing benefit income gains. Fathers are especially affected by labour income volatility, with earnings losses leading to lower warmth scores among higher-income fathers, whereas the opposite is true for those with lower incomes. Findings shed light on how theories, public debates, and policies could be re-tailored to address the consequences of volatility on family life.

Keywords: Income, Parenting, Income volatility

JELS classification: D1, G5

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This paper uses data from Understanding Society, the UK Household Longitudinal Study (University of Essex [ISER], 2022). Funding for this project was granted by an Understanding Society Fellowship awarded by ISER (University of Essex) in 2022. I would like to thank the Understanding Society team and study participants for making this research possible. I would also like to thank Silvia Avram and participants at internal seminars at ISER, the RC28 Spring Meeting 2023, and the Understanding Society Scientific Conference 2023 for their valuable feedback and suggestions. All errors or omissions are the author's own.

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August 10, 2023

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An income gradient in child well-being persists in high-income countries (Cooper & Stewart, 2021). In scholarly and public debates, a prominent explanation for such gradient points to parenting differences across the socioeconomic spectrum (e.g. Lareau, 2011; Doepke & Zilibotti, 2019). As a result, parenting in lower-income households is often deemed a worthy target of remedial interventions. In contrast, the extent to which policies targeting household incomes might have spillover effects on parenting is unclear or disputed (Mayer, 2010; Heckman & Mosso, 2014). If any spillover effects can be expected, early studies (Mayer, 1997; Blau, 1999) that remain influential to this day (Torche, 2015) concluded that differences in household permanent income *levels* are more consequential than transitory income *changes* or volatility for short. Yet, volatility has been rising in the past fifty years (e.g. Dynan et al., 2012; Hardy & Ziliak, 2014; Moffitt & Zhang, 2018), and is greatly influenced by policy intervention or lack thereof (Bartels & Bönke, 2013; Rohde et al., 2014). Countries with more meagre safety nets like the US or the UK have become less effective in mitigating volatility in recent decades (Hardy & Ziliak, 2014; Avram et al., 2022), with only a temporary reversal during the COVID-19 pandemic. Against this changing landscape, it is worth re-examining if and how income volatility might matter for family functioning.

A handful of studies have incorporated income volatility in theoretical models of family functioning (Hill et al., 2013; Hardy, 2014; Morrissey et al., 2020), although the empirical focus has been more on child outcomes than parenting. Research has found evidence of associations between childhood income volatility and cognitive and non-cognitive measures of child development (Hill, 2021; Sosu & Schmidt, 2022), children's behaviour at school (Gennetian et al., 2015), as well as completed education (Hardy, 2014) and mental ill-health diagnoses in early adulthood (Cheng et al., 2020). Differently, the few studies examining whether and how income changes affect parenting have found mostly null results (Dooley & Stewart, 2007; Schmiedeberg & Bozoyan, 2021). Compared to child-centred studies, however, research on parenting has not analysed (long-run) volatility *per se*, focusing instead on short-run income changes regardless of their size and frequency. Available measures of parenting have also been rather coarse and limited to mothers. Finally, causal designs are still needed to disentangle the effects of volatility from (unob-

served) confounding, and thus assess whether and why volatility itself could be the target of policy interventions aimed at ameliorating family life.

Hence, I study if and how income volatility might affect parenting, using data from the UK Household Longitudinal Study (UKHLS, University of Essex [ISER], 2022). These data provide rich, accurate, and high-frequency information on household incomes (Fisher et al., 2019), superior to those of cohort studies often used in the field and collected during a period of macroeconomic and policy-driven financial turmoil (2009-2022, Fetzer, 2019; Crossley et al., 2021; Breinlich et al., 2022). I use these data to construct state-of-the-art measures of income volatility (e.g. Dahl et al., 2011; Moffitt & Zhang, 2018; Avram et al., 2022), so far never examined in tandem with parenting.

UKHLS also features a well-established in-depth survey instrument tapping into parenting styles (Robinson et al., 1995; Olivari et al., 2013). Parenting styles are increasingly understood as a key parental investment that can deliver developmental and competitive advantages to children, even more so in highly unequal contexts such as the UK (e.g. Ermisch, 2008; Chan & Koo, 2011; Fiorini & Keane, 2014; Cobb-Clark et al., 2019; Doepke et al., 2019). I examine parenting styles among both mothers and fathers (cfr. Ermisch, 2008; Cooper, 2020), thus also contributing to the burgeoning field on paternal parenting (e.g. Cabrera et al., 2018; Schoppe-Sullivan & Fagan, 2020).

Finally, I use an innovative causal design whereby the effect of volatility is identified net of a rich set of observed confounders (including, e.g., parental cognitive ability, Big 5 personality traits; Hartas, 2015; Schmiedeberg & Bozoyan, 2021). In addition, "post-outcome" values of volatility are leveraged to proxy for unobserved confounding (Mayer, 1997; Elwert & Pfeffer, 2022). Under a set of transparent assumptions, estimates in this study can thus suggest whether and how policies targeting income volatility may have spillover effects on parenting.

1. Background

1.1. Mechanisms linking income volatility and parenting

Income volatility might influence parenting via several mechanisms. The Family Economic Stress Model (FESM) has long posited that socioeconomic disadvantage may exert economic pressure on parents, adversely affecting their mental health and disrupting parental

relationships and parent-child interactions (Elder, 1974; McLoyd, 1990; Conger & Donnellan, 2007; Masarik & Conger, 2017). In particular, warmer parenting styles featuring intensive emotional engagement with the child might become more challenging to maintain compared to harsher or more permissive styles (Gershoff et al., 2007; Kiernan & Huerta, 2008; Rijlaarsdam et al., 2013; Sosu & Schmidt, 2017). Whilst many in the recent FESM literature have linked these parenting styles to snapshot measures of income poverty or material deprivation, others have also highlighted associations between drivers of income volatility such as job loss and unemployment and harsher or less sensitive parenting (Elder et al., 1985; McLoyd et al., 1994; Solantaus et al., 2004; Layte & McCrory, 2018; Prickett, 2020; Mari & Keizer, 2021). Besides, studies have also found that volatility in financial circumstances might lead to the accumulation of allostatic load and worsen mental health (Prause et al., 2009; Ganzel et al., 2010), changes which are predicted to associate with harsher parenting in the FESM (Conger & Donnellan, 2007; Masarik & Conger, 2017).

Beyond the FESM, behavioural theories of scarcity (e.g. Mullainathan & Shafir, 2013) suggest that lack of income might shift cognitive and other efforts towards day-to-day money management and away from other activities including parenting (Cobb-Clark et al., 2019). Living with low and unstable incomes may further compound this "attention" shift, also depending on the source of income volatility. Whilst any volatility might renew attention to household budgeting (Antonides et al., 2011), labour income volatility following job loss and unemployment might constrain attention due to the additional demands and stressors of job search (Paul & Moser, 2009; Melloy et al., 2018). For those out of work or in low-paid jobs, benefit income volatility might also reflect repeated high-stakes interactions with the benefit system. Attention might be depleted by managing benefit claims or by the loss of (or threat of losing) entitlements due to sanctions, policy changes or fraud suspicions (for a review, Gennetian & Shafir, 2015). These demands faced by people with low and unstable incomes are time-consuming, often require multiple cognitive efforts and can be emotionally draining, as they are tied to feelings of self-worth and social recognition (see also Patrick, 2023). Income volatility might thus deplete the attention parents could otherwise devote to the kind of monitoring and emotional responsiveness

typical of warmer parenting styles (Hill et al., 2013; Cobb-Clark et al., 2019).

Finally, income volatility may either intensify or disrupt the cognitive and emotional labour necessary for all family operations to run smoothly and to ensure that family members are catered to and supported – i.e. the 'mental load' of household labour (Hochschild, 1989, 2003; Offer, 2014; Daminger, 2019; Dean et al., 2022). In line with persisting norms concerning ideal motherhood (Hays, 1996), this mental load often falls disproportionately on women, who are specially tasked to retain a "consciousness of" their children's needs (Walzer, 1996; Dean et al., 2022). On the one hand, mothers might thus anticipate children's response to instability and adopt strategies to shield them from the household's financial struggles. For example, extensive qualitative evidence shows that mothers living with low and unstable incomes might skip meals so that their children can maintain their dietary intake and regular meal routines (Attree, 2005; O'Connell & Brannen, 2021; see also Ridge, 2011; Fisher & Nandi, 2015). On the other hand, previous research also suggests that unstable incomes might contribute to a more chaotic household environment (Corapci & Wachs, 2002; Coldwell et al., 2006; Wachs & Evans, 2010). Income volatility may thus motivate parents to maintain warm and attentive parenting strategies ('shielding') or, vice versa, make parenting more inconsistent or permissive ('chaos').

On balance, income instability might thus hamper parents' ability to maintain emotionally and cognitively demanding parenting styles with their children. Across households, the combination of low and unstable incomes might be particularly detrimental. Within households, mothers might bear the brunt of volatility. Volatility across different income components – labour, benefits – might also deserve special attention.

1.2. Income volatility in context: The UK in the 2010s and early 2020s

Studying parenting responses to income volatility is also motivated by the latter's rise and renewed salience. The rise in volatility is not immediately apparent or uniform across income sources. Household disposable income volatility has remained fairly constant in recent decades thanks to public and private insurance mechanisms: Safety-net programs are predisposed to mitigate earnings losses and (two-earner) households can self-insure if the correlation between earnings shocks across partners is low (Bartels & Bönke, 2013; Avram et al., 2022).

Yet, similar to other high-income countries, earnings instability has been on the rise in recent decades in the UK (Bartels & Bönke, 2013; Hardy, 2014; Moffitt & Zhang, 2018; Avram et al., 2022), with a peak during the Great Recession started in 2008/09. This trend is troublesome if one considers that the British safety net has historically performed less well than those of other European countries when tasked with mitigating labour income volatility (Bartels & Bönke, 2013; Rohde et al., 2014; Avram et al., 2022). In the UK, programs providing income support to households are skewed towards family-related cash transfers, and in-work rather that out-of-work benefits (Gornick & Smeeding, 2018; Brewer & Hoynes, 2019). What is more, studies have shown that the ability of benefit programs to smooth incomes among working-age households has declined after the recession (Avram et al., 2022). The weakened role of the safety net is largely attributable to a raft of reforms, first in 2011-13 and again in 2016, which restricted eligibility rules, tightened work requirements and related sanctions, and froze transfer levels across Britain's core benefit programs (Beatty & Fothergill, 2018). These austerity reforms resulted in benefit and household income losses concentrated among households at the bottom of the income distribution (Cribb et al., 2018; De Agostini et al., 2018; see also Fetzer, 2019).

In addition to austerity, Britain was hit by two other shocks giving renewed salience to income instability in the period under consideration. The Brexit referendum of 2016 had important macroeconomic consequences, resulting, for example, in losses in real incomes due to higher inflation for products with greater import shares (Breinlich et al., 2022). Since 2020, the worldwide COVID-19 pandemic has also spurred new income instability. Despite effective temporary measures such as the furlough scheme and a raise in benefit levels for some welfare recipients, studies have estimated household income losses of 10% or more for around half of the respondents in a nationally representative survey (Crossley et al., 2021). Households at the bottom of the income distribution, younger and job-insecure workers, women, and individuals from minoritised ethnic groups have been most affected (see also Blundell et al., 2020; Adams-Prassl et al., 2020).

Hence, the overall stability in household incomes, at least up to the COVID-19 pandemic, may mask rising labour and benefit income volatility. Instability has been spurred both by shocks affecting all countries including Britain (Great Recession, pandemic) and by

UK-specific shocks (austerity, Brexit). This unique combination of external shocks makes Britain an exemplary case study for the analysis of income volatility and its consequences.

2. Data and methods

2.1. Sample

I use data from Waves 1 to 12 of the UK Household Longitudinal Study (UKHLS, University of Essex [ISER], 2022), covering the period 2009-2022. UKHLS is a well-established household panel following the lives of a representative sample of around 40,000 people since 2009. UKHLS is uniquely suited for this study because it combines rich data on family life with high-quality and high-frequency income data, also differentiated by source (e.g. labour, benefit, etc.; for details, Fisher et al., 2019).

By construction, income volatility measures will require a minimum of two observations per household/person, meaning that the analytical sample will be restricted to observations starting in Wave 3. In addition, the inclusion of leads of volatility in my analyses (see Section 2.3) means that the analytical sample will not comprise waves beyond Wave 10. In short, Waves 1, 2, 11, and 12 contribute to the construction of volatility measures but do not feature in the main analytical sample.

Data on parenting styles was collected via ad-hoc questionnaire modules administered to parents whenever children in the household reached age 10. Whilst constraining my analyses to one parenting measurement per parent-child dyad, UKHLS measures at age 10 are taken at a crucial developmental stage in which the impact of financial resources and parenting styles is still strong (e.g. Steinberg & Morris, 2001; Pinquart, 2017; Noonan et al., 2018). In line with theoretical expectations and previous studies, I will examine mothers and fathers separately. Each parent may contribute to the dataset more than once if they have multiple children aged 10 in the same wave or if they reply to the questionnaire in different waves for different children. After listwise deletion, data on parenting styles are available for 3,933 unique parents and 5,167 total observations: 3,220 total observations for 2,467 mothers and 1,947 observations for 1,466 fathers.

2.2. Measures

2.2.1. Incomes

Following previous research, I start from the arcpercentage change (APC) in income. Specifically, for a given pair of study waves, the APC is given by:

$$APC_{i,w} = 100 \times \frac{Y_{i,w} - Y_{i,w-1}}{(Y_{i,w} + Y_{i,w-1})/2} \quad (1)$$

where $Y_{i,w}$ is income for person i in the current wave and $Y_{i,w-1}$ is income for the same person in the previous wave. Besides household disposable income, I also examine household benefit income and individual labour income, as income volatility from each component may distinctively affect parenting (see Background). All income measures are equivalised using the 'modified OECD' scale (Avram et al., 2022) and indexed at 2019 prices using the Consumer Price Index (CPI). The change in income across two subsequent waves ($Y_{i,w} - Y_{i,w-1}$) is then divided by the average income in those same waves to minimise the influence of outliers. The procedure also results in a symmetric measure, where the size of a given income change does not depend on the ordering of incomes across subsequent waves. The APC values range from –200% (positive income in wave $w - 1$ and no income in wave w) to +200% (no income in wave $w - 1$ and positive income in wave w), with 0 indicating no change (same income in both waves).

I compute the APCs for each pair of waves up to and including the one in which a given parenting outcome is measured and then take the standard deviation of the APCs observed until then as my measure of volatility. The standard deviation of the APCs *in a given sample or population* is a common measure of volatility in studies assessing volatility trends (e.g. Ziliak et al., 2011; Moffitt & Zhang, 2018; Avram et al., 2022), whilst I rely on the *within-person* standard deviation of the APCs in line with previous literature concerned with the effects of volatility on family functioning (Prause et al., 2009; Hill, 2021). For example, to a respondent whose parenting styles have been measured at Wave 8, I will assign the standard deviation of all "her" APCs observed up to and including Wave 8. A parent observed multiple times will be assigned each time the corresponding cumulative value of volatility up to and including the wave in which each parenting

measurement has been taken. Values are capped at 200 to minimise the influence of outliers.

Measuring income volatility by relying on the (history of) APCs has several advantages. The APC is computed based on raw income changes and does not require parametric assumptions (although it is closely related to measures derived from parametric models, Ziliak et al., 2011; Moffitt & Zhang, 2018). In addition, using the full history of income changes may help minimise concerns over measurement error, whilst leveraging all available data on exposure to volatility (Hardy, 2014). In my sample, volatility measures are collected over a minimum of three waves and an average of around six waves up to the measurement of parenting outcomes when children are aged 10, and thus covering the middle childhood period. The APC is also simple to calculate and easily incorporates the case where incomes are zero in either or both waves (Ziliak et al., 2011; Avram et al., 2022). On the other hand, one disadvantage is that the standard deviation of the APC is always positive, with higher values corresponding to more volatility due to both income losses and gains. In addition, instability might be especially consequential if income changes are relatively large and frequent, and the standard deviation of the APC may not help to disentangle these dynamics from small or isolated income shifts[1]. Finally, a small SD of the APC may reflect either stable incomes or consecutive losses or gains of similar size. To investigate possible asymmetric effects of (large and frequent) gains and losses, I will also rely on indicators for the number of arcpercentage changes larger than 25% (Dahl et al., 2011; Hardy, 2014; Hill, 2021) accumulated up to when parenting was measured for each respondent. I will examine one counter for positive income changes and one for negative income changes. Both are top-coded at three or more large income changes, with "no large positive (negative) income changes" serving as the comparison group.

In line with previous research, my goal is "to compare families with stable and unstable income at similar average income levels" (Hill et al., 2013: 86), as the combination of low and unstable incomes might be particularly impactful. Yet, income levels in a given wave

[1]For example, a household moving in and out of benefit receipt (corresponding to APCs of 200% and –200%, respectively) will be assigned large values of volatility regardless of the amount of benefit income gained or lost each time.

are affected by past income volatility. I thus rely on household disposable income levels in the first wave of interviews (for most respondents, Wave 1). I divide parents into a lower- and higher-income sub-sample based on whether their starting incomes fell below or above the median computed in Wave 1 for all UKHLS households in which the youngest child is aged 0-10 (around 29,250 GBP in 2019 prices). Income levels at one point in time are good proxies of permanent income (Brady et al., 2018). Splitting at the median is in continuity with previous studies (Hill, 2021). It also allows me to maximise the sample size in each group. Nevertheless, income levels at one point in time may be imprecise due to regression-to-the-mean dynamics. Household income levels, more generally, may not adequately capture households' ability to absorb income shocks compared to other measures of socioeconomic status such as wealth (e.g. Morrissey et al., 2020. Sensitivity analyses reported in Appendix S4 show that my main results are unaltered when using home-ownership status at baseline as an alternative definition of socioeconomic status, proxying for wealth (*ibidem*).

Figure 1 shows trends in the median within-person volatility as measured by the SD of arcpercentage changes. For descriptive purposes, I use all the information available in UKHLS for members of the analytical sample (unweighted n = 34,748 for 3,933 parents). The overall stability in household disposable income in the period – at least up to the COVID-19 period – masks growing instability in income components. Household benefit income volatility increased for both lower and higher-income parents. Austerity reforms are undoubtedly a major driver of such instability: Whilst disproportionately targeting lower-income households, some of the reforms also affected middle- and top-income income groups (Beatty & Fothergill, 2018). Across core state-benefit programs, the higher-income group in our sample typically receives universal child benefits as well as tax credits, whereas the lower-income group also receives housing benefits and out-of-work benefits - all of which were subject to austerity reforms in the period. It is worth stressing, however, that instability concerns different income amounts for different income groups, as state benefits account for a much smaller portion of total income for households in the upper parts of the income distribution compared to the bottom[2] (Fisher et

[2]In the analytical sample, benefit income accounts for around 40% of household disposable income in the lower income group and 12% in the higher income group.

al., 2019).

As for individual labour income, instability rises substantially among mothers. The economic reprise after the Great Recession, the more stringent work requirements attached to benefit receipt by austerity reforms, and the income and occupational dynamics associated with the return to work after childbirth are likely explanations (Portes & Reed, 2018; Harkness et al., 2019). Among lower-income fathers, labour income instability peaked in the midst of the Great Recession and again during the COVID-19 pandemic, whilst higher-income men have experienced growing but relatively less instability in the period.

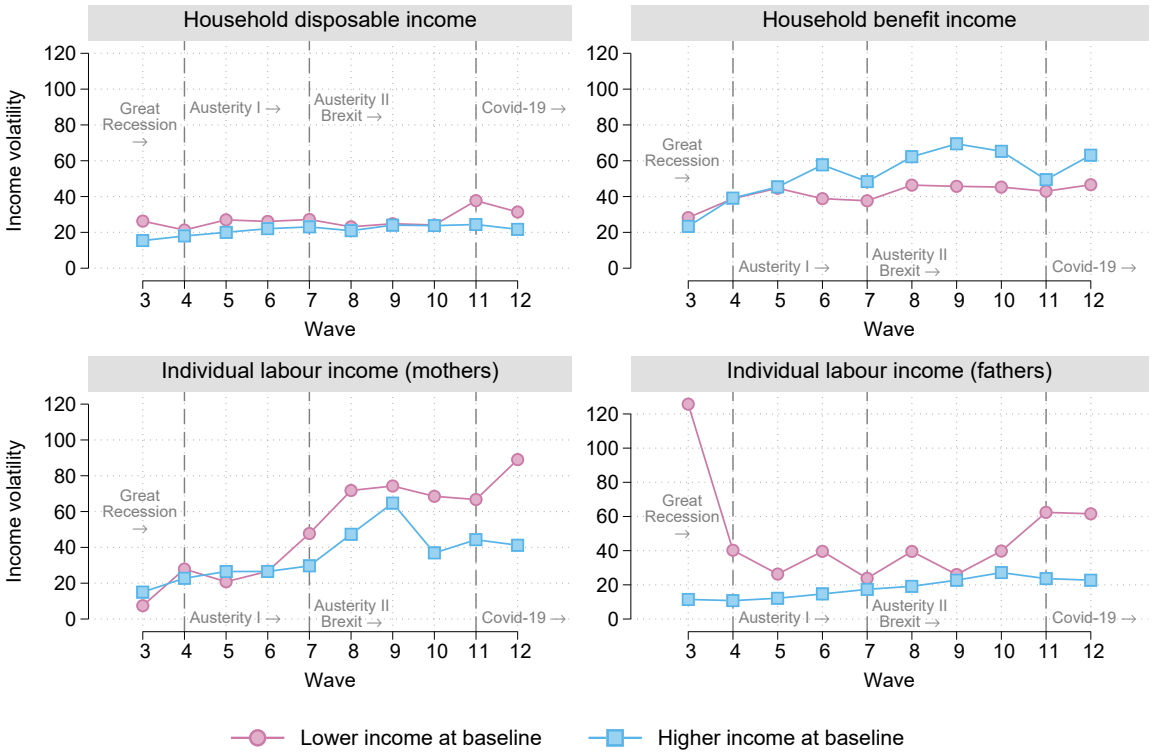

Figure 1: Median income volatility by study wave. Volatility is measured by the within-person standard deviation of the arcpercentage changes in different income components. Dashed lines in grey provide an indicative starting date for macroeconomic, policy, and other shocks in the study period. Longitudinal survey weights are applied.

2.2.2. Parenting styles

I examine parenting styles by relying on a 32-item version of the Parenting Style and Dimensions Questionnaire (PSDQ, Robinson et al., 1995) administered to parents whenever children in the household reach the age of 10. The PSDQ is a well-established survey instrument to measure parenting styles (for a review, Olivari et al., 2013). Parents are asked

about how they interact with their children; each item's response options range from 1 (never) to 5 (always). Self-reported measures are common in the field, especially for large samples where direct observations of parent-child interactions are impractical and costly. In addition, the PSDQ is a more in-depth and extensive questionnaire compared to the more coarse measures deployed in the few studies to date examining income changes and parenting (Dooley & Stewart, 2007; Schmiedeberg & Bozoyan, 2021). Nonetheless, relying on "one-shot" self-reported measures has limitations. In supplementary analyses in Appendix S5, I show that parental reports in the PSDQ correlate well with available youth reports of parental behaviours at ages 10 and 15, suggesting good cross-informant validity of PSDQ measures, also over time.

Based on long-standing research in the social and developmental sciences (e.g. Baumrind, 1991; Chan & Koo, 2010; Fiorini & Keane, 2014; Pinquart, 2017), I define three styles: warm, harsh, and permissive. A warm style features affection and responsiveness towards the child, coupled with rules set by reasoning together with the child. In the PSDQ, warm parenting is captured by items such as "I am responsive to the child's feelings and needs" or "I explain to the child how I feel about their good and bad behaviour". Differently, harsh parenting implies a more unilateral enforcement of discipline, also characterised by verbal hostility, physical punishments or other punitive strategies. Harsh parenting is captured by items such as "I yell or shout when the child misbehaves" or "I spank the child when they are disobedient". Finally, permissive parenting is receptive towards children's impulses and desires, but rules are enforced laxly by ignoring misbehaviour or not following through with punishments. The PSDQ taps into permissive parenting via items such as "I find it difficult to discipline the child" or "I spoil the child". Although parental inputs cannot be easily reduced to one or the other style, the consensus in the literature is that warm parenting is beneficial to child development and harsh and permissive parenting can be detrimental, especially when it comes to socio-emotional skills (e.g. Ermisch, 2008; Fiorini & Keane, 2014; Pinquart, 2017).

I match each item to the different styles as per well-established guidelines (Olivari et al., 2013) and run a confirmatory factor analysis. I ran two separate measurement models, one for mothers and fathers (Wong et al., 2022), and adjust specifications in light of mod-

ification indices to improve model fit[3]. Thanks to full-information maximum likelihood estimation, confirmatory factor analysis also allows me to include observations that have missing values on some but not all questionnaire items. Goodness-of-fit metrics are in line with accepted standards in the literature (for mothers: RMSEA = .035; CFI = .945; for fathers: RMSEA = .043; CFI = .921). Each predicted score is then standardized to have a mean of zero and a standard deviation equal to one.

Figure 2 displays the distribution of the three parenting styles among mothers and fathers in each income group. There is considerable overlap among parenting styles between income groups. Parents in lower-income households are somewhat over-represented at the bottom (and, among mothers, top) of the distribution of warm parenting scores. Parents with lower incomes at baseline are also more likely to be found at the top of the distribution of harsh and permissive parenting scores. Differences in parenting scores across income groups are similar for mothers and fathers.

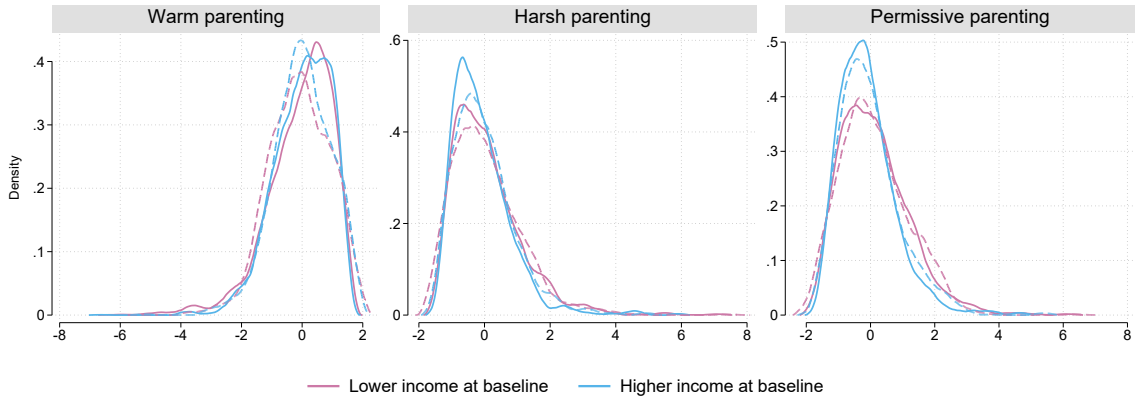

Figure 2: Parenting styles z-scores by income group.

2.3. Empirical approach

Focusing on income volatility as measured by the SD of arcpercentage changes, the target estimand τ in this study (Lundberg et al., 2021) can be formalised as follows:

$$\tau = E(Y_i(v) - Y_i(v-1)|G_i = g) \tag{2}$$

[3]I proceed iteratively adding covariances and cross-loadings that would lower the model's chi-square by at least 40 points (to avoid over-fitting).

where τ is the dose-response treatment effect for a unitary increase in income volatility $(Y_i(v) - Y_i(v-1))$ in households in the same income group g at baseline.

The key assumption necessary to approximate this causal quantity is that parents within each income group are not exposed to different levels of income volatility due to confounding factors also affecting their parenting behaviour. As mentioned in Section 1.2, the period under consideration featured several macroeconomic and policy shocks affecting household, benefit, and labour incomes. These exogenous shocks might have given impetus to income volatility in the period, regardless, at least in part, of parental characteristics (level of education, personality traits, etc.) that might be associated with parenting behaviours. Nevertheless, to strengthen causal claims, I modify the estimand as per Equation 2 by including measures of candidate confounding sources in the relation between volatility and parenting. The resulting vector of covariates X_i[4] comprises a quartic for age, level of education, region of residence, single-parent status at baseline, housing tenure at baseline, whether employed at baseline, whether the respondent had a long-term illness or disability at baseline, number of children (in four age brackets: 0-2, 3-4, 5-11, 12-15), cognitive ability[5] and Big 5 personality traits[6] (see, e.g., Dooley & Stewart, 2007; Chan & Koo, 2011; Hardy, 2014; Hartas, 2015; Schmiedeberg & Bozoyan, 2021). Further, to account for time-dependent dynamics (ϕ_t), I include wave and month-of-interview fixed effects.

Besides observed confounding, unobserved sources of confounding may still hinder causal claims. To address such concerns, I augment Equation 2 by including the "future" value of volatility (fv; Mayer, 1997; Elwert & Pfeffer, 2022), i.e. as computed over the waves following the measurement of a given parenting outcome. Future volatility cannot retrospectively affect parenting. Yet, future and current measures of volatility might share common observed *and* unobserved driving factors, which might, in turn, also affect parenting. For example, insofar as unobserved health shocks fuel both current and future

[4]In a few instances, I omit candidate confounders to avoid common support problems. Specifically, I omit the single-parent dummy in all models for men, and region fixed effects in models for income losses among men. Results are unaltered by these choices.

[5]Cognitive ability is measured as a latent construct in a confirmatory factor analysis based on cognitive assessments of memory, concentration, literacy, and numeracy available in Wave 3 (for details, McFall, n.d.).

[6]Data on personality traits were collected based on the Big 5 inventory (John & Srivastava, 1999) in Wave 3.

volatility, as well as influence parenting, adjusting for future volatility might purge the association between current volatility and parenting from unobserved confounding due to health shocks.

As formalised by Elwert and Pfeffer (2022), adjustment for future values of the exposure is strictly bias-reducing if two conditions are met: (1) the outcome does not influence future values of the exposure (no reverse causality); (2) future values of the exposure do not strongly depend on current values of the exposure ('low to moderate 'true" state dependence). Whilst it is reasonable to assume away reverse causality between parenting and volatility, the extent of state dependence in income volatility can be assessed empirically. Results presented in Appendix 1A are derived from regression models predicting future volatility on the basis of current volatility (and net of X_i and ϕ_t). For each additional unit (SD) of income volatility measured up to the current wave, future volatility increases by around .10-.33 units in the following waves. These coefficients are within the bounds identified in prior studies (*ibidem*) to denote settings in which adjusting for future values of the exposure is bias-reducing even in the case of moderately strong omitted confounders.

In my setting, an additional threat to identification is the possibility that volatility may affect parenting (also) by inducing worries in anticipation of future volatility (Mayer, 1997; Dooley & Stewart, 2007). Adjusting for future volatility might thus mute one of the channels via which past and current volatility affect parenting, leading to over-control bias. However, concerns about bias are only warranted to the extent that future volatility is a good proxy for respondents' current worries. Arguably, income instability was often unpredictable in the period being considered: For example, households could not (perfectly) anticipate how their finances were going to be moulded by the economic reprise after the Great Recession, the eligibility rules of austerity reforms, or the pandemic.

Hence, the empirical estimand in this study can be written as:

$$\theta = E(Y_i(v) - Y_i(v-1)|G_i = g, X_i, \phi_t, fv) \tag{3}$$

where θ is the dose-response treatment effect for a unitary increase in income volatility $(Y_i(v) - Y_i(v-1))$ in households in the same income group g at baseline, conditioning on

observed covariates and time-dependent dynamics (X_i, ϕ_t), and future values of income volatility (fv). In the remainder, I will refer to estimates of the parameter described in Equation 3 as conditional associations; their causal interpretation holds if and to the extent that X_i and ϕ_t capture observed sources of confounding, and how well fv proxies for unobserved sources. I will further assess sensitivity to unmeasured confounding in Section 4 (Cinelli & Hazlett, 2020).

To estimate θ, I rely on doubly-robust methods for two main reasons. Doubly-robust estimation requires correctly specifying only one of two models, either the one for the exposure or the one for the outcome. Besides, estimating a model for the exposure can flag a lack of overlap across covariates, which might otherwise lead to noisy or misleading estimates due to linear extrapolation if I was to only run a linear outcome model. Hence, I first derive inverse probability of treatment weights by predicting treatment intensity (for the SD of arcpercentage changes) or probability (for discrete income changes) based on X_i, ϕ_t, and fv (e.g. Hernán et al., 2004). Specifically, I derive stabilised weights to minimise concerns over extreme weights and lack of overlap across covariate values among units with different values of the treatment (e.g. Hernán & Robins, 2020). Treatment weights generally have mean around 1 and relatively small standard deviations, indicating good overlap in covariate values across treatment doses or conditions. After combining treatment weights with custom weights addressing sample non-response and attrition in UKHLS (see Kaminska & Lynn, 2019), I ran a weighted outcome model including all covariates. Estimation is carried out via OLS[7] and with robust standard errors. Differences across income groups are assessed by pooling income groups in a single model in which the exposure and all covariates are interacted with a dummy for income group.

[7]When examining discrete income changes, the estimand becomes:

$$\tau = E(Y_i(V = v) - Y_i(0)|G_i = g, X_i, \phi_t, fv) \tag{4}$$

where v equals one, two, or three or more income changes of at least 25%. Generally, less than half of respondents are exposed to one large income change, less than a quarter to two, and less than a tenth to three or more changes. OLS will place larger weights on smaller groups (the treated, in this case), thus yielding approximations of the average treatment effects *on the treated* (ATT, $\tau* = E(Y_i(V = v) - Y_i(0)|V = v, G_i = g)$; Słoczyński, 2022).

3. Findings

3.1. Income instability and parenting styles

Table 1 displays results for mothers based on the within-person SD of arcpercentage changes in income and split by income group at baseline. Figure 2A in the Appendix complements this evidence by plotting predicted scores against sensible values of volatility (up to around the 90th percentile), as well as p-values for the test of equality of coefficients across income groups. Estimates come from fully adjusted regression specifications including covariates, period effects, and future values of volatility to proxy for unobserved confounding. Coefficients are scaled by a factor of 10 for ease of exposition.

Overall, Table 1 displays little evidence that parenting is elastic to income instability among mothers, albeit with some exceptions. For a 10-unit increase in household income volatility, higher-income mothers score lower on the warm parenting measure by an average 2.4% of a SD ($p = .037$). The association seems to be driven by labour income volatility, whose increase by 10 units corresponds to a decline of about 1.5% of a SD in the warm parenting measure among higher-income mothers ($p = .006$). The associations between volatility measures and warm parenting scores are relatively smaller for lower-income women, although evidence of differences across income groups is not clear-cut (e.g. $p = .068$ for the equality of coefficients associated with labour income volatility; Figure 2A). When it comes to harsh parenting, I cannot detect associations between volatility measures nor differences in these associations across income groups. Differently, I find that lower-income mothers score 2.2% of a SD lower in permissive parenting when benefit income volatility increases by 10 units, and this estimate is statistically different at the 5% level from its counterpart for higher-income mothers ($p = .017$, see also Figure 2A).

Turning to fathers, Table 2 (Figure 3A in the Appendix) reports more clear-cut evidence of associations between income volatility and parenting styles. Instability in household income is associated with lower warmth (–3.3% of a SD, $p = .006$) and higher harshness (3.5% of a SD, $p = .010$) and permissiveness (2.5%, $p = .098$) among higher-income fathers. For harsh and permissive parenting, estimates for lower-income fathers are of similar size and not statistically different. Zooming in on different income components,

Table 1: Point estimates for the association between income volatility measures and parenting styles z-scores among lower- and higher-income mothers. Linear regression models are adjusted for covariates, future values of volatility, and period effects as detailed in the main text. Robust standard errors in parentheses.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Volatility (SD of APC)						
Household income	-0.006	-0.024**	-0.005	-0.004	-0.011	0.001
	(0.013)	(0.011)	(0.014)	(0.011)	(0.011)	(0.012)
Benefit income	-0.005	-0.008	-0.006	0.003	-0.022**	0.006†
	(0.010)	(0.006)	(0.010)	(0.006)	(0.010)	(0.006)
Labour income	-0.000	-0.015***	-0.007	0.004	-0.006	0.002
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)
Unweighted n	1,401	1,819	1,401	1,819	1,401	1,819

Note: * $p \leq .1$, ** $p \leq .05$, *** $p \leq .01$; † $p \leq .05$ for the equality of coefficients between lower- and higher-income parents.

however, I find evidence of opposite effects across income groups. Benefit income volatility leads to a 2.2% increase in the SD of harsh parenting ($p = 007$) and 1.6% for permissive parenting ($p = .033$) among higher-income groups. Both estimates are statistically different from the respective lower-income counterparts ($p = .013$ and .006, respectively). Labour income volatility, on the other hand, leads to higher scores on warm parenting among fathers with lower incomes at baseline (2% of a SD, $p = .012$) and lower scores for higher-income ones (2.4% of a SD, $p = .003$). The difference between the two estimates is statistically significant at conventional levels ($p < .001$).

Hence, higher-income parents, and especially fathers, seem most often responsive to income volatility. Differently from lower-income parents, and contrary to theoretical expectations, volatility in the higher-income group is often associated with lower scores on warmth and higher scores on harshness and permissiveness.

3.2. Instability due to gains v. losses among mothers

Companion analyses reveal that mothers are most affected by instability due to large and repeated income gains rather than losses. Figure 3 plots the main supporting evidence for this finding (see Table 1A and 2A in the Appendix for full results). As per panel A, higher-income mothers score lower in warmth in response to one (–16% of a SD, $p = .003$), two (–15%, $p = .051$), or three or more income gains (–44% of a SD, $p = .010$) larger than 25%, compared to the reference group experiencing no such changes. Higher-income mothers also score higher on harshness and permissiveness when accumulating three or

Table 2: Point estimates for the association between income volatility measures and parenting styles z-scores among lower- and higher-income fathers. Linear regression models are adjusted for covariates, future values of volatility, and period effects as detailed in the main text. Robust standard errors in parentheses.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Volatility (SD of APC)						
Household income	0.003	-0.033***	0.027	0.035***	0.018	0.025*
	(0.019)	(0.012)	(0.021)	(0.014)	(0.019)	(0.015)
Benefit income	0.021	0.001	-0.015	0.022***†	-0.024*	0.016**†
	(0.014)	(0.007)	(0.012)	(0.008)	(0.013)	(0.008)
Labour income	0.020**	-0.024***†	-0.004	0.007	0.004	-0.007
	(0.008)	(0.008)	(0.008)	(0.007)	(0.009)	(0.007)
Unweighted n	683	1,264	683	1,264	683	1,264

Note: * $p \leq .1$, ** $p \leq .05$, *** $p \leq .01$; † $p \leq .05$ for the equality of coefficients between lower- and higher-income parents.

more household income gains. Differently, estimates for lower-income mothers all point in the opposite direction.

Panel B at the bottom of Figure 3 shows that these patterns are somewhat driven by labour income gains for warmth and by benefit income gains for harsh and permissive parenting. Three or more labour income gains are associated with a decline of around 20.6% of a SD ($p = .039$) among higher-income mothers. For benefit income gains, effects flow in opposite directions for lower- and higher-income mothers, closely mirroring the top panel and earlier results in Table 1. Lower-income mothers score lower on harsh parenting when experiencing two (–18.8% of a SD, $p = .032$) and three or more (–29.4% of a SD, $p = .022$) benefit income gains, whereas estimates for higher-income mothers are similar in size but positively signed (e.g. 21.1% for three or more benefit income gains, $p = .059$). Permissive parenting scores decline by around 28.2% of a SD among lower-income mothers experiencing three or more benefit income gains ($p = .025$), whereas they increase by around 29.9% of a SD for higher-income mothers exposed to the same instability ($p = .005$).

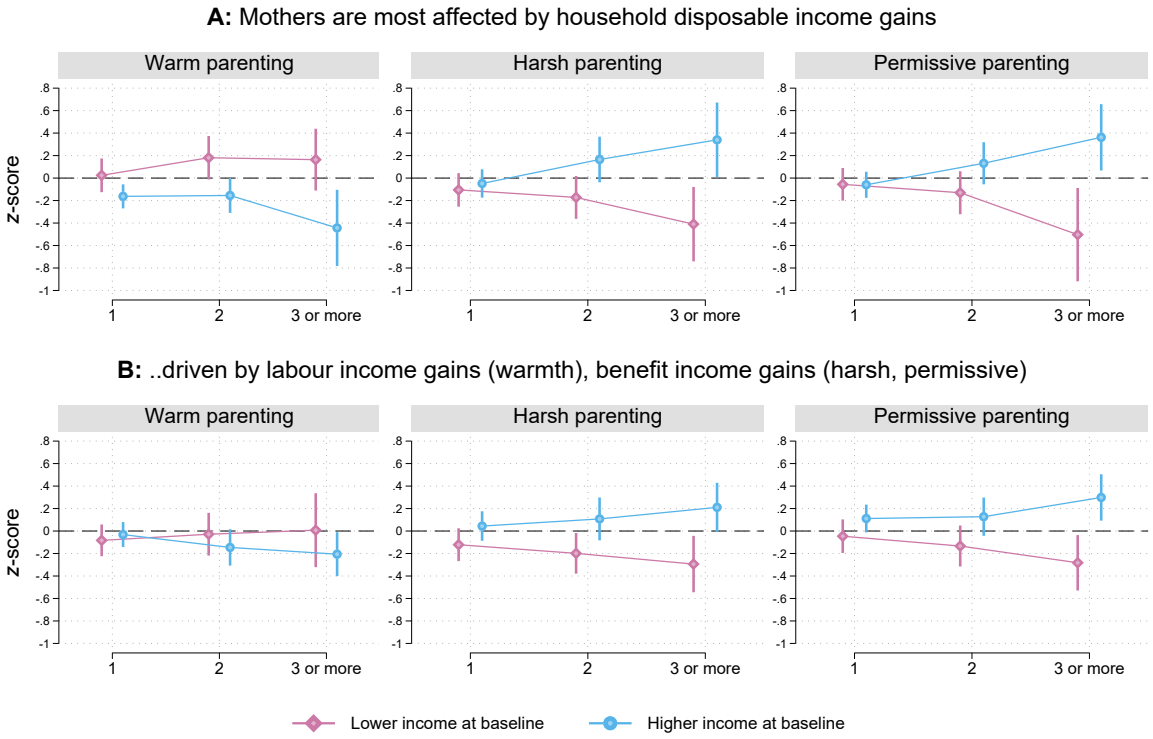

Figure 3: Point estimates and 95% confidence intervals for the association between income volatility measures and parenting styles z-scores among lower- and higher-income mothers. Household income volatility is examined in panel A; labour income (warmth) and benefit income (harsh, permissive) in panel B. Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

Additional results reported in Table 2A showcase that maternal parenting scores are rather inelastic to large and repeated income losses, especially those pertaining to household benefit income. The only relevant exception is household disposable income losses and higher-income mothers' warmth scores, which are found to decrease by around 37.3% of a SD among those affected by three or more losses ($p = .016$). Once again, labour income losses appear to drive such declines in warmth for higher-income mothers, although estimates for lower-income mothers are similar in size and sign. On balance, complementing evidence in Table 1, household income instability - driven by labour income gains and losses - seems to drive warmth down among higher-income mothers. Differences across income groups, however, are not always clear-cut when it comes to warmth. Differently, income gains, especially concerning benefit income, have opposite effects on harsh and permissive parenting scores across income groups, bolstering them for higher-income women and mitigating them for lower-income women.

3.3. Instability due to gains v. losses among fathers

For fathers, the headline finding accompanying results in Table 2 concerns labour income losses, as displayed in Figure 4 (full results are available in Table 3A and 4A in the Appendix). In line with Table 2, I find that labour income losses are associated with higher warmth scores among lower-income fathers, from 34% of a SD for one labour income loss (p = .001) to 42.7% for two (p = .003) and 54% for three or more (p = .003). Differently, estimates for higher-income fathers are similarly sized but point to lower warmth (e.g. for two labour income losses, –51.8% of a SD, p = .001).

When analysing harsh and permissive parenting scales, I find more similarities between lower- and higher-income men exposed to labour income losses. Estimates for lower-income men in Figure 4 are somewhat more clear-cut, overall, and point to declines in both harsh (e.g. –35.1% of a SD, p = .011 for two losses) and permissive parenting (e.g. –39.6% of a SD, p = .004 for two losses).

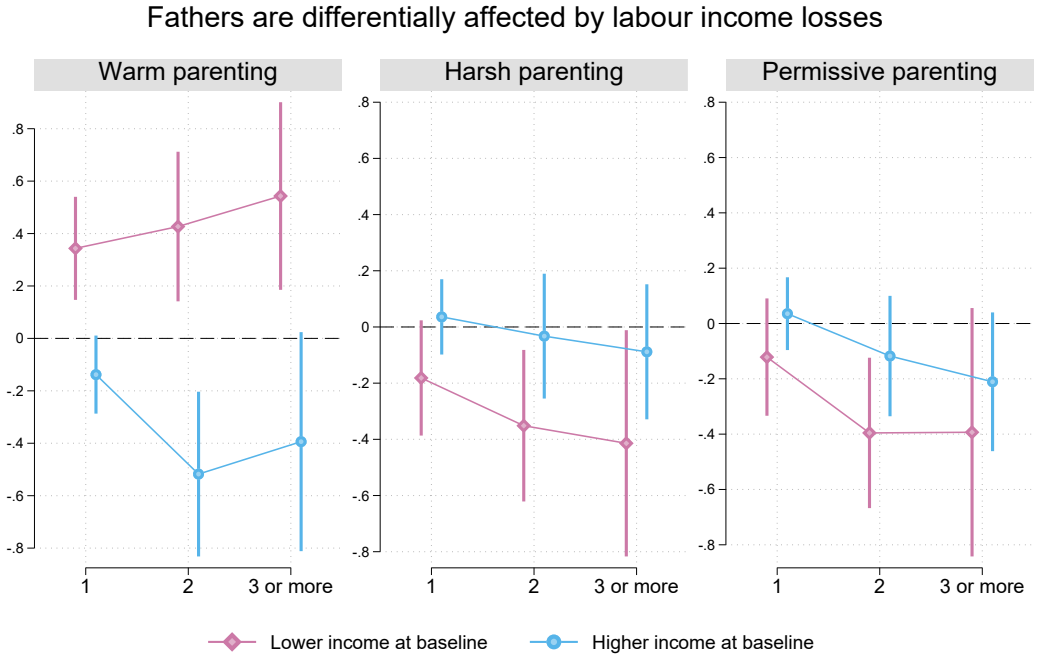

Figure 4: Point estimates and 95% confidence intervals for the association between labour income losses and parenting styles z-scores among lower- and higher-income fathers. Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

Similar to mothers, I also find that higher-income fathers are adversely affected by ben-

efit income gains (Table 3A in the Appendix), albeit differences between lower- and higher-income men are less clear-cut in this respect compared to mothers and also to the corresponding estimates in Table 2. Nonetheless, when accumulating three or more benefit income gains, higher-income fathers score lower on warmth (–39.2% of a SD, p = .017), higher on harshness (56.6% of a SD, p = .007), and higher on permissiveness (40.2%, p = .013).

Finally, lower-income fathers appear to score higher on warmth in relation to household income gains (e.g. 55.7% of a SD for 3 or more, p = .005), once again driven by the differential effect of labour income gains for this group compared to higher-income men (Table 3A). Taken together with results in Table 2, opposite responses to labour income instability across income groups appear driven by both labour income losses and gains. Labour income losses also spur previously undetected responses in terms of lower harshness and permissiveness among lower-income fathers. Adverse effects on harsh and permissive parenting scores corresponding to benefit income instability seem to be driven by income gains among higher-income fathers, similar to higher-income mothers.

4. Robustness checks

As for concerns relative to my outcome measures and empirical approach, I examine the former's cross-informant validity and the latter's internal validity. Parenting measurements are self-reported at one point in time and may differ from youth experiences at that time or in the long run. In UKHLS, adolescents aged 10-15 are administered a separate questionnaire comprising items on parent-child interactions. I can thus rely on one item surveying how frequently children "talk about things that matter" and one about how often they "quarrel" with their parents. Adolescents report separately on their mother and father, with response options ranging from "hardly ever" (1) to "on most days" (4). I select responses at age 10, similar to parenting measurements in the adult questionnaire, and age 15, the latest data point available for adolescents. Although matching is imperfect, as I cannot identify which child parents referred to when completing the PSDQ[8], I retain around 56% of the analytical sample of parents when matched to youth reports at

[8]When multiple children of the same age could be matched to the same parent, I took their average response to the survey items on parent-child interactions. Results are unaltered when focusing only on singletons.

age 10, and around 33% of the sample when matched to reports at age 15.

With these caveats in mind, I examine whether parent-reported parenting styles predict youth-reported parent-child interactions. Conditional associations[9] net of the same controls included in the main model are reported in Figure 6A in the Appendix. Results are in the direction one might expect, suggesting god cross-informant validity. At age 10, warm parenting measures for each parent are associated with a higher likelihood of talking about things that matter and a lower likelihood of quarrelling with that parent. The pattern reverses for harsh and permissive parenting, albeit with more clear-cut evidence concerning their positive association with quarrelling. At age 15, associations are smaller in size, possibly reflecting unobserved changes in parenting in the long run or a fading-out effect of earlier parenting styles on current parent-child interactions. Nevertheless, the same patterns observed at age 10 also hold at age 15, buttressing claims on the cross-informant validity of the main outcome measures in this study.

As for internal validity, a causal interpretation of my analyses rests on a selection-on-observables assumption. Future values of income volatility are added to address unmeasured and omitted confounding (Mayer, 1997; Elwert & Pfeffer, 2022), yet they might not fully capture it due to measurement error and depending on the strength with which omitted confounders relate to future volatility. I present here a bounding exercise (Cinelli & Hazlett, 2020) which simulates how estimates would change in the presence of an omitted confounder as strong as one of the control variables I could include. Confounding strength is assessed in terms of partial R-squared with respect to both the treatment (volatility) and the outcome (parenting styles). As a benchmark confounder, I rely on the available measures of Big 5 personality traits. These traits are one of the strongest independent predictors of the outcome in this sample (see also Hartas, 2015; Schmiedeberg & Bozoyan, 2021). Vice versa, I could not find equally strong predictors of the treatment among my control variables, a further indication that volatility might have been by and large driven by exogenous shocks in the period.

For illustrative purposes, Figure 5 reports contour plots for the t-statistic corresponding to selected associations between volatility and parenting styles. Estimates in both panels

[9]Results refer to linear regression models. Conclusions are unaltered when using ordered logistic regression.

suggest that omitted confounders three to four times as strong as Big 5 personality traits would be needed to invalidate conclusions on the effects of volatility (t <|1.96|). Such an omitted confounder would have to share a partial R-squared of .12-.20 with parenting styles and less than .05 with the volatility measures.

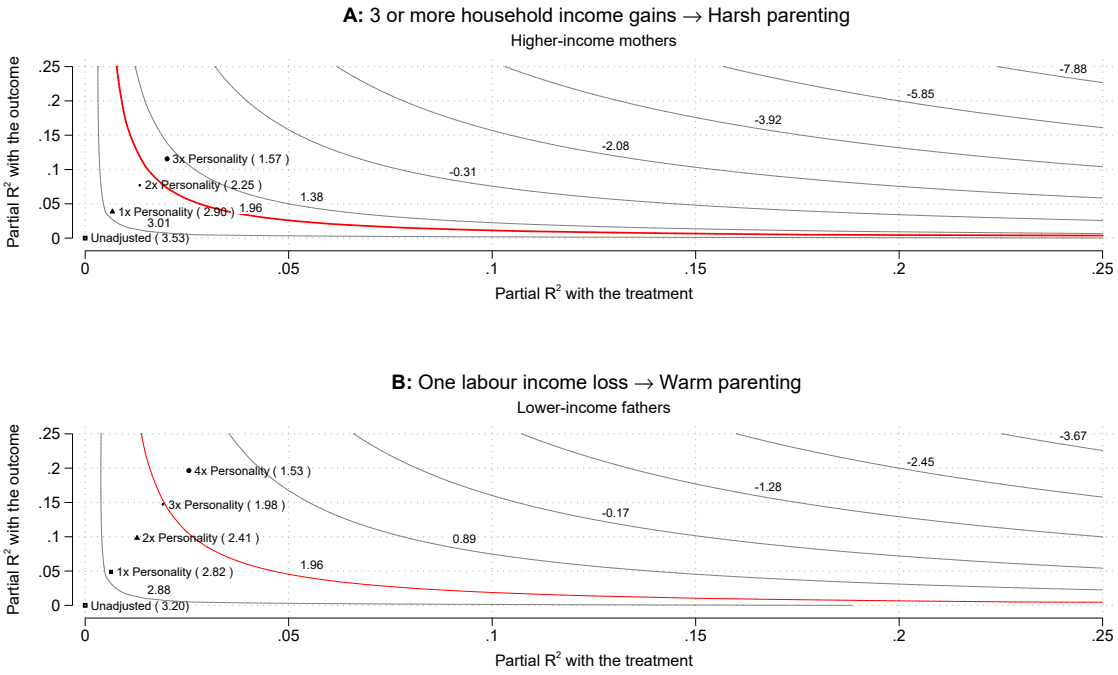

Figure 5: Sensitivity contour plots (t-statistic) in the partial R^2 scale. The analyses simulate an omitted confounder as strong as Big 5 personality traits for selected combinations of treatment (volatility), outcome (parenting styles), and sub-sample (higher/lower-income, mothers/fathers).

5. Discussion

This paper examined if and how income volatility affects parenting styles among mothers and fathers across the income distribution. There are four main findings. First, parenting styles are overall more responsive to income instability among parents with relatively higher incomes at baseline. Second, whilst not always clear-cut, differences between higher- and lower-income parents typically point to "adverse" effects on parenting for the former (lower warmth, higher harshness and permissiveness) and "positive" effects on parenting for the latter (higher warmth, lower harshness and permissiveness). Third, overall instability is often not consequential among mothers, but income gains are more conse-

quential than losses. Instability in disposable income, driven by individual labour income, is associated with lower warmth scores among higher-income mothers. Household (benefit) income gains, instead, lead to higher scores on harshness and permissiveness among higher-income mothers, whereas the opposite is true for lower-income mothers. Finally, income instability is associated with changes in paternal parenting styles, and more clearly so when it comes to labour income losses. For example, higher-income (lower-income) fathers facing unstable labour incomes score lower (higher) on a measure of warm parenting. Instability in household and benefit incomes is also detrimental for higher-income fathers, but differences with lower-income fathers are not always detected.

Findings suggest that transitory income changes might have a role to play in family functioning. Evidence supports recent calls to focus on income volatility (Hill et al., 2013; Morrissey et al., 2020), lending credence to models of intergenerational transmission in which family income enters not just in *levels*, but also in *changes* (cfr. Hardy, 2014; Torche, 2015). The magnitude of estimates corresponding to large and/or repeated income changes, in particular, is typically around 30% of a SD or higher. Comparable effect sizes have been ascertained when examining the effects of interventions directly targeted at parenting styles and practices (e.g. Jeong et al., 2021). On the other hand, it is worth stressing that a marginal increase of around 10% in income instability is found to be associated with fairly modest parenting responses of around 2-3% of a SD (Table 1 and 2). Hence, a promising direction for future empirical research is to focus on income instability, especially if large and/or frequent, and further pin down the magnitude of its effects, for which this study provides both lower and upper bounds.

Future studies and public debate might also benefit from recasting their focus on both lower- *and* higher-income mothers *and* fathers (e.g. Cabrera et al., 2018; Cooper, 2020). Existing theories emphasise the combination of low and unstable incomes (Hill et al., 2013; Conger & Donnellan, 2007; Gennetian & Shafir, 2015) and are typically centred around mothers (Dean et al., 2022). While there is abundant evidence that low income levels exert negative spillover effects on families (Cooper & Stewart, 2021), parenting responses to income changes are often nil or feature increases in warmth and decreases in harshness and permissiveness among lower-income mothers and fathers in this study. In contrast

with predictions from the Family Stress Model or behavioural theories of scarcity (Conger & Donnellan, 2007; Gennetian & Shafir, 2015), evidence thus points to shielding and compensatory parenting efforts in lower-income households facing instability, expanding on previous evidence of "positive" parenting practices in these environments (e.g. Cooper, 2020; Kirby et al., 2020). As such, findings further call into question public discourses often associating "poor parents" with "poor" parenting (for critical reviews, Jensen, 2018; Cooper, 2020).

Evidence that parents with higher incomes are affected by volatility also departs from Family Stress and behavioural theories. Yet, findings fall in line with studies showing adverse outcomes of income instability among relatively well-off children (Hardy, 2014; Cheng et al., 2020). This study thus contributes to the growing body of literature on the intergenerational consequences of income instability, suggesting a possible mechanism linking parents and children. Besides, evidence on mental load has mainly pertained to relatively affluent families (mothers in particular, Daminger, 2019; Dean et al., 2022). I find that lower scores for warmth and higher scores for harshness and permissiveness are often associated with instability due to income gains among higher-income parents. Such parenting responses could reflect overload due to intensifying demands at work or stressful interactions with the benefit system. Future research could gain from re-tailoring theories and samples to further investigate why parenting may be responsive to income instability among higher-income parents.

This study has some general limitations. The first is that income volatility is assessed at yearly or multi-year intervals corresponding to the fielding of UKHLS. As a result, I could not capture intra-year instability, which studies have already connected to child outcomes (Gennetian et al., 2015). At the same time, this study could rely on more precise and frequent information on incomes compared to past research (Dooley & Stewart, 2007; Schmiedeberg & Bozoyan, 2021), and also showcased the insights gained from being able to separate different sources of instability (cf. Hill, 2021). Similar, repeated measurements of parenting styles are also lacking. Therefore, I could not rely on parenting variation within families and over time. Studies could benefit from exploiting such sources of variation, especially in tandem with high-frequency income information,

although it is worth stressing that extant theories focus on income variation between families and within-family designs present empirical challenges of their own (Engzell & Hällsten, 2023). Finally, parenting is multi-dimensional, of course. This study only speaks to available measures of parenting styles. More comprehensive conclusions could be achieved by analysing parental time inputs and activities, parent-child routines, parental logics underlying child-rearing practices, and more (e.g. Ermisch, 2008; Lareau, 2011; Fiorini & Keane, 2014; Cooper, 2020).

Conclusions

This study provided some of the first evidence on how and for whom income volatility may affect parenting. It is thus possible that, partly through parenting, income volatility may have consequences that reverberate across generations with rich implications for the design of safety-net policies. One of the crucial roles of safety nets is to mitigate volatility, a role that has somewhat weakened in the past decades (Hardy & Ziliak, 2014; Avram et al., 2022), only to acquire unprecedented salience during the recent pandemic (e.g. Crossley et al., 2021). Results in this study suggest that lower-income parents stand to gain from benefit income gains, whilst being resilient to losses. The latter is an encouraging finding after a decade of austerity in the UK, although it is counterbalanced by evidence that lower-income parents bore other costs in the period, most clearly in terms of worse mental health (e.g. Reeves et al., 2020; Brewer et al., 2022). Differently, parents in relatively well-off families seem to be adversely hit by interactions with the benefit system, despite the extra income gains, while also being insufficiently insured against labour income losses. Taken together, findings in this study suggest that tailoring safety nets to address the spillover effects of volatility on parents (and children) might be a worthwhile, albeit not straightforward, endeavour.

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Appendix

S1. State dependence

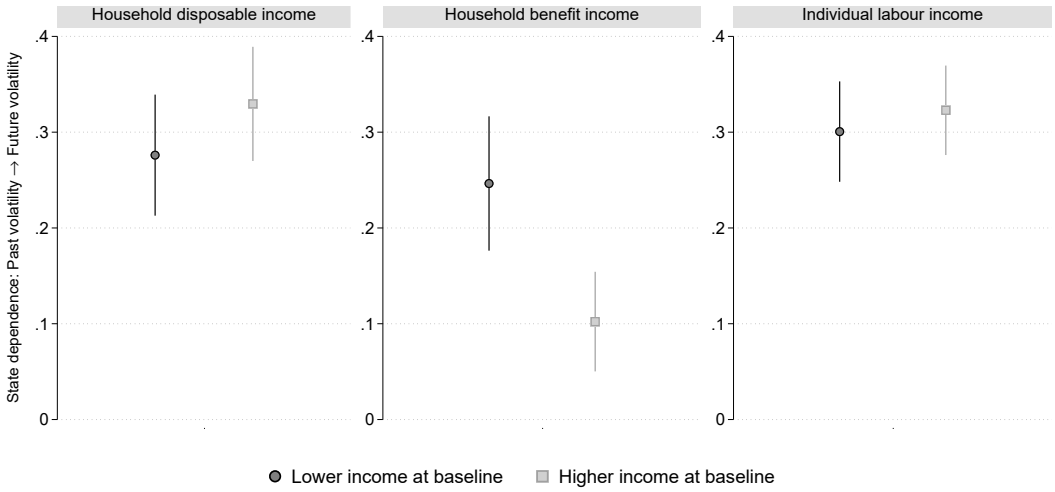

Figure 1A: Point estimates and 95% CIs for the association between past volatility and future volatility. All models are adjusted for the full set of controls detailed in the main text to approximate "true" state dependence. Pooled analytical sample (Unweighted $n =$ 5,167).

S2. Income volatility (SD of arcpercentage changes) and parenting styles

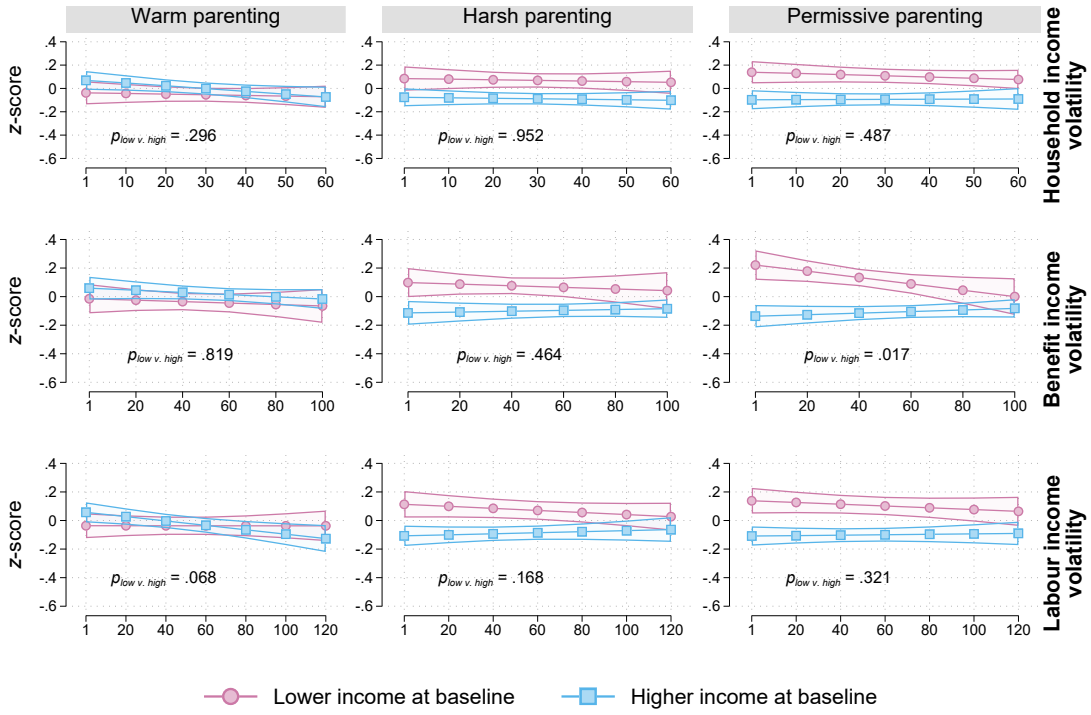

Figure 2A: Linear predictions and 95% confidence intervals for the association between income volatility measures and parenting styles z-scores among lower- and higher-income mothers. Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

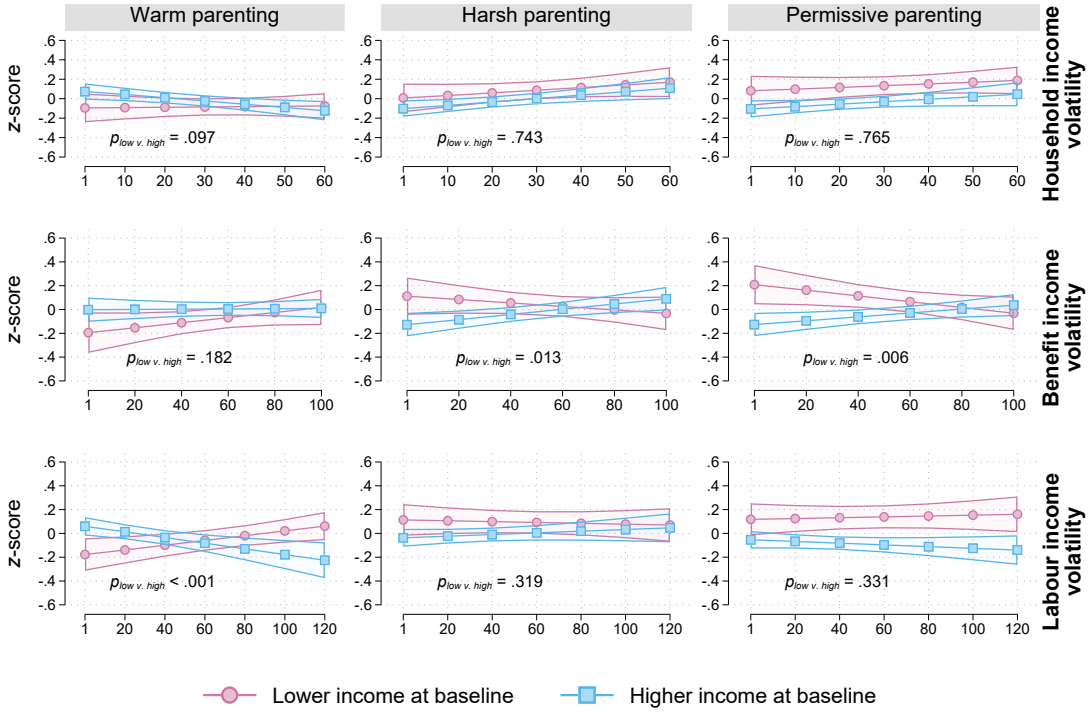

Figure 3A: Linear predictions and 95% confidence intervals for the association between income volatility measures and parenting styles z-scores among lower- and higher-income fathers. Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

S3. Income volatility (changes of 25% or more) and parenting styles

Table 1A: Point estimates and robust standard errors for the associations between positive income changes and parenting among mothers.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Household income gains						
1	0.024	-0.163***	-0.105	-0.048	-0.056	-0.060
	(0.076)	(0.055)	(0.076)	(0.064)	(0.074)	(0.059)
2	0.181*	-0.155*	-0.172*	0.165	-0.130	0.132
	(0.099)	(0.079)	(0.097)	(0.103)	(0.097)	(0.095)
3 or more	0.164	-0.443**	-0.410**	0.340**	-0.503**	0.362**
	(0.140)	(0.173)	(0.169)	(0.169)	(0.211)	(0.150)
Benefit income gains						
1	-0.009	-0.048	-0.122	0.044	-0.046	0.111*
	(0.075)	(0.056)	(0.074)	(0.067)	(0.076)	(0.063)
2	-0.138	-0.092	-0.198**	0.108	-0.133	0.128
	(0.112)	(0.079)	(0.092)	(0.097)	(0.093)	(0.087)
3 or more	0.056	-0.145	-0.294**	0.211*	-0.282**	0.299***
	(0.122)	(0.102)	(0.128)	(0.111)	(0.126)	(0.105)
Labour income gains						
1	-0.083	-0.031	-0.016	-0.076	0.006	-0.085
	(0.072)	(0.056)	(0.070)	(0.066)	(0.073)	(0.058)
2	-0.028	-0.146*	-0.035	0.030	0.014	0.028
	(0.097)	(0.082)	(0.112)	(0.093)	(0.120)	(0.096)
3 or more	0.008	-0.206**	-0.087	0.019	-0.041	-0.049
	(0.168)	(0.100)	(0.188)	(0.118)	(0.185)	(0.104)
Unweighted n	1,401	1,819	1,401	1,819	1,401	1,819

Table 2A: Point estimates and robust standard errors for the associations between negative income changes and parenting among mothers.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Household income losses						
1	0.011	-0.036	-0.050	-0.034	-0.067	0.014
	(0.070)	(0.053)	(0.072)	(0.060)	(0.070)	(0.059)
2	0.185*	-0.019	0.051	0.074	0.109	0.089
	(0.107)	(0.078)	(0.131)	(0.092)	(0.142)	(0.088)
3 or more	0.093	-0.373**	-0.021	0.161	0.044	0.072
	(0.202)	(0.155)	(0.195)	(0.152)	(0.188)	(0.149)
Benefit income losses						
1	0.089	0.059	0.007	-0.160**	0.017	-0.113*
	(0.085)	(0.063)	(0.076)	(0.070)	(0.081)	(0.065)
2	0.080	-0.012	-0.082	-0.137*	-0.099	-0.084
	(0.101)	(0.078)	(0.098)	(0.082)	(0.100)	(0.075)
3 or more	0.084	-0.074	-0.009	-0.056	0.011	-0.033
	(0.121)	(0.098)	(0.120)	(0.107)	(0.121)	(0.100)
Labour income losses						
1	-0.064	-0.034	0.072	0.067	0.014	0.079
	(0.071)	(0.053)	(0.080)	(0.057)	(0.083)	(0.057)
2	0.037	-0.185**	-0.076	0.065	-0.074	-0.023
	(0.112)	(0.083)	(0.130)	(0.078)	(0.133)	(0.074)
3 or more	-0.276	-0.196	0.089	0.048	-0.075	-0.098
	(0.252)	(0.124)	(0.210)	(0.132)	(0.226)	(0.167)
Unweighted n	1,401	1,819	1,401	1,819	1,401	1,819

Table 3A: Point estimates and robust standard errors for the associations between positive income changes and parenting among fathers.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Household income gains						
1	0.202**	-0.054	0.051	0.001	0.081	-0.015
	(0.100)	(0.065)	(0.101)	(0.071)	(0.105)	(0.074)
2	0.259*	-0.115	0.140	0.057	0.031	-0.020
	(0.142)	(0.108)	(0.170)	(0.107)	(0.163)	(0.100)
3 or more	0.557***	0.214	-0.000	-0.024	-0.202	-0.038
	(0.198)	(0.191)	(0.228)	(0.261)	(0.210)	(0.319)
Benefit income gains						
1	0.131	0.126*	-0.174	0.172**	-0.146	0.155*
	(0.118)	(0.068)	(0.120)	(0.082)	(0.122)	(0.083)
2	0.157	-0.228**	0.055	0.224*	0.024	0.108
	(0.138)	(0.111)	(0.139)	(0.118)	(0.144)	(0.104)
3 or more	-0.067	-0.392**	0.238	0.566***	0.131	0.402**
	(0.184)	(0.164)	(0.203)	(0.211)	(0.208)	(0.161)
Labour income gains						
1	0.066	0.021	0.023	0.006	0.002	0.021
	(0.110)	(0.071)	(0.116)	(0.074)	(0.118)	(0.070)
2	0.288**	0.019	-0.002	-0.060	0.087	-0.119
	(0.146)	(0.110)	(0.183)	(0.128)	(0.205)	(0.128)
3 or more	0.360**	-0.288	-0.296*	0.077	-0.326*	-0.018
	(0.178)	(0.190)	(0.170)	(0.178)	(0.178)	(0.208)
Unweighted n	683	1,264	683	1,264	683	1,264

Table 4A: Point estimates and robust standard errors for the associations between negative income changes and parenting among fathers.

	Warm parenting		**Harsh parenting**		**Permissive parenting**	
	Lower income	Higher income	Lower income	Higher income	Lower income	Higher income
Household income losses						
1	0.144	-0.073	0.136	0.055	0.107	-0.042
	(0.100)	(0.071)	(0.104)	(0.070)	(0.115)	(0.066)
2	-0.121	-0.322***	0.021	0.155	0.035	0.077
	(0.153)	(0.101)	(0.158)	(0.120)	(0.175)	(0.118)
3 or more	0.624***	-0.199	-0.397	-0.064	-0.265	-0.138
	(0.225)	(0.171)	(0.347)	(0.199)	(0.412)	(0.206)
Benefit income losses						
1	-0.049	-0.080	0.250**	-0.117	0.319**	-0.139
	(0.124)	(0.077)	(0.126)	(0.089)	(0.137)	(0.090)
2	0.087	0.003	0.072	-0.040	-0.013	-0.039
	(0.144)	(0.093)	(0.157)	(0.104)	(0.163)	(0.106)
3 or more	0.309	-0.150	-0.071	-0.059	-0.228	-0.100
	(0.197)	(0.133)	(0.201)	(0.128)	(0.202)	(0.126)
Labour income losses						
1	0.343***	-0.138*	-0.181*	0.036	-0.121	0.036
	(0.100)	(0.076)	(0.104)	(0.068)	(0.108)	(0.067)
2	0.427***	-0.518***	-0.351**	-0.033	-0.396***	-0.118
	(0.145)	(0.160)	(0.137)	(0.113)	(0.138)	(0.111)
3 or more	0.543***	-0.394*	-0.414**	-0.088	-0.393*	-0.211*
	(0.182)	(0.213)	(0.205)	(0.122)	(0.229)	(0.128)
Unweighted n	683	1,264	683	1,264	683	1,264

S4. Alternative socioeconomic groups

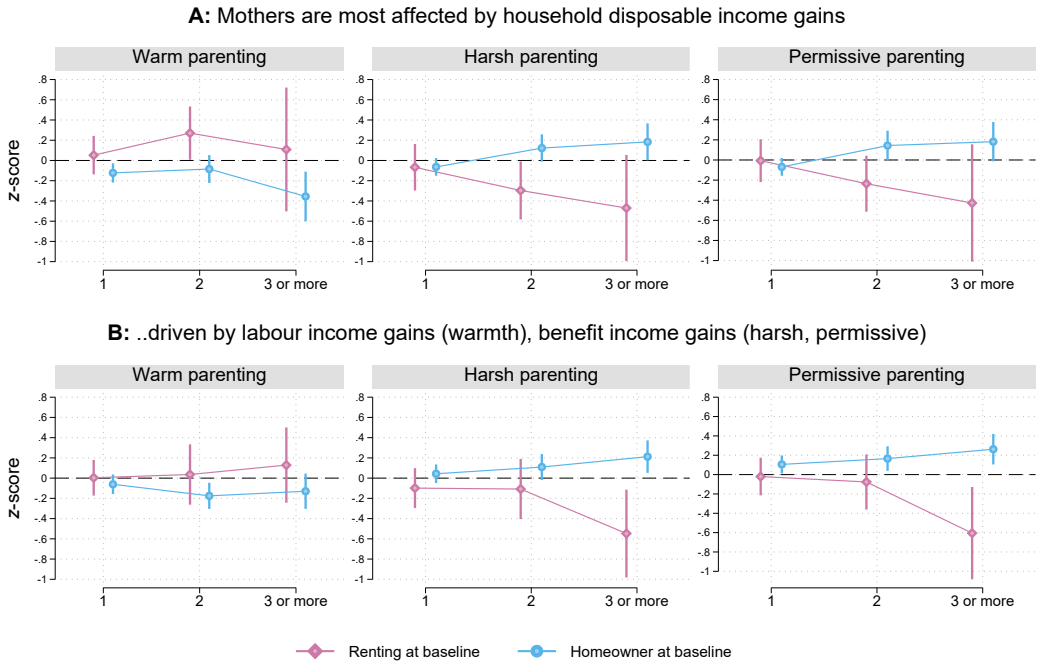

Figure 4A: Point estimates and 95% confidence intervals for the association between labour income losses and parenting styles z-scores among mothers renting or homeowners (with a mortgage or outright) at baseline (unweighted $n = 959$ and 2,261, respectively). Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

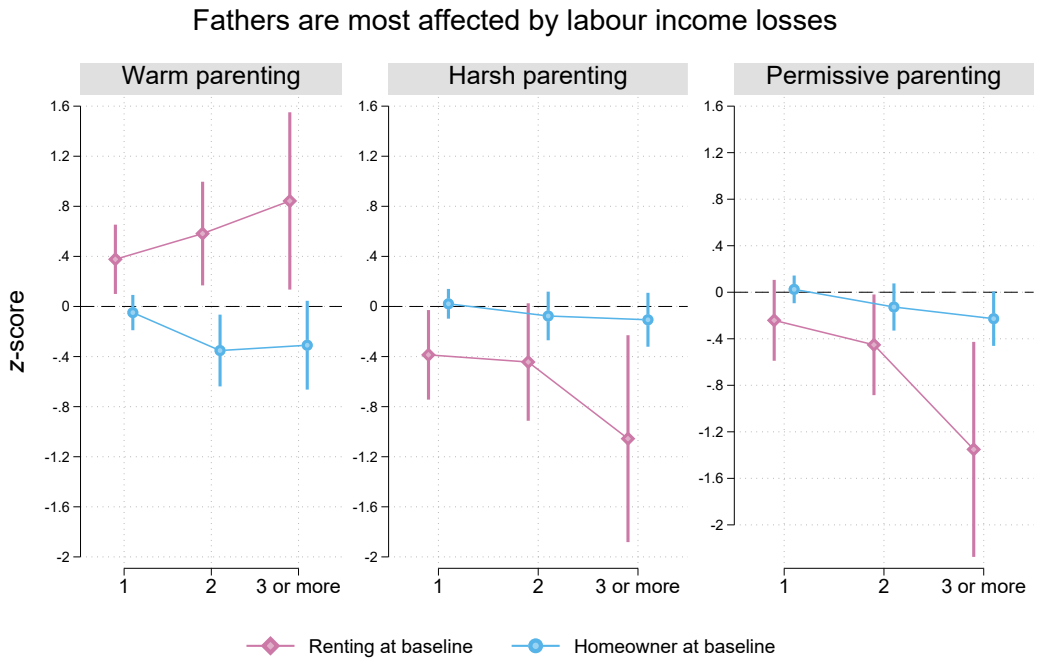

Figure 5A: Point estimates and 95% confidence intervals for the association between labour income losses and parenting styles z-scores among fathers renting or homeowners (with a mortgage or outright) at baseline (unweighted $n = 389$ and 1,558, respectively). Linear regression models are estimated with robust standard errors and are adjusted for covariates, future values of volatility, and period effects as detailed in the main text.

S5. Parenting styles and youth reports

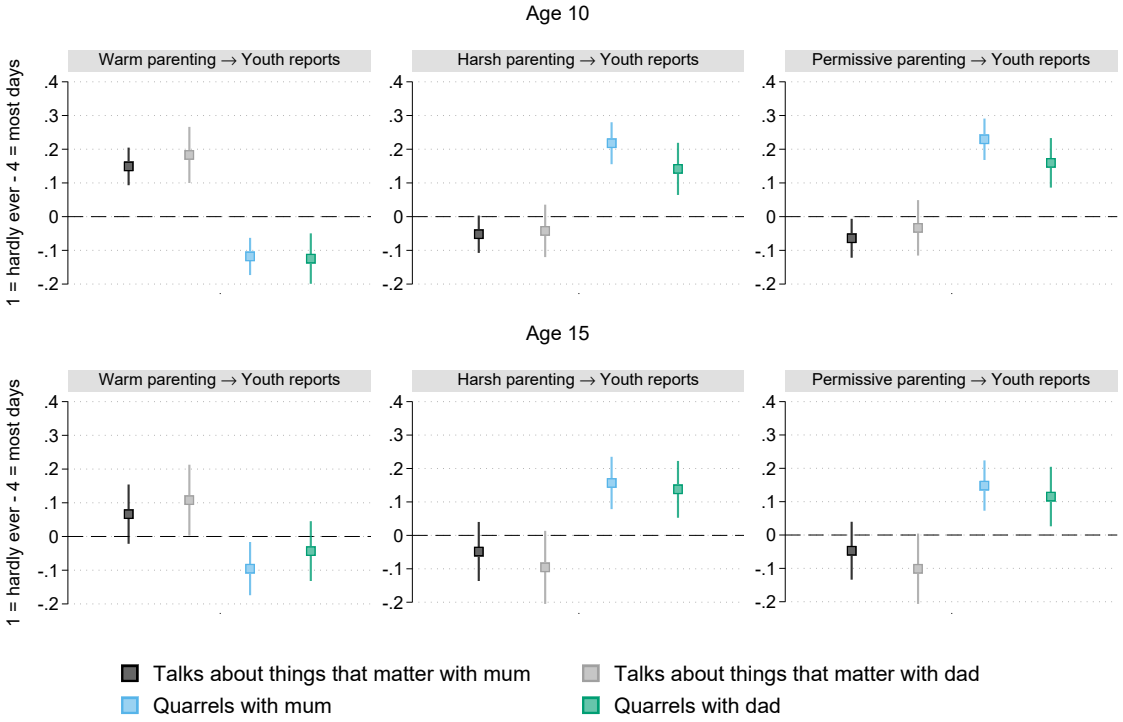

Figure 6A: Point estimates and 95% confidence intervals for the association between parenting styles and youth reports of parent-child interactions at age 10 and 15. Parenting styles for each parent are associated with youth reports about the same parent (unweighted n at age 10 = 1,825 mother-youth pairs and 1,078 father-youth pairs; unweighted n at age 15 = 1,084 mother-youth pairs and 694 father-youth pairs).