Growing up with an Unemployed Mother*

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Abstract

We study the long-term consequences of maternal unemployment on children's labor market outcomes. Using the NLSY, we show that children exposed to maternal unemployment during childhood have lower wages and employment probabilities as adults. These effects remain even after controlling for family income, indicating that income loss alone cannot explain the observed scarring effects. Our results suggest that (i) greater parental time availability does not mitigate the damage and (ii) non-income channels play a key role. Finally, we find that the negative effects are concentrated in adolescence, with maternal unemployment during these years leading to earlier labor force entry and reduced educational attainment.

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1 Introduction

Losing a job and becoming unemployed is a traumatic and costly experience for workers and their families. Unemployment is associated with large and persistent declines in income and health among workers, as well as worse labor market, educational, and health outcomes for their children. Most evidence on the effects of parental job loss comes from comparing displaced and otherwise similar non-displaced workers. However, displacement simultaneously reduces household income, increases parental time availability, and may impose other psychological or social costs. Because these effects operate at the same time, it is difficult to disentangle the channels through which parental unemployment affects children.[1]

In this paper, we provide a new perspective on the long-term effects of parental unemployment, with an emphasis on mothers. We construct a measure of children's exposure to maternal unemployment and estimate its effect on children's labor market outcomes, while separating the income-loss channel from other potential mechanisms. We find that greater maternal unemployment during childhood is negatively associated with children's future employment probability and wages. Importantly, this relationship persists even after controlling for family income, indicating that the adverse effects of unemployment extend beyond lost income and that additional maternal time spent at home does not offset these costs.

We then conduct a comprehensive analysis of heterogeneity in these effects. We show that maternal unemployment is most detrimental during adolescence. We further utilize multiple datasets and examine a broad range of outcomes, including children's test scores, mothers' time allocation, family expenditures, and age at labor force entry, to understand the underlying mechanisms.

Our primary datasets are the National Longitudinal Survey of Youth 1979 cohort (NLSY79) and the NLSY79 Child and Young Adult cohort (NLSY79-CYA). Essential for our approach, the NLSY79 includes the labor force status of each respondent on a week-by-week basis, which allows us to construct an exposure measure of each child to his/her mother's unemployment. The NLSY79-CYA follows the biological children of the women in the original 1979 cohort from childhood and into adulthood, collecting detailed information on all life stages of these children and allowing for intergenerational analysis. We explore the labor market outcomes of these children when they are young adults and relate them to their mothers' unemployment when they were growing up.

Children who spent more time with an unemployed mother when growing up have lower

[1]Previous papers on parental job loss have also explored the mechanisms underlying these effects, focusing on income loss as well as non-monetary channels such as parents' and children's mental health (Carneiro, Salvanes, Willage, and Willén, 2024) and children's crime, teenage work, and fertility (Britto, Melo, and Sampaio, 2023). Our contribution is not to deny these efforts but to emphasize that, because job displacement simultaneously lowers household income and imposes other non-monetary costs, it remains difficult to isolate and quantify the relative importance of each channel.

employment probability and lower future wages. In particular, a child who is one standard deviation above the mean exposure to maternal unemployment (approximately 67 additional childhood weeks with his/her mother unemployed) has a 4% lower wage than the average child in the distribution. This negative return is approximately 30-50% of the return to one year of schooling, which is, on average, between 8 and 13% (Card, 1999). Interestingly, we do not find an impact of maternal unemployment on the number of hours worked.

Unemployment simultaneously reduces family income, increases parental time availability, and generates other non-income costs. To separate the income channel from other mechanisms, we follow Carneiro and Heckman (2003) and construct a measure of family per capita permanent income. We show that income loss explains only part of the negative impact of maternal unemployment. A child with one standard deviation above the mean exposure to maternal unemployment still displays a 3% lower future wage than the average child in the distribution with the same level of family income. This result suggests that maternal unemployment has scarring effects beyond lower income. Moreover, we show that this scarring effect of unemployment cannot be fully explained by children's educational attainment, even though maternal unemployment does negatively affect it.

Our cumulative exposure approach allows us to control for family income during childhood, but it also raises concerns about endogeneity due to the lack of clear quasi-experimental variation. In particular, duration and probability of maternal unemployment may be correlated with unobserved maternal characteristics. We show that our results are not driven by this correlation. We control for several proxies of maternal abilities – such as education levels, cognitive ability measures, and other background characteristics – and find that most of the scarring effect remains unexplained. We also use exposure to maternal unemployment outside the child's formative years as a placebo-type control. The logic of this exercise is as follows: if maternal traits fully explained the negative association, unemployment at any stage of the mother's life should predict children's outcomes. If, instead, exposure during childhood is the key driver, only unemployment during this period should matter. We find, indeed, that only maternal unemployment during the child's formative years is correlated with children's labor market outcomes.

We build on Caetano (2015) to show that our controls adequately address selection on unobservables.[2] Since mothers cannot have negative unemployment exposure, our sample mechanically exhibits marked bunching of children at zero maternal unemployment exposure. This means the zero-exposure bin disproportionately includes mothers with very strong labor-market attachment–those who, absent truncation, would have had "negative unemployment." Consequently, the zero-exposure group has a higher concentration of high-ability mothers compared to those with small

[2]The idea that bunching can be used to test endogeneity is further developed in Caetano, Caetano, Fe, and Nielsen (2024a), Caetano, Caetano, and Nielsen (2024b), Caetano, Caetano, Nielsen, and Sanfelice (2024c), and Caetano, Mansfield, and Slichter (2024d).

positive exposure. If unobserved maternal characteristics drive our results, we should observe a discontinuity at zero: children of zero-exposure mothers would, on average, have better outcomes than those whose mothers had minimal positive exposure. By contrast, if unemployment itself causally harms children, outcomes should vary smoothly through zero, since 0.00 and 0.01 years of exposure are fundamentally similar. We find that once maternal ability proxies are included, children's wages vary smoothly at zero, confirming that our controls capture the relevant dimensions of maternal quality and that unemployment has a causal effect on children.

Another concern is potential confounding factors. Unemployment may itself be caused by contemporaneous household shocks that also directly affect children's outcomes. To address this, we include two sets of controls: (i) family environment variables such as family structure and the presence of fathers and grandparents in the household, and (ii) local labor market and community conditions drawn from the NLSY Geocode data. Finally, we consider measurement errors in income and show that they do not explain our findings.[3]

In the final section of the paper, we investigate the mechanisms underlying the scarring effect of maternal unemployment. We estimate how the effect of maternal unemployment depends on the stage of the childhood by breaking down the 18-year maternal unemployment exposure measure into 6-year exposure measures. We find strong evidence of age-dependent effects: maternal unemployment has no detectable impact on young children but generates significant negative effects during adolescence.

To understand why we do not find effects in early childhood, we examine maternal investments in children using data from the American Time Use Survey (ATUS), the Consumer Expenditure Survey (CE), and the NLSY. First, the ATUS data show that unemployed mothers spend nearly twice as much time with their children compared to employed mothers (even though the size of the increase is small). This additional time is reflected not only in total parent-child interaction but also in specific activities, such as helping with educational activities. Second, the CE data show that unemployed mothers spend significantly less on key child-related categories, particularly education and formal childcare. This suggests a pattern of input substitution, where unemployed mothers compensate for reduced financial resources by increasing their direct time involvement with their children.

Supporting the input substitution hypothesis, we find that children exposed to maternal un-

[3]Our findings are robust to various alternative specifications and sensitivity checks. We test for potential non-linearities and heterogeneous effects by gender and family structure. We also implement a strategy that breaks non-employment into voluntary and involuntary components, with the latter including so-called "discouraged workers" or "hidden unemployment." To isolate the involuntary component, we instrument maternal non-employment using exposure to cyclical industries, exploiting the idea that job loss in these sectors is more likely driven by external aggregate shocks. The instrumented results yield a point estimate similar to our previous findings on unemployment, reinforcing the conclusion that a mother's inability to find work hurts children's prospects.

employment in their first five years do not perform worse on standardized tests by age 10. This lack of effect suggests that the additional parental time investment might be sufficient to offset the potential scarring effects of early childhood unemployment exposure.

To understand the mechanisms through which unemployment impacts older children, we look at the age when the children enter the labor force for their first full-time job and subsequent career trajectories. We document that exposure to maternal unemployment during ages 13-18 is associated with earlier labor force entry, suggesting that having an unemployed mother accelerates workforce entry at the expense of educational investment. Consistent with this interpretation, we find that maternal unemployment is associated with lower educational attainment. We also document that affected children sort into occupations with lower earnings risk, suggesting that exposure to maternal job loss shapes career choices. These findings indicate that the critical mechanisms operate during adolescence, when educational and career decisions are made, and highlight the importance of policies that help families maintain educational investments during periods of parental job loss.

Roadmap. Section 2 describes our empirical approach. Section 3 introduces our data sources and measurement strategy, detailing how we construct exposure to maternal unemployment and family permanent income. Section 4 presents our main findings, documenting the persistent negative effects of maternal unemployment on children's future wages and employment probabilities. Section 5 examines the underlying mechanisms, exploring unemployment exposure at different childhood stages. Section 6 summarizes our contributions and discusses broader implications. Additional empirical results and robustness analyses are provided in the appendix.

Related Literature

Our paper relates to two strands of work: one examining the consequences of job loss and unemployment, particularly their intergenerational effects, and another studying childhood determinants of long-run labor market outcomes, with a focus on the trade-off between parental income, time investments, and other determinants.

A large literature shows that displaced workers suffer persistent earnings losses (Jacobson, LaLonde, and Sullivan, 1993; Stevens, 1997; Couch and Placzek, 2010; Raposo, Portugal, and Carneiro, 2021), as well as declines in health and even increases in mortality (Schaller and Stevens, 2015; Cygan-Rehm, Kuehnle, and Oberfichtner, 2017; Sullivan and Von Wachter, 2009).[4] More recently, attention has turned to intergenerational consequences. Evidence here is mixed: Oreopoulos, Page, and Stevens (2008) find wage declines for displaced workers' children in Canada, especially at the lower end of the distribution; by contrast, Fradkin, Panier, and Tojerow (2019) find no wage effects in Belgium but an increase in early labor supply, Hilger (2016) finds no impact on earnings or college

[4]See Fallick (1996) and Kletzer (1998) for detailed literature reviews.

enrollment in the United States, and Bratberg, Nilsen, and Vaage (2008) find null results in Norway. Other studies document effects on non-labor outcomes such as birth weight, grade retention, and school performance (Coelli, 2011; Lindo, 2011; Rege, Telle, and Votruba, 2011; Stevens and Schaller, 2011). More recently, Britto et al. (2023) provide evidence from Brazil, showing that parental job loss reduces school enrollment, increases teenage work, crime, and early pregnancy, and shifts children to lower-quality schools.

We make two contributions to this discussion. First, we focus on maternal unemployment exposure, rather than discrete displacement events. Our specification makes it easier to distinguish the roles of income loss and non-monetary channels. Prior work has also explored mechanisms by examining subgroup heterogeneity and other outcomes, such as child health (Lindo, 2011; Carneiro et al., 2024), education and school performance (Coelli, 2011; Rege et al., 2011; Stevens and Schaller, 2011; Britto et al., 2023), and early labor market behavior (Fradkin et al., 2019). However, because job displacement simultaneously lowers household income and imposes other non-monetary costs, it remains difficult to separate these channels. We show that non-income channels have significant scarring effects.[5]

Second, we document how the impact of maternal unemployment varies across childhood stages. We are the first to show this in the U.S., finding that long-run effects are concentrated among teenagers. This result is consistent with Carneiro et al. (2024) for Norway, who show that family income is not the main driver of the effects. Related evidence comes from Uguccioni (2022) for Canada and Bingley, Cappellari, and Ovidi (2024) for Denmark, who likewise document that the timing of parental job loss matters for children's outcomes.

Our study also connects to research on the childhood determinants of long-run outcomes. This literature highlights the role of family environment (Caucutt and Lochner, 2020; Pedtke, 2025), insurance coverage (Goodman-Bacon, 2021), teacher quality (Chetty, Friedman, and Rockoff, 2014a), health conditions (Case, Fertig, and Paxson, 2005; Smith, 2009), and neighborhood effects (Chetty, Hendren, Kline, and Saez, 2014b; Chetty, Hendren, and Katz, 2016). Of particular relevance to our study is the focus on the trade-off between parental income and time. Some studies examine this trade-off using natural experiments, such as tax credit expansions (Agostinelli and Sorrenti, 2022; Bastian and Lochner, 2022).[6] Other studies estimate the trade-off between maternal employment and childcare decisions (Bernal, 2008; Baker, Gruber, and Milligan, 2019), some using maternity leave expansions (Dustmann and Schönberg, 2012; Carneiro, Løken, and Salvanes, 2015). More recently,

[5] Similar to Hilger (2016), we find evidence that part of the negative effect of maternal unemployment reflects selection on unobservables. Yet placebo regressions and bunching tests (Caetano et al., 2024a) show that selection alone cannot account for the results: exposure before birth or after age 18 has no effect, and children of mothers with zero unemployment do not differ from those with small positive exposure.

[6] Other studies also explore the effects of the EITC; for instance, Dahl and Lochner (2012, 2017) find positive effects on childhood test scores, while Bastian and Michelmore (2018) find positive effects on education and employment outcomes.

Caetano et al. (2024c) use bunching methods and find that increased maternal labor supply adversely affects children's early cognitive development.

We show that maternal unemployment does not generate sufficient compensatory benefits through increased parental time, implying that other non-income channels are important. However, we find null effects for young children, suggesting that additional time with mothers may offset some of the income loss at early ages. To investigate this, we use the American Time Use Survey (ATUS) to study how unemployed mothers allocate their time, with a focus on child-related activities. This contrasts with Krueger and Mueller (2012), who use ATUS and the Survey of Unemployed Workers in New Jersey to study time allocation among unemployed workers more broadly.

2 Conceptual Framework and Empirical Strategy

In this section, we motivate and describe our empirical strategy used to document the association between mothers' unemployment and their children's long-term outcomes. We also discuss potential issues with our strategy. For the ease of interpretation, we use linear models for our main analysis. In Appendix B.2 we examine whether the effects of maternal non-employment are in fact non-linear.

2.1 Conceptual Framework

There are three main channels through which maternal unemployment can shape children's long-run labor market outcomes. First, unemployment reduces household income, thereby limiting the market inputs available for investments in children's human capital. Second, it increases mothers' time at home, which could partly offset the loss of financial resources if that time is allocated to activities that build children's human capital. Third, maternal unemployment may operate through other non-income channels, such as stress and depression (experienced by either mothers or children), other health-related concerns, family instability, or social stigma.

The importance of these channels is likely to vary with the timing of exposure. In early childhood, parental time and market inputs may function as substitutes (or complements), so that the income loss from unemployment may be less (or more) damaging. By contrast, during adolescence, when schooling and career decisions are made, non-income channels such as stress, health concerns, and family instability likely dominate, generating long-lasting effects.

Most existing work has studied parental job displacement events, often exploiting variation from plant closures. That approach identifies the full impact of job loss but makes it difficult to separate the income channel from other mechanisms. Our approach differs: rather than focusing on a discrete event, we construct a measure of total exposure to maternal unemployment during childhood. This measure captures the cumulative intensity of maternal unemployment throughout childhood and is used to understand children's labor market outcomes when they become adults.

Our approach of measuring cumulative exposure allows us to test whether income or non-income channels dominate at different stages of childhood, and to identify at what ages maternal unemployment is particularly detrimental. The trade-off is that this strategy inevitably raises endogeneity concerns. We return to these challenges in the next subsection, where we present our empirical specification and discuss potential threats in detail.

2.2 Estimating the Impact of Maternal Unemployment on Children's Labor Market Outcomes

We estimate the effect of exposure to maternal unemployment by projecting labor market outcomes of young adult i at time t, y_{it}, on measures of maternal unemployment and out-of-the-labor force exposure, UNEMP_i and OLF_i:

$$y_{it} = \alpha + \beta_1 \mathit{UNEMP}_i + \beta_2 \mathit{OLF}_i + \beta_3 \mathit{PI}_i + \gamma_1 X_i + \gamma_2 Z_{it} + \epsilon_{it} \ . \tag{1}$$

y_{it} denotes a young adult's labor market outcome. UNEMP_i and OLF_i capture cumulative childhood exposure, while PI_i is family permanent income. X_i and Z_{it} include time-invariant and time-variant child characteristics, and ϵ_{it} is an error term. Section 3 provides detailed variable definitions.

One interpretation of β_1 is the effect of one additional week of maternal unemployment exposure, regardless of whether that week reflects part of a longer unemployment spell or an isolated short spell. In Appendix B.4, we decompose exposure into the number of unemployment spells and the average duration per spell. The results suggest that the frequency of spells is the more important driver of the negative association with children's future wages.

The timing of the variables is central. Exposure measures and permanent income are calculated before age 18, while outcomes are measured when the child reaches working age. For those with a high school degree or less, this would be older than 21 years, and for those with a college degree, it would be older than 25 years. Thus, equation (1) is best viewed as a forecasting exercise, where early-life conditions predict later-life outcomes, in the spirit of Carneiro and Heckman (2003), who showed that family permanent income is a strong predictor of children's future educational attainment.

Equation (1) is estimated by OLS. Our identifying assumption is that, conditional on controls, maternal unemployment is driven by exogenous shocks. This assumption is plausible for involuntary job loss but less credible for labor force non-participation, which often reflects active choices correlated with unobserved maternal characteristics. For this reason, our analysis focuses on unemployment exposure.[7] The identifying assumption may still be violated if: (i) maternal unemployment correlates with unobserved time-invariant maternal traits, (ii) unemployment results from contemporaneous family shocks, or (iii) permanent income is mismeasured and correlated with unemployment. We now discuss these challenges and our empirical strategies for addressing them.

2.3 Challenges: Selection on Time-Invariant Unobservables

Maternal unemployment may be correlated with unobserved maternal traits that also affect children's outcomes, such as non-cognitive skills, emotional stability, or parenting style. If so, our estimates could capture both the effect of unemployment and different maternal quality or traits.

We address this concern in two steps. First, we control for proxies of maternal quality, including educational attainment and standardized test scores. As an additional check, we use a rich set of birth and prenatal characteristics that have been shown to predict later outcomes, following the discussion in Dougan, García, and Polovnikov (2025). Second, we assess how well these proxies capture maternal traits by implementing bunching tests. The bunching tests indicate that our controls substantially reduce concerns about omitted maternal traits. Nevertheless, they might not fully capture unobserved characteristics, and the absence of a clear quasi-experiment remains a limitation of the study.

2.4 Challenges: Selection on Time-Varying Unobservables

Maternal unemployment during childhood may also be correlated with contemporaneous shocks that independently affect children's outcomes. Child-driven shocks (such as illness or behavioral problems) may cause mothers to leave work, while mother-driven shocks (such as health crises, divorce, or family violence) may generate both unemployment and worse child outcomes.

We partially address this concern by controlling for observable characteristics of the child's environment, including maternal age at childbirth, spousal labor supply, family structure, and the presence of fathers and grandparents, as well as community characteristics using restricted-use county identifiers. These controls help isolate the effect of unemployment from correlated family circumstances. After including these factors, the estimated impact of maternal unemployment on children's future wages remains negative and statistically significant. However, while unemployment is measured weekly, we lack precise timing for other family shocks (health crises, divorce, etc.), which prevent us from establishing the ordering of events.

[7] To make the coefficients of OLF more comparable with those of UNEMP, we report in Appendix B.9 an IV strategy using industry cyclicality to capture involuntary non-employment. The resulting point estimates are consistent with our main results.

2.5 Challenges: Measurement Error

Measurement error in family permanent income may bias our estimates. If unemployment is correlated with true permanent income and our measure of permanent income is noisy, then our results may partly reflect the effect of true permanent income that our measure does not capture. To address this concern, we construct an alternative measure of permanent income and use it as an instrument for our baseline measure. Under the assumption that measurement errors in the two measures are uncorrelated, this instrumental variables approach isolates the true effect of unemployment from bias due to income mismeasurement. We describe the data and the construction of both permanent income measures in the next section. In Appendix C.3, we show that an implausibly large permanent income coefficient would be required to fully eliminate the unemployment effect.

3 Data and Measurement

In this section, we describe the dataset and measurement choices used to study the association between maternal unemployment and children's long-term outcomes. The data provide detailed information on mothers' labor market status and follow their children into adulthood, allowing us to link maternal unemployment histories to their offspring's labor market outcomes. Our key measure is cumulative exposure to maternal unemployment during childhood.

3.1 Data: The NLSY79 and NLSY79-CYA

We use data from two National Longitudinal Surveys.

First, we use the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 consists of a nationally representative sample of over 12,000 individuals ages 14-22 in 1979. The survey follows individuals longitudinally and has information about their employment, education, family background, and other life circumstances. Importantly for our exercise, the NLSY79 keeps a detailed record of each respondent's employment, unemployment, and non-employment spells at a weekly frequency. It has records from January 1, 1978, to the date of the last interview. We use this labor force status array to construct our measure of a child's exposure to maternal unemployment, which will be described in the following subsection.

Second, we use data from the NLSY79 Child and Young Adult (NLSY79-CYA). The NLSY79-CYA has information on children born to women in the original NLSY79 cohort. These children were born between 1980 and 2016 and followed longitudinally from birth through adulthood. This dataset includes extensive information on these children, including cognitive, socioemotional, and physical development. It also includes information on their educational attainment, employment status, and earnings by following children into their adulthood. The richness of the data allows us to explore how children's exposure to maternal unemployment affects their development, well-being, transition to adulthood, and, especially, their long-term labor market outcomes.[8]

In our analysis, we focus on children of mothers from the original NLSY79 cohort who have reached working age. The final sample consists of respondents who were interviewed at least once as adults. We further restrict the sample to young adults who are at least 21 years old with a high school diploma or less, or at least 25 years old with a college degree or higher. These restrictions yield a sample of 6,861 individuals born between 1980 and 1997, observed in the labor market between 2005 and 2016, and with an age range of 21–35. On average, respondents are observed 3.3 times, yielding a total of 22,920 respondent–year observations. Among the employed, 4,276 respondents provide non-missing wage information, with an average of 2.03 observations per person, resulting in 8,682 wage observations. Each respondent is linked to their mother. Appendix A reports summary statistics and further details on sample construction.[9]

We study a range of adult outcomes: log earnings, log wages, log hours worked, and indicators for educational attainment and labor force participation. The NLSY records all jobs ever held, so respondents may report multiple jobs in a given year. In our baseline specification, we select the main match, defined as the job with the most hours at the interview date, and pool observations across all survey waves, creating a panel where individuals contribute multiple person-year observations. This approach raises a potential concern: since less-educated individuals enter the labor force earlier, they contribute more observations to the sample, which could bias our estimates. To assess robustness, we implement four alternative sample selection methods: (1) using only the first observed main match, (2) using only the last observed main match, (3) including all jobs across all surveys, and (4) averaging job characteristics within each survey year. Table B1 shows that our results are robust to these alternatives.

Our main specification relates variables measured during childhood to outcomes in adulthood. Control variables observed repeatedly over the 18 years of childhood are aggregated using the mode for categorical measures (e.g., marital status) and the mean for continuous measures (e.g., spouse's workweek).

[8]We construct hourly wages for NLSY79-CYA respondents following the methodology used by the Bureau of Labor Statistics for the main NLSY79 cohort. While BLS provides constructed hourly wage variables for NLSY79 and NLSY97 respondents, this variable is not available for children in the NLSY79-CYA. We replicate the BLS procedure documented in the HRP:PROC code described in the NLSY79 Codebook Supplement, Addendum to Appendix 18: Work History Data (page 318).

[9]We use the NLSY instead of the Panel Study of Income Dynamics (PSID) for two main reasons. First, the NLSY provides a detailed weekly array of labor market statuses, the key feature we rely on to create our unemployment exposure measure. The PSID measures unemployment by asking respondents how many weeks they were unemployed the previous year. Second, the NLSY offers detailed data on children's backgrounds during their childhood and their labor market outcomes in adulthood. This second reason may be less critical since the PSID includes two supplements, the Child Development Supplement (CDS) and the Transition into Adulthood Supplement (TAS), which could, in principle, allow similar analysis.

3.2 Constructing a Measure of Exposure to Maternal Unemployment

We measure children's exposure to maternal unemployment as the fraction of their childhood that their mothers spent unemployed. This measure captures the intensity of mothers' unemployment, distinguishing between someone who lost a job once and immediately found a new one and someone who has been consistently unemployed. For example, a single, short spell means a small fraction of childhood spent with an unemployed mother, while multiple, long spells mean a larger fraction. Previously, the literature has mainly looked at the effects of parental job loss in event study settings. Our approach complements the previous literature by using a different measure that captures the intensity of unemployment and allows us to isolate the income-loss effect from other potential effects of maternal unemployment.

To compute the fraction of children's childhood that their mothers spent unemployed, we explore the detailed labor force status array recorded in the NLSY79. It contains weekly information on the respondent's labor market status, whether "employed," "unemployed," "out of the labor force," or "on active military service." In the case of employment, it also recodes some job characteristics. In each interview, respondents are asked to report their labor market status for every week since their last interview, which means that the weekly data covers the entire time the respondent participated in the survey, even including years when they were not interviewed, and provides a comprehensive work history for each respondent.

More formally, we define the exposure of child i to his/her mother's unemployment as:

$$\textit{Exposure to Unemployed}_i = \frac{\sum_{\{t|age_{i,t}\in[0,18]\}} \mathbb{1}\{\textit{labor status}_{mother(i),t} = \text{unemployed}\}}{\sum_{\{t|age_{i,t}\in[0,18]\}} \mathbb{1}\{\textit{week}_{mother(i),t} = \text{observed}\}}. \quad (2)$$

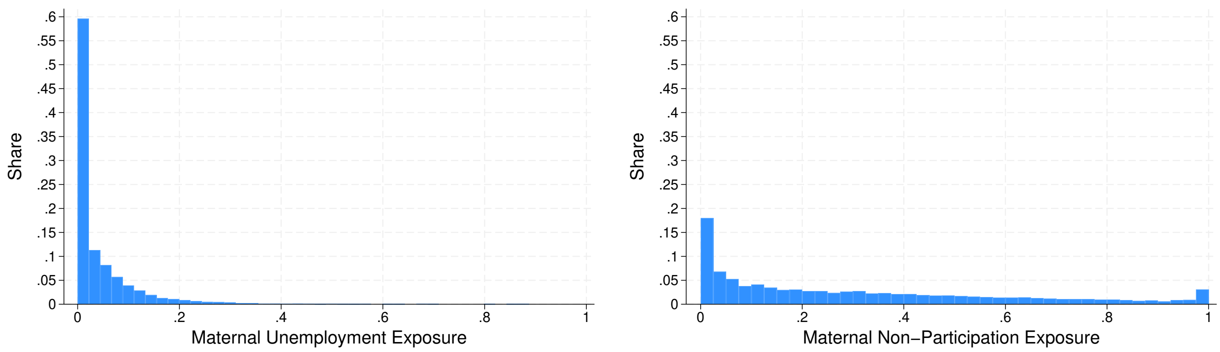
In the numerator, we count the number of weeks during which the mother reported being unemployed in the labor force status array. Here, $mother(i)$ is a function that denotes the identifier of child i's mother, and $\textit{labor status}_{mother(i),t}$ is a variable indicating her labor market status in week t. The summation is carried out over $\{t|age_{i,t} \in [0, 18]\}$, which represents all the weeks t in which the child is between 0 and 18 years old. In the denominator, we count the number of weeks during which the mother's labor force status is observed. $\textit{week}_{mother(i),t}$ is a variable that indicates whether the labor market status of the mother in week t is observed.[10]

[10] We implement our measure by first calculating maternal unemployment exposure for each year of a child's childhood. For instance, we count unemployed weeks and total observed weeks when the child was between the ages of 0 and 1, 1 and 2, and so on. Then, we compute the ratio between unemployed weeks and total observed weeks for each childhood year. We have a total of 206,436 child-year observations. We treat years with 50 or fewer observed weeks as missing, which accounts for only 0.46% of child–year observations (948 in total), all of which occur when children are ages 0–18 before 1978 or after 2018, reflecting the fact that labor force status data are only available starting in 1978 and our sample extends through the 2019 release. As the last step, we average these ratios over the 18 years of childhood to construct a measure representing the exposure to maternal unemployment.

Following the same logic, we construct other exposure measures capturing the fraction of a child's childhood in which his/her mother was out of the labor force or employed. The three constructed measures of exposure sum to one by definition. Blau and Grossberg (1992) also use the share of weeks that mothers worked as a measure of the quantity of maternal time available to invest in children. However, we focus on unemployment, which is plausibly exogenous, unlike out-of-the-labor-force status that reflects choice.

Figure 1 shows the distributions of exposures to mothers being unemployed and out of the labor force. There is much more variation in the out-of-the-labor-force exposure than in unemployment. During the first 18 years of their lives, young adults with at least one wage observation were, on average, exposed to 1 year of maternal unemployment, to 6.8 years of their mother being out of the labor force, and to 9.9 years of maternal employment. Standard deviations are 1.4 years for the exposure to unemployment, 5.3 years for the exposure to a mother being out of the labor force, and 5.6 years for the exposure to maternal employment. We later use the bunching of mothers with zero unemployment exposure, in the spirit of Caetano (2015) and Caetano et al. (2024a), to test for selection and argue that it is minimal after controls.

Figure 1: Fraction of Time in Each Labor Market State



Note: Maternal unemployment and non-participation exposures are defined as in equation (2) and can take values from 0 to 1. Each bar represents the share of young adults who were exposed to a particular level of maternal unemployment/non-participation during the first 18 years of their lives.

3.3 Measuring Family Permanent Income

Unemployment can affect families through three main channels: (i) reduced family income, (ii) increased parental time availability, and (iii) other non-income factors such as health concerns or family instability. We construct a measure of family income during childhood and use it to assess whether unemployment influences children beyond income reduction and to evaluate the relative importance of non-income channels.

More formally, we measure family income by constructing a discounted average of family per capita income over the child's childhood. This measure was previously used by Carneiro and Heckman (2003), from whom we borrow the name *family permanent income*. The child i's family permanent income is defined as:

$$PI_i = \sum_{\{t|age_{i,t}\in[0,18]\}} \frac{Y_{i,t}}{(1+r)^t} \cdot \frac{\frac{1}{1+r} - 1}{(\frac{1}{1+r}^{19} - 1)} \, . \tag{3}$$

$Y_{i,t}$ is a measure of the per-capita income of i's family at time t. We use a constant interest rate, r, to express income in terms of the year the child was born.[11] Lastly, we sum income for all the child's childhood years and divide it by the sum of the discount factors to compute average income.

Our measure of per capita income is net family income divided by the family size. The BLS created the total net family income variable by summing different income sources for all household members. These include, for example, labor income as wages and salary, farm and business income, asset income, and government transfers. We manually create a family income measure for respondents for whom this variable is unavailable. The BLS also created the variable family size by counting the number of people who live in the household and are related to the respondent by blood, marriage, or adoption.[12] All values are expressed in $10,000 in 1993 US Dollars, using the CPI. In our sample of young adults with non-missing wage observations, the average family permanent income is 0.865, with a standard deviation of 0.897.

Ideally, we would have the BLS's family income for each year of a child's childhood. However, this is not the case for two reasons. First, the NLSY began as an annual survey but transitioned to a bi-annual schedule after 1994. Therefore, by survey design, we do not observe family income for all the years of a child's childhood. Second, if a respondent misses a survey, he/she is not asked about income in the missed years. On average, we observe income for 10.17 years with a standard deviation of 4.03 years. We explain below how we correct for measurement error in our results.

3.4 Addressing selection on time-invariant unobservables

A concern with our exposure measure is that it may capture latent maternal characteristics rather than the effect of maternal unemployment. For example, mothers with weaker non-cognitive skills, lower emotional stability, or less effective parenting styles may both invest less in their children's human capital and experience higher unemployment.

[11] We calibrate $r = 0.05$, but our main results are robust to other values.

[12] When calculating family size, the BLS only counts family members related to the respondent by blood, marriage, or adoption. Foster relationships, partners, boarders, guardians, and others are not considered. Similarly, the income and earnings of spouses are included in the total family income, but those of partners are not. The reason for this choice, according to the BLS, is that inferring a financial relationship among individuals who are not legally related is more uncertain than inferring such a relationship among legal family members.

We address this concern using several strategies. First, we control for three proxies of maternal quality: mothers' educational attainment, their scores on the Armed Forces Qualification Test (AFQT), and unemployment outside the child's formative years. The first two proxies assume that more educated mothers and those with higher AFQT scores invest better in their children and face lower unemployment risks. The third proxy measures unemployment between ages 25 and 60, excluding the child's first 18 years. The logic goes: if maternal traits alone explained the association between children's outcomes and maternal unemployment, then unemployment at any stage of the mother's life should be predictive, not only unemployment measured during the child's formative years. Comparing results with and without these controls allows us to infer the role of unobserved maternal quality. As an additional check, we include a broad set of prenatal and birth characteristics shown to predict children's outcomes (Dougan et al., 2025).

Finally, we implement a bunching test following Caetano (2015) to assess whether selection bias remains after including maternal quality controls. The test exploits a composition issue created by truncation at zero: mothers cannot have negative unemployment exposure. This mechanical constraint means the zero-exposure bin disproportionately includes mothers with very strong labor-market attachment. This composition difference has testable implications. If unobserved maternal characteristics drive child outcomes, we should observe a discontinuity at zero: children of zero-exposure mothers would systematically outperform children whose mothers had minimal positive exposure, reflecting better maternal selection at exactly zero. By contrast, if unemployment itself causally harms children, outcomes should vary smoothly through zero, since 0.00 and 0.01 years of exposure should have similar effects on children, conditional on observables. Our evidence shows that once maternal quality proxies are included, children's wages vary smoothly at zero, with no detectable discontinuity. This suggests that our estimates are not substantially biased by selection on time-invariant unobservables.

3.5 Addressing selection on time-variant unobservables

A second concern with our exposure measure is that maternal unemployment may be correlated with contemporaneous shocks that directly affect children's outcomes. For example, child-driven shocks such as illness or behavioral problems may cause mothers to leave work, while mother-driven shocks such as health crises or divorce may simultaneously increase unemployment risk and reduce children's well-being.

We take two steps to address these concerns. First, we incorporate detailed information on the family environment during childhood. Specifically, we add controls for (i) a polynomial in maternal age at childbirth, (ii) spousal labor supply, (iii) family structure and location, including the number of children and urban/rural residence, and (iv) the fraction of years in which fathers and grandparents were present in the household. These controls capture variation in household resources and support networks that may co-vary with maternal unemployment. Second, we account for local labor market and community conditions using restricted-use county geocodes (NLSY Geocode data). Although less granular than neighborhood-level variation emphasized in influential work (Chetty et al., 2014b, 2016; Chetty, Dobbie, Goldman, Porter, and Yang, 2024), county-level measures of unemployment and income provide meaningful information about community context.

Including these additional controls does not attenuate our estimates: the negative association between maternal unemployment and children's adult wages remains statistically significant, and in some cases increases in magnitude. This suggests that the effects we document are not simply driven by differences in family composition or local labor market conditions.

3.6 Addressing Measurement Error

A final concern is measurement error in family permanent income.[13] To address this problem, we construct an alternative income measure using a different source of information in the NLSY. Specifically, we use the labor market status array to build a weekly earnings series, defined as usual hours worked times the wage at the primary job, aggregate this to annual earnings, and then construct a permanent income measure as in equation (3). This alternative measure has the advantage of covering the entire childhood period, including years when the mother was not interviewed.

However, this measure is not a perfect substitute for family permanent income: it only includes the mother's labor earnings and omits other important components such as spouse's income, asset income, and transfers. Directly controlling for it would therefore bias the interpretation of our regressions toward "maternal labor earnings" rather than "family permanent income." Instead, we use it as an instrument. By doing so, we exploit the correlation between the alternative and the original measure to correct for attenuation bias, while preserving the broader family income concept. To improve relevance, we interact the instrument with indicators of mothers' marital status and whether she is the household's primary earner. These dummies are also included as controls in the main regression, so that only their interactions enter as excluded instruments.

When dealing with measurement error, we estimate equation (1) using two-stage least squares with this instrument. The relevance condition is satisfied by construction, since both measures capture the same underlying concept of permanent income but with different sources of variation. The exogeneity condition requires that the measurement error in family income and unobserved

[13] Family permanent income is subject to measurement error for two reasons. First, survey responses on income are well known to be noisy. Second, we do not observe family income for each year of the child's childhood. Thus, the average family income is computed from only a subset of years. On the other hand, our measure of exposure to maternal unemployment is less likely to be subject to measurement error since it is constructed using the labor market status array. The BLS spends a significant amount of time constructing those and ensuring their quality.

maternal abilities are uncorrelated with the instrument; we mitigate the latter concern by including controls that proxy for maternal ability.

4 Main Results

We document the long-run impact of exposure to maternal unemployment on children's labor market outcomes. In particular, our results show that children who were more exposed to maternal unemployment have lower wages and a lower likelihood of being employed. These negative effects persist even after accounting for differences in mothers' characteristics, changes in family circumstances over time, and possible measurement error.

4.1 Effects on Labor Market Outcomes

First, we estimate equation (1) without including permanent income to document the full impact of maternal labor status on the labor market outcomes of their children. We look at the effects on total earnings, wages, workweek, and employment probability. In Appendix B, we also show results for a measure of occupation risk.[14]

In Table 1, Column 1, we document that children whose mothers spent more time unemployed or out of the labor force during their childhood had lower earnings in their adulthood. The estimated coefficient on the unemployment exposure measure is -0.66 (standard error 0.15) and on the labor-force non-participation exposure measure is -0.32 (standard error 0.04). For the magnitude of the effect, a child whose exposure to maternal unemployment was 1 standard deviation above the mean had, on average, 5% lower earnings. In the case of labor-force non-participation exposure, a 1 standard deviation above the mean is associated with 9% lower earnings.

In Columns 2 and 3, we show that the decline in earnings comes mainly from a decline in wages and not a decline in the workweek. The estimated coefficient of the unemployment exposure measure on wages is -0.50 (standard error 0.09), meaning that a 1 standard deviation higher exposure to maternal unemployment is associated with a 4% lower wage. By contrast, the association between unemployment exposure and hours worked is positive but statistically insignificant. We also find that higher exposure to maternal labor-force non-participation is associated with lower wages (-0.16, standard error 0.02) and fewer hours.

[14] In Appendix B, Tables B10 and B11 show that greater maternal unemployment exposure during childhood is associated with lower occupational earning risk in adulthood, consistent with individuals self-selecting into safer but lower-earning occupations. The effect is more pronounced for individuals over 30 years old, likely because older workers have had more time to transition into occupations that better match their risk preferences acquired from childhood experiences. Since job search and career changes take time, the impact of early-life factors on occupational sorting strengthens as workers age. This finding aligns with Hegarty (2022), who documents a similar mechanism using the Panel Study of Income Dynamics (PSID).

We focus on young adults who were employed during the survey week in Columns 1-3, while, in Column 4, we look at the sample of young adults who reported any employment status. Notice that the sample size is 2.5 times bigger in Column 4 than in Columns 1-3. While 70% of those in Column 4 are employed, only half reported wages and hours. We document that those who are more exposed to maternal non-employment are less likely to be employed. Notice that the effects of being out of the labor force and being unemployed are pretty similar in their effects on the employment probability. So, while the impact of the exposure measures on the intensive margin of labor supply is small, their impact on the extensive margin is large and significant.

Table 1: The Impact of Mother's Labor Market Status on Child's Outcomes

	(1) log(total earn.)	(2) log(wage)	(3) log(wkly hours)	(4) employed
OLF	-0.321**	-0.161**	-0.035**	-0.226**
	(0.038)	(0.021)	(0.009)	(0.015)
UNEMP	-0.664**	-0.500**	0.035	-0.340**
	(0.152)	(0.085)	(0.034)	(0.065)
Observations	8,683	8,683	8,683	22,920
R2	0.167	0.167	0.060	0.067

Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Controls include dummy variables for children's race and gender, a cubic polynomial in children's age, and fixed effects for the survey year. All models are estimated by ordinary least squares. Standard errors are clustered at the children's level and reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

4.2 Beyond Income: The Scarring Effects of Maternal Unemployment

Maternal unemployment can affect child outcomes by reducing family income and, consequently, parents' investment in the child's human capital. To separate the income channel from other non-income channels, we estimate equation (1) again, but now controlling for the household's permanent income. We restrict ourselves to analyzing the impact of exposure to maternal unemployment and labor market participation on children's wages and employment probability since the other labor market outcomes were insignificant in the previous table.

Maternal unemployment and labor-market participation have long-run negative impacts on children, even after controlling for family income. Table 2, Columns 2 and 4 show the impact on wages and employment probability, respectively. Even after controlling for family income, children exposed to more maternal unemployment or lower labor-market participation have lower wages and lower employment probability. The estimated impacts are almost unchanged from the baseline results, decreasing between 31% and 9% after controlling for family income. Columns 1 and 3 reproduce the results without controlling for family income for comparison.

It could be that maternal unemployment has a negative impact on children's outcomes, controlling for permanent income, because the family income is measured with error and unemployment is correlated with the latent true permanent income. We deal with it by estimating equation (1) by two-stage least squares and using an alternative measure of permanent income constructed using a different set of questions in the NLSY. We interact our instrument with dummies that capture whether the mother is the household's primary earner and whether she is married. These dummies are included in the main regression to control for their direct effect on children's outcomes, and only the interactions between these variables are excluded from the main regression. More details are provided in Subsection 3.6.

Table 2, Column 3 shows that the impact of permanent income on wages increases from 0.099 to 0.154 when corrected for measurement error. This coefficient implies that each $10,000 increase in average family income when children are growing up increases their future wages by 0.154 log points. Column 6 shows that the same is true for the impact on employment probability, with each $10,000 increase in family income increasing their employment probability by 4.7 percentage points. After controlling for permanent income and correcting for measurement error, the estimated effects of exposure to maternal unemployment and labor-market non-participation decrease relative to the OLS results but remain negative and significant. Contrasting the magnitude of the estimated coefficient of unemployment exposure and permanent income, one standard deviation more exposure to unemployment is similar to having a $1,794 lower average family income when growing up.

Carneiro and Heckman (2003), Caucutt and Lochner (2020), and others have shown that family income predicts children's educational attainment. They do it by regressing educational attainment dummies on average family income. In Table 3, we perform the same exercise as they did but also allow our exposure measures to enter the specification and instrument for measurement error in permanent income, as in Table 2. Our estimated coefficients on permanent income, which are similar to the ones reported by Caucutt and Lochner (2020), suggest that permanent income reduces the likelihood of a child dropping out of high school while increasing their chances of attending and graduating from college.

Interestingly, we find that, conditional on permanent income, higher exposure to maternal unemployment and labor-force participation increases the likelihood of a child dropping out of high school while decreasing their chances of attending and graduating from college. In monetary terms, one standard deviation more exposure to unemployment decreases the likelihood of graduating from college by 1.13%. This effect is similar to having a $3,750 lower average family income when growing up.

Since higher exposure to maternal unemployment and labor-force participation impacts edu-

Table 2: The Impact on Child's Outcomes After Controlling for Family Income

	OLS		IV	OLS		IV
	(1)	(2)	(3)	(4)	(5)	(6)
	log(wage)	log(wage)	log(wage)	emp.	emp.	emp.
OLF	-0.176**	-0.123**	-0.103**	-0.201**	-0.184**	-0.189**
	(0.024)	(0.024)	(0.026)	(0.017)	(0.017)	(0.018)
UNEMP	-0.393**	-0.329**	-0.305**	-0.303**	-0.275**	-0.284**
	(0.085)	(0.078)	(0.078)	(0.068)	(0.067)	(0.068)
Permanent Income		0.099**	0.136**		0.032**	0.022*
		(0.010)	(0.021)		(0.006)	(0.012)
F statistic			59.500			104.215
Observations	8,378	8,378	8,378	21,737	21,737	21,737
R2	0.175	0.196	0.193	0.065	0.067	0.066

Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Controls include dummy variables for children's race and gender, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, and fixed effects for the survey year. Columns 1, 2, 4, and 5 are estimated by ordinary least squares. Columns 3 and 6 are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

cational attainment, we investigate how much of documented scaring effects on wages and employment come through the education channel. For that, we estimate our equation (1), now controlling for education attainment dummies. Table 4, Columns 2 and 4 present the results, while Columns 1 and 3 reproduce the results only controlling for permanent income. After controlling for education attainment, the coefficients decrease between 22% and 49%, implying that the scarring effects of unemployment and labor-market participation go beyond their impact on education attainment.

4.3 Addressing Selection on Mothers' Unobserved Heterogeneity Using Proxies

We documented that the scarring effects of unemployment and non-participation go beyond their impact on family income and children's education. Potentially, this scarring effect can be explained by unobserved mothers' heterogeneity, such as in their ability to invest in their children's human capital. For example, if mothers who are bad at taking care and educating their children are also the ones who spend more time unemployed or out of the labor force, our exposure measures might be capturing the effect of latent mothers' characteristics and not the direct effect of the market status. In this subsection, we argue that latent characteristics are not the explanation.

Table 3: The Impact on Education Attainment

	(1) HS dropout (ages 21–24)	(2) Attended college (ages 24–27)	(3) Graduated college (ages 24–27)
OLF	0.124**	-0.165**	-0.057**
	(0.038)	(0.021)	(0.013)
UNEMP	0.340**	-0.395**	-0.145**
	(0.128)	(0.073)	(0.047)
Permanent Income	-0.075**	0.072**	0.037**
	(0.034)	(0.017)	(0.011)
F statistic	55.555	89.597	89.597
Observations	3,516	7,719	7,719
R2	0.306	0.257	0.368

Note: The dependent variables are indicator variables derived from the respondent's highest grade completed at the survey date. OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Controls include dummy variables for children's race and gender, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, and fixed effects for the survey year. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

In Table 5, we control for three proxies that capture mothers' abilities to invest in children's human capital: (1) mothers' education attainment, (2) cognitive test scores, and (3) time spent unemployed outside of the first 18 years of her child. Columns 2-4 gradually introduce these proxies of mothers' abilities as controls. For comparison, Column 1 reports the results without any proxy.

Looking across Columns 2 to 4, we can see that our proxies of ability explain only a small portion of the negative effect on wages associated with exposure to unemployment. For example, when controlling for all proxies in Column 4, we find that one standard deviation higher exposure is associated with -1.5% lower wages. Similarly, our proxies explain a small portion of the effect associated with exposure to lower labor-market participation. The decrease in the effect is sufficient to make the impact of exposure to labor-market participation statistically insignificant. Overall, a significant portion of the long-term negative impact of unemployment on children's prospects is still not accounted for. In the case of the negative effect of non-participation, a significant fraction can be explained by our proxies and other controls.

In Table 5, Column 5, we present the relation between the exposure to maternal unemployment and labor-force participation and a child's employment probability when controlling for our three proxies of mothers' abilities. Overall, the proxies and other controls explain only a small part of the

Table 4: The Impact After Controlling for Family Income and Education Attainment

	(1) log(wage)	(2) log(wage)	(3) emp.	(4) emp.
OLF	-0.100**	-0.054**	-0.187**	-0.145**
	(0.026)	(0.025)	(0.019)	(0.018)
UNEMP	-0.322**	-0.243**	-0.299**	-0.216**
	(0.081)	(0.073)	(0.068)	(0.067)
Permanent Income	0.137**	0.115**	0.023*	0.008
	(0.022)	(0.021)	(0.012)	(0.012)
High School		0.026*		0.076**
		(0.015)		(0.012)
Some College		0.174**		0.173**
		(0.017)		(0.012)
College or more		0.359**		0.248**
		(0.021)		(0.014)
F statistic	58.371	56.465	102.777	100.833
Observations	8,017	8,017	20,857	20,857
R2	0.187	0.261	0.065	0.091

Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Controls include dummy variables for children's race, gender, and educational attainment, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, and fixed effects for the survey year. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

effect of unemployment and labor-force participation.

Birth and Prenatal Characteristics We also analyze birth and prenatal characteristics and their relationship to labor market outcomes. Specifically, we examine the child's birth order, gestation length (in weeks), birth weight (in ounces), and the shortest birth spacing in cases where the child has siblings. The last control is of particular interest in light of evidence from Dougan et al. (2025), who show that the effectiveness of the Infant Health and Development Program (IHDP) depends on whether a child is a twin and, more broadly, on the spacing between births. Table B8 shows no evidence that these controls affect our estimates of the impact of maternal unemployment.

Table 5: Measures of Mother's Ability: Effects on Wages and Employment

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) emp.
OLF	-0.055**	-0.041	-0.036	-0.035	-0.132**
	(0.025)	(0.026)	(0.026)	(0.026)	(0.019)
UNEMP	-0.266**	-0.252**	-0.229**	-0.212**	-0.137*
	(0.073)	(0.073)	(0.073)	(0.078)	(0.070)
Permanent Income	0.115**	0.107**	0.102**	0.102**	0.008
	(0.022)	(0.024)	(0.024)	(0.024)	(0.014)
Educ Dummies	Yes	Yes	Yes	Yes	Yes
Mom Educ Dummies		Yes	Yes	Yes	Yes
Mom AFQT			Yes	Yes	Yes
Remaining Unemp				Yes	Yes
F statistic	58.868	51.283	49.798	49.977	77.825
Observations	7,710	7,710	7,710	7,710	20,102
R2	0.268	0.270	0.273	0.273	0.094

Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Column 1 only has the baseline controls, while other columns have additional controls. Column 2 includes mother's education attainment dummies. Column 3 additionally controls for mothers' Armed Forces Qualification Test (AFQT) scores. We include the test scores as quantiles of the score distribution. Column 4 also includes the mother's time spent unemployed between ages 25-60, excluding the first 18 years of the child's life. This control captures unobservable characteristics that cannot be captured by education achievements or cognitive test scores. Baseline controls include dummy variables for children's race, gender, and educational attainment, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, and fixed effects for the survey year. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

4.4 Testing Selection on Mothers' Unobserved Heterogeneity Using Placebo Tests

The critical assumption in our analysis is that maternal unemployment is driven by exogenous shocks rather than by time-invariant maternal characteristics. As an additional check, we conduct a placebo test by replacing our main explanatory variable–maternal unemployment during ages 0-18–with maternal unemployment outside that period (before the child's birth and after age 25). The logic is straightforward: if unemployment merely reflects persistent maternal traits (low ability, poor health, etc) that independently harm children, then unemployment at any point in the mother's career should predict worse child outcomes. Instead, we find that maternal unemployment outside the child's formative years is uncorrelated with their adult labor market outcomes. This supports our interpretation that the timing of exposure matters: unemployment harms children through disruptions during childhood rather than through selection on persistent maternal characteristics.

The results of the placebo test are presented in Table 6. As explanatory variables, we include unemployment exposure when children are already adults, between the ages of 25 and 30, and before they are born, 5 years before birth to 1 month before birth. Columns 1-3 show that maternal unemployment outside the child's formative years does not have any statistically significant correlation with the child's future income. This contrasts with our main result in Table 5, where maternal unemployment during the first 18 years of the child's life is negatively associated with the child's wage. Column 4 of Table 6 similarly shows that unemployment outside of the child's formative years is not correlated with the child's future employment probability, again contrasting with the result in Table 5.

Table 6: Placebo test

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) emp.
OLF when child is bw 25-30	-0.024		-0.005	-0.052**
	(0.018)		(0.023)	(0.017)
OLF when child is bw -5-0		-0.042	-0.044*	-0.006
		(0.026)	(0.027)	(0.021)
UNEMP when child is bw 25-30	-0.056		-0.042	-0.091
	(0.051)		(0.066)	(0.060)
UNEMP when child is bw -5-0		0.016	0.015	-0.033
		(0.072)	(0.073)	(0.053)
Permanent Income	0.120**	0.058**	0.058**	0.044**
	(0.023)	(0.024)	(0.023)	(0.016)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom Controls	Yes	Yes	Yes	Yes
F statistic	81.338	48.639	49.460	75.737
Observations	7,672	4,164	4,123	11,489
R2	0.269	0.291	0.291	0.102

Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Controls include dummy variables for children's race, gender, and educational attainment, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, mothers' ability controls, and fixed effects for the survey year. For mothers' ability, we use mothers' education attainment dummies and mothers' Armed Forces Qualification Test (AFQT) scores, which we include as quantiles. We do not include the mother's time spent unemployed between ages 25-60, excluding the first 18 years of the child's life. This variable is extremely correlated with our placebo measures. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

4.5 Testing Selection on Mothers' Unobserved Heterogeneity Using Bunching Tests

In the last two sections, we used proxies for mothers' abilities and placebo tests to argue that time-invariant maternal characteristics do not simultaneously affect mothers' likelihood of unemployment and child outcomes. Instead, we argue that exogenous factors drive maternal unemployment. As an additional test to empirically validate this exogeneity assumption, we employ a bunching test at the point of zero unemployment (Caetano, 2015; Caetano et al., 2024a). Details on the implementation are given in the figure note.

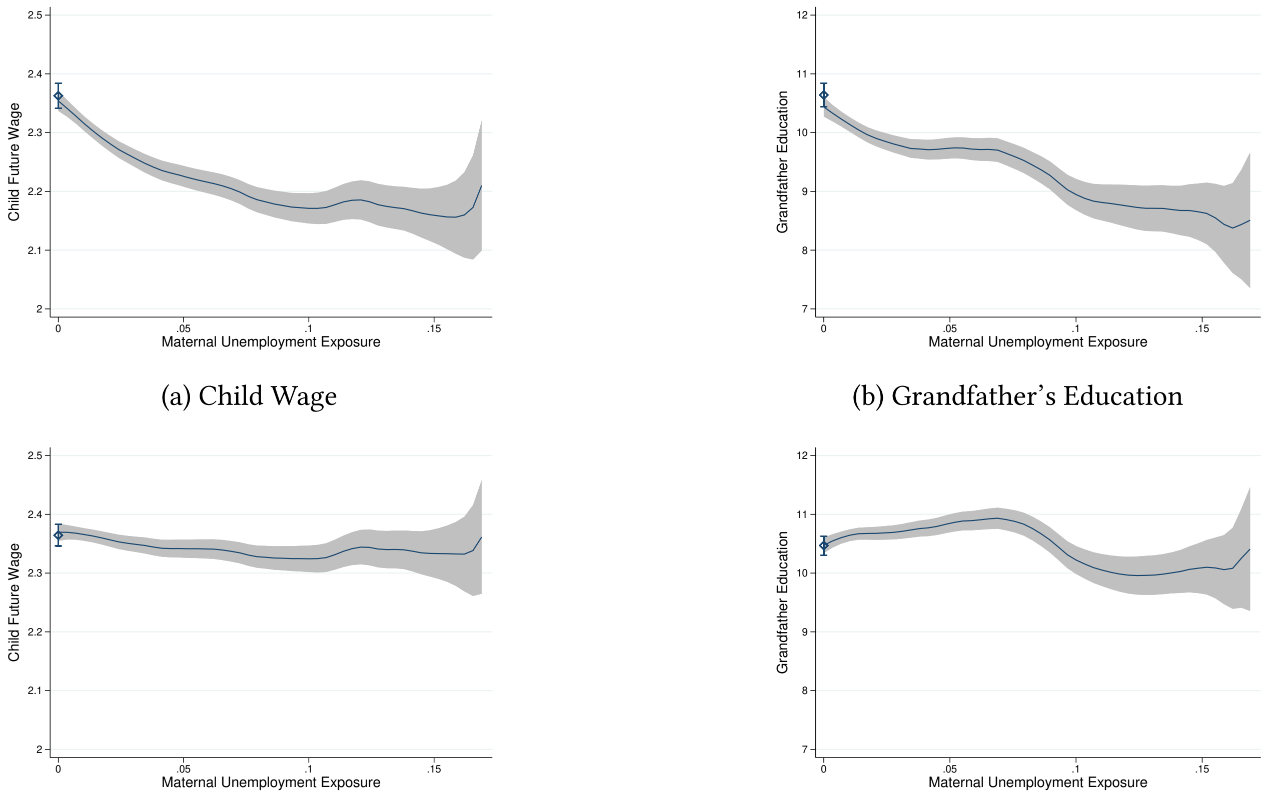
Unemployment exposure is truncated at zero: mothers cannot have negative exposure. This mechanical constraint creates a composition issue at the boundary. The zero-exposure bin disproportionately includes mothers with very strong labor-market attachment–mothers who, absent the truncation, would have had "negative unemployment." As a result, the zero-exposure group has a higher concentration of high-ability, stable-employment mothers compared to the group with small positive exposure.

This composition difference has testable implications for identification. If unobserved maternal characteristics (ability, labor-market attachment) drive child outcomes, we should observe a discontinuity at zero: children of zero-exposure mothers would, on average, outperform children whose mothers had minimal positive exposure, reflecting unobserved maternal characteristics. By contrast, if unemployment itself causally harms children, outcomes should vary smoothly through zero. Under a causal interpretation, a mother with zero exposure and a mother with 0.01 years of exposure should have children with similar outcomes, conditional on observables, since the difference in exposure is negligible.

We first implement this test graphically using two local linear regressions. First, we regress children's wages against exposure to maternal unemployment. Second, we regress maternal grandfather's education against exposure to maternal unemployment to detect spurious correlations induced by endogeneity. Grandfather's education is a placebo outcome that should not be affected by maternal unemployment. Figure 2, Figure 2, Panels (a) and (b), show that, without controls, both outcomes display discontinuities at zero unemployment, raising initial concerns about selection bias.

However, Figure 2, Panels (c) and (d), show that after including proxies for maternal ability and other controls, the graphical analysis reveals no discontinuities at the zero-unemployment threshold. Crucially, even after conditioning on these observables, maternal unemployment continues to negatively affect children's labor market outcomes across the full range of exposure. By contrast, the relationship between grandfather's education and maternal unemployment exposure changes sign: it is positive for exposures up to 1.8 years (21 months) and turns slightly negative thereafter. Taken together, this evidence alleviates concerns that the negative impact of maternal unemployment is driven by endogeneity.

Figure 2: Bunching Test: Outcome and Placebo Outcome



(a) Child Wage

(b) Grandfather's Education

(c) Residualized Child Wage

(d) Residualized Grandfather's Education

Note: This figure presents the bunching test results for both the primary outcome variable (Child Wage) and a placebo outcome (Grandfather's Education). Panels (a) and (b) show raw outcomes, while Panels (c) and (d) display residualized outcomes, controlling for relevant covariates. To compute residualized outcomes, we regress the relevant variable on maternal unemployment exposure and a full set of controls, including child race, sex, age, year-fixed effects, education dummies, and our maternal ability proxies. We add back the variation accounted for maternal unemployment exposure using the estimated coefficient. Lastly, we rescale the estimates of Panels (c) and (d) by adding the mean at zero unemployment computed in Panels (a) and (b). The local polynomial smoothing uses a bandwidth of 0.015. Bootstrapped standard errors are computed using 1,000 replications.

Ideally, the placebo outcome, the grandfather's education, would be unrelated to exposure to maternal unemployment after accounting for our controls. The fact that, in Figure 2, Panel (d), the grandfather's education is low for exposure to maternal unemployment higher than 1.8 years (21 months) raises the question of whether these tail events are driving the results. In Table 7, we reestimate our model restricting the upper part of the distribution. Column 1 shows the full sample, while Columns 2, 3, and 4 restrict the sample to observations below percentiles 95, 90, and 85, respectively. If something, the coefficient on the exposure measure of unemployment increases when restricting the sample, implying that tail events are not driving the results.

In Table 7, we also conduct a regression-based test for bunching to formalize our graphical results. This specification includes a dummy variable indicating zero unemployment exposure. If mothers at zero exposure differ systematically from those with small positive exposure, the zero-exposure dummy should have a significant coefficient. Instead, we find that the coefficient is statistically indistinguishable from zero, confirming the visual evidence of smoothness at the threshold. Following Caetano (2015); Caetano et al. (2024a), this test can be interpreted as a local balancing test: rather than testing whether unemployment correlates with observables on average (which could reflect selection anywhere in the distribution), we specifically test for selection at the margin between zero and small positive exposure. The null finding is consistent with maternal unemployment being exogenous, conditional on our controls. The result also holds when excluding tail events (Columns 2–4).

Table 7: Restricting Tail Events: Effects on Wages

	All Sample	UNEMP < p(95)	UNEMP < p(90)	UNEMP < p(85)
	(1)	(2)	(3)	(4)
	log(wage)	log(wage)	log(wage)	log(wage)
$1\{\text{UNEMP} = 0\}$	-0.005	-0.011	-0.013	-0.018
	(0.016)	(0.017)	(0.017)	(0.017)
OLF	-0.036	-0.028	-0.030	-0.028
	(0.026)	(0.026)	(0.027)	(0.027)
UNEMP	-0.220**	-0.394**	-0.376*	-0.636**
	(0.080)	(0.152)	(0.213)	(0.252)
Permanent Income	0.103**	0.103**	0.104**	0.103**
	(0.025)	(0.026)	(0.027)	(0.027)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Remaining Unemp	Yes	Yes	Yes	Yes
F statistic	46.297	47.182	45.784	43.485
Observations	7,713	7,328	6,938	6,557
R2	0.273	0.268	0.265	0.268

Note: See Table 5 for the definition of variables and mothers' ability control. All models are estimated by two-stage least squares. Excluded instruments used in all Columns are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

4.6 Addressing Selection on Changes in Family Circumstances

Maternal unemployment during childhood may coincide with changes in family circumstances that independently affect children's outcomes. For instance, shifts in family structure, the presence of extended family members, or local economic conditions could confound the relationship between maternal unemployment and children's later labor market outcomes.

Appendix B provides additional results. Table B3 adds controls for a range of family characteristics observed while the child was growing up. Specifically, we include (i) a polynomial in the mother's age at childbirth, (ii) spousal labor supply, (iii) measures of family structure and location, such as the number of children in the household and whether the family lived in an urban or rural area, and (iv) the fraction of years in which the father and grandparents were present. Extended family members, in particular, have been shown to influence children's educational attainment and test scores (Loury, 2006). After accounting for these factors, the estimated effect of maternal unemployment on adult wages becomes larger in magnitude and remains statistically significant.

We also address the role of local environments. A large literature documents the importance of neighborhoods in shaping children's long-term outcomes (Chetty et al., 2014b, 2016, 2024). To ensure our findings are not simply driven by geographic location, we use restricted-use county geocode data to control for local labor market and community conditions. While county-level data are less granular than the neighborhood-level variation emphasized in prior work, they still provide meaningful information on local unemployment and income. Table B15 shows that county characteristics predict children's future wages but cannot account for the negative impact of maternal unemployment exposure. This suggests that the mechanisms we document go beyond family composition and local labor market conditions.

4.7 Effects Across Labor Market Outcomes

We documented that maternal unemployment reduces children's labor market outcomes measured in adulthood and that these scaring effects extend beyond income loss and are not caused by endogeneity concerns. The scaring effects of maternal unemployment persist after controlling for proxies of mothers' ability and correcting for measurement error. They also pass placebo and bunching tests. However, most of our analysis only focuses on wages and employment probability. In this last subsection, we analyze other labor market outcomes: (1) total earnings, (2) wages, (3) weekly hours, and (4) employment probability. Table 8 presents the results of Table 5, Column 4 to these additional outcomes.

Column 1 shows that total earnings are negatively associated with non-participation exposure (significant) but exhibit no significant relationship with unemployment exposure. This lack of significance appears to result from opposing effects: Column 2 shows that wages decline with unemployment exposure (significant negative effect), while Column 3 shows that the workweek increases (significant positive effect). These effects offset each other, leading to a null impact on total earnings. Permanent income has a positive and significant effect on both total earnings and wages but does not significantly influence the workweek. Column 4 examines employment probability, finding no relationship between non-participation exposure and a significant negative association with unemployment exposure.

Table 8: All Labor Market Outcomes

	(1) log(total earn.)	(2) log(wage)	(3) log(hours)	(4) employed
OLF	-0.152**	-0.035	-0.031**	-0.132**
	(0.046)	(0.026)	(0.012)	(0.019)
UNEMP	-0.171	-0.213**	0.086**	-0.137*
	(0.150)	(0.078)	(0.037)	(0.070)
Permanent Income	0.086**	0.103**	-0.006	0.008
	(0.038)	(0.024)	(0.009)	(0.014)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Add. unemp	Yes	Yes	Yes	Yes
F statistic	50.051	50.051	50.051	77.825
Observations	7,713	7,713	7,713	20,102
R2	0.232	0.273	0.069	0.094

Note: See Table 5 for the definition of variables and mothers' ability control. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

4.8 Additional Robustness and Sensitivity Analyses

We conduct a series of robustness checks and heterogeneity analyses to assess the stability and interpretation of our main results.

Functional Form and Specification Checks. Table B2 tests for non-linear effects of unemployment exposure on wages and employment probabilities and finds no strong departures from linearity, a result confirmed by local regressions in Subsection 4.4. Table B4 decomposes exposure into the number and length of unemployment spells, showing that the frequency of maternal unemployment is negatively related to children's future wages, while average duration is imprecisely estimated.

The panel structure of the NLSY allows us to exploit variation between siblings who experienced different amounts of maternal unemployment. Table B5 reports estimates from models with mother fixed effects. In all specifications, the coefficients on unemployment exposure, non-participation exposure, and permanent income are statistically insignificant and often have signs opposite to the baseline results. We interpret this as evidence of limited within-family variation: siblings typically share nearly identical conditions during childhood. For instance, two siblings spaced two years apart share 16 of the same 18 years of maternal unemployment exposure. The implausible negative coefficient on permanent income, which contradicts both theory and other empirical evidence linking income positively to children's outcomes, reinforces the view that the fixed-effects specification lacks sufficient identifying variation.

Lastly, we control for permanent income to isolate the portion of the unemployment effect not explained by the income channel. A key challenge is that the unemployment coefficient depends on the estimated coefficient on permanent income. To assess sensitivity, we run constrained regressions in which the coefficient on permanent income is fixed at different values within a pre-specified grid (Figure C3). The results show that, for the unemployment effect to disappear, the true coefficient on permanent income would need to be about 0.5 – five times larger than our OLS estimate and more than three times larger than our IV estimate. Such a value is implausibly large, implying that income alone cannot account for the observed effects. Instead, maternal unemployment appears to have an independent impact beyond its influence through income.

Heterogeneity Analyses. Table B6 shows that maternal non-participation reduces the employment probability of daughters but has little effect on sons, consistent with the transmission of gender roles (Galassi, Koll, and Mayr, 2021); no gender differences are found for wages. Table B7 shows that results do not differ substantially between children of single and married mothers. Figure C1 shows no heterogeneity by maternal education or AFQT score. Figure C2 shows that the negative effects of maternal unemployment persist as children age, rather than dissipating over time.

Distinguishing Between Voluntary and Involuntary Unemployment. Throughout most of the paper, we distinguish unemployment from non-participation. Unemployment is typically involuntary and driven by external shocks, whereas non-participation can reflect voluntary choices (e.g., caregiving, schooling) or involuntary circumstances (e.g., discouraged unemployment). The latter is likely to affect children in ways similar to unemployment. To separate these cases, we combine unemployment and non-participation into a single measure of non-employment and instrument it with children's exposure to maternal employment in cyclical industries. Mothers employed in more cyclical industries are more likely to experience involuntary job loss due to industry downturns, so we interpret the predicted component of non-employment from this instrument as the involuntary portion. The results in Table B9 are imprecise, likely due to weak instruments, and most coefficients are not statistically significant. Nonetheless, the point estimates align with our main results, suggesting that maternal involuntary non-employment also leaves lasting scars on children's future labor market outcomes.

5 Mechanisms and Channels

We now explore the mechanisms underlying the scarring effects of maternal unemployment. Our analysis proceeds in three steps. First, we examine heterogeneity by age of exposure, testing whether maternal unemployment affects children uniformly across childhood or whether effects are concentrated in specific developmental periods. Second, we investigate the channels through which maternal unemployment operates. We use the American Time Use Survey (ATUS) and Consumer Expenditure Survey (CE) to document how unemployed mothers reallocate time and expenditures. We also examine whether exposure to unemployment correlates with children's home environment and cognitive development using the NLSY-CYA assessment tests. Third, we test whether maternal unemployment disrupts adolescents' educational and career trajectories by examining the timing of labor market entry.

We find strong evidence of age-dependent effects: maternal unemployment has no detectable impact on young children but generates significant negative effects during adolescence. For young children, unemployed mothers partially substitute increased time for reduced expenditures on development activities, and we find no association between maternal unemployment and measures of home environment or cognitive development. By contrast, adolescents exposed to maternal unemployment enter the labor force significantly earlier, suggesting that unemployment during this critical period disrupts educational investments and accelerates workforce entry.

5.1 The Role of Timing: Exposure Across Childhood Stages

Our previous exercises documented the effect of total exposure to maternal non-employment during children's first 18 years. However, it could be that this effect varies across different stages of the child's development, especially since we know that the effect of family income varies across different development stages. For example, Caucutt and Lochner (2020) found stronger estimated effects of early income (relative to late income) on college attendance. They interpret their results through the lens of a structural model and conclude that early financial constraints are binding for some young parents. Also, based on other results, they further conclude that later financial constraints are also binding.

Therefore, our first step in uncovering the mechanisms behind the scarring effects of maternal unemployment is to examine how these effects vary across children's development stages. To do this, we estimate a modified version of Equation (1), allowing the measure of exposure to maternal unemployment and non-participation to differ across development stages:

$$y_{it} = \alpha + \sum_j \beta_{1,j} UNEMP_{i,j} + \sum_j \beta_{2,j} OLF_{1,j} + \sum_j \beta_{3,j} PI_{i,j} + \gamma_1 X_i + \gamma_2 Z_{it} + \epsilon_{it} \ . \tag{4}$$

We use 6-year bins for the development stages to smooth the results, grouping them as 0–5, 6–12, and 13–18. We also construct permanent income measures for the same age periods. We estimate this specification using both OLS and IV, and show that the results are similar.[15]

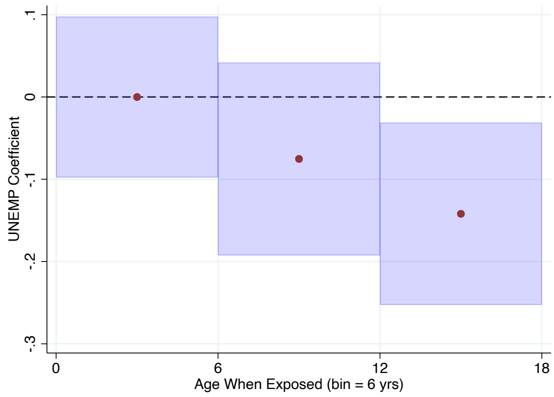
Figures 3 (a) and (c) visualize the results of the OLS regression, with permanent income split by age groups. We find that the impact of maternal unemployment is stronger when it occurs during adolescence (ages 13–18). Early childhood exposure (ages 0–6) shows minimal long-term wage effects, with estimates centered around zero and wide confidence intervals. Mid-childhood exposure (ages 6–12) has a negative point estimate, but it is insignificant. These results show that exposure to maternal unemployment during adolescence likely disrupts choices about when and how to enter the labor market and key human capital accumulation decisions.

Interestingly, exposure to the mother being out of the labor force has positive significant effects on future wages when the child is between 0 and 5 years old, and negative ones when the child is 13-18 years old. One interpretation is that labor force non-participation during early childhood often reflects deliberate maternal investment in child development, while non-participation during adolescence may reflect discouraged-worker dynamics similar to unemployment. However, given the endogeneity of the labor force participation decision, we caution against strong causal interpretations of these patterns.

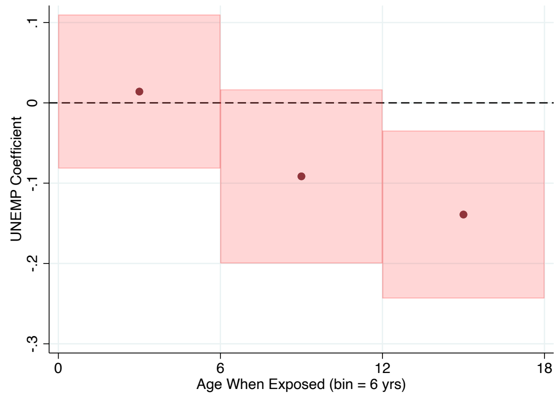
Figures 3 (b) and (c) present IV estimates where permanent income is instrumented to correct for measurement error. We use a single permanent income measure for ages 0-18 rather than age-specific measures due to weak first-stage identification with multiple instruments. The IV results closely match the OLS age pattern, confirming that measurement error does not meaningfully bias our finding that unemployment effects are concentrated during adolescence.

[15] We restrict analysis to ages 0-18 and exclude pre-birth unemployment. While pre-birth unemployment serves as a useful placebo test (Section 4.4), including it in the life-cycle specification reduces the sample from 7,672 to 4,164 observations (Table 6) because many mothers are not yet active in the labor force before childbirth. This sample restriction creates composition effects that confound interpretation of age-specific patterns.

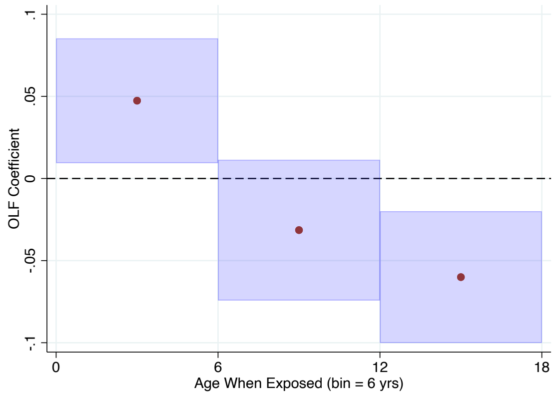
Figure 3: Impact of Maternal Employment Status on Wages by Childhood Stage



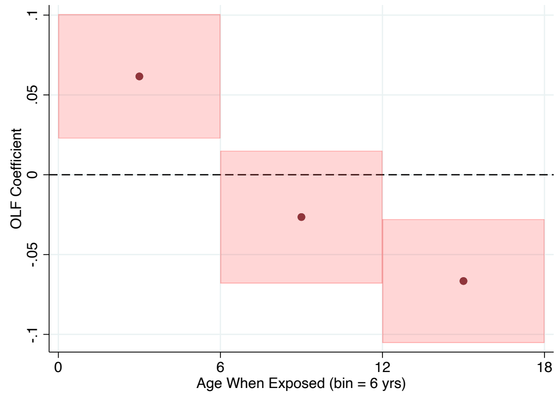
(a) Unemployment effects, OLS



(b) Unemployment effects, IV



(c) Out-of-the-labor-force effects, OLS



(d) Out-of-the-labor-force effects, IV

Note: This figure plots the estimated effects of maternal unemployment (panels a and b) and labor force non-participation (panels c and d) on children's log wages, by age of exposure. Panels (a) and (c) show OLS estimates; panels (b) and (d) show IV estimates where permanent income is instrumented to correct for measurement error. The OLS specifications include age-specific permanent income controls (0-5, 6-12, 13-18), while the IV specifications use a single permanent income measure for ages 0-18 due to weak first-stage identification with multiple instruments. All specifications control for child demographics (race, sex, age polynomials, education), maternal characteristics (marital status, primary earner status), and maternal quality proxies (AFQT, maternal education). The x-axis represents the child's age at exposure, grouped into 6-year bins. Point estimates are shown with 95% confidence intervals, with standard errors clustered at the individual level.

5.2 Early Childhood: Parental Time, Expenditure, and Human Capital Formation

Because we found no impact of early childhood exposure to maternal unemployment (ages 0–15) on children's long-term wages, we now examine potential mechanisms that could explain this result. In particular, we use the ATUS and CE to analyze how mothers invest in their children's human capital development. We find evidence that mothers decrease expenditures because of job loss but increase investment time, suggesting some input substitution during unemployment. To investigate this mechanism further, we examine other child outcomes during early childhood. Specifically, we assess whether maternal unemployment affects the home environment and cognitive development. Our findings indicate no significant impact, which may explain why early-life exposure to maternal unemployment does not translate into lower wages for children.

Amount of Time Investment by Mothers' Labor Market Status

Unemployment decreases income but increases the time available to spend with children. Because we found no impact of early childhood exposure to maternal unemployment on children's long-term wages, a hypothesis is that the increased available time compensates for lower family income. Therefore, we use the Annual American Time Use Survey (ATUS) to investigate how mothers' time allocation varies given their labor market statuses. The ATUS measures how individuals divide their time among various activities. We look at the time allocation of women between 25 and 50 who reported having children under 18 living in the household. We provide a detailed description of the ATUS data and our sample in Appendix A.

In Table 9, we compare the time allocation of these women when they are employed, unemployed, and out of the labor force. For that, we compute the mean time spent on different activities and reweight the observations to correct for differences in the numbers of children across women with different employment statuses. The reweighting factor is the inverse of each employment status conditional probability on the number of children. We use four bins to discretize the number of children.

The most significant difference between employed and unemployed mothers is the substantially less time the latter spends on work and work-related activities. An employed mother works, on average, 6 hours and a half, while an unemployed mother works only half an hour. Unemployed mothers distribute this extra time in several other activities. They increase the time allocated to personal care and household activities by 137 minutes, to leisure by 105 minutes, and, crucially, to activities related to children's education by only 8 extra minutes. This increase seems small in magnitude, but it represents an increase of 77% relative to employed mothers. So, unemployed mothers almost double the time allocated to activities related to children's education.

Mothers out of the labor force spend more time with their children than those unemployed. We speculate that this can reflect self-selection into not working. Mothers who choose to be out of the labor force are likely to be more willing to spend time with their children and to invest their time in children's education and well-being. This can explain why the negative effects of mothers' non-participation are much lower than those of mothers being unemployed, which tends to be an involuntary state.

In Tables B12, B13, and B14, we do the reweighting to correct for other observable differences among labor market status groups. In particular, we correct for (i) differences in family income, (ii) differences in education levels, and (iii) differences in the age of the youngest child and the number of children. The time allocated to children's education is always higher for unemployed mothers than employed mothers, ranging from 70% to 80%. Therefore, it does not matter how we adjust for the differences in observables; unemployed mothers significantly increase their time investing in children.

Amount of Money Investment by Mothers' Labor Market Status

Unemployed mothers almost double the time allocated to activities related to children's education, as shown in Table 9. Since they increase their home-based childcare, we now investigate whether these mothers spend less in categories associated with market-based investment in human capital. We use the Consumer Expenditure Survey (CE) to investigate the relationship between parental labor status and children-related expenditures. We provide a detailed description of the CE data and our sample in Appendix A.

The CE provides information on monthly household expenditures, family structure, and employment statuses of each household member. First, we aggregate expenditures into quarters. We also identify children-related expenditures based on the corresponding expenditure names in the CE. Examples are expenditures related to childcare, school and college tuition, apparel, and entertainment. Appendix A provides a complete description of these expenditure categories. Second, we record the employment status of both parents, where a parent can be either working or non-working. Non-working parents are further classified as unemployed and out of the labor force. A person is identified as unemployed if they (i) are not working and (ii-a) report inability to find a job as the reason for not working or (ii-b) have received unemployment benefits over the past 12 months. All other non-working parents are classified as out of the labor force.

We run OLS regressions where the dependent variables are a combination of all children-related expenditures. The main variables of interest are the labor market statuses of both parents. Controls include total expenditures, family size, the number of children younger than 16 years old, parental education, and time-fixed effects. Households with no children under the age of 16 are

Table 9: Average Number of Minutes per Mother' Status

	Emp	Unemp	OLF
Total Time Spent on All Activities	1440.00	1440.00	1440.00
Caring for and Helping Household Members	112.44	155.89	193.77
Caring for and Helping Children	90.83	132.75	169.39
Education-Related Activities for Children	10.74	19.00	21.10
Health-Related Activities for Children	3.17	4.77	4.79
Other Caring Activities for Children	76.92	108.97	143.50
Caring for and Helping Non-Household Members	6.40	14.47	11.92
Working and Work-Related Activities	384.05	35.84	5.25
Leisure and Social Activities	172.24	276.96	266.43
Purchasing Goods and Eating	103.09	125.69	130.45
Personal Care and Household Activities	634.48	771.85	778.23
Educational Activities	7.25	27.58	20.38
Other and Communication Activities	20.05	31.72	33.57

Note: The sample consists of women between 25 and 50 who reported having children under 18 living in the household. We look at the days of the week and non-holidays. We have 14,755 employed, 1,014 unemployed, and 5,135 out-of-the-labor-force respondents. Time allocation categories are the major BLS aggregate categories. Each cell represents averages. We use survey sample weights and construct additional weights to correct for differences in the number of children across employment status groups.

excluded from the sample.

Table 10, Column (1) shows that households with out-of-the-labor-force and unemployed mothers spend around 30% less on children-related expenditures relative to families with employed mothers. Similarly, households with out-of-the-labor-force and unemployed fathers also spend less on children-related expenditures, although the magnitude of the effect is weaker. Column (2) includes additional control for parental education, which reduces the negative effect of being non-employed. The estimated effects are stronger for out-of-the-labor-force parents than for unemployed parents, which could reflect unemployed parents spending time looking for jobs and not fully substituting market-based inputs or unobservable differences between out-of-the-labor-force and unemployed parents.

Columns (3) and (4) look at the share of expenditures dedicated specifically to childcare among households with children under the age of two. Share of childcare expenditures is, on average, 6-7 pt lower in households with non-working mothers relative to families where mothers are employed. The effect for out-of-the-labor-force mothers is stronger, consistent with the results in Columns (1) and (2). Moreover, the effect of paternal employment status is much weaker, indicating that fathers do not tend to substitute market-based childcare services. Importantly, these results are consistent with our evidence on the time of use, showing that there is a substitution of home-based and market-based inputs depending on the labor market statuses of the mothers.

Table 10: Child-related expenditures (CE)

	(1) log kids exp	(2) log kids exp	(3) childcare share	(4) childcare share
Mother OLF	-0.339**	-0.314**	-0.075**	-0.072**
	(0.012)	(0.011)	(0.003)	(0.003)
Mother Unemployed	-0.282**	-0.246**	-0.060**	-0.056**
	(0.056)	(0.056)	(0.013)	(0.012)
Father OLF	-0.166**	-0.148**	-0.014**	-0.015**
	(0.025)	(0.024)	(0.007)	(0.007)
Father Unemployed	-0.192**	-0.171**	-0.036**	-0.029**
	(0.063)	(0.062)	(0.015)	(0.015)
Constant	-0.658**	0.001	0.087**	0.175**
	(0.068)	(0.174)	(0.017)	(0.038)
Number of children	Yes	Yes	Yes	Yes
Family size	Yes	Yes	Yes	Yes
Total expenditures	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Parental education		Yes		Yes
Observations	61,424	61,424	13,636	13,636
R2	0.220	0.233	0.093	0.111
Engel curve			1.100	0.929

Note: Mother OLF/Unemployed and Father OLF/Unemployed represent dummy variables corresponding to labor statuses of each parent. Base categories are working mother and working father. Baseline controls include number of children younger than 16 years old, number of household members, total expenditures, year and quarter fixed effects. Columns (2) and (4) additionally account for parental education defined as the maximum of the maternal and paternal educational levels. Sample size in Columns (3) and (4) is smaller, since it includes only families with children under the age of 2. All models are estimated by ordinary least squares. Standard errors are reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

The Impact on Home Environment and School Performance

We further investigate our finding that exposure to maternal unemployment appears to have little impact when experienced in early childhood. In particular, we use two assessment tests available in the NLSY-CYA to document how maternal unemployment impacts children's development. First, we use the HOME (Home Observation for Measurement of the Environment) Inventory, which measures the quality of the child's home environment. Second, we use the Peabody Individual Achievement Test (PIAT), which measures children's cognitive achievements.

In Table 11, we investigate these relations by projecting the standardized test scores on maternal unemployment exposure, maternal labor-market non-participation exposure, and family permanent income. Test scores are measured when children are between 7 and 9 years old, while the other variables are measured when they are between 0 and 5 years old. We include maternal education attainment and AFQT quintile dummies to capture mothers' latent abilities and childcare skills, dummy variables for children's race and gender, a cubic polynomial in children's age, and survey-year fixed effect. We also correct for measurement error.

In Column 1, we document a negative but statistically insignificant relationship between exposure to maternal unemployment and the overall home environment. In Column 2, we find a negative relationship between maternal unemployment exposure and emotional support, though this estimate is not statistically significant. In Column 3, the relationship between maternal unemployment and cognitive stimulation is small and also statistically insignificant. In Columns 4 and 5, we document positive but statistically insignificant relationships between maternal unemployment exposure and both the average PIAT and Math PIAT scores. In sum, the results indicate that maternal unemployment does not have a statistically significant impact on the home environment or cognitive development measures.

5.3 Late Childhood: School-to-Work Transition

We found that maternal unemployment significantly impacts children when experienced during adolescence (ages 13–18). We now test a key mechanism: that unemployment during this critical period disrupts the timing of labor market entry. Adolescence coincides with important educational and career decisions, and premature workforce entry may generate persistent scarring through two channels: reduced educational attainment and inferior job matching. If financial pressure from maternal unemployment forces adolescents into the labor market earlier, they may sacrifice schooling quality and accept worse initial matches, resulting in the lower wages we observe in adulthood.

To measure labor market entry timing, we use NLSY-CYA work history records containing the calendar month and year each job began. We define labor market entry as the age at which the respondent first holds a full-time position (average weekly hours exceeding 35), restricting attention to jobs starting at age 14 or later to exclude casual youth employment. This definition captures meaningful workforce entry rather than temporary or part-time work during school.

Figure 4 presents OLS and IV estimates of the effect of maternal unemployment exposure on entry age. Exposure during adolescence (ages 13-18) is associated with earlier labor force entry.

Table 11: The Impact on Home Environment and Overall Development

	(1) HOME Total	(2) Emotional	(3) Cognitive	(4) Avg. PIAT	(5) Math PIAT
OLF 0-5	-0.054	-0.042	-0.028	0.000	-0.036
	(0.062)	(0.071)	(0.068)	(0.069)	(0.067)
UNEMP 0-5	-0.256	-0.327	-0.021	0.034	0.180
	(0.211)	(0.217)	(0.236)	(0.209)	(0.217)
Permanent Income 0-5	0.220**	0.079	0.274**	0.176**	0.090
	(0.056)	(0.060)	(0.060)	(0.067)	(0.063)
Mom Educ Dummies	Yes	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes	Yes
F statistic	36.693	36.914	35.709	40.474	40.474
Observations	5,845	5,577	5,712	5,640	5,640
R2	0.340	0.262	0.253	0.172	0.169

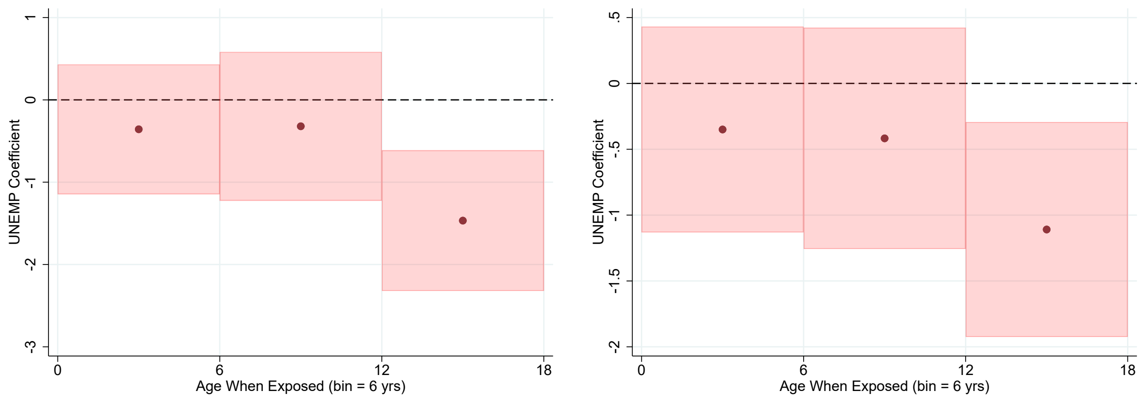
Note: OLF and UNEMP are our exposure measures to maternal labor-force non-participation and unemployment, respectively. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. OLF, UNEMP, and PI are measured when children are between 0 and 5 years old. Controls include maternal education attainment and AFQT quintile dummies, dummy variables for children's race and gender, a cubic polynomial in children's age, and survey-year-fixed effect. Dependent variables are total HOME score, emotional support HOME score, cognitive stimulation HOME score, average PIAT score, and PIAT MATH score. All are measured when children are between 7 and 9 years old. The average PIAT score is an average of the PIAT Math, PIAT Reading Recognition, and Reading Comprehension. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

By contrast, exposure during early childhood shows no significant relationship with entry timing. These results are consistent with Fradkin et al. (2019), who find that children of displaced workers in Belgium enter the labor market earlier and experience lower long-term earnings as a result.

Earlier labor market entry likely reflects interrupted educational investment. Recall that Table 3 documents that unemployment exposure increases high school dropout rates and reduces college attendance and graduation. Combined with our finding of earlier workforce entry during adolescence, this pattern suggests that maternal unemployment simultaneously pulls children into the labor market and pushes them out of school.

Beyond educational attainment and entry timing, exposure to maternal unemployment may also affect occupational choices. Children who experience maternal job loss during the formative years may become more risk-averse, leading them to select safer occupations with lower earnings volatility. We test this hypothesis by constructing measures of occupational earnings risk and returns following Bonin, Dohmen, Falk, Huffman, and Sunde (2007), Hartog and Vijverberg (2007), and Necker and Voskort (2014). Table B10 shows that greater unemployment exposure is associated with employment in occupations with significantly lower earnings risk. This relationship is stronger among workers over age 30 (Column 4), consistent with cumulative self-selection into risk-averse career paths over time. These findings align with Hegarty (2022), who document that parental layoffs lead children to work in lower-risk occupations using PSID data.

Figure 4: Labor Force Entry



(a) OLS results

(b) IV results

Note: This figure plots the estimated effects of maternal unemployment on children's age at labor market entry, by age of exposure. Panel (a) shows OLS estimates; panel (b) shows IV estimates where permanent income is instrumented to correct for measurement error. Labor market entry is defined as the age at which the respondent first holds a full-time position (average weekly hours exceeding 35 hours), restricting attention to jobs starting at age 14 or later. The OLS specification includes age-specific permanent income controls (0-5, 6-12, 13-18), while the IV specification uses a single permanent income measure for ages 0-18. Both specifications control for child demographics (race, sex, age polynomials, education), maternal characteristics (marital status, primary earner status), and maternal quality proxies (AFQT, maternal education). The x-axis represents the child's age at exposure, grouped into 6-year bins. Point estimates are shown with 95% confidence intervals, with standard errors clustered at the individual level.

6 Conclusion

In this paper, we revisit the question of whether maternal unemployment affects children's future labor market outcomes. Our approach allows us to disentangle the income and non-income effects of having an unemployed mother and to account for the intensive margin of the exposure to maternal unemployment. In particular, we construct a continuous measure of children's exposure to maternal unemployment using NLSY79 and NLSY79-CYA. We show that the amount of time a mother spends unemployed when her child is growing up is negatively associated with the child's employment probability and future wage. Moreover, we document that this negative relation remains even after controlling for family permanent income, suggesting that the negative effect of unemployment extends beyond the income decline.

We further provide evidence that our results are unlikely to be driven by measurement error or unobservable mother characteristics. On the contrary, our results suggest that mothers' inability to find a job directly affects children's labor market outcomes. We investigate potential mechanisms by examining how the impact of unemployment differs across childhood stages. We document that the impact is predominantly driven by the exposure to maternal unemployment in adolescence, and provide evidence that this effect operates through the earlier entry into the labor market and reduced educational attainment.

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A Data

A.1 NLSY Data Details

Our primary datasets are the National Longitudinal Survey of Youth 1979 (NLSY79) and the NLSY79 Child and Young Adult (NLSY79-CYA). The NLSY79 is a nationally representative sample of over 12,000 individuals aged 14-22 in 1979, providing a comprehensive longitudinal study of their lives and labor market experiences. The NLSY79-CYA contains information on children born to women in the original NLSY79 cohort, spanning from 1980 to 2018. The BLS has identified 11,551 children as having been born to the original 6,283 NLSY79 female respondents (as of 2018).

Table A1: Summary Statistics by Employment and Wage Availability

	Non-missing Emp Status	Non-missing Wage
Sample size	22,920	8,681
Unemployment Exposure	0.052	0.047
	(0.074)	(0.070)
Non-participation Exposure	0.355	0.332
	(0.295)	(0.281)
Permanent Income	0.798	0.857
	(0.810)	(0.848)
Age	28.30	28.10
	(4.63)	(4.27)
Log Wage	2.23	2.25
	(0.44)	(0.42)
Log Hours	3.72	3.72
	(0.20)	(0.19)

The Young Adult sample within the NLSY79-CYA is what enables us to study the long-term labor market impacts of maternal unemployment on children. The Young Adult sample refers to a questionnaire that has been administered to children aged 15 and older since 1994. The questionnaire was designed to facilitate life-cycle and cross-generational analyses. Of all identified children, 4,354 were interviewed as young adults. Details on the sizes and eligibility criteria vary by year. Those interested should look for details in the NLSY documentation.

Table A.1 has the descriptive statistics of our final samples. We include only young adults who have reached working age in our analysis. Column 1 shows the statistics for the respondents with non-missing values for employment status, child's gender, child's race, and child's age. Overall we have 22920 person-year non-missing employment observations. Column 2 shows the statistics for those who have non-missing values for wage, hours worked, total income, and child's gender, child's race, and child's age. There are 8682 person-year non0missing wage observations. The continuous variables display the mean and the standard deviation in parenthesis, while the categorical variables are described as fractions corresponding to each level.

UNEMP and OLF correspond to the exposures to maternal unemployment and non-participation in the labor force and are defined as in 2, taking values from 0 to 1. Permanent income is defined in 3 and is expressed in 10000s of 1993 US dollars. Age corresponds to the young adult's age at the time of the survey. Log wage refers to a logarithm of hourly wages, while log hours – to a logarithm of weekly hours. Education and Mother's Education indicate the highest attained education level for the young adults and their mothers respectively.

Notice, that Column 1 includes non-employed young adults, while Column 2 does not. This might explain why education levels are lower in Column 1. The rest of the variables have similar distributions across the two samples.

A.2 CES Data Details

The Current Employment Statistics (CES) data is a monthly survey conducted by the Bureau of Labor Statistics (BLS). It provides detailed information on employment, hours, and earnings of workers on nonfarm payrolls. Our exercise uses employment estimates for total nonfarm employment and by industry sector.

We use three-digit industries, but to describe the data in this appendix, we divide employment into 15 industries, as seen in Table A2. We use data spanning January 1972 to December 2019. Because, during these years, the US experienced significant changes in its sectorial composition, we show the total number of employees in each industry and their share of total employment in December 1972 and December 2019. The industries that gained employment share were financial activities, professional and business services, and private education and health services. Those that lost were construction, durable goods, and other services. We deliberately exclude the COVID-19 pandemic years from the sample.

We estimate the cyclical sensitivities of industries following the method of McLaughlin and Bils (2001). To account for the trend in sectorial composition, we use a cubic trend, which is flexible enough to capture it in these sectors. In the text, we mentioned that we estimate equation (5) in first-difference, but we actually use quarter changes, i.e., $E_{it} - E_{it-3}$. The reason is that we are interested in business cycle fluctuations, and we believe that monthly changes are too high-frequency. Ultimately, there is not much difference in the cyclical sensitivity β_i^e when using monthly or quarterly changes. Our estimated β_i^e's are displayed in Column 6.

A.3 ATUS Data Details

The American Time Use Survey (ATUS) is an annual survey conducted by the Bureau of Labor Statistics (BLS) to measure how the American population spends their time on different activities such as paid work, childcare, volunteering, and socializing. Participants are interviewed once and asked to describe their previous day's activities, which are then categorized.

The ATUS sample is derived from households that have conducted their eighth interview for the Current

Table A2: Cyclical Sensitivities

	dez/1972		dez/2019		
	Empl. (1,000)	Share	Empl. (1,000)	Share	β_i^e
All industries	**75,268**	**100.0%**	**151,666**	**100.0%**	
Mining and logging	677	0.9%	708	0.5%	0.05
Construction	3,937	5.2%	7,541	5.0%	2.09
Durable goods	11,036	14.7%	8,022	5.3%	1.35
Nondurable goods	7,122	9.5%	4,794	3.2%	-0.06
Wholesale trade	3,578	4.8%	5,895	3.9%	0.14
Retail trade	8,227	10.9%	15,509	10.2%	-0.05
Transportation and warehousing	2,678	3.6%	5,736	3.8%	0.43
Utilities	564	0.7%	548	0.4%	-0.95
Information	2,096	2.8%	2,885	1.9%	0.24
Financial activities	3,842	5.1%	8,814	5.8%	-0.29
Professional and business services	5,668	7.5%	21,443	14.1%	0.21
Private education and health services	4,975	6.6%	24,391	16.1%	-0.77
Leisure and hospitality	5,240	7.0%	16,761	11.1%	-0.17
Other services	1,944	2.6%	5,911	3.9%	-0.49
Government	13,684	18.2%	22,709	15.0%	-0.85

Note: See text for details.

Population Survey (CPS). The ATUS survey selects roughly 25% of these households randomly to participate in the time-use survey. One individual aged 15 or above is chosen from each household to participate in the survey. Key demographic data collected in the CPS is transferred to the ATUS. This includes household membership, employment status, earnings, and other characteristics. We use data spanning 2003 and 2021 and focus on women between 25 and 50 who reported having children under 18 living in the household.

A.4 CE Data Details

Consumer Expenditure Surveys (CE) are conducted by the Census Bureau for BLS. For our analysis we use Interview Surveys of CE for 2000-2020, which collect information on monthly household expenditures, incomes, and household characteristics. Households are surveyed for five consecutive quarters and are then dropped from the sample. The survey collects detailed data on each household member, including income, employment status, education, gender, and relation to the respondent. We use this information to determine parental employment statuses and education levels.

The expenditures are recorded using Universal Classification Codes (UCC) and cover a wide range of personal consumption categories. We identify children-related expenditures based on whether the name of the expenditure contains one of the following children-related keywords: "infant", "nursery", "baby", "child", "boy", "girl", "school", "toys", "playground", "college", "tutoring". We then classify expenditures associated with babysitting, childcare, and daycare (UCC codes: 340210-340212, 670310) as childcare-related.

B Additional Tables

B.1 Robustness to Alternative Sample Selection Rules

In our baseline analysis, we use the main match, defined as the job in which respondents work the most hours at the survey interview date. This implies that respondents may appear multiple times in the sample if they participated in several survey waves as adults. To assess robustness, we implement four alternative sample selection rules: (1) selecting only the first observed match, so each respondent appears once; (2) selecting only the last observed match, again with each respondent appearing once; (3) including all jobs across all surveys, allowing respondents to contribute multiple observations even within the same year; and (4) averaging job characteristics (wages, hours) across all matches for each respondent.

Table B1 estimates equation (1) under these alternative selection rules. The results show that the estimated effects of maternal unemployment and labor force non-participation on children's wages are robust to how we select the sample. The coefficients remain similar in magnitude and significance across all specifications, confirming that the specific choice of match definition does not drive our baseline findings.

Table B1: Other Matches

	Main Match	First Match	Last Match	All Matches	Avg. of Matches
	(1)	(2)	(3)	(4)	(5)
	log(wage)	log(wage)	log(wage)	log(wage)	log(wage)
OLF	-0.121**	-0.099**	-0.095**	-0.114**	-0.097**
	(0.024)	(0.024)	(0.025)	(0.023)	(0.021)
UNEMP	-0.377**	-0.446**	-0.437**	-0.345**	-0.487**
	(0.076)	(0.079)	(0.085)	(0.074)	(0.076)
Permanent Income	0.105**	0.094**	0.111**	0.099**	0.108**
	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)
Observations	8,376	4,116	4,116	10,943	4,269
R2	0.193	0.187	0.188	0.198	0.183

Note: This table presents robustness checks using alternative sample selection rules. Column (1) uses the baseline main match specification. Column (2) uses only the first observed match per respondent. Column (3) uses only the last observed match. Column (4) includes all jobs across all surveys. Column (5) averages job characteristics across all matches per respondent. UNEMP and OLF measure exposure to maternal unemployment and labor force non-participation, respectively (see equation 2). Permanent Income is measured in $10,000s (discounted present value as of birth year; see equation 3). All specifications control for child demographics (race, gender, cubic in age) and survey year fixed effects. Estimates use ordinary least squares with standard errors clustered at the individual level (in parentheses). * and ** indicate statistically significant at the 10% and 5% levels.

B.2 Non-Linear Effects on Wages and Employment Probabilities

Table B2 tests for non-linearity in the relationship between maternal non-employment and children's labor market outcomes. Rather than assuming linear effects, we divide exposure into quartiles and estimate equation (1) with dummy variables for each quartile (omitting the first quartile as the reference group). Because the distributions of unemployment and labor force non-participation exposure differ substantially, the quartile cutpoints vary by measure. For unemployment exposure, the 25th, 50th, and 75th percentiles correspond to approximately 0.04, 0.42, and 1.25 years. For non-participation exposure, these percentiles correspond to approximately 1.75, 5.25, and 9.83 years.

The results reveal different patterns for the two measures. For maternal unemployment (Columns 1-2), the effects on children's wages are approximately linear: each quartile shows progressively larger negative effects, with all three upper quartiles significantly different from the reference group. For labor force non-participation (Columns 1-2), effects are concentrated in the top quartile—only mothers out of the labor force for more than 9.8 years show significant negative impacts on children's wages.

The pattern differs slightly for employment probability (Columns 3-4). Unemployment exposure below one year (first two quartiles) shows no significant effect on children's employment probability, with effects emerging only for longer exposures. In Subsection 4.5, we report local polynomial regressions that further support approximate linearity for unemployment effects on wages.

Table B2: Exploring Non-linearity in the Effects of Maternal Labor-Force Non-Participation

	OLS	IV	OLS	IV
	(1) log(wage)	(2) log(wage)	(3) emp.	(4) emp.
OLF p25-50	-0.017	0.018	-0.015	-0.007
	(0.017)	(0.018)	(0.012)	(0.012)
OLF p50-75	-0.042**	0.002	-0.074**	-0.064**
	(0.017)	(0.018)	(0.012)	(0.013)
OLF p75-100	-0.113**	-0.046**	-0.149**	-0.109**
	(0.018)	(0.020)	(0.012)	(0.014)
UNEMP p25-50	-0.040**	-0.007	-0.019	-0.016
	(0.017)	(0.017)	(0.012)	(0.012)
UNEMP p50-75	-0.105**	-0.061**	-0.006	-0.005
	(0.018)	(0.018)	(0.013)	(0.013)
UNEMP p75-100	-0.137**	-0.072**	-0.063**	-0.050**
	(0.019)	(0.019)	(0.013)	(0.014)
Permanent Income		0.157**		0.037**
		(0.022)		(0.013)
F statistic		58.428		105.206
Observations	9,203	9,203	22,920	21,737
R2	0.184	0.209	0.063	0.065

Note: This table presents non-linear specifications where unemployment (UNEMP) and labor force non-participation (OLF) exposure measures are replaced with quartile dummy variables. The omitted category is the first quartile (lowest exposure). Columns (1) and (3) show OLS estimates; Columns (2) and (4) show IV estimates where permanent income is instrumented using an alternative measure interacted with maternal characteristics. Dependent variables are log wages (Columns 1-2) and employment probability (Columns 3-4). Permanent Income is measured in $10,000s (discounted present value as of birth year; see equation 3). All specifications control for child demographics (race, gender, education, cubic in age), survey year fixed effects, and maternal characteristics (marital status, primary earner status). Standard errors are clustered at the individual level (in parentheses).
* and ** indicate statistically significant at the 10% and 5% levels.

B.3 Additional Controls

In our baseline analysis, we control for permanent income, child demographics, maternal characteristics, and three proxies for maternal ability (education, AFQT score, and unemployment outside ages 0-18). Table B3 assesses robustness to including additional controls capturing other aspects of children's environment during childhood.

Column 1 replicates our baseline IV specification from Table 5, Column 4. Column 2 adds a polynomial in the mother's age at childbirth. Column 3 controls for spousal labor supply (average weekly hours and weeks worked). Column 4 includes family structure and location variables (average number of children in the household, urban/rural residence). Column 5 adds the fraction of years that grandparents and fathers were present in the household. Column 6 includes all additional controls simultaneously.

The results show that our findings are robust—indeed, strengthened—by these additional controls. The estimated effect of maternal unemployment becomes more negative, increasing in magnitude from -0.21 (SE 0.10) to -0.32 (SE 0.11) when all controls are included, and remains highly significant. This pattern suggests that the additional controls capture positive correlates of both maternal unemployment and child wages (such as extended family support), so controlling for them reveals a larger negative causal effect. By contrast, the coefficient on maternal labor force non-participation remains small and statistically insignificant across all specifications, consistent with our interpretation that this measure captures heterogeneous behaviors that do not have consistent causal effects on children.

Table B3: Robustness: Additional Controls

	Baseline	w/ Mom's Age	Mom's Spouse	Family Envir.	HH Composition	All Controls
	(1)	(2)	(3)	(4)	(5)	(6)
	log(wage)	log(wage)	log(wage)	log(wage)	log(wage)	log(wage)
OLF	-0.035	-0.033	-0.026	-0.037	-0.034	-0.025
	(0.026)	(0.026)	(0.028)	(0.026)	(0.026)	(0.028)
UNEMP	-0.214**	-0.214**	-0.324**	-0.214**	-0.212**	-0.323**
	(0.078)	(0.078)	(0.088)	(0.078)	(0.078)	(0.087)
Permanent Income	0.102**	0.101**	0.108**	0.102**	0.103**	0.104**
	(0.024)	(0.025)	(0.025)	(0.026)	(0.024)	(0.027)
Educ Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes	Yes	Yes
Add. unemp	Yes	Yes	Yes	Yes	Yes	Yes
Mom Cubic in Age		Yes				Yes
Mom' Spouse Labor Supply			Yes			Yes
Family Environment				Yes		Yes
Household Composition					Yes	Yes
F statistic	49.962	49.199	48.614	44.030	49.383	40.467
Observations	7,707	7,707	6,905	7,695	7,707	6,893
R2	0.273	0.274	0.268	0.276	0.273	0.272

Note: This table presents robustness checks with additional control variables. The dependent variable is log wages. Column (1) replicates the baseline IV specification from Table 5, Column 4. Additional controls are: (2) cubic polynomial in mother's age at childbirth; (3) spouse's average weekly hours and weeks worked; (4) average number of children in household and urban/rural residence; (5) fraction of years with grandparents and father present; (6) all additional controls simultaneously. All specifications include baseline controls: permanent income, child demographics, maternal characteristics, and maternal ability proxies (education, AFQT, unemployment outside ages 0-18). All models estimated by two-stage least squares, instrumenting permanent income with an alternative measure interacted with maternal characteristics. Standard errors clustered at the individual level (in parentheses). The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

B.4 Number of Unemployment Spells

Our baseline analysis measures total exposure to maternal unemployment during childhood (ages 0-18). We now decompose this total exposure into two components: the number of unemployment spells and the average duration per spell. We define an unemployment spell as a transition from any labor market status (employment or out-of-labor-force) to unemployment. For each mother, we count the total number of such transitions during the child's first 18 years. Average duration per spell is calculated as total unemployment exposure divided by the number of spells (defined as zero for mothers with no unemployment spells). Note that by construction, total exposure equals the number of spells times average duration per spell.

Table B4 presents the results. Column 1 shows that a greater number of maternal unemployment spells is significantly associated with lower children's wages: each additional spell reduces wages by 0.7% (standard error 0.003). The number of out-of-labor-force spells shows no significant relationship. Column 2 estimates the effect of average duration per spell while controlling for permanent income and other baseline controls. The coefficient on average unemployment duration is negative (-0.359) but imprecisely estimated (standard error 0.218). Lastly, in Column 3, we includes both measures simultaneously. The coefficient on the number of unemployment spells remains negative and statistically significant, while the coefficient on average unemployment exposure remains negative but imprecisely estimated. This suggests that frequency rather than duration per spell may be the more important mechanism.

Table B4: Robustness: Number of Unemployment Spells

	(1) log(wage)	(2) log(wage)	(3) log(wage)
Number of OLF Spells	0.001		0.000
	(0.002)		(0.002)
Average OLF		-0.020	-0.061
		(0.050)	(0.053)
Number of UNEMP Spells	-0.007**		-0.007**
	(0.003)		(0.003)
Average UNEMP		-0.359	-0.254
		(0.305)	(0.301)
Permanent Income	0.112**	0.117**	0.103**
	(0.024)	(0.023)	(0.024)
Educ Dummies	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes
Remaining Unemp	Yes	Yes	Yes
F statistic	66.670	77.717	61.233
Observations	7,672	7,672	7,672
R2	0.273	0.270	0.274

Note: See Tables 5 for the definition of mothers' ability control and for the construction of other variables. See text for additional controls. All models are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

B.5 Fixed-Effect Results

Our baseline analysis exploits cross-sectional variation between children. The panel structure of the NLSY also allows us to examine within-family variation by comparing siblings exposed to different amounts of maternal unemployment. We estimate equation (1) including mother fixed effects, which control for all time-invariant maternal characteristics and identify effects solely from differences in exposure between siblings. Table B5 presents the results. Columns 1-2 show OLS estimates, and Columns 3-4 show IV estimates, all with mother fixed effects. None of the coefficients on unemployment exposure, labor force non-participation, or permanent income are statistically significant. Moreover, the coefficient signs differ from our baseline cross-sectional results.

This null finding reflects insufficient within-family variation rather than absence of effects. Siblings close in age share most of their childhood years and thus experience highly similar maternal employment patterns. For example, two siblings born two years apart share 16 of 18 childhood years, leaving minimal variation in maternal unemployment exposure between them. The lack of identifying variation is further evidenced by the insignificant (and sometimes negative) coefficient on permanent income in the fixed-effects specification, contradicting both economic theory and robust empirical evidence that family income positively affects children's outcomes. We conclude that mother fixed effects absorb too much relevant variation to provide informative estimates in our setting. Our baseline between-family comparisons, combined with the extensive controls for maternal ability and the bunching tests validating our identification strategy, provide more credible estimates of the causal effects of maternal unemployment.

Table B5: Fixed-Effect Results: Wages

	OLS		IV	
	(1)	(2)	(3)	(4)
	log(wage)	log(wage)	log(wage)	Emp.
OLF	0.055	0.045	0.023	-0.035
	(0.094)	(0.094)	(0.072)	(0.050)
UNEMP	0.260	0.297	0.340	-0.099
	(0.285)	(0.293)	(0.238)	(0.140)
Permanent Income	-0.029	-0.016	0.160	0.185**
	(0.046)	(0.047)	(0.136)	(0.077)
Educ Dummies		Yes	Yes	Yes
Mom Controls		Yes	Yes	Yes
F statistic			54.327	229.878
Observations	8,680	8,287	7,350	20,565
R2	0.678	0.691	0.156	0.022

Note: Column 1 includes dummy variables for children's race, a cubic polynomial in children's age, and fixed effects for the survey year. Column 2 includes, in addition, dummy variables for educational attainment, mothers' marital status, and primary earner status. These equations were estimated by ordinary least squares. Columns 3 and 4 include dummy variables for children's race, a cubic polynomial in children's age, fixed effects for the survey year, children's educational attainment dummies, and mothers' marital and primary earner status dummies. These equations were estimated by two-stage least squares. Excluded instruments used in all columns are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. All columns include mother-fixed effects. Standard errors are reported in parentheses. They are clustered at the children's level for Columns 1 to 2 but not for Columns 3 and 4. Clustering made the estimated covariance matrix not of full rank. The F statistic is the Cragg-Donald Wald F statistic, which assumes that the errors are independent and identically distributed. This might not be true in our context, and the results should be interpreted with caution.
* and ** indicate statistically significant at the 10% and 5% levels.

B.6 Effects by the Gender of the Child

Our baseline analysis assumes homogeneous effects across children of different genders. However, if maternal employment patterns influence children through role modeling or gender-specific socialization, effects may differ by child gender. To test for such heterogeneity, we re-estimate equation 1 including interactions between child gender and maternal unemployment and labor force non-participation exposure. Table B6 presents the results. Columns 1 and 3 replicate baseline estimates without interactions; Columns 2 and 4 include gender interaction terms. Column 2 shows no significant gender differences in the effects of maternal non-employment on children's wages–both sons and daughters experience similar wage penalties from maternal unemployment exposure.

However, Column 4 reveals gender-specific patterns for employment probability. For example, exposure to an out-of-the-labor-force mother significantly reduces the employment probability of daughters but not of sons. This may result from gender-norm transmission within the family. Daughters who grow up with non-working mothers are more likely to choose not to work. On the other hand, maternal unemployment reduces the probability of employment for sons but not for daughters. This might be because sons are more likely to be negatively affected by a stressful home environment. However, we caution that these are exploratory findings.

Table B6: Differences by Gender

	(1) log(wage)	(2) log(wage)	(3) emp.	(4) emp.
OLF	-0.035	-0.043	-0.132**	-0.057**
	(0.026)	(0.034)	(0.019)	(0.024)
UNEMP	-0.214**	-0.210*	-0.137*	-0.293**
	(0.078)	(0.111)	(0.070)	(0.096)
Permanent Income	0.102**	0.103**	0.008	0.008
	(0.024)	(0.024)	(0.014)	(0.014)
Female	-0.103**	-0.108**	-0.054**	-0.018
	(0.012)	(0.019)	(0.009)	(0.014)
Female # OLF		0.017		-0.152**
		(0.043)		(0.032)
Female # UNEMP		-0.006		0.291**
		(0.136)		(0.121)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Add. unemp	Yes	Yes	Yes	Yes
F statistic	49.962	50.161	77.825	78.168
Observations	7,707	7,707	20,102	20,102
R2	0.273	0.273	0.094	0.097

Note: See Tables 5 for the definition of mothers' ability control and for the construction of other variables. Controls include dummy variables for children's race, and educational attainment, a cubic polynomial in children's age, and fixed effects for the survey year, mothers' marital and primary earner statuses. All columns are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses.

* and ** indicate statistically significant at the 10% and 5% levels.

B.7 Effects by the Single Mom

We use household composition variables to measure the presence of a spouse in the household over time. Specifically, we count the number of survey waves in which the respondent's spouse was present and divide the sample into two groups: (i) respondents with spousal presence frequency below the median and (ii) respondents with spousal presence frequency above the median. We then estimate separate regressions for each group. Table B7 shows no substantial difference in the unemployment coefficient across groups, suggesting that having a spouse present does not significantly alter the long-term effects of maternal unemployment on wages.

Table B7: Differences by Single Mom

	No Spouse Present	Spouse Present
	(1)	(2)
	log(wage)	log(wage)
OLF	-0.159**	-0.111**
	(0.031)	(0.016)
UNEMP	-0.142*	-0.327**
	(0.084)	(0.076)
Permanent Income	0.030	0.003
	(0.027)	(0.011)
Educ Dummies	Yes	Yes
F statistic	163.124	.
Observations	11,019	9,529
R2	0.087	0.076

Note: See Tables 5 for the definition of mothers' ability control and for the construction of other variables. Controls include dummy variables for children's race and educational attainment, a cubic polynomial in children's age, fixed effects for the survey year, and dummies for primary earner status. We do not include marital status dummies. All columns are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

B.8 Birth and Prenatal Characteristics

We examine whether birth and prenatal characteristics – birth order, gestation length, birth weight, and birth spacing among siblings – affect our estimates of the impact of maternal unemployment on children's future wages. Including birth spacing is motivated by evidence from Dougan et al. (2025), who show that the effectiveness of the Infant Health and Development Program (IHDP) depends on whether a child is a twin and, more broadly, on the spacing between births. As shown in Table B8, controlling for these factors does not alter the estimated negative and statistically significant effect of maternal unemployment. Most birth-related controls are small and insignificant, except for existence of sibling, which shows a modest positive effect. These results indicate that early-life conditions do not confound our main finding.

Table B8: Birth and Prenatal Characteristics

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) log(wage)	(6) log(wage)
OLF	-0.035	-0.036	-0.040	-0.044*	-0.037	-0.044
	(0.026)	(0.026)	(0.027)	(0.027)	(0.026)	(0.027)
UNEMP	-0.213**	-0.214**	-0.221**	-0.231**	-0.216**	-0.227**
	(0.078)	(0.078)	(0.081)	(0.079)	(0.078)	(0.082)
Permanent Income	0.103**	0.108**	0.099**	0.100**	0.111**	0.109**
	(0.024)	(0.026)	(0.025)	(0.025)	(0.026)	(0.027)
Birth order of child		0.010				0.005
		(0.007)				(0.008)
Length of gestation (weeks)			0.004			0.003
			(0.003)			(0.003)
Birth weight (ounces)				0.000		0.000
				(0.000)		(0.000)
Has siblings					0.058**	0.050*
					(0.028)	(0.029)
Has siblings=1 × Birth space					0.002	0.001
					(0.002)	(0.002)
Educ Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes	Yes	Yes
Remaining Unemp	Yes	Yes	Yes	Yes	Yes	Yes
F statistic	50.051	44.996	46.226	46.400	41.934	35.840
Observations	7,713	7,713	7,214	7,409	7,713	7,068
R2	0.273	0.272	0.277	0.271	0.273	0.275

Note: See Tables 5 for the definition of mothers' ability control and for the construction of other variables. All columns are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

B.9 Distinguishing Between Voluntary and Involuntary Unemployment

Throughout most of the paper, we have examined two labor market statuses: unemployment and non-participation in the labor force. Unemployment is usually involuntary and caused by external factors beyond an individual's control; however, non-participation can be either voluntary or involuntary. For example, non-participation is voluntary if mothers choose not to work for personal reasons, such as caring for children or other family members. It can be involuntary if mothers are not employed and have stopped looking for work due to reasons beyond their control, such as inability to find suitable employment, lack of job opportunities, or disability.

Some of the mothers in our sample might have self-selected to not participate in the labor market, raising concerns about the interpretation of the coefficient on the non-participation measure. In this subsection, we identify the effect of exposure to involuntary labor-market non-participation on children's outcomes. We combine our unemployment and non-participation exposure measures into a single measure of non-employment exposure and instrument it with children's exposure to maternal employment in cyclical industries.

The idea is that mothers who work in more cyclical industries are more likely to experience involuntary job loss due to factors beyond their control. We interpret a mother's non-employment predicted by her industry's cyclical exposure as the involuntary portion of her non-employment. To construct instruments for this exercise, we follow these steps: (1) we calculate a measure of cyclical sensitivity β_i^e for each industry i, (2) using the NLSY labor history array, we associate each job with its industry's measure of cyclical sensitivity, (3) we multiply the cyclical sensitivity measure by a business cycle indicator for each month, and (4) we take the average cyclical sensitivity that each child was exposed to during childhood.

We use two measures of cyclical sensitivities of industries. First, we follow the method of McLaughlin and Bils (2001) and project the industry i employment E_{it} on aggregate employment E_t and a cubic trend,

$$\ln(E_{it}/E_t) = a_i + a_{1i}t + a_{2i}t^2 + a_{3i}t^3 + \beta_i^e \ln E_t + e_{it} \,. \tag{5}$$

Our measure of industry i's cyclical sensitivity is β_i^e. We estimate equation (5) for each industry i in the first difference. The employment variables come from the Current Employment Statistics (CES) data, a survey conducted monthly by the Bureau of Labor Statistics (BLS). We use three-digit industries in the estimation. Our time series spans from 1972 until 2019. We give a more detailed data description in the Appendix A.

Our second measure of industries' cyclical sensitivities is the durability of the goods associated with them. According to consumer theory, the durability of goods should predict the cyclical behavior of expenditures on these goods. This is because durable goods require substantial investment to increase their stock, with small increases in stock potentially leading to a proportionally larger increase in spending. Consequently, the demand for durable goods is highly sensitive to the economic cycle. This prediction is confirmed by Bils and Klenow (1998) and Bils, Klenow, and Malin (2013). We utilize durability measures for seventy goods, as constructed by Bils et al. (2013). These measures are derived from data sourced from the US National Income and Product Accounts (NIPA) and estimates provided by an insurance company.

We use two instruments for each cyclical measure. The first instrument is the job's cyclical sensitivity, where simply working in a more cyclical job is assumed to predict higher involuntary non-employment, regardless of business cycle conditions. The second instrument is the job's cyclical sensitivity interacted with a business cycle measure. For our baseline estimate, we use annual real GDP growth as our measure of the business cycle, where working in a cyclical job during periods of negative growth is assumed to predict involuntary non-employment.

In Table B9, Column 2 reports results using the job's cyclical sensitivity and its interaction with the GDP growth for predicting involuntary non-employment, while Column 3 uses the durability measure and its interaction with the GDP growth. Column 4 uses the job's cyclical sensitivity and the durability measure jointly. For comparison, Column 1 shows the results without instrumenting for involuntary non-employment. The coefficients from the instrumented regressions are significantly larger than the non-instrumented estimates. Moreover, the coefficients from the instrumented regressions are statistically significant when using the job's cyclical sensitivity as an instrument in Column 2. The specification that only uses the durability measure is not significant potentially because of large standard errors driven by a weak-IV problem. However, the point estimate is in line with the other columns.

The coefficient on non-employment exposure in Table B9, Column 4 is -0.241, similar to the coefficient of -0.207 on unemployment exposure in Table 5, Column 5. This stark similarity when accounting for involuntary non-participation implies that maternal involuntary non-employment also has scarring effects on children's future labor market outcomes. Specifically, under the assumption that all non-employment is involuntary, these results suggest that a child exposed to one standard deviation higher involuntary maternal non-employment is estimated to experience a 7.0% reduction in adult wages. Lastly, Table B9, Column 5 reports the effect of maternal non-employment on children's employment probability.

Table B9: Using Exposure to Cyclical Industries to Disentangle Voluntary and Involuntary OLF: Effects on Wages

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) emp.
OLF + UNEMP	-0.049*	-0.171*	-0.060	-0.110	-0.182**
	(0.026)	(0.101)	(0.099)	(0.092)	(0.083)
Permanent Income	0.103**	0.055	0.101**	0.081**	-0.010
	(0.025)	(0.045)	(0.044)	(0.041)	(0.033)
Inst. for Measurement Error	Yes	Yes	Yes	Yes	Yes
Cyclical-Job Measure		Yes		Yes	Yes
Durability Measure			Yes	Yes	Yes
F statistic	50.357	9.791	10.252	8.806	7.222
Observations	7,527	7,527	7,527	7,527	19,486
R2	0.270	0.270	0.271	0.272	0.089

Note: OLF + UNEMP are our exposure measures to maternal non-employment. See equation (2) for construction details. Permanent Income is in $10,000s (discounted present value) as of birth year. See equation (3) for construction details. Controls include dummy variables for children's race, gender, and educational attainment, a cubic polynomial in children's age, dummy variables for mothers' marital and primary earner statuses, mothers' ability controls, and fixed effects for the survey year. See Tables 5 and 5 for the definition of mothers' ability control. All models are estimated by two-stage least squares. Excluded instruments used in all Columns are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Columns 2, 3, and 4 use different measures of exposure to cyclical industries as additional instruments. See text for details. Standard errors are clustered at the children's level and reported in parentheses. The F statistic is the Kleibergen-Paap F statistic.
$^{+}$, *, and ** indicate statistically significant at the 15%, 10%, and 5% levels.

B.10 Other Labor Market Outcomes: Occupation Return and Risk

We documented that exposure to maternal unemployment and labor-market non-participation predicts children's lower earnings. It is plausible to imagine that a child who is exposed to maternal unemployment self-selects into safer occupations, presumably because the child might develop a risk aversion against earning risk. Hegarty (2022) finds evidence of the exact mechanism using the Panel Study of Income Dynamics (PSID). She construct a measure of lifetime earnings risk for 22 occupations and documents that parental layoffs are correlated with children earning less in their early careers and working in occupations with lower risk.

In this subsection, we document that children with more exposure to maternal unemployment work in low-risk occupations, but we do not find that these occupations have low returns. We measure occupational earning return and risk using Mincer equation regressions following Bonin et al. (2007), Hartog and Vijverberg (2007), and Necker and Voskort (2014). First, we stack the March Current Population Survey (CPS) data from 1998 to 2021. We use the Autor-Dorn crosswalk to harmonize occupation codes across years. Second, we regress the logarithm of wages on a cubic polynomial in potential experience, educational attainment dummies, sex and race dummies, year-fixed effects, and occupation-fixed effects. Our measure of occupation mean returns is the estimated coefficients on the occupation dummies. Our measure of occupation risk is the standard deviation of the residuals within each occupation. Our sample includes occupations with at least 100 individuals and normalizes the measures by their standard deviation. Therefore, the regression coefficient should be interpreted as the impact of working in an occupation with one standard deviation above the average return or risk.

Table B10 shows the results when we use 3-digit occupation codes to create our measures. Columns 1 and 3 show that occupation returns are unrelated to the unemployment exposure measure. Columns 3 and 4 show that more unemployment exposure is associated with lower occupational earning risk. As mentioned, this finding is consistent with Hegarty (2022) and the mechanism that exposure to unemployment or parental layoffs during childhood leads individuals to self-select into lower-earning but safer occupations. In Column 4, we focus on individuals over 30 years old. Since we interpret the previous result as related to risk aversion, we expect a stronger effect for older workers (i.e., even lower occupational risk). Job search and career transitions take time, so older workers had more opportunities to self-select into occupations consistent with their risk preferences. Consistent with this interpretation, we find a stronger effect for this subgroup.

Our baseline measure of occupational earning risk is constructed using 3-digit occupations. Table B11 uses 2-digit occupations codes. In Columns 1 and 3, we continue to find occupation returns that are unrelated to the unemployment exposure measure. In Columns 3 and 4, we observe that the impact of unemployment exposure is still negative but not significant. Moreover, the result is stronger for the older sample but, again, not significant. We conclude that having finer occupation codes is essential for the documented result.

Table B10: Other Labor Market Outcomes: Earning Risk

	All Sample	Above 30 Yr	All Sample	Above 30 Yr
	(1)	(2)	(3)	(4)
	3d-Occ Return	3d-Occ Return	3d-Occ Risk	3d-Occ Risk
OLF	-0.007	-0.035	0.049	0.028
	(0.015)	(0.025)	(0.070)	(0.111)
UNEMP	0.044	0.057	-0.520**	-0.960**
	(0.051)	(0.085)	(0.210)	(0.269)
Permanent Income	0.028**	0.014	0.009	-0.028
	(0.013)	(0.024)	(0.060)	(0.098)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Add. unemp	Yes	Yes	Yes	Yes
F statistic	49.403	18.169	48.430	18.066
Observations	7,517	2,191	7,501	2,194
R2	0.224	0.183	0.030	0.047

Note: See text for details how how earning risk is constructed. See Tables 5 for the definition of mothers' ability control and for the construction of other variables. All columns are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

Table B11: Other Labor Market Outcomes: Earning Risk

	All Sample	Above 30 Yr	All Sample	Above 30 Yr
	(1)	(2)	(3)	(4)
	2d-Occ Return	2d-Occ Return	2d-Occ Risk	2d-Occ Risk
OLF	-0.004	-0.035	0.070	0.034
	(0.013)	(0.024)	(0.066)	(0.109)
UNEMP	0.010	0.037	-0.322	-0.480
	(0.050)	(0.083)	(0.228)	(0.310)
Permanent Income	0.019	0.015	0.084	0.082
	(0.012)	(0.024)	(0.060)	(0.091)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Add. unemp	Yes	Yes	Yes	Yes
F statistic	50.051	18.731	50.048	18.731
Observations	7,713	2,248	7,711	2,248
R2	0.207	0.163	0.025	0.054

Note: See text for details how how earning risk is constructed. See Tables 5 for the definition of mothers' ability control and for the construction of other variables. All columns are estimated by two-stage least squares. Excluded instruments are the alternative permanent income measure and its interactions with dummy variables for mothers' marital and primary earner statuses. Standard errors are clustered at the children's level and reported in parentheses.
* and ** indicate statistically significant at the 10% and 5% levels.

B.11 Additional Results on Mothers' Time of Use

Table B12: Average Number of Minutes per Mother' Status and Family Income

	Emp	Unemp	OLF
Total Time Spent on All Activities	1440.00	1440.00	1440.00
Caring for and Helping Household Members	109.38	162.20	205.07
Caring for and Helping Children	88.16	138.46	179.15
Education-Related Activities for Children	10.28	17.50	23.12
Health-Related Activities for Children	3.07	5.80	4.98
Other Caring Activities for Children	74.80	115.15	151.05
Caring for and Helping Non-Household Members	6.55	14.21	11.11
Working and Work-Related Activities	385.55	38.41	4.90
Leisure and Social Activities	173.24	271.58	258.46
Purchasing Goods and Eating	102.14	135.82	132.13
Personal Care and Household Activities	635.74	761.39	774.72
Educational Activities	7.43	22.38	19.28
Other and Communication Activities	19.96	34.02	34.33

Note: The sample consists of women between 25 and 50 who reported having children under 18 living in the household. We look at the days of the week and non-holidays. Time allocation categories are the major BLS aggregate categories. Each cell represents averages. We use survey sample weights and construct additional weights to correct for differences in family income across employment status groups.

Table B13: Average Number of Minutes per Mother' Status and School Groups

	Emp	Unemp	OLF
Total Time Spent on All Activities	1440.00	1440.00	1440.00
Caring for and Helping Household Members	106.88	161.73	204.20
Caring for and Helping Children	85.93	138.58	178.52
Education-Related Activities for Children	10.24	18.08	22.77
Health-Related Activities for Children	2.97	5.75	5.17
Other Caring Activities for Children	72.72	114.74	150.58
Caring for and Helping Non-Household Members	6.76	14.17	11.37
Working and Work-Related Activities	384.04	37.10	4.93
Leisure and Social Activities	174.26	271.48	259.80
Purchasing Goods and Eating	101.81	132.47	131.46
Personal Care and Household Activities	639.88	761.37	774.19
Educational Activities	6.78	27.76	19.99
Other and Communication Activities	19.59	33.91	34.06

Note: The sample consists of women between 25 and 50 who reported having children under 18 living in the household. We look at the days of the week and non-holidays. Time allocation categories are the major BLS aggregate categories. Each cell represents averages. We use survey sample weights and construct additional weights to correct for differences in education levels across employment status groups.

Table B14: Average Number of Minutes per Mother' Status, Age of the Youngest Child, and the Number of Children

	Emp	Unemp	OLF
Total Time Spent on All Activities	1440.00	1440.00	1440.00
Caring for and Helping Household Members	113.85	153.17	183.77
Caring for and Helping Children	92.25	130.47	158.66
Education-Related Activities for Children	10.33	18.68	22.74
Health-Related Activities for Children	3.22	4.63	4.70
Other Caring Activities for Children	78.70	107.16	131.21
Caring for and Helping Non-Household Members	6.46	14.72	12.62
Working and Work-Related Activities	383.45	36.86	5.33
Leisure and Social Activities	171.74	277.99	271.00
Purchasing Goods and Eating	103.09	125.94	131.12
Personal Care and Household Activities	634.10	771.80	781.20
Educational Activities	7.24	28.13	21.12
Other and Communication Activities	20.08	31.40	33.84

Note: The sample consists of women between 25 and 50 who reported having children under 18 living in the household. We look at the days of the week and non-holidays. Time allocation categories are the major BLS aggregate categories. Each cell represents averages. We use survey sample weights and construct additional weights to correct for differences in the age of the youngest child and the number of children across employment status groups.

B.12 Local Labor Market and Mobility

Chetty et al. (2014b), Chetty et al. (2016), and Chetty et al. (2024) document the importance of neighborhood environments in shaping children's long-term outcomes. To account for this channel, we use restricted-use County Geocode data to ensure that our findings are not simply driven by geographic location. While our data is limited to the county level – less granular than the neighborhood-level variation emphasized in prior work – it still allows a meaningful examination of the local environment. These geocode files provide detailed locational information on survey respondents, including their county of residence at various points in time. This allows us to complement our analysis of maternal unemployment exposure with an investigation into how county-level conditions influence children's future labor market outcomes.

We construct three measures of the local environment during childhood: (1) Exposure to County Unemployment, defined as the average county-level unemployment rate during the child's formative years; (2) Exposure to County Income, measured using per capita personal income; and (3) Number of Counties Lived In, capturing the extent of residential mobility, which may reflect instability and have independent effects on child development. County unemployment data come from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS), which provide monthly estimates of labor force conditions at the county level. County income data are obtained from the Bureau of Economic Analysis' Regional Economic Accounts, which report annual measures of personal income and population.

Table B.12 shows that county-level unemployment and income are both predictive of children's future wages. However, they are insufficient to account for the negative impact of maternal unemployment exposure, suggesting that the mechanisms at play go beyond local labor market conditions. Columns 1 and 3 show the baseline results for comparison.

Table B15: Robustness: Controlling for Local Labor Market and Mobility

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)
OLF	-0.037	-0.056**	-0.035	-0.033
	(0.026)	(0.026)	(0.026)	(0.026)
UNEMP	-0.216**	-0.198**	-0.213**	-0.211**
	(0.078)	(0.077)	(0.078)	(0.078)
Permanent Income	0.099**	0.067**	0.103**	0.103**
	(0.024)	(0.026)	(0.024)	(0.024)
County Unemp		0.011**		
		(0.003)		
County Income		0.281**		
		(0.037)		
Number of moves				-0.004
				(0.006)
Educ Dummies	Yes	Yes	Yes	Yes
Mom Educ Dummies	Yes	Yes	Yes	Yes
Mom AFQT	Yes	Yes	Yes	Yes
Remaining Unemp	Yes	Yes	Yes	Yes
F statistic	49.423	44.010	50.051	50.100
Observations	7,652	7,652	7,713	7,713
R^2	0.273	0.289	0.273	0.273

Notes: OLF and UNEMP capture exposure to maternal non-participation and unemployment, respectively (see equation 2). Permanent Income is measured in $10,000s as the discounted value at birth year (see equation 3). County-level unemployment and income are included to account for local labor market conditions. All models control for children's demographics and education, maternal characteristics, and survey-year fixed effects. Standard errors clustered at the child level are shown in parentheses. The F statistic is the Kleibergen-Paap statistic.
* and ** indicate statistically significant at the 10% and 5% levels.

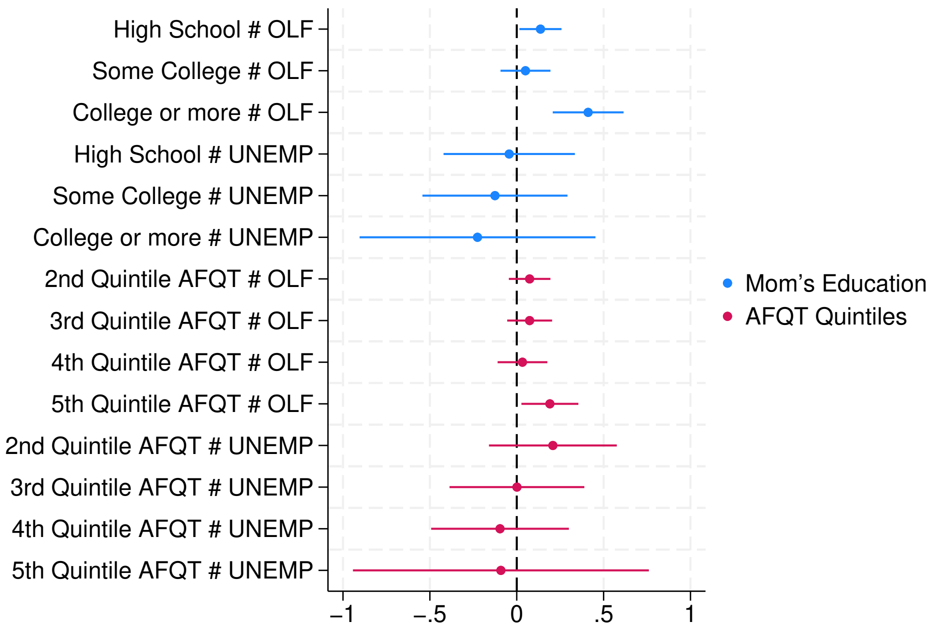
C Additional Figures

C.1 Heterogeneous Effect by Mother's Ability

We test for heterogeneity by interacting unemployment and labor force non-participation exposure with maternal education dummies and AFQT scores. Figure C1 plots the interaction coefficients.

The interaction terms are statistically insignificant, indicating no strong evidence of heterogeneity in the effects of maternal unemployment by maternal education or ability. However, point estimates suggest unemployment may be slightly more harmful for children of college-educated mothers, while labor force non-participation shows the opposite pattern—children of college-educated mothers who are out of the labor force have better outcomes than children of less-educated mothers who are out of the labor force. This asymmetry is consistent with differential selection: non-participation likely reflects deliberate childcare investment for college-educated mothers but discouraged-worker effects for less-educated mothers.

Figure C1: Heterogeneous Effect: Measures of Mother's Ability



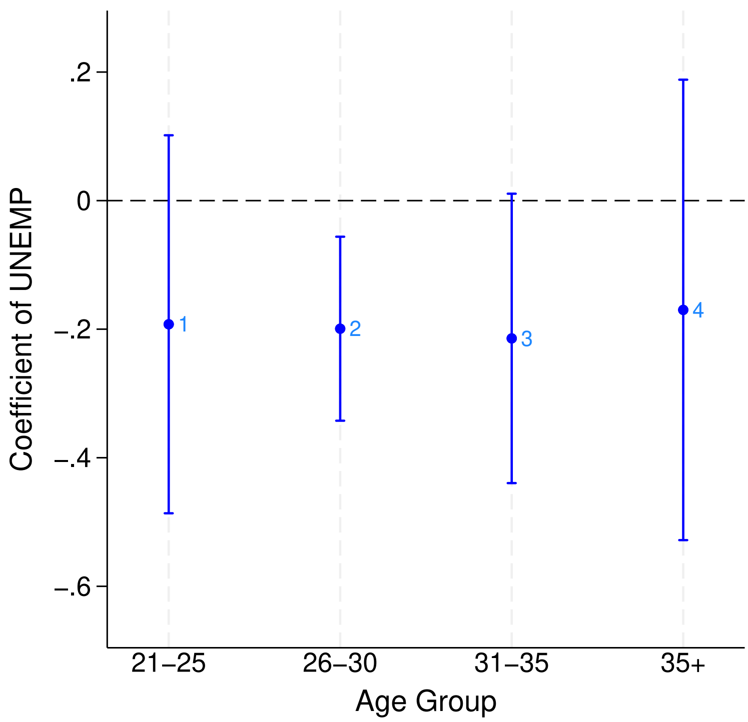
Note: This figure plots the coefficients from estimating equation 1 with children's wages as outcomes, augmented with the interaction terms between our measures of exposure to maternal non-employment (OLF and UNEMP) and measures of mother's cognitive abilities (education and AFQT quintiles). Each dot represents a point estimate of the effect of a variable displayed on the left-hand side on children's future wages, with the confidence interval plotted around. The magnitudes of the point estimates and the confidence intervals can be determined by looking at the X-axis.

C.2 Persistent Effect of Unemployment Over Age Groups

We also check how the impact of maternal unemployment varies across different age groups. The goal is to determine whether the effects of experiencing unemployment during childhood persist as individuals grow older or if they diminish over time. We estimate the effect of unemployment by dividing the sample into four age groups: (i) 21-25 years, (ii) 26-30 years, (iii) 31-35 years, and (iv) 35+ years. We estimate using our specification corrected for measurement error and with proxies for the mother's quality.

Figure C2 presents the estimated coefficients across the four age groups, with confidence intervals represented by vertical error bars. The estimated effect of unemployment is negative across all age groups and has a similar magnitude, with no clear upward or downward trend over time. The coefficient is insignificant in some specifications because of the reduced number of observations.

Figure C2: Persistent Effect of Unemployment Over Age Groups



Note: This figure plots the coefficients of exposure to maternal unemployment on children's wages. Each dot represents a point estimate of the effect of unemployment at a given age group on children's future wages, with confidence intervals displayed around the estimates. The magnitude of the point estimates and the width of the confidence intervals can be interpreted by referring to the Y-axis.

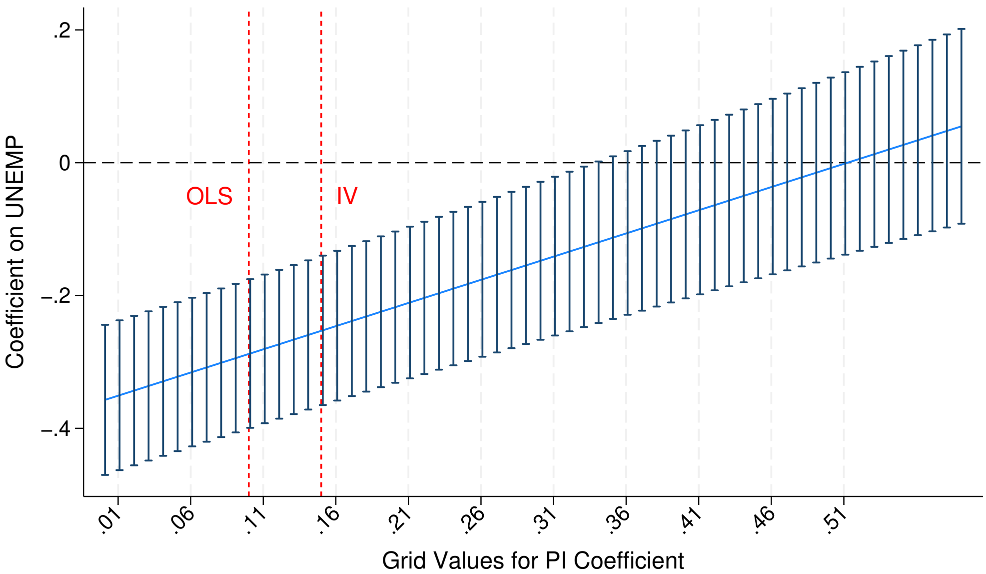
C.3 Assessing the Downstream Impact of Unemployment Beyond Income

Maternal unemployment impacts the outcome directly and through income. So, we control for a measure of permanent income to estimate the impact of unemployment not explained by the income channel. However, for that, it is crucial to estimate the coefficient on permanent income correctly. We deal with this concern first using instrumental variable estimation to correct for measurement error in permanent income. Additionally, we conduct a constrained regression where the coefficient on permanent income is fixed at different values within a pre-specified grid. This approach allows us to assess how the estimated impact of maternal unemployment changes as we impose different assumptions about the true value of the income coefficient.

Figure C3 shows that, if the true PI coefficient were large enough, the estimated impact of unemployment could approach zero—indicating that income fully accounts for any observed effects of unemployment. In particular, the graph plots the estimated coefficient on UNEMP (y-axis) as a function of different fixed values of the PI coefficient (x-axis). Two vertical lines represent the OLS and IV estimates of the PI coefficient.

For the estimated effect of unemployment to be fully accounted for by income (i.e., for the unemployment coefficient to reach zero), the true PI coefficient would need to be around 0.5. This value is five times larger than the OLS estimate and more than three times larger than the IV estimate. This suggests that income alone cannot fully explain the effects of unemployment unless one assumes an implausibly large coefficient on permanent income. In other words, unemployment likely has an independent impact beyond its effect through income. This highlights the importance of considering non-income channels when studying the consequences of unemployment.

Figure C3: Assessing the Downstream Impact of Unemployment Beyond Income



Note: This figure presents results from a constrained regression exercise where the coefficient on permanent income (PI) is fixed at different values along a predefined grid. By imposing these constraints, we examine how the estimated coefficient on unemployment (UNEMP) changes. This approach helps assess the extent to which income alone accounts for the observed impact of unemployment.