Cash Transfers and Intergenerational Insurance: Evidence from Mass Layoffs in Israel

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Abstract

We study the role of government transfers in alleviating the repercussions of parental employment shocks on the education outcomes of children. A comprehensive reduction in Israel's system of universal child cash benefits, cutting total government transfers to families with children by more than 30%, is shown to have adversely affected children whose parents were displaced in a mass layoff event. First, we find that children of laid-off parents suffer from lower high-school performance and are less likely to secure a matriculation certificate (*Bagrut*). These effects are present only in lower-income families and are inversely related to the child's age at the time of the shock. Second, we find that cuts to child benefits at the household level exacerbate these negative effects for low-income families while leaving high-income families unaffected. Our findings suggest that cash transfers have a mitigating role in determining children's outcomes among families with low socio-economic status.

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1 Introduction

Abundant evidence exists regarding the long-term effects of job displacement on the earnings, health, and family circumstances of adults (e.g., Sullivan and von Wachter, 2009; Britto et al., 2022; Bertheau et al., 2022). Government insurance programs such as Unemployment Insurance (UI), along with progressive taxes and transfers, provide adults with insurance against such risks (Chetty and Finkelstein, 2013; Schmieder and Von Wachter, 2016; Card et al., 2018; Stepner, 2019). Despite growing evidence suggesting that negative labor market shocks propagate to children and affect their long-run outcomes (see Mörk et al., 2020, for a review), the effectiveness of government programs in protecting children has not been sufficiently studied. Understanding the intergenerational insurance capacity of tax and transfer policies is critical, not only from an equity perspective but also from an efficiency standpoint, since social policies that generate positive results for children have been shown to be socially beneficial (Hendren and Sprung-Keyser, 2020).

Most of the existing research on the relationship between government programs and children's long-run outcomes highlights the effectiveness of government programs in improving the lives of disadvantaged children (Hoynes et al., 2016; Dahl and Lochner, 2012; Dahl and Gielen, 2021; Shanan, 2020; Kott, 2022). A recent contribution from Brazil (Britto et al., 2021) provides the first evidence that the UI system mitigates some of the adverse effects of parental job displacement on children. However, to our knowledge, no studies have explored the effectiveness of universal government transfers that are not tied to unemployment or moral hazard at mitigating adverse effects on children.

In this paper, we study the intergenerational insuring capacity of cash transfers by leveraging a unique combination of rich administrative data from Israel and a rare setting that allows us to observe shocks to children's parental employment amidst a major change in government transfers' generosity. Following Jacobson et al. (1993) (JLS), we use mass layoff events as a negative parental shock, focusing on the period of 1999-2009, a decade with a relatively high incidence of layoffs. To learn about the insuring capacity of government transfers, we exploit a comprehensive reform to universal child cash benefits. Combined, these allow us to evaluate the importance of government transfers in mitigating the harm children experience due to a shock to their parents' careers.

We exploit a substantial policy change from the end of 2002 when the Israeli government slashed social benefits expenditures. This change repositioned Israel from a country spending above the OECD average on per-capita GDP family cash benefits to one well below this average (see Figure 1). We focus on the change in child benefits, a universal and automatic monthly cash transfer that depends *solely* on the number of children under 18. In this reform, child benefits changed dramatically as a function of family size. Prior to the reform, marginal per-child benefits were increasing with the number of children, whereas post-reform, the marginal per-child benefit was standardized and reduced to the level of a one-child family's benefits. As a result, the total government expenditure on child benefits was cut by more than a third. Using this reform, we focus on evaluating whether the drop in unconditional transfers affected the extent to which parental employment shocks affect children's outcomes.

Our analysis starts by studying the direct costs of job loss borne by parents and their children. We use firm-level mass-layoff events to identify the effects of job loss, akin to the strategy employed in the literature (Jacobson et al., 1993; Couch and Placzek, 2010; Lachowska et al., 2020). This strategy involves identifying the firms that close or go through mass-layoffs and comparing the displaced workers to a control group of non-displaced workers. We find that the job loss effect on parents in Israel in the years 1999-2009 is deep and persistent. In the short run, displaced parents experience a 45 percent decline in earnings compared to their pre-displacement earnings, along with a nearly 30 percent drop in employment. In the long run, the job loss scar lasts at least ten years and levels at around 10-20% lower earnings. The effects we find in Israel are substantial in the short run, akin to Portugal, Italy, and Spain, but shrink in size in the long run to levels more similar to Scandinavian countries (Bertheau et al., 2022).

Next, we evaluate the effects of job loss on children and find that parental job loss causes a substantial and significant drop in high school education outcomes. We measure education outcomes by performance in the *Bagrut* math exam and the probability of attaining a *Bagrut* certificate (or matriculation certificate), a series of standardized national exams taken in the last two years of high school, and a pre-requisite for university. We estimate a reduction of 1.4 percentage points in the probability of securing a Bagrut certificate due to parental job loss and almost one percentage point in the math exam. Interestingly, the mean results mask substantial heterogeneity. The entire effect is driven by children from families with below-median income, amounting to a 3.7 (2.2) percentage points decrease in Bagrut certificate attainment

(math scores), which is a reduction of almost 10 percent of the baseline average in this group. These findings suggest that low-income families have a limited capacity to shield their children from the adverse impacts of unforeseen employment shocks.

Echoing the results in Uguccioni (2021), we show that the impact of parental job loss on children's education diminishes with age at the time of job loss, implying younger children's heightened vulnerability. This finding is aligned with theories highlighting the importance of early years in human capital development (Cunha and Heckman, 2007), or those suggesting that effects accumulate over time (Chetty and Hendren, 2018a,b). In addition, we consider the effect of job loss on both fathers and mothers. Our data provide noisy, suggestive evidence that the impact is mainly due to paternal displacement. This finding either hints at a possible trade-off mothers face between family financial resources and investment in childcare home production or is driven by the fact that women in our sample are less likely to be the primary earners in the household, hence, their job loss is less impactful on the household.

Having established the effects of job loss on parents and their children, we then study the potential of government cash transfers to alleviate the adverse effects of job loss on children, together with estimating the effects of these cash transfers on parents' labor market outcomes post-job-loss. We rely on the fact that the child benefits program's generosity changed differentially by family size, where larger families experienced greater changes both in absolute terms and per child, while families with one or two children were minimally affected. For large families with three or more children, this change could correspond to 4%-40% of the average monthly wage at the time of the reform. This meant a dramatic reduction in financial resources for a large low-income family dealing with a recent job loss. We note that while the reform was not limited to child benefits alone and included changes to other government programs, such as unemployment insurance, these other changes were not directly related to family size and are not expected to affect the households in our sample.

We estimate the reform's impact on children of parents who have lost their employment by running a triple-difference regression. In this design, job loss effects post-reform are compared with pre-reform effects, utilizing the number of children as an intensive margin for the reform's treatment. We observe larger job loss penalties on children post-2003, which also increase approximately linearly with the number of children in the household. Combining these estimates, we find that the reform exacerbated the impact of job loss on low-income families by one per-

centage point for every additional child on the math outcome and almost 1.5 percentage points on Bagrut attainment.

In terms of cash transfers, for each additional 1,000 Shekels ($\approx$ \$300) lost due to the reform, children from low-income job-loss-affected families experienced an additional 0.2 percentage point decrease in math performance and a 0.5 percentage point decrease in the probability of attaining a Bagrut certificate. Given the average loss in benefits, these effects correspond to a third of the total effect of job loss on math and half of the effect of job loss on the Bagrut for low-income children. With an average level of 10,000 Shekels annually in child benefits prior to the reform, these effects imply elasticities of job loss penalty with respect to benefits lost of 1.3 in Bagrut attainment and 0.9 in math among low-income families. A back-of-the-envelope calculation of the effects of the reform on a child's earnings at age 30 suggests that every 1,000 Shekels in cash benefits lost translates into a decrease of 52 Shekels in a child's annual earnings at age 30.

The reform also affected the parents who were laid off. We find that among low-income parents, the change in child benefits mitigated the short-run effects of job loss on employment by around 15 percent and on earnings by around 10 percent. Stratifying by parents' gender, we find noisy evidence that most of the positive effect is attributed to fathers. Taken together, the policy's effects on parents and the effects on children generate a trade-off between the short-run effects on parents' labor supply response and the long-run effects on children. This parallels the trade-off studied in (Aizer et al., 2022) in the context of safety net policies in the United States.

We also find suggestive, albeit noisy, evidence for heterogeneity in the mitigating effects of the reform on children by the gender of the displaced parent. Children of low-income fathers who experienced job loss underwent larger detrimental effects on their outcomes due to the reform, while mothers' impacts are imprecise. This finding indicates the different tradeoffs men and women face in child human capital production and career aspirations (Goldin, 1995; Bertrand et al., 2010; Goldin, 2014; Wasserman, 2023).

The main contribution of our paper is in causally estimating the insuring capacity of government transfers to families on children's outcomes against shocks to the household. The only related evidence is a study from Brazil Britto et al. (2021), and a parallel work in progress in the US (Cole et al., 2023), both estimating the positive effects of Unemployment Insurance (UI)

on children's educational outcomes. The important distinction of our paper is the insuring instrument being studied, where our paper is the first to evaluate the intergenerational insurance capacity of unconditional transfers.

Our findings align with the recent literature emphasizing the importance of government transfers and safety-net programs for children from low socio-economic status (e.g. Dahl and Lochner, 2012; Hoynes et al., 2016). In the Israeli context, Kott (2022) studied the direct effect of this reform in child benefits using a regression discontinuity design and found a 2.3 percentage points drop in the probability of securing a matriculation certificate for every USD 1,000 benefit lost. Additionally, our paper extends the discussion about the effects of government transfers on the impact of job loss beyond the effects on children and estimates how cash transfers impact the labor supply of parents who have been laid off. This allows us to evaluate the trade-off between parents' labor market incentives and children's well-being. In doing so, we add to the debate presented in Aizer et al. (2022), extending it to the context of job loss.

Lastly, our work advances our understanding of the effects of job loss on children. While several studies find large adverse effects of parental job loss on children's outcomes (Oreopoulos et al., 2008; Stevens and Schaller, 2011; Coelli, 2011; Uguccioni, 2021; Britto et al., 2021), others point to null or very modest effects on school performance and labor market outcomes, such as Bratberg et al. (2008) in Norway, Hilger (2016) in the US, and Mörk et al. (2020) in Sweden. We add to this literature by finding substantial effects in our setting and provide evidence that these effects are causal, adhering to the concerns raised in Hilger (2016) for selection bias in previous studies such as Oreopoulos et al. (2008). Moreover, by showing that these effects can be mitigated by cash transfers in the case of disadvantaged families, we shed light on the mechanism through which job loss propagates from parents to children, namely that the family's financial resources serve as an important mediator.

The rest of this paper is organized as follows. Section 2 provides a detailed account of the changes brought about by the reform, along with a description of the general setting. Section 3 defines the targeted parameters in the analysis. Section 4 presents the data sources and present the outcomes of interest. In Section 6, we estimate the effects of job loss on the labor market outcomes of parents and the high school outcomes of children, and in Section 7, we investigate the role of cash transfers in mitigating the effects of job loss. Finally, Section 8 offers a summary and conclusions.

2 Israel Welfare Reform

The beginning of the 2000s in Israel was abound with consequential events. Following the 2001 world recession and the Palestinian uprising against Israel, also known as "The Second Intifada", Israel experienced high levels of unemployment rates and instability, which resulted in a government coalition shift. The newly elected Parliament ("Ha-Knesset") passed a series of expedited bills in 2002-2003 as a part of a reform aimed to shrink government debt by lowering expenditures and promoting labor force participation (Ami-Mei, 2008). Quoting from the annual report for 2002-2003 of the Israeli National Insurance Institute (NII):[1]

"In 2002-2003, the Israeli government decided on a real turnabout in its social policy, which will bear its mark on the Israeli welfare state in the long run. [...] *The series of cuts to benefits in 2002-2003 and those that are expected in future years is long and did not pass over even one of the benefit-targeted populations. The depth of the immediate cuts were most pronounced in labor force age populations - unemployment insurance, income support, and child benefits. The cut in each of these systems has summed to a third of all payments made before the new legislation"* (Achdut, 2004).

To illustrate the magnitude of the changes, Figure 1 presents the public spending on family cash benefits as a percent of GDP per capita in the OECD countries. Before the reform, Israel was among the more generous countries in terms of public spending on family benefits compared to other OECD countries. Post reform, these spendings dropped to well below OECD average levels in a span of 3 years.[2]

As quoted above, the reform includes substantial changes to child benefits, unemployment insurance, and the income support program. In this work, we focus on the change to child benefits, which are universal and automatic cash transfers that depend solely on the number of children and their date of birth. We discuss the different programs, as well as the details of the reform below, and explain in detail why the other changes are not relevant to our analysis.

[1] The NII is the Israeli institution responsible for collecting health and insurance contributions and providing unemployment and disability insurance, child benefits, maternity leave, grants for widows, old-age pensions, and grants to recent immigrants.

[2] The gradual decrease is primarily a reflection of the economic recession in Israel in these years in which both the GDP per capita decreased together with the social benefits.

2.1 Reform in Children Benefits

Children benefits are universal, automatic, and nontaxable monthly transfers to families with children aged 18 or below, with a take-up of almost 100% (Gotlib, 2021). The amount of money transferred depends solely on the number of children in the household and is transferred automatically to all eligible parents.[3] Changes to child benefits were described as the "deepest cuts" in the reform in terms of government expenditures. While Child Benefits expenditure amounted to around 1.7 percent of Israel's GDP in the late 1990s and early 2000s, it declined after the reform to levels well below 1 percent.

Figure 2 displays the main change in child benefits over time as a percent of the average yearly earnings in 2001 by the number of children in the household. Before the reform, the marginal subsidy per child was increasing with the number of children. After the reform, the marginal subsidy was constant and at a lower level than the pre-reform. The shaded areas in Figure 2 represent the heterogeneity in the change in benefits by the child's date of birth. To alleviate the change in family transfers, families were endowed with different amounts for children born before or after June 2003, such that the marginal benefit for the third child and above was higher than the constant post-reform amount but lower than it was pre-reform, making the change more gradual.

While the levels of benefits changed, eligibility criteria did not. The full schedule of the benefits by year is presented in the top panel of Table 2. Benefits were first reduced on average by 15% in 2002 and were no longer to be updated to inflation (6.5% that year). In the following year, benefits were cut even further, by around 40% on average, and the marginal transfer per child was made constant. Although benefits were reduced for all families, the largest changes were made to larger families. The bottom panel of Table 2 presents the range of potential lost benefits for families before and after the reform. The changes to total yearly benefits range from around 4% of the average wage for a family with 3 kids and up to 18-31% for a family of 6 kids.[4]

[3] Birth information is transferred from hospitals to the National Social Insurance Institute, and no intervention on the side of the parents is required. Some special cases require submitting a request, such as home birth, adoption, and so forth.

[4] It's worth noting that the total fertility rate (average number of children per woman) is around 3 in Israel, the *highest* among the OECD countries, which average at around 1.6.

2.1.1 Literature on the Impacts of the Reform

Past research has shown that the reform in child benefits exerts some impacts on children and parents in general, as opposed to our focus on insuring effects. In a recent contribution, Kott (2022) uses a regression discontinuity design that leverages the fact that, by design, benefit changes were less acute for children born before June 2003, to study the impacts of the reform in child benefits on children's educational outcomes. He finds an effect of a 2.3 percentage point drop in the probability of securing a matriculation certificate for every USD 1,000 lost. However, most of the impact he finds is concentrated among low-income Jewish boys from the youngest cohorts, which were born after 1995, the youngest cohort in our sample. Accordingly, in our sample that includes children who were born before 1995, we find little evidence for impacts on the regression discontinuity design. For further detail, see Appendix C.3. The change in child allowance also affected fertility rates (Toledano et al., 2011; Cohen et al., 2013)[5] and there is some evidence that they also affected the labor supply of females with multiple children (Mazar et al., 2018).[6]

2.2 Other Main Changes

In addition to the reform in children's benefits, the government introduced changes to two main social welfare programs: unemployment insurance and income support. Details of the programs and the changes made in the reform are in Appendix B. We briefly discuss the scope of these changes and their relation to this work.

Unemployment insurance (UI): Unemployment insurance in Israel is a function of pre-displacement wage and is given to workers who are laid off until they find a job, or up to a maximal period which depends on demographics.

Changes to UI were made both in the extensive and intensive margins. First, UI eligibility

[5] Toledano et al. (2011) find that child benefits for larger families increased the probability of an Arab woman giving birth by approximately 6-7%, while the probability for an ultra-orthodox Jewish woman increased by about 3%. Cohen et al. (2013) corroborates this, demonstrating a positive, statistically significant effect of child subsidy on overall fertility. Cohen et al. (2013) also highlights an income effect on fertility, which was found to be negative at low-income levels and positive at high-income levels.

[6] Mazar et al. (2018), using a difference-in-differences design, show an increase of approximately 4.3 percentage points in the labor supply of women with four or five children and an approximate 2.8 percentage point increase for men with the same number of children. The combined findings from these studies reveal a myriad of impacts of the child benefit reforms, family income, fertility, and labor supply, with some implications for children's educational outcomes.

requirements were made more stringent. Before the reform, to be eligible for UI, a laid-off worker was required to work for a qualifying period of at least six months out of 12 or 9 out of 18 before the unemployment spell started. After the reform, the qualifying period was changed to 12 out of 18 months. This change will not affect the eligibility of workers in our sample, as we restrict our attention to high-tenured workers working at least three years in the same firm, all of whom are eligible both under pre- and post-reform conditions. Further detail on the sample restrictions and definitions are in Section 5

Second, amounts were changed in several ways, amongst which are corrections to the discount rate scheme and changes in benefit periods. These changes, however, are minor, especially relative to changes in child benefits. To illustrate this, we assign each family their potential unemployment insurance, based on their work history and all other relevant observables according to the UI schedules.[7] In Figure B.2, we present the benefits calculated for families of different characteristics, such as wage levels, age, and the number of children. As can be seen, UI amounts are stable across the period.

Income Support (IS): Income support is a governmental financial aid designed to ensure a minimum standard of living for families with low to no income. This benefit is akin to conventional income support systems implemented in various other countries, often reflecting a social safety net objective. In Israel, it is either provided as basic income in cases of no income at all or as a supplementary income for those who earn less than the maximal IS benefit. Eligibility is based on two main tests, income, and employment, but is also conditional on owning assets, such as a car or real estate, which revoke eligibility.

The amount paid to beneficiaries depends mainly on their income, family status, and the number of children. The changes that were introduced in the reform were intricate since they were made to all parameters that determine the amounts and are set in proportion to the average wage (at around 35% on average in 2002). Overall, an eligible family before the reform was to receive between 25 to 50 percent of the average wage and 20 to 40 percent after it.[8]

For several reasons, the change in IS is not expected to affect our population of interest, namely, our selected job-loss families. The workers in our population of interest are relatively high-tenured and stable earners before being laid off. Full-time workers likely own some assets,

[7] Details of the way we simulate benefits, and the exact changes to UI, are in Appendix B.
[8] The Statistical Quarterly of The National Insurance Institute, retrieved using Wayback Machine

such as a car, that would revoke their eligibility. Additionally, in our main analysis, we exploit the variation in the number of children, which is not a formal criterion for either eligibility or the amount of income support benefits. Furthermore, even for the small portion of workers that might have been eligible, eligibility and amounts received depend on income that includes UI benefits. That is, only if UI benefits (including all other household incomes) are lower than the maximal IS benefit, then a household is eligible for it during the UI period. Even if this condition holds, the amount paid would only be the difference between the UI benefit and the IS maximum. Lastly, IS take-up in our period of interest is low, estimated at around 50% in 2013 (Gotlib, 2021) among the eligible families.[9]

To illustrate the changes in IS as well as child benefits and UI, We simulate the benefits a family would receive in the first year after job loss for every year in our sample period. When calculating IS benefits, we account for two sources of uncertainty in determining eligibility and amounts. First, the low take-up rate. In our simulations, we use the estimated value of %50 from the year 2013, which is expected to be even higher than in the early 2000s (Gotlib, 2021). Second, inferring IS benefits from the administrative data is inaccurate. Out of those who are potentially eligible according to their income and characteristics in the administrative data, only half are actually eligible for IS. We incorporate the estimates for take-up by family characteristics and inference errors from Gotlib (2021) to calculate the expected benefit a family would receive in our sample. As can be seen in Figure B.2, the implied change in IS due to the reform is negligible for our sample.

3 Parameters of Interest

Our study aims to estimate the impact of government transfers on the treatment effects of job loss. To identify such a parameter, we exploit two research designs. The first is based on comparing displaced and non-displaced workers conditional on a set of controls assuming selection of observables. The second utilizes a difference-in-differences approach, exploiting the sudden change in child benefits generosity. In this section, we briefly outline the assumptions and target parameters of each part. A more formal and detailed description using a potential outcome framework is provided in Appendix C.

[9] Gotlib (2021) argue that the low take-up is a result of stringent eligibility conditions, which include meeting an official and is perceived as stigmatizing.

We have a population of N^p parents, indexed by j, each of which has $n_j > 0$ children, indexed by i. Therefore, the child population is comprised of $N^c = \sum_j^{N^p} n_j$ children. We use the function $J : \mathbb{Z} \to \mathbb{Z}$ to indicate that $J(i)$ is the parent of the child i. We start by presenting the job loss treatment effect parameters and then turn to the impacts of the reform.

3.1 Job Loss Effects

Our first parameter of interest is the treatment effect of parental job displacement, which we denote as Δ^p for the effect on parents, and Δ^c for the effect on children. In the absence of an experiment, estimating Δ by naively comparing the outcomes of parents and children by whether the parents were laid off poses significant challenges in identifying the true causal effect. In particular, unemployed workers tend to be negatively selected (Davis and von Wachter, 2011), and a parent's innate abilities are inherited by their children (Sacerdote, 2007; Fagereng et al., 2021). To overcome these issues, we assume that selection into treatment is explained by a vector of child-parent pair pre-displacement characteristics $Z_i = (X_{J(i)}, X_i)$. Therefore, comparisons of displaced and non-displaced workers within Z_i values identify the impact of job loss.

Even with rich and comprehensive data such as ours, this is a strong assumption. We rely on a long literature in labor economics estimating the effects of job displacement in mass layoffs on adults based on the Jacobson et al. (1993) (JLS) approach and show that our results are comparable to the job loss effects estimated in developed countries. Moreover, by estimating Δ^p before displacement on outcomes that are not included in X_j, we present evidence on pre-displacement differences between treated and control units.[10]

In addition, estimating Δ requires an additional standard assumption of overlap over values of the propensity score among displaced and non-displaced workers. In Section 6, we describe our estimation strategy and provide further evidence that our setting satisfies the common support assumption.

[10]Note that this is a stronger assumption than the "parallel trends assumption". We do not only reject differences between treated and control units in the rate of change of outcome pre-displacement, but we also require that the levels should be the same on average.

3.2 The Impact of Government Transfers on the Impact of Job Loss

The second set of parameters we set to estimate is φ^p and φ^c, the effects of cash transfers on the effects of job loss for parents and children, accordingly. To that end, we recognize that the reform in children's benefits generates variation in exposure to cash transfers both by time, before and after 2003, and by family size, in particular for families with above and below three children. We exploit this variation by using a difference-in-differences research design. While this design is standard in the literature, our approach is different as it employs a difference-in-differences strategy on job loss impacts rather than on outcomes directly.

Among parents, the difference-in-difference parameter compares the impact of job loss on labor market outcomes before the reform to the impact of job loss after the reform among families of different numbers of children. Among children, we restrict attention to the first-order impact of government transfers in the year of job loss. i.e., we compare children whose parents experienced job loss in the years before the reform to children whose parents experienced job loss in the years after the reform. In Appendix Figure D.9, we provide evidence of no meaningful effects of the second-order interaction between job loss and change in transfers on different years, i.e., we find no significant additional impact of losing a job in year t and losing benefits only a years later rather than in the year of job loss or before. The identification assumption of the difference-in-difference parameter is the well-known parallel trend assumption. It states that the average job-loss impacts for families with high and low levels of benefits would have followed parallel paths in the absence of the treatment.

To estimate φ, note that the difference-in-difference parameter is comprised of three differences: job-loss status, benefits eligibility status, and time. Hence, it can be estimated using a triple-difference (DDD) model interacting both job loss, time, and family size after adjusting properly for covariates Z_i. To support the parallel trends assumption, we test for pre-trends in the DDD coefficient of the impacts of the reform before the reform was enacted.

In the following sections, we first present the data sample restrictions and treatment definitions as the basis of our empirical analysis. We then turn to estimate Δ^p in Section 6.1, and in Section 6.2 we estimate Δ^c. Finally, we estimate φ^p and φ^c in Section 7.

4 Data

In this work, we use administrative data assembled by the Israeli Central Bureau of Statistics (CBS). The data covers the entire registered Israeli citizens for the cohorts of 1950-1995 and their parents, totaling roughly 100 thousand individuals per cohort on average. The data is composed of three main sources: Tax records, education records, and the population registry. From the Tax Authority, the data includes employer-employee and self-employed tax records at a yearly level for the years 1995-2019.[11] At the firm level, the data includes a unique firm id, number of employees, 3-digit industry code, and total payroll for each year. At the worker level, this data records separate jobs on a yearly basis at each employer, with the number of months of employment at each job and gross yearly earnings.

From the Ministry of Education, we merge to each individual their school identifiers and city, high-school matriculation scores, subjects and majors, and parents' education attainment.[12] From the civil registry records, we match detailed information on demographics, including gender, year of birth, date of immigration and country of origin of children, parents and grandparents, identifiers of siblings, an identifier of a spouse, and the month and year of birth for every child.

Although we do not directly observe benefits transferred to parents, we can reconstruct these transfers from the available information. Further details on how we simulate benefits over time are in Appendix B.4, and the sources we used to trace the details of the reform are in Appendix B.

Outcomes: The primary outcomes for children in our study are educational achievements, specifically high school matriculation exam scores and attainment of the matriculation certificate, known as "Bagrut". To secure a Bagrut certificate, students must pass a series of standardized national exams administered during the final two years of high school, with the majority of these exams taking place in the last year when students are typically 17-18 years old. Each

[11] Information on self-employment is a unique feature of our data, which we use to construct household income. Note, however, that we observe self-employment only for the years 1995, 1999, 2000, ..., 2017.

[12] Parental education is generated from various sources, from which we comprise a unique variable for every parent that codes whether the parent has an academic degree. We do so by combining information from the Ministry of Education, the 1995 and 1983 censuses, and CBS's social and immigrant surveys for the individuals who were surveyed. Using this method, 20% of the parents have at least a Bachelor's degree, aligned with publicly available information.

successful exam contributes between one and five points towards the minimum requirement of twenty points needed for a Bagrut. Both students and their teachers have the flexibility to choose the proficiency level of the exam each student takes, with higher proficiency levels yielding more points. They also have the option to select elective subjects in addition to the required core subjects.[13] The attainment of a Bagrut certificate is a critical factor in children's future labor market outcomes, as nearly all post-secondary institutions mandate it for admission. Consequently, failure to acquire a Bagrut can significantly restrict students' opportunities for further education and, ultimately, their prospects in the labor market.

We define two primary outcomes for children. First, we define Bagrut certificate attainment as a binary variable assigned a value of 1 if the student has received the certificate and 0 otherwise. The second outcome is the math exam result, which is also a binary variable assigned a value of 1 if the student's score exceeds the mean within their cohort, and 0 otherwise. It's important to note that in both instances, a value of 0 is attributed to students who either fail or do not have a registered score.

For parental labor market outcomes, we use employment and yearly earnings from work. Employment status is defined by a binary variable that is assigned a value of 1 if the worker's annual earnings exceed 10,000 NIS (approximately 3,000 dollars) and 0 otherwise. We also consider the spouse's earnings, as well as earnings from self-employment, and the total household income, which is the sum of parental earnings from all sources in our data.

5 Job Loss Definition and Sample Restriction

Similarly to administrative data sets in the U.S. and other countries, our data contains no direct information regarding the reason for job separation. Therefore, we follow the literature (Bertheau et al., 2022; Schmieder et al., 2022) in using a data-driven approach to identify exogenous job separations due to mass layoffs and firm closure events. We start by restricting the sample to all the parents with at least three years of tenure with the same primary employer. Therefore, our analysis refers to long-tenured workers who are highly attached to the labor market before displacement. We consider job displacement events in the years 1999-2009,

[13] For instance, math, English, and history are core subjects, while physics, biology, or theatre are considered electives.

thereby ensuring that we observe each displaced worker at least five years before displacement and ten after. We include only parents with at least one child born between the years 1974 to 1995, the cohorts for which we have Bagrut data, graduating in 1992-2013. Therefore, our children's age at displacement range from 4-35. Appendix Tables D.1 provide the frequencies of treated children in our sample by cohort and the age when the parent was displaced from the job.

On the firm side, we restrict attention to firms with at least 40 employees[14] excluding workers in nonrelevant industries as detailed in Appendix Section A, such as government employees, similarly to Schmieder et al. (2022), to avoid false detection of mass layoffs in sectors that are unlikely to have them, or that are under very different employment agreements and conditions compared to the private sector. We classify a firm as experiencing a mass layoff event if it experienced a drop of at least 30% in monthly employment in one year. Importantly, we distinguish between actual permanent separations and events such as mergers, takeovers, or changes in employer identification numbers. To avoid classifying a firm undergoing acquisition, outsourcing, or a change in firm identifier as one that experiences mass layoff event, we generate a cross-flow matrix of workers between firms and consider a firm experiencing a mass layoff event only if fewer than 20% percent of the laid-off workers are going to the same single employer.

The comparison group is constructed of the sample of all workers who do not belong to the above population of displaced workers at any period in the data. Then, we restrict attention to long-tenure workers who follow the same restrictions on firm size and tenure in non-mass-layoff firms. To avoid detection of retirement, we restrict the sample to workers aged 25-55 in the year preceding the job displacement event. Further details on the sample restrictions and construction procedure are in Appendix Section A.

Propensity Score Having defined the treated (displaced) and non-treated populations in our data and following our identification strategy outlined in Sections 3, we estimate a logit-based propensity score to predict the probability of each child-parent pair experiencing a mass layoff event. We exploit the richness of our data by including a long list of features in the propensity estimation, including both parent characteristics, $X_{j(i)}$, and child characteristics, X_i. Parent

[14]The minimal firm size used in the literature is 50. Instead, we use 40 to increase our sample size in subsequent analyses. Using 50 does not qualitatively affect our result.

characteristics include second-order polynomial of earnings in the three years before displacement, pre-displacement firm characteristics such as firm size decile dummies, firm fixed effect decile dummies, 1-digit industry dummies, tenure years, cubic age times gender, the job loss year dummies, district of residence at the time of displacement, ethnic group, post-secondary degree attainment both parents and spouse's earnings in the two years before displacement. Child characteristics (X_i) include the child's year of birth dummies and gender, as well as the number of siblings born up to the displacement year.

For every child-parent pair (i, j), for which $J(i) = j$, we denote $\hat{p}_{ij} = \hat{\Pr}(D_j = 1|X_j, X_i)$, the predicted propensity score, where $D_j \in \{0, 1\}$, is an indicator that equals one if parent j was displaced in a mass layoff event, and zero otherwise. We exploit $\hat{p}_{ij}$ to estimate the Average Treatment effect on the Treated (ATT) using inverse probability weighting (IPW). Specifically, to estimate Δ^c on the children population, we use the weights $\hat{w}^p \propto \frac{\hat{p}_{ij}}{1-\hat{p}_{ij}}$ to reweight each child observation with a non-displaced parent to match the distribution of characteristics of the displaced workers. To estimate Δ^p on the parents' population, we attain the propensity score at the parent level, $\hat{p}_j$, by employing the law of total probability calculating $\hat{p}_j$ with a within-parent weighted average of each parent-child pair $\hat{p}_{ij}$.[15] We then re-weight the control group of parents with the same weights, thereby matching the distribution of parents' non-displaced characteristics to displaced.[16]

As mentioned in Section 3, identification requires sufficient overlap across the values of the propensity score between treated and control units. We trim our sample to include observations with a propensity score in the minimal overlap range between the displaced and non-displaced groups and omit observations with weights that are larger than the top 99.5%. In Appendix Figue D.1 we present the full distribution of the propensity scores of displaced and non-displaced groups.

[15] By the law of total probability:

$$p_j = \Pr(D_j = 1|X_j = c) = \sum_x \Pr(X_i = x|X_j = c) \Pr(D_j = 1|X_j = c, X_i = x)$$

because X_j includes continuous and fine measures of workers' characteristics, we approximate $\Pr(X_i = x|X_j = c)$ with $\Pr(X_i = x|J(i) = j)$.

[16] We have conducted a similar analysis using the more common approach in the literature of matching and get the same qualitative results with larger standard errors. For further discussion about IPW and a comparison to matching methods, see Busso et al. (2014).

5.1 Summary Statistics

Table 1 presents the means and standard deviation of children and parents' pre-displacement characteristics. Column (1) presents the characteristics of the non-displaced population, from which we draw families to match the job loss population. Columns (2) and (3) present the treated and control groups after applying a reweighing to the control group, while column (4) presents the difference between treatment and control means with standard errors in parenthesis.

First, note that covariates balance across the two groups after matching. Second, it can be seen from the table that job loss families (and the control group) are negatively selected compared to the entire population. Job loss parents are more likely to have lower earnings before displacement, are less educated, work in smaller, lower-paying firms, and are more likely to be of minority groups, such as Ethiopians and Arabs.

6 The Impacts of Job Displacement

6.1 Job Loss Effects on Parental Outcomes

Figure 3 presents the average labor market outcomes of parents for both job loss-affected families and the inverse probability weighted control group, where t are the years relative to the job loss event year, $t = 0$. Outcomes include employment, earnings, spouse earnings, self-employment earnings, and total household earnings. We use these results for two goals, first, to study the impact of job displacement on parents, and second, to validate our identification assumptions from Section 3 and Appendix Section C.

The impact of job displacement on parents in Israel is substantial and long-lasting. Compared to previous findings from other countries (e.g. Bertheau et al., 2022), we find that the effects of job loss in Israel on labor market outcomes for displaced workers are deep, persistent, and on the higher end of the range of results from other developed countries.[17] The short-run drop in

[17]The short-run effects of job loss on parents in Israel are large relative to results from other countries, while the long-run effects are similar in size to other studies. For example, Lindo (2011) find a drop of around 40% in first-year earnings, and a 26% drop, in the long run, using the PSID. Using tax records data, Hilger (2016) finds a 30% drop in father earnings in the short-run, while family income dropped by only 14% in the first year. Uguccioni (2021) finds a constant 30% drop in father income using administrative data in Canada. In Scandinavian countries, effects are

employment, namely in the first year, is almost 30% as shown in Panel a), implying that %30 of parents did not find any job in the year following their displacement. The drop in earnings relative to pre-displacement earnings is around 50% (Panel B), or 70,000 Israeli Shekels (Panel C). In the longer run, ten years after displacement, these effects shrink to a level of around 7% in employment and around 15-20% (30-35 thousand Israeli Shekels) lower earnings.

Job displacement generates a shock to the family as a whole, which we can observe here in terms of the worker's spouse's labor market outcomes. Panel (d) depicts a short-run uptick in spousal earnings in the year of job loss for those who have a spouse, suggesting a labor supply response at the household level and, importantly, that the job loss shock affects not only the outcomes of the displaced worker. This increase in spouse earnings, along with a small increase in self-employment earnings (Panel (e)), offsets the effect of job loss on total household income only in the year of displacement but not in the years after the first year nor in the long run, as shown in Panel (f). This result resembles in spirit the idea of the added-worker effect as in Lundberg (1985) and is generally in line with the results in Stephens (2002), which shows that the spouse response is short-lived and small.[18]

To test our identification assumptions, we leverage the fact that we did not match parents based on all of these outcomes in pre-displacement periods. We estimate the propensity score based on pre-displacement outcomes from the three years before displacement, excluding self-employment earnings (panel e). We find that pre-displacement self-employment earnings *levels* are equal on average between the displaced and non-displaced workers, supporting the conditional independence Assumption A1.

6.2 Job Loss Effects on Children's Education

Next, we turn to estimate Δ^c, the propagating effects of parental job loss on children's high school outcomes. We first estimate the aggregate effects of job loss and then turn to study the dynamics of the effects with age and their heterogeneity with respect to parental income and

even smaller, with, for example, an initial drop of 30% in father earnings in Finland (Huttunen and Riukula, 2019), and small effects on both disposable income and unemployment in Sweden, with values under 10% (Mörk et al., 2020). However, effect sizes in Israel do resemble those found in Brazil by Britto et al. (2021), at around 50% initial drop in earnings, and a very small effect on spousal income, such that household income resembles the displaced worker's earnings.

[18] Our results are somewhat at odds with Halla et al. (2020), who also find that the spouse's labor response has limited effect on total household income, but there the impact lasts for longer, and at least five years after husband displacement whilst we find only shorter-term effects.

displaced parent's gender.

In our analysis, we focus on the effects of parental job loss on key high school performance indicators: the probability of attaining a Bagrut certificate and the probability of completing all the math Bagrut exams and scoring above the median. Since these outcomes are realized between the ages 17-18, we estimate Δ^c separately for children whose parents were displaced before and after age 18. Therefore, the effect of job loss after age 18 serves as a placebo test. In all our estimations, we re-weight the control group with the ATT inverse probability weights introduced in section 5. To estimate Δ^c by age groups, we run:

$$y_i = \sum_{k=1}^{J} \Delta_k^c \times \mathbb{1}\{age_i \in k\} \times \mathrm{D}_i + \sum_{k=1}^{J} \gamma_k \times \mathbb{1}\{age_i \in k\} + \varepsilon_i \tag{1}$$

Where D_i is a dummy for whether child i's parent experienced job loss, and age_i is the age of the child when the parent was displaced. $1, ..., J$ are the age bins of children's age at parent's displacement. Δ_k^c, therefore, gives the effect of parent job loss among children whose parent was displaced when she was in age group k.

Table 3 presents the estimation results of Equation 1 stratifying the age group to above and below age 18. Column (1) shows that children whose parents lost their jobs by age 18 are 1.4 percentage points less likely to secure a Bagrut certificate. The estimates for the effects post outcome realization (above 18) are small and insignificant, supporting our identification assumption of no selection into treatment. Interestingly, the total mean effect masks substantial heterogeneity by family earnings. As seen in column (2), the effect on the matriculation certificate more than doubles in size when we introduce an interaction with a dummy for family income above the median. This implies that children born to below-median income families whose parents lost their jobs unexpectedly suffer from a 3.7 percentage points lower probability of securing a matriculation certificate, a reduction of more than 10 percent effect relative to the unconditional mean in Matriculation attainment in our sample. Columns (3) and (4) display a very similar pattern for the effect on math exam scores, which is an important indicator for performance in the matriculation exams, and found to be strongly related to later labor market outcomes (Lavy, 2020; Angrist and Lavy, 2009; Ben-David and Kimhi, 2020). This effect amounts to about 7 percent effect relative to the sample mean once we focus on lower-income families.

Table 3 presents the estimated effects of parental job loss on children's high school outcomes. The coefficient of the interaction between job loss and high-income families is qualitatively small, positive, and statistically insignificant from zero, implying that the effects of job loss on children are concentrated among children from low-income families. Such results could be driven by several mechanisms. For example, financially constrained families may have a harder time coping with the loss of income and struggle to prevent the propagation of the shock to children, as suggested in Cooper and Stewart (2017). Another explanation could be that the time spent at home for higher income families, which are on average associated with higher human capital, could outweigh the effects of lost earnings, as suggested in Liu and Zhao (2014). The aggregate effects we find are in line with the general conclusions in the literature: job loss appears to have detrimental impacts on children's school performance, and the effects are stronger for low-income families.[19]. This result can be partly explained by the patterns in our data, as discussed in Section 6.1, regarding the effects of job loss on parents by income. There we show that these families enjoy much higher levels of income in absolute terms, even after job loss and despite the drop in earnings, which may mean lower elasticities of child human capital investments with respect to income. In addition, the earnings shock relative to pre-displacement levels is similar between low and high-income families, while the self-employment and spousal income responses somewhat dampen the effects in higher-earnings families.

Figure 4 extends this analysis to finer age groups at parental job loss and whether it was the father or mother displaced. Subfigure (a) presents the results from securing a Bagrut certificate, and subfigure (b) for math exam performance, i.e., scoring above the median in the matriculation math exam. First, in accord with the findings in Table 3, only children from lower-income families experience negative effects, and only before outcome realization.

Interestingly, we find substantial heterogeneity in the impacts of job loss on children by the child's age at the time of parental job loss, whereby the effects on children from low-income families decrease with age. That is, the younger the child is at parental job loss, the larger the harm to her education outcomes. The effects are as high as 6 percent on the math exam outcome and 8 percent on the Bagrut certificate for the younger age groups. While we find substantial heterogeneity in the impact of job loss on children by their age at loss, in Appendix Figure D.3, we do not find a similar pattern for parents. This suggests that the child age effect variation

[19]For a review, see Ruiz-Valenzuela (2020, 2021)

is not driven by variation in family characteristics that might be correlated with the age of the child at the time of displacement, such as parental age.[20]

The dynamic pattern of job loss effects by age could reflect different mechanisms of the human capital production function. On the one hand, in line with Cunha and Heckman (2007), it could be attributed to the excess importance of younger ages in human capital development. Another possibility is that due to the nature of the job loss scar, which lasts years after the layoff, a shock at a younger age implies longer exposure to a family with fewer resources. Such accumulating exposure effect was also found in the literature on neighborhood effects on children outcomes (Chetty et al., 2016; Chetty and Hendren, 2018a,b). As of now, exploring the mechanisms behind the age-dependent pattern of effects is outside the scope of this paper.

While in this work we present the effects of job loss experienced by both mothers and fathers, the more common practice in the literature is to focus on fathers only (e.g. Oreopoulos et al., 2008). Figure 5 presents the dynamic effects of job loss by the gender of the parent who was displaced. Interestingly, the effects appear to be mainly driven by fathers' displacement.[21] This pattern is in line with the models in which investment in child's human capital can be either acquired in the market or produced at home using time (Gronau, 1977; Galor and Weil, 1993), acknowledging the fact that home child care and career aspirations are trade-offs that mothers, rather than fathers, face under the current social norms (Goldin, 1995; Bertrand et al., 2010; Goldin, 2014; Wasserman, 2023). Therefore, if mothers are more effective in rearing and home production of children's human capital, there is a tradeoff between the effects of job loss on the family resources on the one hand and the added childcare home production on the other. This pattern could also be explained by the fact that women are less likely to be the "breadwinners" in the household, and losing their jobs has less of an impact on family resources.

To further shed light on this matter, we circle back to the effects of job loss on parents. Appendix Figure D.5 presents the effects of job loss on parents by the gender of the parent. There we see large differences in earnings and a large difference in the tendency to turn to self-employment,

[20] This pattern of effects on children by age is similar to that found in Uguccioni (2021). The main difference to our approach is that Uguccioni (2021) uses UI recipients in Canada to identify job losers, as opposed to jobs lost via mass layoff events. Uguccioni (2021) estimates effects on college attainment and later earnings with a similar pattern to ours, from age two up to around age 12, and null thereafter. The effect on college attainment is around 2.5% and around three percentiles on income rank. Our findings are, however, at odds with the findings by Mörk et al. (2020), who use a similar design on Swedish data. They find no effects on children's education outcomes, perhaps due to the relatively small job loss effects on parents that they observe in their setting and the comprehensive social welfare instituted in Sweden.

[21] We caveat that the children whose mothers were displaced are a smaller group, hence the noisier estimates

whereby fathers who experience job loss increase their income from other sources almost three times more than mothers, while mothers tend to remain unemployed slightly more than men after the first year of job loss, implying that mothers are more likely to turn to home production compared to fathers after job loss. Notwithstanding this, it is also evident that the proportion of earnings of fathers out of the total family income is twice as large as that of mothers, meaning that the loss of a father's job is more impactful for family resources than the loss of a mother's job on average.

To test for the validity of Assumption $A1$, we add a placebo test by introducing outcomes that are realized even sooner than high-school graduation, and before job loss. To do so, we utilize the children's scores in the national standardized school evaluation exams, *Meitzav*, administered by the Israeli Ministry of Education.[22] The *Meitzav* exams cover several key subjects, including mathematics, science, English, and Hebrew/Arab language skills, mimics the PISA exam administrated by the OECD. This exam is administered at a random representative sample of schools, which in our sample amounts to a tenth of the children in the full analysis sample. The Meitzav exams are taken by students in the 5th and 8th grades. Hence, if the parental shock affects performance, we expect to find null effects in the years after the exam is taken, and not only after age 18, as we saw in the effects on high-school matriculation exams.

Table 4 presents Δ^c in terms of standard deviations by three age groups. Column (1) presents Δ^c for the 5th-grade exam, and column (2) presents the results for the 8th-grade exam. In the rows, we look at Δ^c by age categories of the child at parent's displacement: before the exam age (before age 11 for the 5th-grade exam, and before age 13, for the 8th-grade exam), after the exam age but still in school (up to age 18), and after age 18. Due to the small number of observations, of around a tenth of the full job loss sample size, the results are noisy. However, focusing on the point estimates, we do find a large negative point estimate for the effects before the exam of around 0.024 to 0.031 lower standard deviations. In addition, the point estimates after the realization of the exam are much smaller and closer to zero.

[22] "Meitzav" is an acronym in Hebrew that translates to "School Efficiency and Growth Measures".

7 The Child Benefits Reform and the Mitigating Role of Government Transfers

Thus far, we have established that the repercussions of job loss are substantial, affecting parents and their children alike. These observations underscore the significance of parental career stability in shaping children's human capital. In this section, we turn to estimate φ, the effect of the cut in child benefits in mitigating the impacts of job displacements.

7.1 Showing the Data: Baseline Effects in Difference in Differences Tables

In our estimation, we rely on the variation across three dimensions: the timing of job loss relative to the reform, the incidence of job loss itself, and the number of children in the household, which determines the drops induced by the reform in child benefits. To clearly outline the variation in outcomes along these dimensions, we first present the averages for each group of job loss and control households, before and after the reform, and separately by family sizes, in 2-by-2 difference-in-differences tables.[23]

Table 5 demonstrates how earnings and employment have changed before and after 2003 for displaced and non-displaced workers, for both the control and laid-off groups.[24] We note two main conclusions from these tables. First, the average outcomes of the control group are practically unchanged after the reform relative to before the reform in both workers' earnings trends and employment. Second, the point estimates of the double differences for both employment and earnings are small yet positive, with values of 0.4 percent in earnings and 1.2 percent in employment, although not statistically significant.

Note, however, that it is not guaranteed that the differences in the job-loss treatment effects, both for parents and children over the years, reflect the causal effects of the reform. It may reflect, for example, changes in the composition of workers who have experienced job loss,[25]

[23] For the full dynamics of the effects of job loss on parents by the dimensions of our triple differences approach, also showing the trends before displacement for each group in this analysis, see Appendix Figure D.6 and D.4.

[24] For control workers who were not laid-off, we assign a "potential" job loss year, as described in Appendix Section A. One year after the job loss year for these workers means a year that follows at least three consecutive years of year-round employment with the same employer.

[25] As we show in this paper, as well as been shown in the literature, the effects of job loss are highly heterogeneous with respect to worker characteristics (Schmieder et al., 2022; Seim, 2019; Krolikowski et al., 2020; Lachowska et al., 2020; Illing et al., 2021, e.g.,).

or a change in the labor market conditions that affect the impacts of job loss (Schmieder et al., 2022). For this reason, we introduce the third difference, which reflects the intensity of the impact of the reform on households - the number of children that are under the age of 18 at the time of job loss. We illustrate this relation in Figure 2, which shows that the more children are in a household, the larger the drop in benefits received after the reform.

To observe the variation in effects with household size, Table 6 presents the same difference in differences comparisons, broken down to small (up to 3 children) and large (4 children or more) families. We find that in both earnings and employment, the reform has made small non-significant effects on smaller families, while larger families experienced shrunk effects of job loss on earnings and employment. That is, workers from larger families experience smaller job loss effects after the reform. Note, however, that the impacts on employment are noisy. These results suggest that the reform has indeed affected the labor market response of workers to a job loss shock since the main change that larger families experienced before and after the reform and relative to the non-job-loss group, is to their monthly cash transfers. The response in employment showcases a labor income effect: as income decreases, labor supply increases. Concurrently, we find a large positive effect on earnings, which is not necessarily predicted given the effect on employment. Specifically, workers from larger families, faced with an abrupt job loss and a decrease in cash benefits, may be compelled to secure employment hastily, potentially leading them to settle for lower-wage positions.

Tables 7 presents similar difference-in-differences tables with children's outcomes. First, we see that there is a general trend of decreasing children's outcomes before and after the reform, according to the control group.[26] Thus, the average Bagrut rate for the untreated sample drops by almost half of a percent in the Bagrut, and there is a larger 2.5 percent decrease in math. A larger trend in math outcomes may point to a general decrease in the quality of the high-school diploma that students achieve or to a change in the standards and requirements of the exams over time. As for the effects of job loss, affected students have around 1.3 percent lower average outcomes in the baseline period in both outcomes. These effects increase substantially in the period after the reform, to -3.3 in the Bagrut and -3.1 in Math.

Similarly to the exercise above with parents' outcomes, in Tables 8, we break the differences

[26]This is also the case for the outcome means over the years for the unrestricted sample of students in Israel, omitting our sample's treatment and control students. We show this in Appendix Figure D.8, which presents the outcome averages across years for the entire population of students.

down to small and large families. Again, if the reform has had a differential effect on larger families, we expect it to manifest as a higher difference-in-differences for larger families. This is indeed what we find. With the Bagrut certificate as an outcome, we find that smaller families experience a reform effect of 1.1 percentage points, as evidenced by the difference-in-differences estimate, while larger families experienced an increase in the effect of job loss after the reform of almost 3 percentage points. The same pattern arises for the math performance outcome, whereby the effects of job loss increase after the reform for both large and small families, by a small increase for small families and a larger increase of 2.2 percentage points for larger families. Note, however, that these findings are only suggestive of the changes that occurred, as the coefficients are non-significantly different from zero.

To support the parallel trends assumption underpinning the identification of these estimates, we need to demonstrate that in the absence of the reform, the impacts of job loss wouldn't have diverged for those workers with larger families who were laid off post-reform. Appendix Figures D.7 and D.10 present the event study coefficients β_{7t} from running the following model:

$$y_i = \alpha + \beta_1 D_i + \beta_2 K_i + \beta_3 D_i \times K_i \tag{2}$$

$$+ \sum_{t=1999\ t\neq 2001}^{2009} \left(\beta_{4t}\text{Year}_i^t + \beta_{5t} D_i \times \text{Year}^t i + \beta_{6t} K_i \times \text{Year}^t i\right) \tag{3}$$

$$+ \sum_{t=1999\ t\neq 2001}^{2009} \left(\beta_{7t} K_i \times \text{Year}_i^t \times D_i\right) + \varepsilon_i \tag{4}$$

Where y_i is the outcome for either child or parent i. D_i is a dummy for job loss. K_i is either the number of children in the household or a dummy for a large family (with three children or more) at the time of job loss. The dummy variables $\text{Year}^t i$ represent the year of job loss, with 2001 as the baseline year in the summation terms. The interaction term $\beta_{7t} K_i \times \text{Year}^t i \times D_i$ allows us to estimate the differential effect of having more children at the time of job loss by relative to 2001, with $\beta 8t$ being the 3D coefficients of interest. Unfortunately, this exercise is underpowered and produces noisy estimates. Nevertheless, we do not detect significant pre-trends in any of the outcomes and groups.

7.2 Effects by the Number of Children

To draw closer to understanding how the reform has changed the outcomes for workers and their children, in this section, we study the relationship between the effects and family size and map the effects from the number of children in the household to the money lost due to the reform. We find that the relation between benefits lost and the effects of job loss on children are approximately linear, which supports our parametric assumption in our triple differences regression to follow, which produces our main result.

To establish the relation between the number of children to the effects of the reform, we turn to estimate the effects of the reform separately by the number of children in the household by running the following set of difference in differences regressions:

$$y_{ik} = \beta_{1k} + \beta_{2k} After_{ik}^{2003} + \beta_{3k} D_{ik} + \beta_{4k} D_{ik} \times After_{ik}^{2003} + \epsilon_{ik} \tag{5}$$

where $After_i^{2003}$ is a dummy of whether the job loss occurred after the reform, and D_i is the job loss indicator. y_{ik} is either parent or children outcomes. These regressions mimic the analysis above presented in 2-by-2 tables in regression form, separately by finer family size categories.

Figure 6 presents the estimates β_{4k} both for parents and for children wherein subfigure (a) we display the impact on children, and in subfigure (b) we display the impact on parents' employment and earnings before job loss (t=-1,0), and in the first two years after job loss (t=1 and 2). Panel (a) shows that the more children are in the household, the larger the job loss penalty after the reform relative to before the reform for children. Interestingly, we find that the effects change approximately linearly with the number of children in the household, with a slope of around 0.42 percent per child in the Bagrut and 0.26 in math. That is, a child with one more sibling whose parent experiences job loss will experience an additional decrease of 0.42 (0.26) percentage points lower probability of securing a Bagrut certificate (math).

Panel (b) illustrates the corresponding estimates on parents, focusing on employment and earnings by the years relative to the job loss event year (before, during, and after). Notably, positive effects on earnings and employment appear only for families with over three children, following a stepwise pattern post-job loss. For families with three or fewer children, no significant changes were observed, suggesting the reform predominantly impacts larger families.

For larger families, earnings experienced a 5-12% increase post-reform, albeit with substantial confidence intervals. Employment showed a significant effect of almost 8 percent only in the first year post-job loss and only for families with at least five children.

The estimates from the year of job loss and the previous year provide a placebo test. Given our focus on previously employed workers, the employment impact before displacement is, by definition, zero. The pre-displacement earnings impact is near-zero, except for slight but significant effects for smaller families.

Next, we turn to estimate the monetary value of cash transfers in the first year after job loss on families that experience job loss. We map the estimates presented above to the approximate cash transfer drop in Figure 7. We do so by assigning to each family size category their yearly total of child benefits change due to the 2003 reform in thousands of New Israeli Shekels, as shown in Table 2, denoted B_i. Note that variation in B_i is driven almost entirely by the number of children at job loss year and their birth years, as described in Section 2, but that the actual benefits also depend on the date of birth of every child.[27]

Figure 7 provides a visual IV of the relationship between the change in family benefits and the impact of the reform on the effects of job loss, as if we treat the number of children in the household as an instrument for the change in benefits. The slope of the linear fit describes, therefore, the relationship between government transfers and children's high school outcomes and parents' labor market outcomes. In Panel (a), we show that a decrease of 1,000 Israeli Shekels in annual benefits received implies a decrease of 0.15 percentage points in the probability of securing a Bagrut certificate and a decrease of 0.08 percentage points in the probability of taking the math exam and score above median among families that experience a parental job loss. From Table 3, the average job loss penalty before the reform was 1.3 (1.4) in the Bagrut (math), meaning that every 1,000 Shekels lost increases the effect of job loss by 11.5% (6%) on average.

In Panel (b), we conduct the same exercise on displaced parents' earnings and employment one year after displacement. Imposing this linear relation, the fitted line has a slope of almost one percentage point in earnings and half a percentage point in employment. This implies that a decrease of 1,000 Shekels generates an increase of 1 percent in earnings and 0.5 percent in employment, illustrating that the reform did generate an income effect among parents and

[27] For the purposes of this exercise, this is a good approximation. In our analysis to follow, we assign each child and parent their accurate benefits according to all the relevant information.

increased, to some extent, their labor supply. The impact of job loss before the reform one year after displacement is 47 percent on earnings and 27 percent on employment (see Table 5). Thus, every 1,000 Shekels lost in the reform translate to around 2% smaller job loss effect on both earnings and employment.

7.3 Triple Difference Estimation: The Shekel-Value Effects

The goal of this section is to estimate the Shekel value of the reform on the impact of job loss. We do so by assigning to each household their yearly total of child benefits for each year in our firm events period, as if they had lost their jobs in that year, keeping household characteristics fixed to their job loss year. For example, for a parent who lost her job in 2005, we assign the child benefits they would have received if the event happened in 2001 and 2004, given their 2005 characteristics. We define $B_i = Benefits_{i,2001} - Benefits_{i,2004}$ to be the potential loss of child i due to the 2003 reform in thousands of New Israeli Shekels. Note that variation in B_i is driven almost entirely by the number of children at job loss year and their birth years, as described in Section 2, although here we also incorporate the information about the year and month of birth of each child in the family, for a more accurate account of the transfers received.

We rely on the results above and combine them in a triple difference model. We impose additional structure to the analysis of a linear relationship between the effects and the loss in cash transfers and use the following regression specification:

$$\begin{aligned} y_i =& \alpha + \beta_1 After_i^{2003} + \beta_2 B_i + \beta_3 D_i \\ &+ \beta_4 D_i \times After_i^{2003} + \beta_5 After_i^{2003} \times B_i + \beta_6 D_i \times B_i \\ &+ \varphi B_i \times After_i^{2003} \times D_i + X_i'\delta + \varepsilon_i \end{aligned} \tag{6}$$

where the outcome variables, y_i, represent both parental and child outcomes. The dummy variable $After_i^{2003}$ indicates whether the job loss took place post-2003, while D_i is a binary variable denoting job loss. B_i quantifies the potential benefit loss for household i due to the 2003 reform, measured in thousands of New Israeli Shekels. In our results, we also present the number of children under 18 in a household as an alternative for the benefit change variable B_i. A set of control variables (X_{it}) is also incorporated into the model, including pre-reform

parental earnings, the child's gender, ethnicity, and year of birth dummies for the parent or the child. The interaction term $B_i \times After_i^{2003} \times D_i$ captures the differential effect of the reform on families who experienced job loss, with the associated coefficient φ being our parameter of interest. Specifically, φ quantifies the impact of a yearly loss of approximately 1,000 Shekels in child benefits due to the reform on families experiencing job loss. Informed by the results from Section 6.2, we run these regressions stratified over family income levels as well.

Parents: Figure 8 presents the φ coefficient from Equation 6 on parents' employment and earnings for different years after the job loss year, separately by parents' earning levels. Each coefficient in the graph corresponds to a separate regression and measures the effect of the reform of the job loss penalty on employment and yearly earnings. The top panels present the effect of having an additional child at the time of job loss, while the bottom panels present the effect of lost benefits.[28] In line with the results in previous sub-sections, we find that the labor supply of low-income parents who experienced job loss responds positively to the number of children and lost cash transfers. That is, job-losing workers who experienced a higher loss in benefits returned to work sooner, and their total yearly earnings increased and remained higher compared to job-losing workers who did not lose benefits for at least four years after displacement.

From Panels (a) and (b), we learn that in low-income households, employment increased in the three years after job loss by around two percent for every additional child. That is the effects of job loss *shrink* by two percent for every additional child in the household. Similarly, the effects of job loss on earnings are reduced by around two to three percent in the first three years after the job loss event, although estimates are only marginally significant. For both outcomes, the estimates are close to zero in the long run. The corresponding effects on high-income families are negative and non-significant for the entire post-job loss period.

Correspondingly, Panels (c) and (d) present the effects of 1,000 Shekels in benefits lost on the effects of job loss. We find that the effects of job loss on employment for low-income parents are one percent smaller for every 1,000 Shekels in benefits lost due to the reform in the first year after job loss, with a longer-term effect that levels at around 0.5 percent. The effects of job loss on earnings are 1.2 percent smaller for every 1,000 Shekels. These effects dim down four

[28] Note that discrepancies between the results using the number of children and lost benefits as the interaction term in the regression stem from several sources. First, the functional form of the effects with respect to the different variables is different, as we show in Figures 6 and 7. Second, child benefits calculated in this section take into account additional information, namely the age of each child in the family, which affects the benefits as described in Table 2.

years after job loss to around 0.8 percent. The effects on high-income parents are all null in the first few years after job loss. Alas, we do estimate significant negative effects in the longer run, above four years after job loss on employment, which is an intriguing suggestive result that invites further investigation.

Given the average benefits level of around 10,000 Shekels in 2001 for low-income families, and with job loss impact pre-reform of 47 percent on earnings and 27 percent on employment (see Table 5), we can calculate the elasticity of the effect with respect to benefits. An estimate of 1.2 or earnings translates to an elasticity of 0.25, and an effect of 1 percent on employment translates to an elasticity of 0.37.

Children: Table 9 presents the effect of the reform on the impact of job loss on education outcomes, the φ coefficient from Equation 6. Panel (A) of Table 9 presents the triple difference estimate of the change in the effect of job loss after 2003, for a child with one more sibling, separately by income level. We find that, on average, one more child in the family increases the job loss penalty by almost half a percent in the Bagrut and by a smaller increase of 0.37 percentage points to the penalty in Math. Interestingly, echoing the finding in Section 6.2, we find that the whole penalty is driven by low-income families. The effect on children from below-median income households, the effect of one more sibling on the job loss penalty is almost 1.5 percentage points on the Bagrut and one percentage point in Math. This result provides further evidence in the direction that children from low-income families are especially vulnerable to parental career shocks, maybe because their parents face tighter financial constraints. Therefore, government transfers play a bigger role in insuring these children in the event of a negative parental shock.

Panel (B) of Table 9 presents the effect of lost benefits in 1,000 Shekels on the effects of job loss. These results mimic the exercise presented in Panel (a) of Figure 7 by calculating the linear relation between the change in cash transfers induced by the reform on the effects of job loss.[29] We find that while in the full sample and for the high-income families, the effect of a 1,000 NIS increase is qualitatively small with coefficients that are generally statistically insignificant from zero, we find that the impacts among the low-income families are substantial. The effect of

[29]Differences between the slope in Figure 7 and this analysis stem from three main differences: (1) Figure 7 presents a simple fitted line on four points with equal weights, (2) child benefits assigned here take into account the date of birth of each child in the family, making it more accurate, and (3) in this analysis we add controls, which, however, do not alter the results dramatically.

every 1,000 NIS ($\approx$ \$300) lost in benefits in the first year of job loss is a half percentage point lower probability of obtaining a Bagrut certificate and 0.2 percentage points lower performance in the math exam. These values, given the average amount of benefits lost ($\approx 4,000$ Shekels), amount to half of the effects of job loss on the Bagrut and a third of the effect of job loss on math. The corresponding elasticities of the effect of job loss with respect to the change in the level of benefits are 1.3 for Bagrut attainment and 0.9 for math.

Note that we do not find that the reform has any impact on the job loss penalty on children from above median earnings families in line with the results for the effect of job loss on children across income groups.

As a placebo exercise, Table 10 presents the results of a regression based on Equation 6, with the difference that instead of child benefits, we use the unemployment insurance change brought about due to the reform as B_i. Details on the changes made in the reform to unemployment insurance are in Appendix B, as well as details on how we simulate the unemployment insurance amounts (see Appendix B.4). In general, the main changes to UI due to the reform were substantial but were mainly made to the eligibility employment duration conditions rather than to the amount provided. Since job loss workers in our sample worked at least three years consecutively before the mass layoff, they are all eligible for similar amounts regardless of the reform and the timing of job loss. As can be seen in Table 10, we do not detect any effect for the changes in potential unemployment insurance, which is reassuring that the effects we find are indeed the result of cash transfers.

Informed by the estimates of the effects of cash transfers on children, we use a simple back-of-the-envelope calculation to assess the potential insurance value of the change in child benefits in terms of the harm to children's education.

Based on our results, a child from a low-income family whose parents were laid off in an unexpected mass layoff event will experience a 0.0048 lower probability of attaining a Bagrut certificate per 1,000 Shekels in cash benefits of benefits. With a 13% Bagrut premium on income at adulthood estimated by (Angrist and Lavy, 2009). In our data, the average low-income family earns 60 thousand Shekels prior to job loss. Using the intergenerational elasticity of earnings of 0.25 estimated in Israel Aloni (2017), the average earnings of the child at around the age of 30 would be almost 80 thousand Shekels. Therefore, for every 1,000 Shekels in cash benefits

benefits lost, the child's earnings at age 30 would decrease by 52 Shekels per year. For the average benefits change in this population of 4,000 Shekels per year, this implies losing around 208 Shekels per year at adulthood on average.

7.4 Reform Effects Heterogeneity by the Gender of the Parent

Parental Labor Market Outcomes: Appendix Figure D.12 displays the results of the triple difference analysis conducted above for the effects of the reform of 1,000 Shekel lost in benefits on the effects of job loss, stratified by the gender of the parent. We find that the overall income effect parents experienced on earnings and employment stems from fathers, whereas the impact on mothers is noisy, with close to zero point estimates on employment and negative point estimates on earnings. This divergence is suggestive of gender-specific dynamics at play in the labor market, hinting towards possible challenges that mothers might face in their re-entry to the labor market after a job loss. This result could be consistent with a model in which mothers, rather than fathers, face a substitution effect in addition to the income effect as a result of the reform and the job loss and might find it more beneficial to spend more time at home providing care to their children rather than working. Alas, the results are only suggestive due to the large standard errors.

Children's Outcomes: The effects on children's education outcomes, presented in Appendix Table D.2 show substantial variation. For children of high-income fathers who lose their jobs, the effects are positive, albeit non-significant for both education outcomes, irrespective of whether we consider the effect of an additional child or the drop in cash transfers (B). For children of low-income fathers, however, our findings are consistent with our previous results: the more children in the family (or the larger the drop in cash transfers), the larger the adverse effects on children. Specifically, the effect of job loss results in a 0.3 percentage point drop in math scores and a more pronounced 0.86 percentage point drop in Bagrut attainment. The pattern observed for mothers is starkly different but also overly noisy, so that we cannot rule out that mothers and fathers have different impacts.

8 Conclusions

In this work, we study the effects of parental career shocks on children's education outcomes in Israel and show that cash transfers can substantially alleviate these adverse effects. Parental job loss hinders high school performance by just under ten percent in both the math exam scores and Bagrut attainment. This is expected to cause significant harm to these children, as the Bagrut certificate is a prerequisite in all universities and most academic institutions in Israel. In addition, we find that these effects are concentrated in the lower part of the income distribution.

Using a comprehensive policy change in the provision of child benefits in Israel, we show that reducing cash transfers to families can exacerbate the adverse effects that low-income children experience when their parents lose their jobs, along with increasing the labor market participation of low-income families after job loss. This suggests that cash transfers have an insuring role for low-income children against the educational setbacks associated with parental job loss.

Lastly, we calculate the total effect of the change in child benefits on the lifetime earnings of both parents and children of low-income families. Our analysis shows that for every 1,000 Shekels lost in government transfers, children's annual earnings in adulthood decreased by 52 Shekels.

Figures and Tables

8.1 Tables

Table 1: Descriptive statistics and balance table of displaced and non-displaced households

Variable	(1) Non-displaced	(2) Non-displaced (weighted)	(3) Displaced	(4) Difference
Panel A: Children				
Male	0.51	0.51	0.51	-0.00
	(0.50)	(0.50)	(0.50)	(0.01)
Child YOB	1986.18	1986.01	1986.00	-0.01
	(5.73)	(5.75)	(5.74)	(0.07)
Total # sibling	2.6	2.88	2.88	-0.00
	(1.71)	(1.98)	(1.91)	(0.02)
T=0 total # sibling	2.31	2.52	2.5	-0.00
	(1.45)	(1.67)	(1.65)	(0.02)
T=0 # sibling under 18	1.55	1.85	1.85	-0.00
	(1.22)	(1.23)	(1.24)	(0.02)
Father BA	0.52	0.46	0.46	0.00
	(0.75)	(0.70)	(0.71)	(0.01)
Mother BA	0.57	0.51	0.51	0.00
	(0.77)	(0.73)	(0.74)	(0.01)
Father displaced	0.55	0.64	0.64	0.00
	(0.50)	(0.49)	(0.49)	(0.01)
Tested in elementary Meitzav	0.11	0.11	0.11	-0.00
	(0.32)	(0.31)	(0.31)	(0.00)
Panel B: Parent				
Parent YOB	1957.94	1958.05	1958.06	0.01
	(5.58)	(5.63)	(5.64)	(0.06)
Arab	0.09	0.12	0.13	0.00
	(0.28)	(0.33)	(0.33)	(0.00)
FSU descendent	0.21	0.19	0.20	0.00
	(0.41)	(0.40)	(0.40)	(0.00)
Ethiopia descendent	0.02	0.03	0.04	0.00
	(0.12	(0.18)	(0.19)	(0.00)
Asia/Africa descendent	0.44	0.37	0.37	0.00
	(0.50)	(0.48)	(0.48)	(0.01)
Europe descendent	0.17	0.14	0.13	-0.01
	(0.37)	(0.34)	(0.34)	(0.00)
Native Israeli	0.17	0.19	0.18	-0.00
	(0.38)	(0.39)	(0.39)	(0.00)
t=-1 Yearly earnings	174,604	139,283	138,742	-541
	(213,982)	(234,691)	(200,212)	(2,485)
t=-2 Yearly earnings	169,333	117,781	119,240	1458
	(207,946)	(145,432)	(168,219)	(1,787)
t=-1 spouse monthly earnings	7,081	6,615	6,566	-48
	(17,145)	(15,532)	(11,406)	(154)
t=-2 spouse monthly earnings	6,899	6,366	6,410	44
	(13,782)	(9,997)	(10,702)	(117)
Panel C: Firm				
# of workers in firm	2,165	1,260	1,210	-49
	(3,421)	(2,773)	(2,549)	(30)
Mean age in firm	45.62	45.76	45.74	-0.01
	3.07	(3.10)	(3.04)	(0.03)
# of unique parents	376,330	355,104	13,095	368,199
# of children	462,853	436,078	16,063	452,141

Note: This table presents means and standard deviations of pre-displacement characteristics of children and parents during the sample period. Column 1 presents the statistics of the full sample. Column 2 presents the non-displaced workers, adjusted using inverse probability weighting. Column 3 shows the statistics for the displaced workers. Column 4 presents the difference between Columns 2 and 3, along with the corresponding standard errors. Earnings are measured in 2016 Israeli Shekels (ILS), worth approximately 0.3 US dollars. The weighted sample in Column 2 is trimmed to the common support of the predicted propensity score, hence the difference in observations between Column 1 and Column 2.

Table 2: Child benefits per child by year and by number of children, current ILS

Panel (A): Monthly benefits per child		Child number 1	2	3	4	5	6	7+
	1999	169	169	338	683	574	633	591
	2000	171	171	342	693	582	642	599
	2001	171	171	343	694	856	856	856
	2002	174	174	342	703	868	868	868
	Starting 7/2002	146	146	289	586	724	724	724
Born until 5/31/2003								
	Starting 8/2003	144	144	195	454	522	522	522
	2004	120	120	164	404	459	459	459
	2005	120	120	156	360	401	401	401
	2006	148	148	178	329	329	329	329
	2008	152	152	182	337	337	337	337
	2009	159	159	191	353	353	353	353
Born after 6/1/2003								
	Starting 8/2003	144	144	144	144	144	144	144
	2004	120	120	120	120	120	120	120
	2005	120	120	120	120	120	120	120
	2006	148	148	148	148	148	148	148
	2008	152	152	152	152	152	152	152
	2009	159	159	159	159	159	159	159

Panel (B): Total yearly benefits	# of children 1	2	3	4	5	6	7
Benefits in 2001	2,052	4,104	8,220	16,548	26,820	37,092	47,364
Benefits in 2004 (max.)	1,440	2,880	4,848	9,696	15,204	20,712	26,220
Benefits in 2004 (min.)	1,440	2,880	4,320	5,760	7,200	8,640	10,080
Min. potential loss	612	1,224	3,372	6,852	11,616	16,380	21,144
as % of avg. wage	**0.7%**	**1.3%**	**3.7%**	**7.5%**	**12.7%**	**17.9%**	**23.1%**
Max. potential loss	612	1,224	3,900	10,788	19,620	28,452	37,284
as % of avg. wage	**0.7%**	**1.3%**	**4.3%**	**11.8%**	**21.4%**	**31.1%**	**40.8%**

Note: This table outlines changes in child benefits by year and family size. Panel (A) illustrates the marginal benefits per child, varying by year and child's birth date, while Panel (B) provides the total annual benefits and potential losses in benefits, calculated as benefits2001 − benefits2004, differentiated by family size. Families with all children born prior to 2003 experience minimal benefit loss, whereas maximal benefit loss applies to families with all but one child born post-2003. Benefit losses are expressed as a percentage of the average wage, determined in relation to the total sum of the average monthly wage in the private sector in 2001 (**Source:** Bank of Israel, Data & Statistics). The benefits increase in 2001 for families with five children is due to the Halpert Law, enacted in January of that year, which temporarily raised the marginal benefit for the 5th child and beyond until its revocation approximately one year later.

Table 3: The impacts of job loss on children's education outcomes

	Bagrut certificate		Math score	
	(1)	(2)	(3)	(4)
Under age 18	-0.014	-0.037	-0.009	-0.022
	(0.006)	(0.006)	(0.006)	(0.007)
Above age 18	0.001	0.001	-0.002	-0.002
	(0.006)	(0.008)	(0.005)	(0.0073)
Under age 18 X high inc.		0.044		0.031
		(0.012)		(0.012)
Above age 18 X high inc.		0.001		-0.02
		(0.13)		(0.13)
Adj. R sq.	0.15	0.16	0.12	0.12
Observations	452,141	452,141	452,141	452,141
Num. Treated	16,063	16,063	16,063	16,063

Note: This table presents the effects of parental job loss on children's education outcomes, by child age at the time of job loss, estimated via Equation 1. Columns (1) and (2) present the effects on Bagrut certificate attainment and columns (3) and (4) present the effect on taking the matriculation math exam and scoring above the median grade. In columns (2) and (4) we introduce an interaction between job loss before age 18 and a dummy for parents' income above the median, altering the interpretation of the first row coefficients from the aggregate effect of job loss to the effects of job loss on children from lower-income families. Standard errors clustered at the parent level are in parenthesis.

Table 4: The effects of job loss on early standardized exams scores, pre job loss

	5th Grade	8th Grade
	(1)	(2)
Before the exam	-0.031	-0.024
	(0.031)	(0.033)
After the exam and below 18	0.012	0.019
	(0.037)	(0.04)
Above 18	-0.002	-0.022
	(0.076)	(0.061)
# of treated obs.	1,867	2,614
Adj. R Squared	0.312	0.523

Note: This table shows the impact of job loss on the composite score in the Meitzav standardized exams, for the subset of students who experienced job loss before taking the exams. These exams are administered to a subset of schools in grades 5 or 8 (ages 11 or 14), as detailed in Section 4. The rows denote the children's ages at the time of their parent's job displacement. Observations are of children affected by job loss. Standard errors clustered at the family level are in parentheses.

Table 5: Difference in differences tables: parents' outcomes 1 year after job loss

Earnings rel. to pre-displacement

	Before 2003	After 2003	Diff.
Control	1.023	1.020	-0.003
			(0.009)
Laid off	0.555	0.555	0.000
			(0.011)
Difference	-0.469	-0.465	0.004
	(0.011)	(0.008)	(0.014)

Employment status

	Before 2003	After 2003	Diff.
Control	0.961	0.961	0.001
			(0.004)
Laid off	0.690	0.702	0.012
			(0.009)
Difference	-0.271	-0.259	0.012
	(0.008)	(0.005)	(0.009)

Note: These tables present the means and differences of job losers and the non-displaced control sample, weighted by the inverse probability weights, before (up to the year 2002) and after the reform (2003 onwards), one year after the job loss. Clustered standard errors at the family level are in parentheses.

Table 6: Difference in differences tables: parents' outcomes 1 year after job loss, by number of children

Earnings, up to 3 children

	Before 2003	After 2003	Diff.
Control	1.028	1.023	-0.005
			(0.010)
Laid off	0.577	0.565	-0.013
			(0.012)
Difference	-0.451	-0.459	-0.008
	(0.013)	(0.009)	(0.016)

Employment, up to 3 children

	Before 2003	After 2003	Diff.
Control	0.961	0.960	-0.001
			(0.004)
Laid off	0.703	0.710	0.007
			(0.009)
Difference	-0.258	-0.250	0.007
	(0.008)	(0.006)	(0.010)

Earnings, 4 kids or more

	Before 2003	After 2003	Diff.
Control	0.994	1.000	0.007
			(0.021)
Laid off	0.421	0.502	0.080
			(0.025)
Difference	-0.572	-0.499	0.074
	(0.026)	(0.021)	(0.033)

Employment, 4 kids or more

	Before 2003	After 2003	Diff.
Control	0.958	0.967	0.008
			(0.009)
Laid off	0.611	0.658	0.048
			(0.023)
Difference	-0.348	-0.308	0.039
	(0.021)	(0.014)	(0.025)

Note: These tables present the means and differences of job losers and the non-displaced control sample separately by the number of children under age 18 in the family, weighted by the inverse probability weights, before (up to the year 2002) and after the reform (2003 onwards), one year after the job loss. Clustered standard errors at the family level are in parentheses.

Table 7: Difference in differences tables: children's outcomes

Bagrut certificate

	Before 2003	After 2003	Diff.
Control	0.557	0.553	-0.004
			(0.005)
Laid off	0.544	0.520	-0.025
			(0.014)
Difference	-0.013	-0.033	-0.020
	(0.010)	(0.010)	(0.014)

Math score

	Before 2003	After 2003	Diff.
Control	0.388	0.363	-0.025
			(0.004)
Laid off	0.375	0.332	-0.043
			(0.012)
Difference	-0.014	-0.031	-0.017
	(0.009)	(0.009)	(0.013)

Note: These tables present the difference in differences tables for children whose parents were displaced when they were under the age of 18 compared to the inverse probability weighted control sample, before (up to the year 2002) and after the reform (2003 onwards). Clustered standard errors at the family level are in parentheses.

Table 8: Difference in differences tables: children's outcome, by number of children

Bagrut certificate, 1 or 2 children

	Before 2003	After 2003	Diff.
Control	0.604	0.591	-0.013
			(0.006)
Laid off	0.589	0.566	-0.023
			(0.017)
Difference	-0.015	-0.025	-0.010
	(0.012)	(0.013)	(0.018)

Math score, 1 or 2 children

	Before 2003	After 2003	Diff.
Control	0.419	0.385	-0.033
			(0.005)
Laid off	0.389	0.341	-0.048
			(0.016)
Difference	-0.030	-0.045	-0.015
	(0.012)	(0.013)	(0.017)

Bagrut certificate, 3 kids or more

	Before 2003	After 2003	Diff.
Control	0.516	0.511	-0.005
			(0.007)
Laid off	0.501	0.468	-0.033
			(0.020)
Difference	-0.015	-0.043	-0.028
	(0.015)	(0.016)	(0.021)

Math score, 3 kids or more

	Before 2003	After 2003	Diff.
Control	0.362	0.329	-0.033
			(0.006)
Laid off	0.362	0.307	-0.055
			(0.017)
Difference	0.000	-0.022	-0.022
	(0.013)	(0.013)	(0.018)

Note: These tables present the difference in differences tables, separately by the number of children in the family, for children whose parents were displaced when they were under the age of 18 compared to the inverse probability weighted control sample, before (up to the year 2002) and after the reform (2003 onwards). Clustered standard errors at the family level are in parentheses.

Table 9: Impact of reform on intergenerational job loss effects, by income levels

		Math score	Bagrut	Obs.
(A): Num. children	Entire sample	-0.0037	-0.0046	197,951
		(0.0031)	(0.0027)	
	Above median income	-0.0014	-0.0016	104,446
		(0.0026)	(0.0035)	
	Below median income	-0.0097	-0.0144	93,505
		(0.0047)	(0.0044)	
(B): Benefits lost	Entire sample	0.0005	-0.0007	197,951
		(0.0006)	(0.0006)	
	Above median income	0.0012	0.0005	104,446
		(0.0007)	(0.0007)	
	Below median income	-0.0020	-0.0048	93,505
		(0.0011)	(0.001)	

Note: This table presents the β_7 coefficient from a triple difference regression described in Equation 6. It presents the impact of the reform on children's education outcomes as a consequence of parental job loss, segmented by household income levels (above and below the median). We present two analyses in this table: Panel (A) shows the effects based on the number of children in the family introduced linearly to the regression, and Panel (B) examines the effects based on the benefits lost due to the reform, calculated as the benefits after the reform (2004) minus the benefits before the reform (2001) in thousands of Israeli Shekels in yearly terms. In each case, the effects on math scores and Bagrut certificate attainment are shown for the entire sample and for the above/below median income categories. Standard errors clustered at a family level are in parentheses, while the number of observations is provided in the last column.

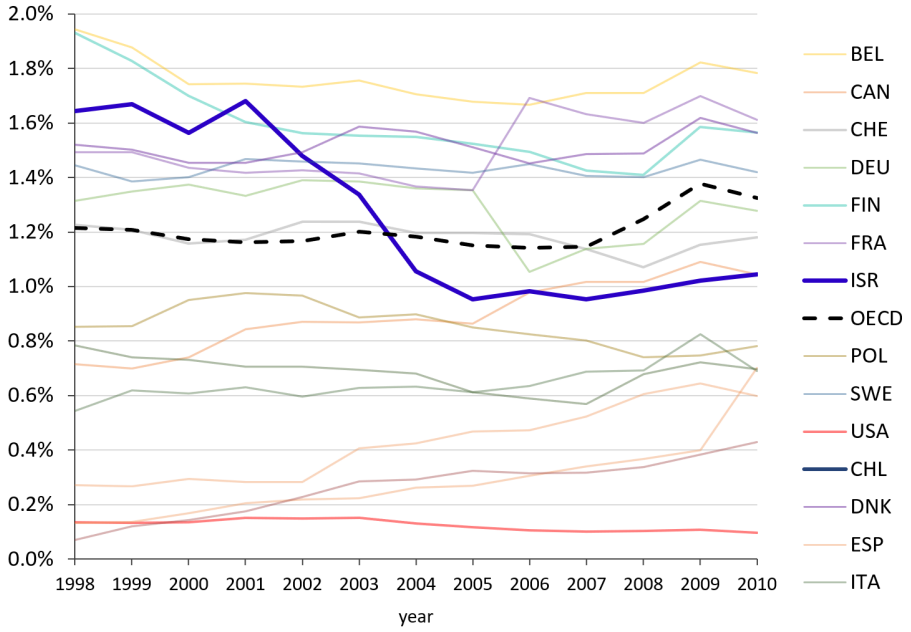
Table 10: Robustnes test: the impact of UI drops on intergenerational job loss effects

	Bagrut	Math score	Obs.
(A) Entire sample	0.0015	0.0068	197,951
	(0.0158)	(0.0142)	
(B) Below median income	0.0059	0.004	93,505
	(0.0256)	(0.025)	
(C) Above median income	0.0068	0.0106	104,446
	(0.0204)	(0.0178)	

Note: This table presents the β_7 coefficient from a triple difference regression outlined in Equation 6, illustrating the impact of UI (Unemployment Insurance) drops on children's education outcomes following parental job loss, post-2002, simulated at the family level as described in Appendix Section B. We present the coefficients segmented by the entire sample and further divided by household income levels (below and above median income). Within each segment, outcomes for children under 18 and above 18 at the time of the reform are presented. The impact on Bagrut certificate attainment and math scores is shown for each category. Standard errors clustered at a family level are shown in parentheses, and the number of observations is provided in the last column.

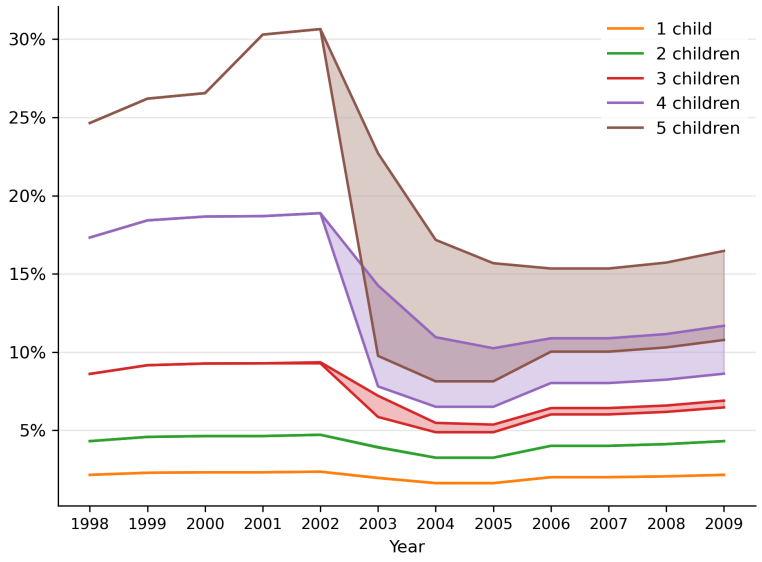
8.2 Figures

Figure 1: Public spending on family cash benefits, % of GDP



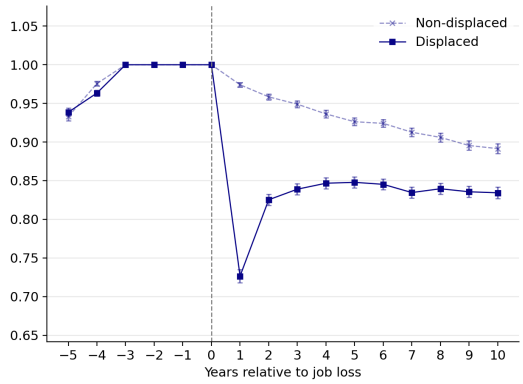
Note: This figure presents public spending on family cash benefits for families with children, as a percent of GDP. Family cash benefits include child-related cash transfers (such as child allowances and income support for sole-parent families), public income support payments during periods of parental leave, and financial support provided through the tax system (including tax exemptions and child tax credits).
Source: OECD (2022)

Figure 2: Child benefits as a percent of the average monthly earnings in 2001

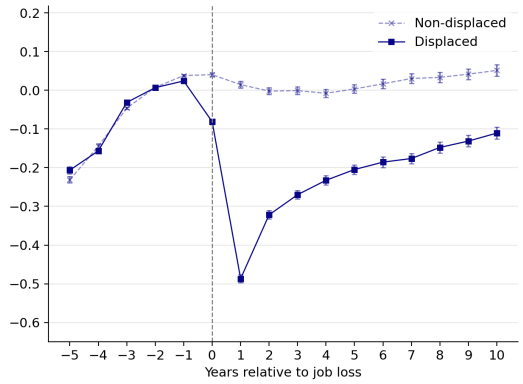


Note: This figure displays the total child benefits each year, expressed as a percentage of the average wage in 2001 (pre-reform year). The shaded regions post-reform reflect the variability in benefit amounts, contingent on the birth date of each child in a family.

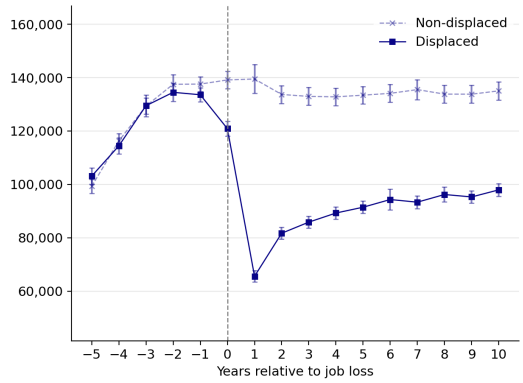
Figure 3: The impact of job loss on household income and parents' employment



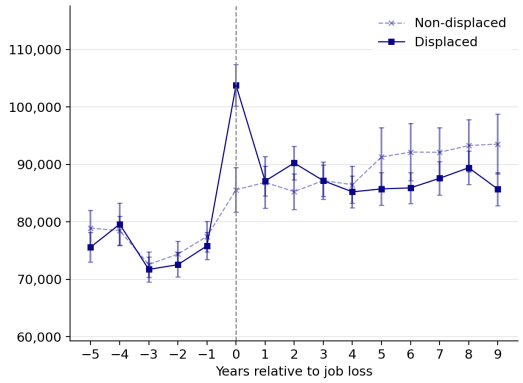
(**a**) Employment status



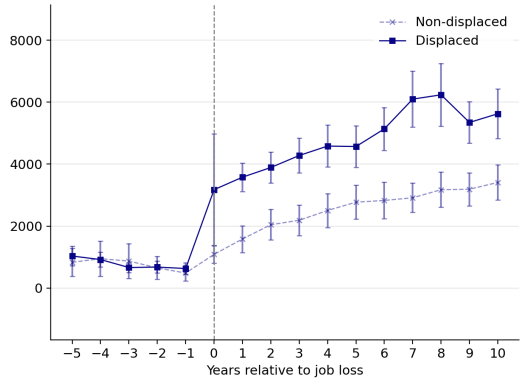
(**b**) Yearly earnings relative to $t = -1$



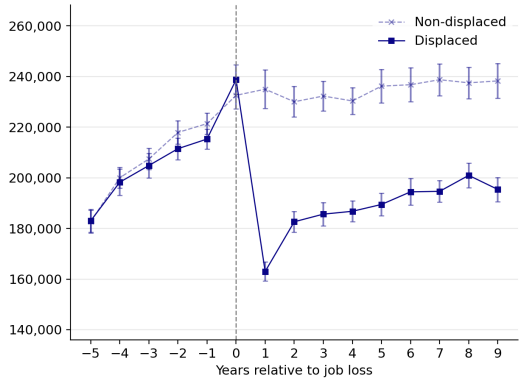
(**c**) Earnings (1ILS $\approx 0.3\$$)



(**d**) Spouse earnings (1ILS $\approx 0.3\$$)



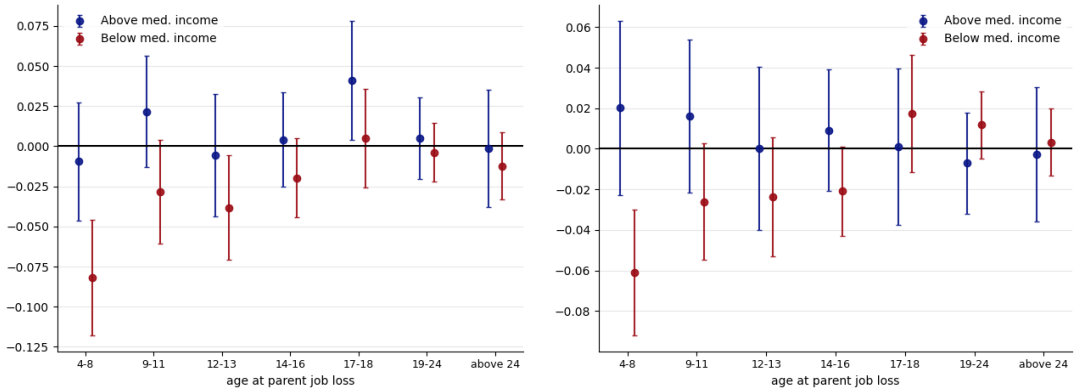
(**e**) Self-employment earnings (1ILS $\approx 0.3\$$)



(**f**) Total household earnings (1ILS $\approx 0.3\$$)

Note: These figures display the impact of job loss on parental labor market outcomes, illustrating the raw means of the treatment and reweighted control groups. Figure (a) shows the average employment status, defined as earning above 10,000 ILS (approx. 3,000 USD) during the year as an employee. Figure (b) presents yearly earnings relative to the average of $t = -1, -2, -3$. Figure (c) presents earnings in Israeli Shekels. Figures (d), (e), and (f) present the spouse's earnings, self-employment earnings, and total household income, respectively. Total household income includes earnings from both employment and self-employment of both parents.

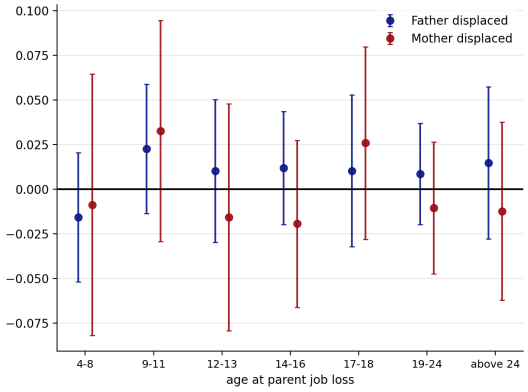
Figure 4: The impacts of job loss on children's education outcomes, by child's age at the time of job loss and by household income



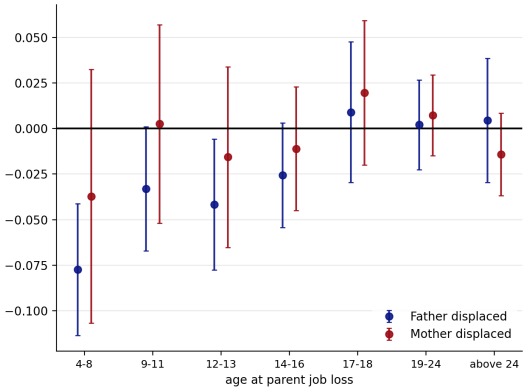
(**a**) Matriculation certificate (**b**) Math score

Note: This figure presents the effects and confidence intervals of parental job loss on children's education outcomes by the age of the child at the year of parental job loss following equation 1. Panel (a) presents the effects on the attainment of the Bagrut certificate and Panel (b) presents the effects on the probability of taking the math exam and achieving a score above the median grade. Each figure is divided by earning levels, determined using the median total parental earnings prior to displacement. Bars indicate 95% confidence intervals based on standard errors clustered at the parent level.

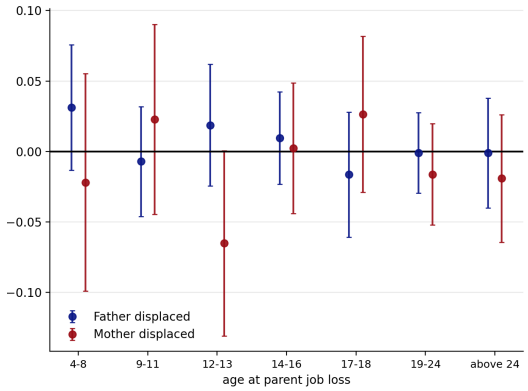
Figure 5: The impacts of job loss on children's education outcomes, by child's age at the time of job loss and by parent's gender



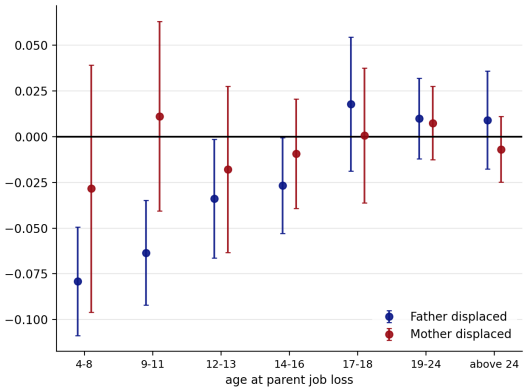
(**a**) Bagrut certificate, high-income



(**b**) Bagrut certificate, low-income



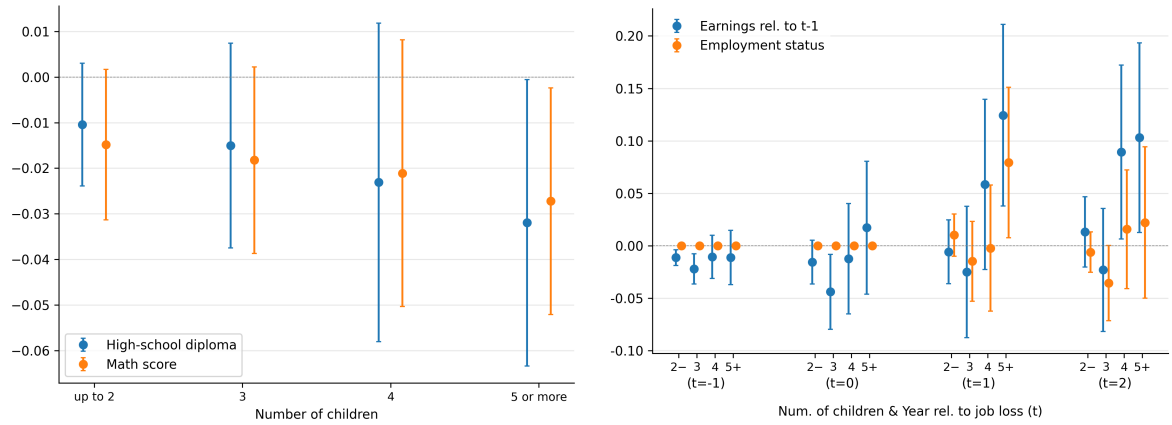
(**c**) Math score, high-income



(**d**) Math score, low-income

Note: This figure shows the effects of parental job loss on children's educational outcomes, segmented by the child's age at the time of displacement, estimated via equation 1, separately by the gender of the parent who was affected by job loss. The top panels present the effects on Bagrut certificate attainment, and the bottom panels present the effects on the probability of taking the math matriculation exam and scoring above the median grade. The left panels, (a) and (c), present the effects on children from high-income families defined by the total parental pre-displacement earnings median, while the right panels, (b) and (d), present the effects on low-income families. Bars indicate 95% confidence intervals based on standard errors clustered at the parent level.

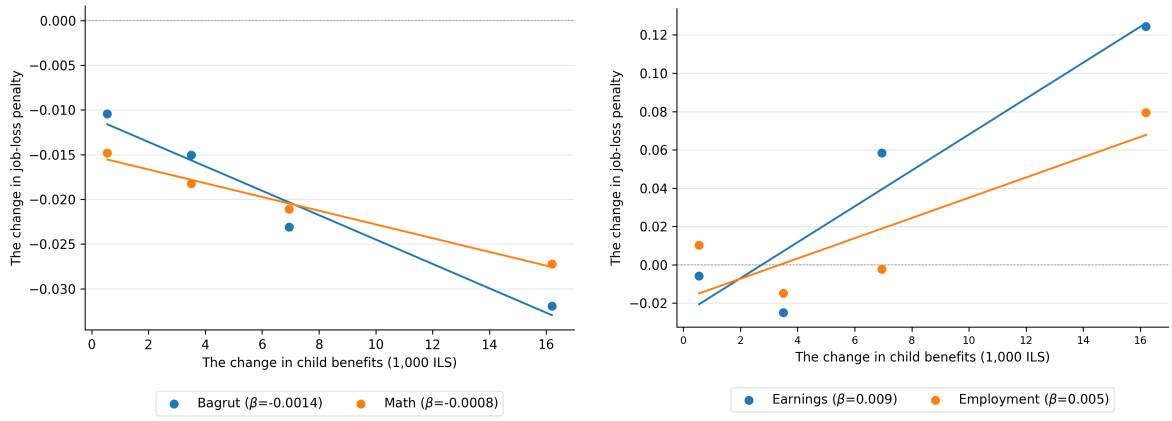
Figure 6: The change in job loss penalty after the reform by the number of children in the household



(**a**) Effects on children (**b**) Effects on parents

Note: This figure presents the β_t coefficients from equation 5 of the change in the effect of job loss after the reform relative to before the reform separately by the number of children in the household that are under age 18. Panel (A) presents the effects on children's education outcomes for the sample of children that experience parental job loss before age 18. Panel (B) presents the effects on parental labor market outcomes, separately for different periods before and after the job loss event. As all parents were employed at $t = -1, 0$, these coefficients are zero. Confidence intervals at the 0.95 level are based on standard errors clustered at the parent level in Panel (A) and at the firm level in Panel (B).

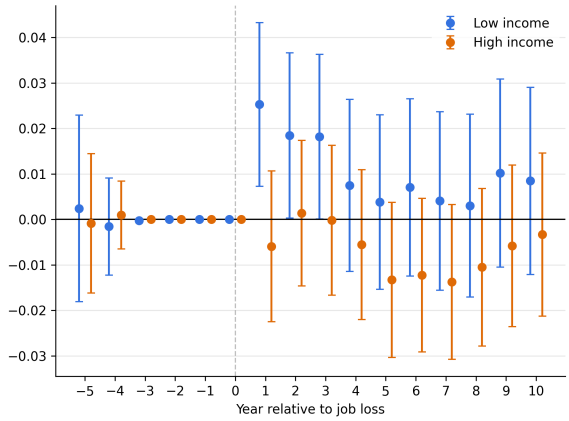
Figure 7: Visual IV of the effect of the reform on the impacts of job loss



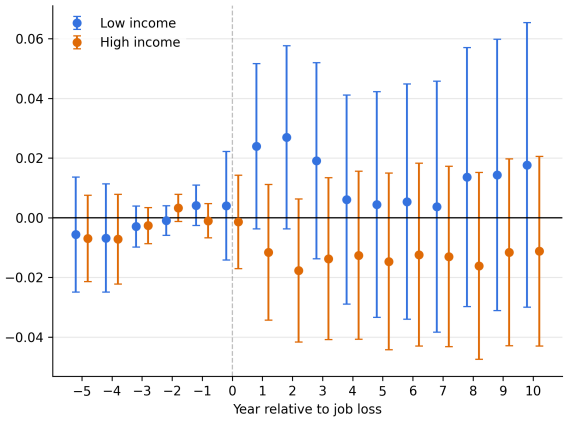
(**a**) Effects on children (**b**) Effects on parents

Note: This figure presents the β_t coefficients from equation 5, depicting the change in the impact of job loss after the reform relative to before the reform for the sample of children that experience a parental job loss before age 18. The coefficients are plotted against the average change in child benefits for each number of children in the household. We plot the effects on children in Panel (A) and the effects on parents in Panel (B). The slopes indicated in the legends are of the linearly fitted lines to the 4 points in the plot.

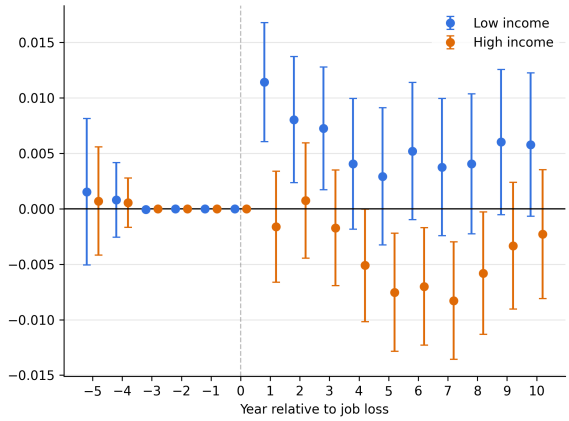
Figure 8: Impact of reform on parents' labor market job loss effects, by income levels



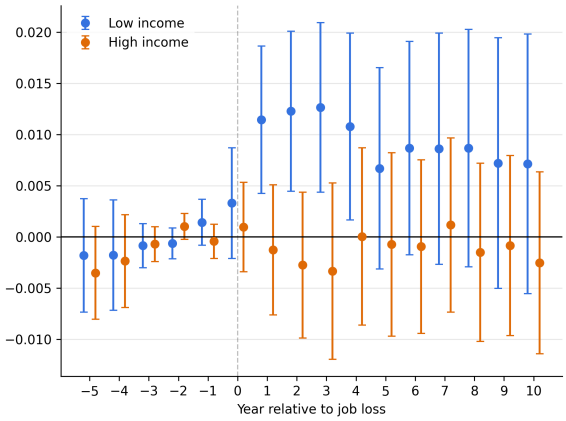
(**a**) Employment status, # of children



(**b**) Yearly earnings, # of children



(**c**) Employment status, benefits loss



(**d**) Yearly earnings, benefits loss

Note: This figure presents the β_8 coefficient from the triple difference regression described in equation 6 for the effect of the reform on the effect of job loss on employment and yearly earnings of displaced parents. The three differences in these regressions are the timing of job loss, the incidence of job loss, and either the number of children in the family (top panels), or the loss in child benefits due to the reform (bottom panels). The change in benefits is calculated as the benefits after the reform (2004) minus the benefits before the reform (2001) in thousands of Israeli Shekels in yearly terms. Regressions are estimated by income levels, defined by pre-displacement earnings. Panels (a) and (C) present the effect on employment, defined as earning at least 10,000 ILS a year from work as an employee, and panels (b) and (d) present the effect on yearly earnings relative to the earnings in the three years before job loss. Bars indicate 95% confidence intervals based on standard errors clustered at the parent level.

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A Defining Firm Mass Layoff Events and Worker Job Displacement

Differentiating between workers who are involuntarily fired and workers who leave a firm due to any other reason is not immediate, given that we do not explicitly observe separation reasons, as is the case in all comprehensive administrative data.[30] Therefore, we build on previous work and focus on mass layoff events, where for reasons that are not worker-specific, firms either have to let go of a substantial fraction of their workers or close altogether. We closely follow the literature to detect such events in the data. Specifically, we rely on Schmieder et al. (2022), who themselves utilize the common methods established in the literature that use administrative data, to detect mass layoff events and worker displacement.

We define a mass layoff event in the data if a firm id disappears from the records in a certain year between 1999 and 2009 and does not appear again, or if between two consecutive years, there is a drop of at least 30% in full-job equivalent worker count in the firm, and this drop is not offset a year later. We restrict our attention to firms with at least 40 workers at the year of the event since larger firms exhibit higher stability in worker count, and large ones are more likely to capture mass layoff events. For example, in a firm of 3 workers, a drop of a third in the number of employees cannot convincingly be independent of worker characteristics. Another concern when using administrative data is that some firms appear to close, but in fact undergo mergers, outsourcing, or changes to firm identifiers. To address this concern, we examine worker flows from each firm that was closed to all other firms. If more than 20% of the workers from the closing firm are employed in another firm that keeps existing in the year following the event, we omit it from the analysis. In addition, we exclude workers who worked in the following sectors prior to their displacement: mining, public administration and health, activities of private households and extra-territorial organizations, and industries that are led by government-owned companies.[31]

The displaced workers' group includes all workers that leave a firm that undergoes a mass layoff event in the same year. We consider tenured workers, who work at the firm for at least

[30] It has been shown, however, that this approach yields very good results in terms of the effect size of job loss on workers (Flaaen et al., 2019).

[31] Specifically, we omit workers from the following industries: B - Mining and quarrying, O - Local administration, public administration and defense; compulsory social security, P - Education, Q - Human health and social work activities, R.91 - Libraries, archives, museums, and other cultural activities, T - Activities of households as employers; undifferentiated goods- and services-producing activities of households for own use, U - Activities of extraterritorial organizations and bodies, H.51 - Air transport

three consecutive years for at least 10 months each year, and who do not work in that firm again after the separation event. This job stability condition is implemented in almost all papers that document the persistent scarring effects of job loss (Bertheau et al., 2022). There are two main reasons to include this condition: (1) decreasing separation hazard rate with tenure implies unexpected displacement, supporting the identifying assumption that displacement is involuntarily and orthogonal to unobserved worker ability, and absent the mass layoff event, workers would have remained employed in these firms, and (2) tenured workers stand to lose more when displaced, due to, e.g., firm-specific accumulated human capital, or higher match quality, which lead to larger effects estimated (for a discussion on this see e.g. Lachowska et al. (2020)).

To construct the sample of non displaced workers group, we consider all workers who do not belong to the above population of displaced workers at any period in the data. We apply the same conditions above, such as on tenure, earnings, and sectors restriction. The only difference in sample restrictions between the displaced and non-displaced is that these workers do not work at firms that we tag as mass-layoff or closing firms. This implies that we do not consider "survivors" of firm-level events in our comparison group. Each year in the data for which a non-displaced worker follows the conditions on tenure, earnings, etc., is a potential job loss year. Finally, note that if a non-displaced worker has K potential job loss years, she enters the non-displaced pool of workers separately K times. We, therefore, weight the data by K.

B Reform Details

In this appendix, we document the changes made in the Israeli welfare system during our sample years and refer to the sources that were used to create the benefit transfers simulation described in Appendix B.4. The changes to eligibility criteria and benefit sizes are complex and not readily available from a single source. To trace these changes accurately, we turn to several different sources: the current laws and archive records of law updates, policy reports from the Knesset (the legislative authority in Israel), archived versions of the National Insurance Institute of Israel (NII) website using web.archive.org going back to 1998, and the help of current and former employees of the NII.

B.1 Children Benefits

The full schedule of children's benefits, along with the changes made in the reform are described in the paper.

B.2 Unemployment Insurance

Unemployment insurance in Israel is given to workers who were laid off, for the period of their unemployment and up to a maximum period that changes with age and number of dependants. The most significant changes to unemployment insurance were to eligibility conditions, and especially to the qualifying period. That is, government expense reduction was mainly achieved in the extensive margin, reducing the size of the eligible unemployed population. Before the reform, a worker was required to work either 6 months out of 12, or 9 months out of the 18 months preceding his unemployment. After July 2002, the qualifying period was changed to 12 out of 18 months only. In addition, all benefits had an additional reduction of 4% after calculation. Lastly, the rates by which the benefit is calculated from the previous wage were changed, along with the maximum eligibility period.[32]

These determining rates, r_1, r_2, r_3, r_4, are used to calculate the benefit in the following manner. Of her wage up to half of the average wage, the recipient receives r_1 times her wage. Of her wage from half and up to three-quarters of the average wage, the recipient receives r_2 times

[32] Previous wage is calculated as the average of the 3 last monthly paychecks.

that amount. Of her wage from three-quarters of the average wage to the average wage, the recipient receives r_3 times that amount. For the part of her wage from the average wage and up to 4 times the average wage, the recipient receives r_4 times that amount.

Before July 2002, the four rates depended on whether the average wage was below or above the average wage in the population, with $r_1 = 0.7, r_2 = 0.5, r_3 = 0.45, r_4 = 0.4$ For recipients with wages above the average wage, and $r_1 = 0.8, r_2 = 0.5, r_3 = 0.45, r_4 = 0$ for those below. The sharp threshold at the average wage created a distortion in which a person making one dollar above the average wage could receive a 10% smaller benefit. After the reform, this dependency was eliminated, with $r_1 = 0.8, r_2 = 0.5, r_3 = 0.45, and r_4 = 0.3$ for all UI recipients.

UI durations have changed several times over the sample period. The duration depends on the number of dependents in a family, and age, and ranged between 100 to 175 days before July 2002, and over the course of the years changed to as low as 50 days for the younger recipients.[33] Its length remained constant for older recipients. Starting in 1999, in the "second period" of the UI, which starts after the first 125 days of the UI period, the equivalent of 5 months, the UI amount is reduced to two-thirds of the original amount.

B.3 Income Support

Income support is a welfare support program for families with low or no income. The main benefit of this means-based program is a monthly transfer that is not limited in time. In addition to money transfers, recipients can also receive other in-kind benefits that include discounts on electricity, telephone, and municipal tax bills, rent and mortgage assistance, and other benefits such as discounts for public transportation.[34] There are three main conditions for receiving the transfers - (a) living in Israel for a minimal period, currently, 12 months, (b) monthly earnings from work, capital income, and some of the SSI benefits, such as UI but not child benefits, are below a threshold, and (c) the requester exhausted his labor market possibilities.[35] In addition,

[33] Counted dependents are each child under 18 years old, and a male spouse if he is above 70 or if he has work income of under 57% of the avg. wage and is above 50 years old, or a female spouse that has work income of under 57% of the avg. wage.

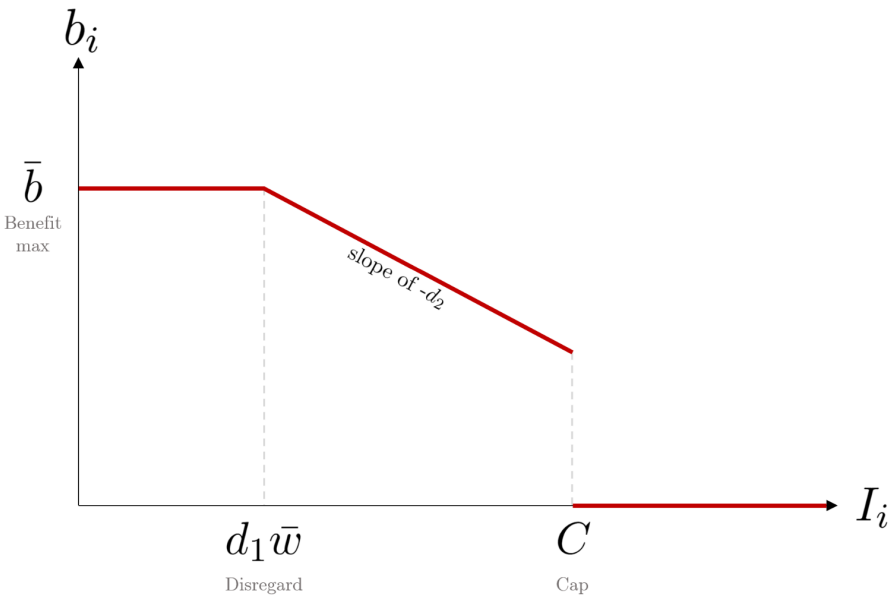
[34] Shanan (2020) estimates these in-kind transfers at around 25,000 ILS per year on average.

[35] This condition implies that the recipient must be enrolled in the Employment Office, report to it on a weekly basis, and not refuse reasonable job offers that match the applicant's health, prior occupation in the last 3 years, or level of expertise and education, located up to 60km from his residence location, and that the wage offered is above the UI amount that the recipient would receive if he were eligible for it. In some cases, this condition does not apply, e.g. for women above the age of 60 and men above 65, or single parents with a child under 2, and also specific cases such as drug addiction, recent jail release, or disability.

owning assets such as a car or real estate may revoke eligibility.

Given eligibility to the benefit, the transfer, b_i, is determined with the following parameters: a determining cap, C, a maximum transfer amount $\bar{b}$, the average wage, $\bar{w}$, a disregard $d_1 \in [0, 1]$, a type-specific slope parameter $d_2 \in [0, 1]$, and the individual income I_i.[36] It is equal to $\bar{b}$ for incomes under the disregard $d_1\bar{w}$, capped at C, and otherwise calculated as $b_i = \bar{b} - d_2(I_i - d_1\bar{w})$. See the illustration below.

Figure B.1: Income support benefit illustration



Note: This figure illustrates the schedule of income support benefit amount, b_i, with respect to total family income I_i in red, and its dependence on the different parameters that changed during the reform.

All of the policy parameters described above, C, $\bar{b}$, $d_1, d_2, \bar{w}$, change over the sample period and depend on age, marital status, and the number of children (0/1/2 or more). We have traced these changes across all recipient types, as we describe below. There were several changes that were implemented in the 2002-03 reform and in other years, that constrained both eligibility criteria and reduced transfer amounts. Here we highlight the main changes during the sample years:

- The benefit max ($\bar{b}$), was reduced by 4% starting July 2002.
- Until May 2003, individuals above age 46 received an enlarged benefit max ($\bar{b}$), of about

[36] The average wage was defined by the actual average wage until it was changed to a "base rate" which diverged from the average wage in 2006.

25% higher than those under age 46. Starting June 2003, the enlarged rate was paid to all those that were being paid the benefit and are 46 and above, or if their age is above 55. That is, the age required for the enlarged amount was increased from age 46 to 55.

- Single parent rates were reduced by 20% to those under age 55 in June 2003.
- The average wage $\bar{w}$ was changed to a "base rate", so that it does not follow the average wage growth, but rather the CPI, starting January 2006.

The determining cap, C, and the benefits max, $\bar{b}$, were provided to us by the NII. The avg. wage/base rates tables are readily available online. To obtain the values of d_1 and d_2 over the period of our sample, as well as other changes to the law, including those pertaining to UI below, we have carefully read the Income Insurance Law, and the National Security Law[37] along with the amendments of the law to trace changes to the law dating back to 1996, and most importantly, the law through which the reform was carried out.[38] As a complement to this, we have used the web archive to visit the old versions of the NII website, to read through the information provided to insurees.[39] The main changes to the disregard rates and slopes were indeed implemented in the 2002 reform. Until June 2002, d_1 was 13% for singles, and 17% for those with a partner or a child, and was left the same for those above age 55. For those under 55, after June 2002 these rates were reduced to 5% and 7%, accordingly. The slope parameter, d_2, was changed for those under 55 and those who are not widowed or single parents, from 0.6 before the reform to all recipients, to 0.625 for couples with 2 children or more, 0.675 for couples with 1 child, and 0.7 otherwise.

B.4 Simulating Benefits

Since we do not observe benefits, we assign to each worker in our data their benefits bundle according to their determining characteristics. We also calculated for each of the workers and their households what their benefits were if they were to be displaced in every year in our sample, keeping their unemployment eve characteristics constant. We then calculate the transfers made to each household during the first year after the job separation.

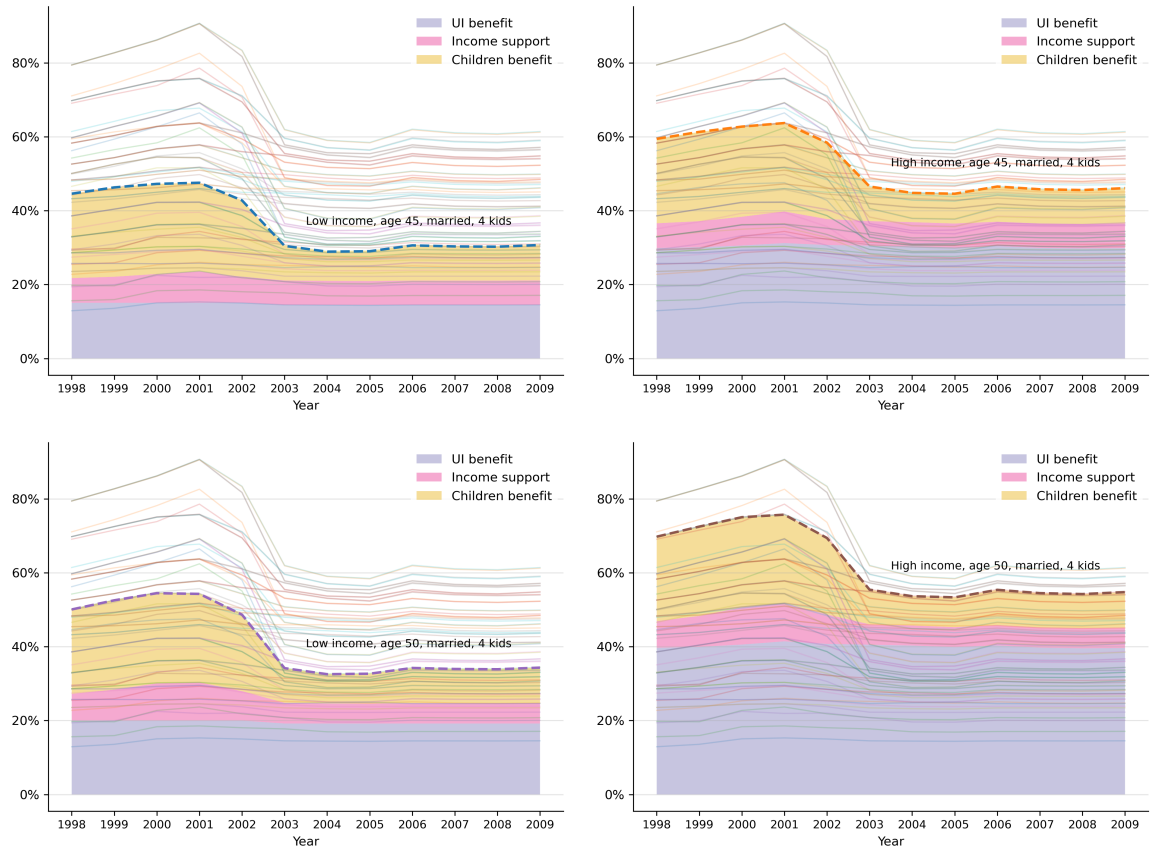
[37] Link to the II law manuscript, link to the NII law manuscript.

[38] The 2002-03 Omnibus Law of Arrangements ("Hok-Hahesderim"), available from the house of representatives website. Link to the law manuscript.

[39] Link to the archived website.

To illustrate the policy changes that were made, we show here several examples of types and their benefits, for each year of potential job loss. We consider two income levels - half the average wage (low), and 1.5 times the average wage (high), different ages, number of children, and marital status.

Figure B.2: Total benefits by their components, as % of avg. wage in 2001, for different types



Note: The graphs in this figure show the potential yearly transfers a two-spouse family would receive, for each year between 1998 and 2009, in the event that the breadwinner lost his/her job and did not return to work in the following 12 months. Benefits are in percentage of the average wage in 2001. All figures pertain to families where the spouse of the worker does not work. where the breadwinner's spouse is not employed. High-income workers (earning 1.5 times the average wage) are represented in the left panels, while the right panels depict low-income workers (earning 0.5 times the average wage).

C Parameters of Interest Details

We have a population of N^p parents, indexed by j, each of which has $n_j > 0$ children, indexed by i. Thereby the children population size amounts to $N^c = \sum_j^{N^p} n_j$ children. We use the function $J : \mathbb{Z} \to \mathbb{Z}$ to indicate that $J(i)$ is the parent of the child i. We denote $D_{jy} \in \{0, 1\}$ an indicator that equals one if parent j was displaced in a mass layoff event in year y, and zero otherwise. Lastly, we use the function $y : \mathbb{Z} \to \mathbb{Z}$, to represent that the job loss year of parent j is y(j).

Each family j receives $\tilde{b}_{jt}$ child benefits in year t, where the child benefit amount depends on the number of children and their ages, and the calendar year, before or after the reform.[40] Therefore $\tilde{b}_{jy(j)}$ is the benefit level received at the parent job loss year. For simplicity, we restrict attention to the variable $b_{jt} \in \{0, 1\}$ that indicates whether the family received high $(b_{jt} = 1)$ or low $(b_{jt} = 0)$ levels of benefits at year t. As explained in Section 2, due to the nature of the reform, most of the variation in b_{jt} is explained by the interaction between the calendar year and family size. Before the reform, families with more than three children were eligible for significantly higher levels of benefits, both per child and for the marginal child, while after the reform, the benefits were reduced dramatically. Furthermore, to simplify notation, and without loss of generality, we refer to the year of job loss $t \in \{0, 1\}$ as receiving two values, either before 2003 $(t = 0)$, the year of the reform, or after $(t = 1)$.

We start by presenting the job loss treatment effect parameters and then turn to the impacts of the reform.

C.1 Job Loss Effects

Parents: We define $Y_{jt}^p(d, b)$ the potential labor market outcome of parent j at year t with job loss status $d \in \{0, 1\}$ and benefits level $b_{tj} = b$. Specifically, $Y_{jt}^p(0, 0)$ is the potential outcome of parent j that didn't experience job loss in year t and was not eligible for a high level of benefits, $Y_{jt}^p(1, 0)$ is the potential outcome of individual j that did experience job-loss in year t and was not eligible for high-level benefits, $Y_{jt}^p(0, 1)$ is the potential outcome of parent j that didn't experience job loss in year t and was eligible for high-level of benefits, and lastly, $Y_{jt}^p(1, 1)$

[40] Families are eligible for benefits for every child below age 18, and families with children born before June 2003 received different benefit amounts after the reform. For more information, see Section 2.

is the potential outcome of parent j that experienced job loss in year t was eligible for high-level of benefits.

For every parent j, we observe D_{jt}, b_{jt} and the labor market outcome Y_{jt}^p in year t:

$$Y_{jt}^p = b_{jt}(D_{jt}Y_j^p(1,1) + (1-D_{jt})Y_j^p(0,1)) + (1-b_{jt})(D_{jt}Y_j^p(1,0) + (1-D_{jt})Y_j^p(0,0))$$

Children: For every child i, we observe their parents' job loss status $D_{j(i)t}$ during childhood. For simplicity, we assume each child experiences at most one event of parental job loss during childhood. Similarly, each family $j(i)$ is eligible for $b_{j(i)0}$ benefits levels before the reform, and $b_{j(i)1}$ after. The potential education outcome of child i is $Y_i^c(D_{j(i)0}, b_{j(i)0}, D_{j(i)1}, b_{j(i)1})$, and the observed outcome is

$$Y_i^c = \sum_{d_0=0}^{1}\sum_{b_0=0}^{1}\sum_{d_1=0}^{1}\sum_{b_1=0}^{1} d_0 \cdot b_0 \cdot d_1 \cdot b_1 \cdot Y_i^c(d_0, b_0, d_1, b_{j(i)1})$$

Equipped with this notation, we define $\Delta_{jt}^p(b) = Y_{jt}^p(0,b) - Y_{jt}^p(1,b)$ to be the individual-level parental job-loss treatment effect for a family with a level of b benefits, and $\Delta_{jt}^p = b_{jt}\Delta_{jt}^p(1) + (1-b_{jt})\Delta_{jt}^p(0)$ the individual-level parental job-loss treatment effect, regardless of family benefits. For children, we define $\Delta_{i0}^c(b) = Y_i^c(0,b_0,0,b_1) - Y_i^c(1,b_0,0,b_1)$, and $\Delta_{i1}^c(b_0,b_1) = Y_i^c(0,b_0,0,b_1) - Y_i^c(0,b_0,1,b_1)$, for every (b_0,b_1) benefits before and after the reform, maintaining the assumption that each family experience job loss only once during the child's childhood. With that notation, we define the individual level job loss treatment effect on children as $\Delta_{it}^c = \sum_{b_0=0}^{1}\sum_{b_1=0}^{1} b_0 \cdot b_1 \cdot \Delta_{it}^c(b_0,b_1)$. Individual-level treatment effects are unobserved, as we never observe both the potential outcome under treatment and the potential outcome under control, a problem that is also known as the "fundamental problem of causal inference" (Holland, 1986; Rubin, 1974).

We start by presenting our first parameters of interest, the Average Treatment Effect on the Treated (ATT) of parental job loss:

$$\Delta^p = \mathbb{E}[\Delta_{jt}^p | D_{jt} = 1]$$
$$\Delta^c = \mathbb{E}[\Delta_{it}^c | D_{j(i)t} = 1]$$

where expectations are taken over the full population of parents or children regardless of ben-

efits levels or the year of job displacement.

Identification: A naive comparison of outcomes of parents and children by whether the parents were laid off poses significant challenges in identifying the true causal effect. In particular, unemployed workers tend to be negatively selected (Davis and von Wachter, 2011), and a parent's innate abilities are inherited by their children (Sacerdote, 2007; Fagereng et al., 2021). In our approach to overcome these issues, we assume the Conditional Independence Assumption, i.e., that selection into treatment is explained by a vector of child-parent pair pre-displacement characteristics $Z_i = (X_{J(i)}, X_i)$. Formally:

Assumption A1 - Conditional Independence - For every child i, benefits levels b_t, and year t

$$(Y^p_{J(i)t}(1, b_t), Y^p_{J(i)t}(0, b_t), Y^c_i(1, b_0, 0, b_1), Y^c_i(0, b_0, 1, b_1), Y^c_i(0, b_0, 0, b_1)) \perp\!\!\!\perp D_{j(i),t}|Z_i$$

Even with rich and comprehensive data such as ours, this is a strong assumption. We rely on a long literature in labor economics estimating the effects of job displacement in mass layoffs on adults based on the JLS approach, relying on the CIA assumption, and show that our results are comparable to the job loss effects estimated in developed counties. Moreover, by estimating Δ^p before displacement on outcomes that are not included in X_j, we present evidence on pre-displacement differences between treated and control units.[41]

Non-parametric identification of the ATT requires the additional standard common support assumption:

Assumption A2 - Common Support

$$0 \geq \Pr(D_{j(i)} = 1|Z_i = z) < 1$$

which calls for overlap over values of the propensity score $\Pr(D_j = 1|Z = z)$ among displaced and non displaced workers. In Section 6, we describe our estimation strategy and provide further evidence that our setting satisfies the common support assumption.

[41] Note that this is a stronger assumption than the "parallel trends assumption". We do not only reject differences between treated and control units in the rate of change of outcome pre-displacement, but we also require that the levels should be the same on average.

C.2 Identifying the Impact of Government Transfers on the Impact of Job Loss

We are interested in studying whether the impacts of job loss vary by the transfers a family receives. For parents, our parameter of interest is:

$$\varphi^p = E[\Delta_{j1}(1) - \Delta_{j1}(0)|b_{j1} = 1],$$

and for children, we restrict attention to the first-order impact of government transfers in the year of job loss, and our parameter of interest is

$$\varphi^c = E[\Delta_{i1}(b_0, 1) - \Delta_{i1}(b_0, 0)|b_{j(i)1} = 1].$$

In Appendix Figure D.9, we provide evidence of no meaningful effects of the second-order interaction between job loss and change in transfers on different years, i.e., we find no significant additional impact of losing a job in year t and losing benefits only a years later rather than in the year of job loss or before.

It is important to note that without further assumptions, $E[\Delta|b = 1] - E[\Delta|b = 0]$ does not identify φ, nor the Average Treatment Effect $E[\Delta_{it}(1) - \Delta_{it}(0)]$. We introduce a new assumption that allows us to use a Difference-in-Difference (DID) model to identify φ:

Assumption A3 - Parallel Trends

$$E[\Delta_{i0}(1) - \Delta_{i0}(0)|b_i = 1] = E[\Delta_{i0}(1) - \Delta_{i0}(0)|b_i = 0]$$

Assumption A3 is the well-known parallel trends assumption. It states that the average job-loss impacts for families with high and low levels of benefits would have followed parallel paths in the absence of the treatment.

Note that the parameter φ is comprised of three differences, job-loss status, benefits eligibility status, and time. Hence, it can be estimated using a triple-difference (DDD) model interacting both job loss, time, and family size, after adjusting properly for covariates Z_i. Therefore, to support Assumption A3, we can test for pre-trends in the DDD coefficient of the impacts of the reform before the reform was enacted.

C.3 The Effect of the Reform on the Non-Job-Loss Population

We estimate the effect of child benefits on the educational achievements of children in Israel, regardless of whether their parents experienced job loss. To do so, we first employ a regression discontinuity design, replicating Kott (2022) using our data.[42] We also conduct a short exposure analysis akin to the main analysis of our paper at the end of this appendix.

For this analysis, our sample includes the entire population of Israel who has had a child born in the two years around June 2003, with at least one older sibling for which we are able to observe educational outcomes. Effectively, for our estimation, we use a 3 months window around the cutoff date.

The identification of the effect of the benefits change relies on a discontinuity in the allowance provided to families according to the date of birth of the child. For a child that was born before June 2003, the allowance drop in the reform was smaller than that of families with children born after. Hence, families with children born shortly before and after the reform received different-sized transfers. Under the assumption that potential outcomes are continuous and smooth relative to the date of birth of the newborn sibling around the reform date, the change in benefits allows to estimate the causal effect of the reform.

We run the following regression:

$$Y_i = \alpha + \beta_1 T_i + \beta_2 (m_i - c) + \beta_3 T_i (m_i - c) + X_i \gamma + \epsilon_i \tag{7}$$

Where:

- Y_i is the educational achievement outcomes: Bagrut certificate and performance in the mathematics exam.
- T_i is the treatment dummy, which is 1 if the child was born after June 2003, thus affected by the policy change, and 0 if the child was born before.
- m_i is the month of birth of the relevant sibling, the "running variable".

[42] In (Kott, 2022), the author finds noisy effects of child benefits on the probability of obtaining a Bagrut certificate.[43] This result, however, is limited to children who are very young when the family experiences the change in benefits, which we do not have at our disposal due to a cohort limitation.

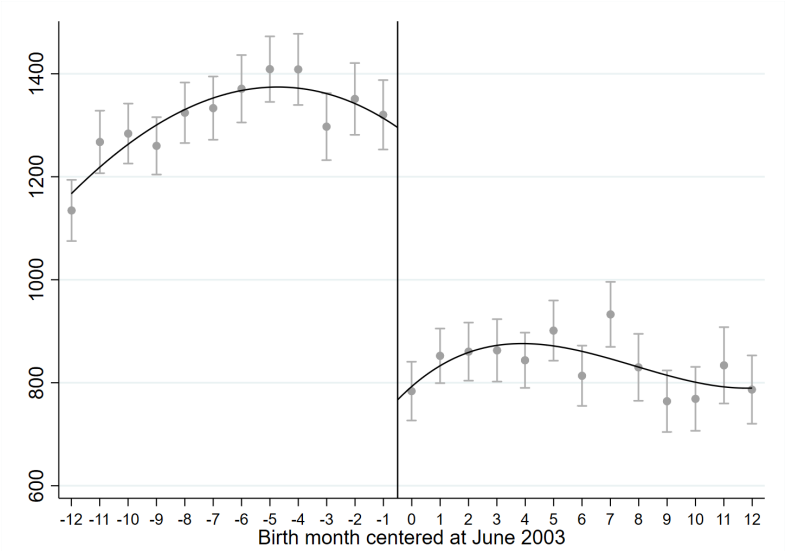
- X_i is a vector of covariates that include dummies for the rank of parental income, both parents' academic degree attainment, dummies for whether the parents are recent migrants, the number of siblings in the household, ethnic group, and age of the parent and of the child.
- c is the cutoff point, June 2003.
- β_1 is the coefficient of interest, which captures the discontinuity at the cutoff and therefore the treatment effect.

To justify this approach, we need to support that (a) there is a discrete change in benefits at the cutoff, (b) there is no manipulation of assignment to treatment, that is, fertility choices were not somehow made to time the birth with respect to the change date, and (3) that potential outcomes are orthogonal to the assignment, that is, that outcomes are smooth around the cutoff.

Table 2 displays the benefits provided to families by family size and the birth date of the child. Similarly to our main analysis in this paper, we impute the child benefits for each family. As mentioned, we are able to do this since (a) child benefits are automatic and universal, with an uptake of around 99%, and (b) child benefits are determined solely based on the number of children and their date of birth which we can observe. Figure C.1 presents the change in the imputed benefits change for the families in our sample. The drop in monthly benefits around the cutoff is substantial, at around 500 ILS, or about 150 USD.

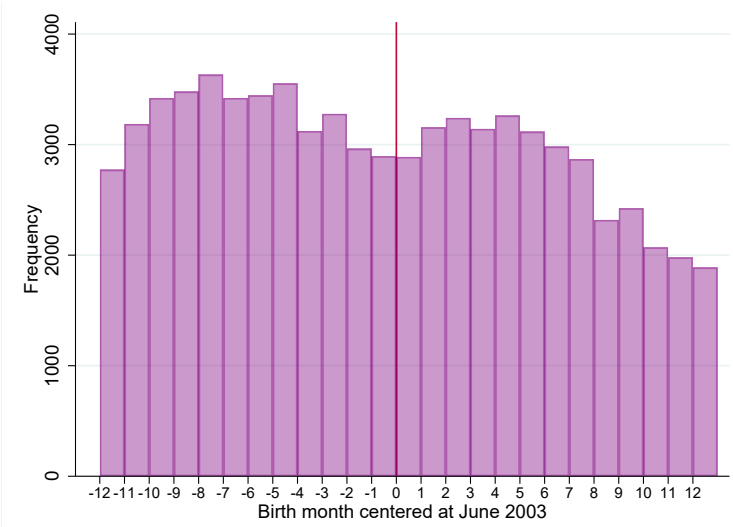
Figure C.2 provides evidence that there is no bunching around the cutoff date. Evidence for manipulation would appear as a difference between the frequency of births in the months before and after the cutoff. Here, the frequencies are almost perfectly symmetric in the few months before and after the cutoff dates.

Figure C.1: First stage: total monthly child benefits (in ILS$\approx$ 0.3$)



Note: These figure plot the total monthly child benefits the family received in Israeli Shekels, against the birth date of the pivotal child, centered around June 2003. Each point represents the average benefits at the monthly level. 3rd polynomial lines of the underlying data are fitted on either side of the cutoff month. The sample contains only children who are below the age of 18 when their sibling was born.

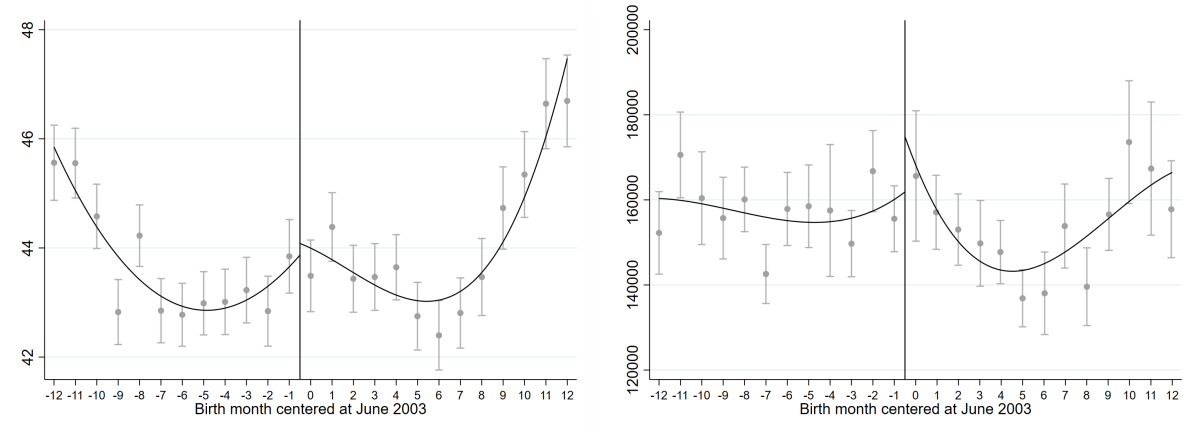
Figure C.2: No bunching: histogram of sibling's birth month centered around the reform



Note: This figure presents the histogram of the "running" variable - birth date, at the monthly level, centered around June 2003, the determining month in the change in benefits.

Lastly, C.3 supports the assumption that potential outcomes are smooth around the cutoff. In this figure, we show that family earnings and rank, which are expected to be correlated with outcomes, are smooth around the cutoff.

Figure C.3: Continuity: the effect on parental earnings



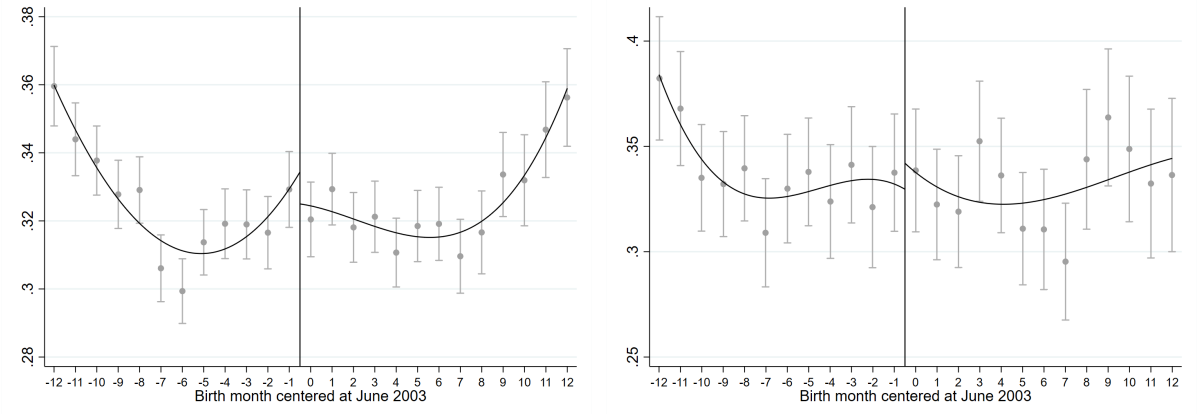
(**a**) Yearly earnings rank (**b**) Yearly earnings (1NIS $\approx$ 0.3\$)

Note: These figures plot the parents' characteristics, against the birth date of the pivotal child, centered around June 2003. Each point represents the average at the monthly level. 3rd polynomial lines of the underlying data are fitted on either side of the cutoff month.

Figure C.4 presents the outcomes of interest around the cutoff. It is evident that there is no discontinuous change in either of these outcomes at the cutoff date.

The estimates for β_1, with a local linear fit, and a window of 3 months before and after the cutoff in both cases are non-significant, with a positive effect of 0.01 for the Bagrut certificate, and a negative 0.01 for math). These results are in line with the small aggregate effects found in Kott (2022), and given the sample differences between the papers, mentioned above.

Figure C.4: The effect on children's education



(**a**) Bagrut certificate (**b**) Math performance

Note: These figures plot high school outcomes against the birth date of the pivotal child, centered around June 2003. Each point represents the average at the monthly level. 3rd polynomial lines of the underlying data are fitted on either side of the cutoff month. The sample contains only children who are below the age of 18 when their sibling was born.

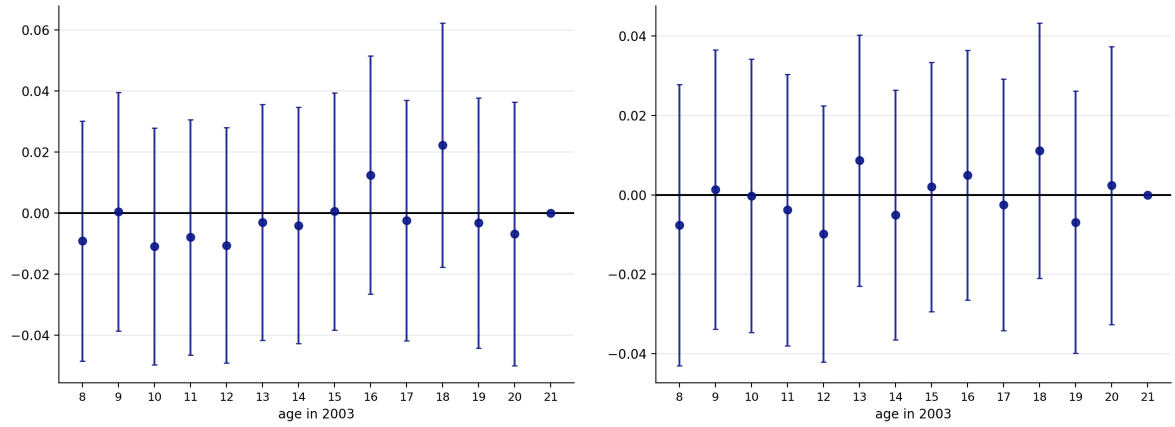
In conclusion, we find null effects of child transfers on education outcomes. We further corroborate this finding by running the following regression on the entire sample of non-job-loss children:

$$\begin{aligned} Y_{ij} = \beta_0 + \beta_1 \cdot \text{Age 2003}_{ij} + \beta_2 \cdot \text{D}_{ij} \\ + \beta_3 \cdot \text{Age 2003}_{ij} \times \text{D}_{ij} + \mu_j + \epsilon_{ij} \end{aligned} \tag{8}$$

Where Y_{ij} is either the Bagrut eligibility or the math score of the child i in family j as a function of the child's age in 2003 (Age 2003_{ij}) with age 21 as the baseline, a dummy variable indicating whether the child is a large family (3 children or more) (D_{ij}), and the interaction term of these two variables. β_3 is the coefficient of interest, which we plot in the following figures. μ_j is the family-level fixed effects. ϵ_{ij} is the error term.

The figure below presents the results: despite the fact that children of large families who are younger in 2003 experienced a longer period during their childhood with lower child benefits, we do not find any evidence that they experience an effect on their education outcomes.

Figure C.5: The reform effect on children's high school outcomes



(**a**) Bagrut certificate (**b**) Math

Note: This figure presents the β_3 coefficients from equation 8 for the effect of the reform on the general population of students not affected by job loss. The regression relies on the fact that the exposure to different amounts of benefits until the outcome realization depends on the age of the child at the time of the reform. The baseline age in 2003 is 21. Standard errors are clustered at the parent level.

D Appendix Figures and Tables

D.1 Appendix Tables

Table D.1: Children Sample Frequency Tables

Age at job loss	Frequency
4	58
5	184
6	317
7	372
8	421
9	502
10	526
11	622
12	674
13	899
14	947
15	926
16	873
17	875
18	837
19	821
20	789
21	772
22	731
23	692
24	616
25	558
26	443
27	384
28	302
29	247
30	208
31	183
32	130
33	101
34	45
35	8

Year of birth	Frequency
1974	249
1975	358
1976	415
1977	450
1978	556
1979	623
1980	635
1981	668
1982	736
1983	822
1984	889
1985	882
1986	902
1987	912
1988	940
1989	846
1990	846
1991	891
1992	878
1993	863
1994	876
1995	826

Note: This tables present the counts of job loss affected children. On the left are the number of children by age at the time of job loss, and on the right is the number of children by birth cohort.

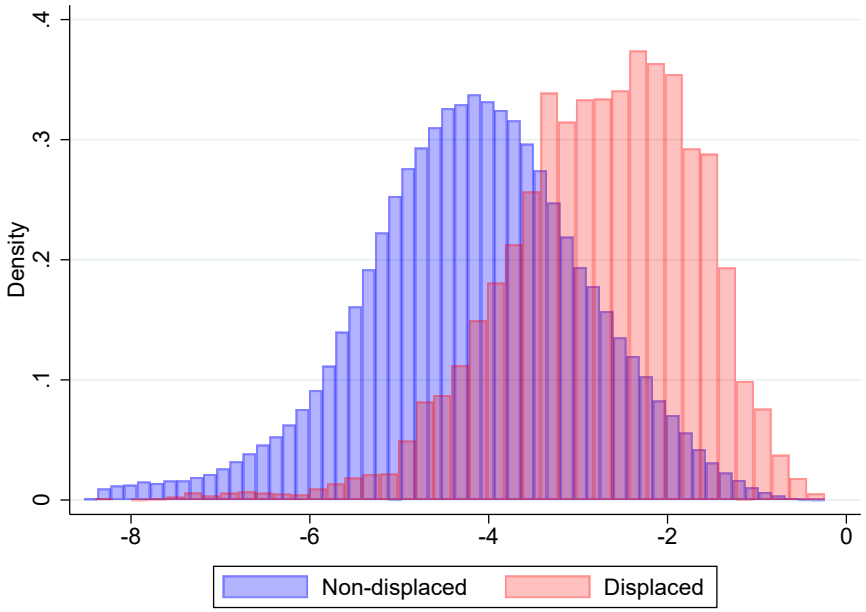
Table D.2: The effect of the reform on the effect of job loss children's education outcomes, by parent and income levels

		Father			Mother		
		Math score	Bagrut	Obs.	Math score	Bagrut	Obs.
(A): Num. children	Entire sample	0.0069	-0.0048	126,836	-0.019	-0.0389	71,114
		(0.0088)	(0.01)		(0.0204)	(0.0178)	
	Above med. inc.	0.0099	-0.0015	74,019	-0.0333	-0.0506	30,427
		(0.009)	(0.0111)		(0.0229)	(0.0198)	
	Below med. inc.	-0.0180	-0.0324	52,818	0.0429	0.0233	40,687
		(0.0196)	(0.0197)		(0.0392)	(0.0370)	
(B): Benefits lost	Entire sample	0.0025	-0.0001	126,836	-0.0078	-0.0116	71,114
		(0.0019)	(0.0022)		(0.0053)	(0.0045)	
	Above med. inc.	0.0016	0.0006	74,019	-0.0112	-0.0146	30,427
		(0.018)	(0.0023)		(0.0059)	(0.0046)	
	Below med. inc.	-0.0037	-0.0086	52,818	0.01	0.0062	40,687
		(0.0037)	(0.0046)		(0.0118)	(0.0102)	

Note: This table presents the β_7 coefficient from a triple difference regression based on Equation 6. It presents the impact of the reform on children's education outcomes as a consequence of parental job loss, segmented by household income levels (above and below the median) and by parent (father or mother). We present two analyses in this table: Panel (A) shows the effects based on the number of children in the family introduced linearly to the regression, and Panel (B) examines the effects based on the benefits lost due to the reform, calculated as the benefits after the reform (2004) minus the benefits before the reform (2001) in thousands of Israeli Shekels in yearly terms. In each case, the effects on math scores and Bagrut certificate attainment are shown for the entire sample and for the above/below median income categories. Robust standard errors are in parentheses, while the number of observations is provided in the last column.

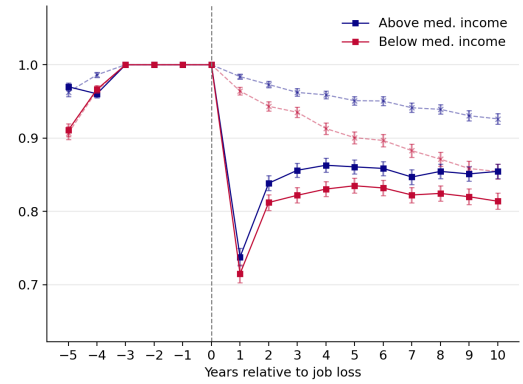
D.2 Appendix Figures

Figure D.1: Propensity scores distributions

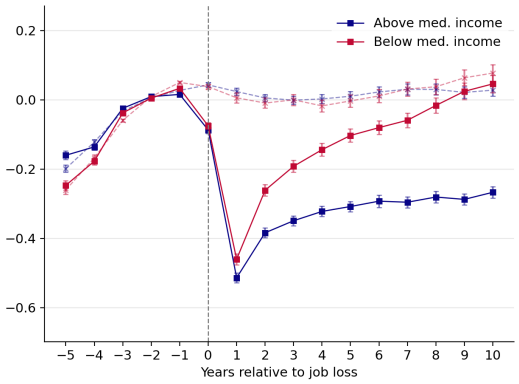


Note: This figure presents the overlap in the log of the estimated propensity scores of children as described in 5.

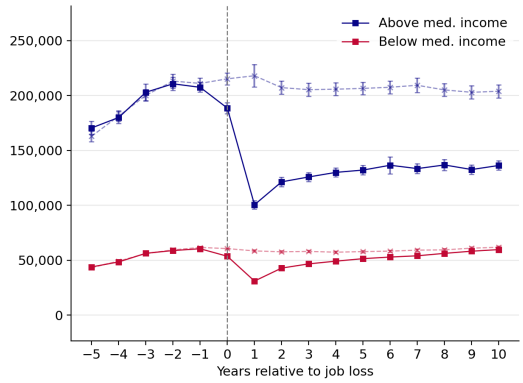
Figure D.2: Effects of job loss on household income and employment by income level



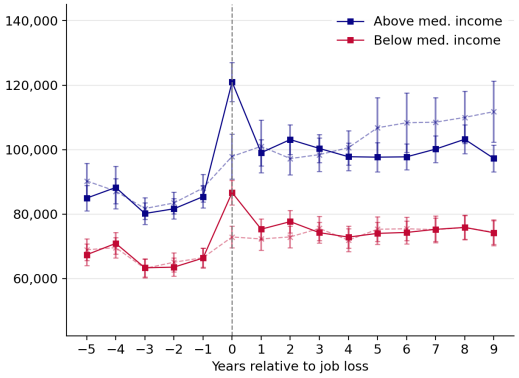
(**a**) Employment status



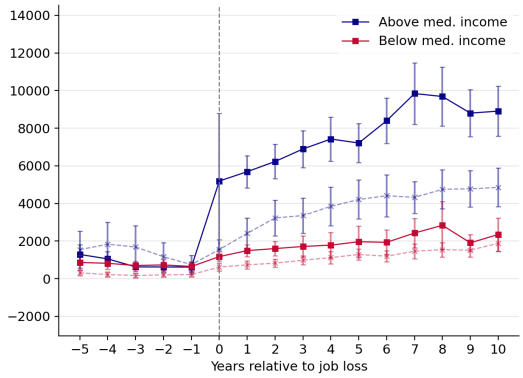
(**b**) Yearly earnings relative to $t < 0$



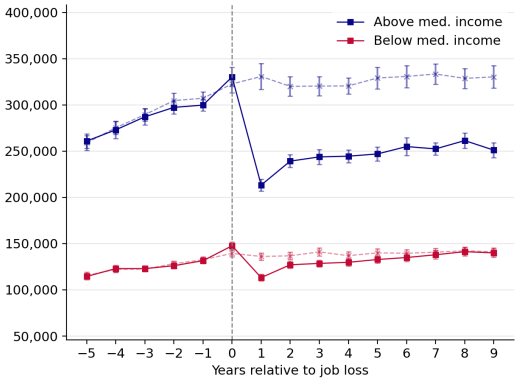
(**c**) Earnings (1ILS $\approx$ 0.3$)



(**d**) Spouse earnings (1ILS $\approx$ 0.3$)



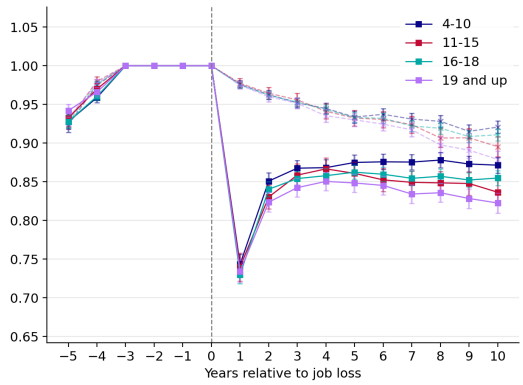
(**e**) Self-employment earnings (1ILS $\approx$ 0.3$)



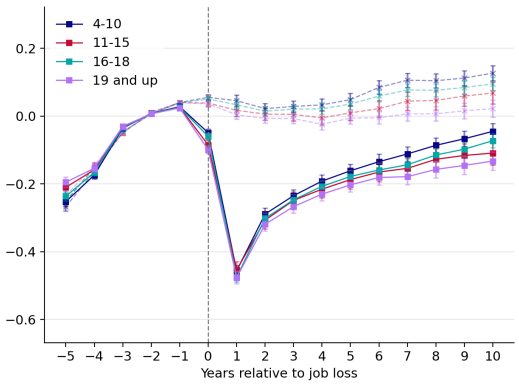
(**f**) Total household earnings (1ILS $\approx$ 0.3$)

Note: These figures present the impacts of job loss on labor market outcomes for individuals above and below median pre-displacement earnings. They depict the raw means of the treatment and control groups. The first three figures show the (a) mean employment status, defined as working and earning above 10,000 ILS (around 3,000 USD) during the calendar year, (b) average annual earnings relative to the average of the three years of earnings before displacement, and (c) average annual earnings in Israeli Shekels. The next three figures present the (d) spouse's annual earnings, (e) earnings from self-employment, and (f) total household income, including both employment and self-employment earnings for the individual and their spouse.

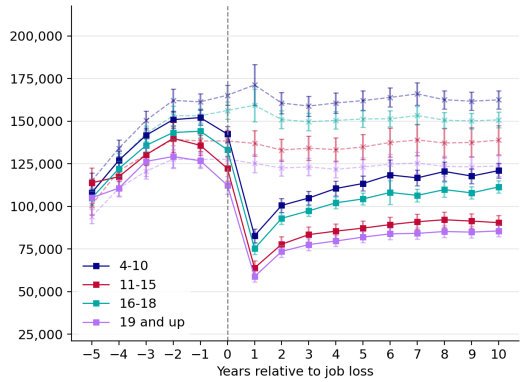
Figure D.3: Effects of job loss on household income and employment by child's age at the time of displacement



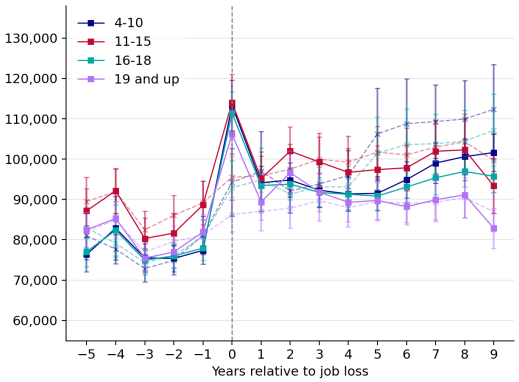
(**a**) Employment status



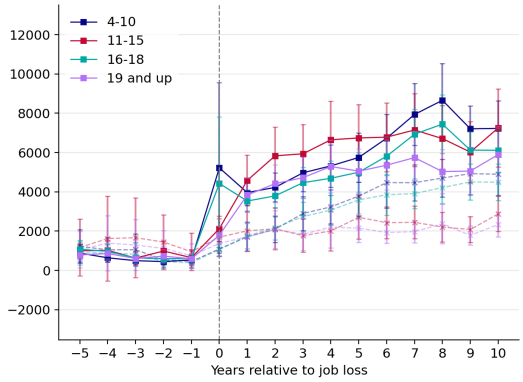
(**b**) Yearly earnings relative to $t < 0$



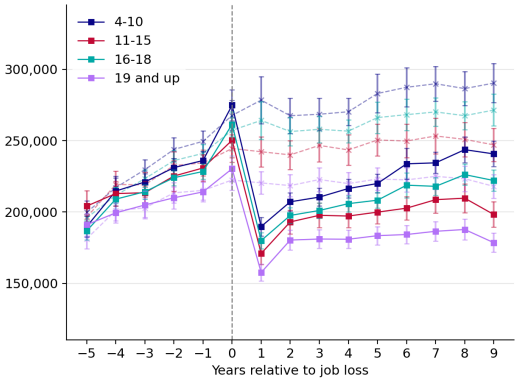
(**c**) Earnings (1ILS $\approx$ 0.3\$)



(**d**) Spouse earnings (1ILS $\approx$ 0.3\$)



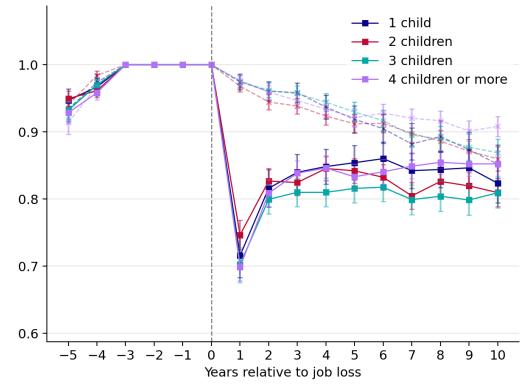
(**e**) Self-employment earnings (1ILS $\approx$ 0.3\$)



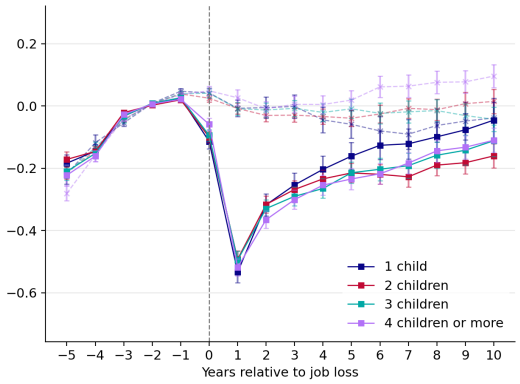
(**f**) Total household earnings (1ILS $\approx$ 0.3\$)

Note: These figures present the impacts of job loss on parents' labor market outcomes for parents, by the age of the child at the time of displacement. They depict the raw means of the treatment and control groups. The first three figures show the (a) mean employment status, defined as working and earning above 10,000 ILS (around 3,000 USD) during the calendar year, (b) average annual earnings relative to the average of the three years of earnings before displacement, and (c) average annual earnings in Israeli Shekels. The next three figures present the (d) spouse's annual earnings, (e) earnings from self-employment, and (f) total household income, including both employment and self-employment earnings for the individual and their spouse.

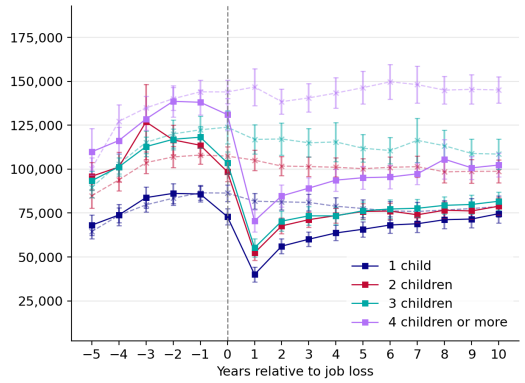
Figure D.4: Effects of job loss on household income and employment by the number of children



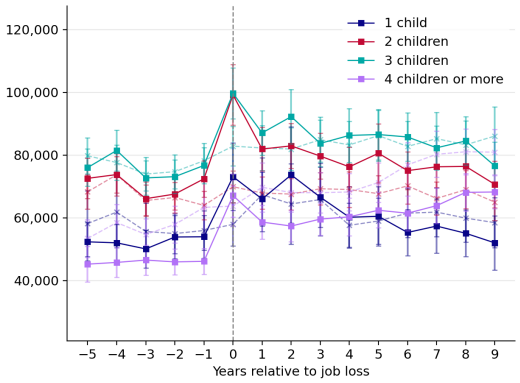
(**a**) Employment status



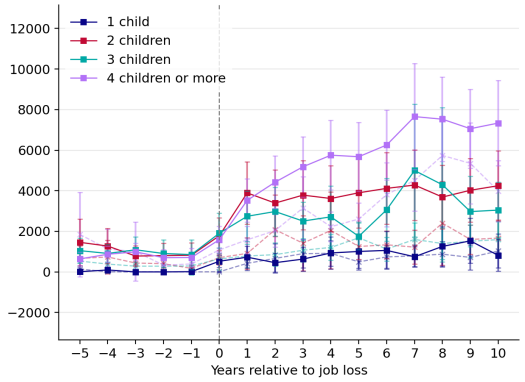
(**b**) Yearly earnings relative to $t < 0$



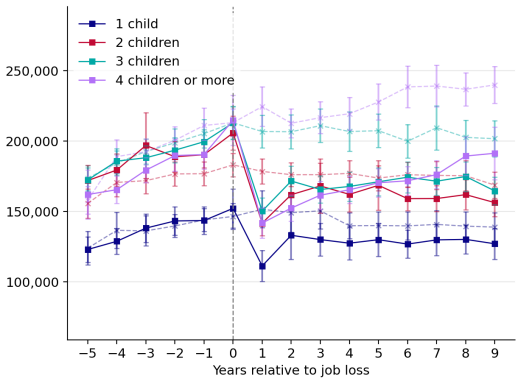
(**c**) Earnings (1ILS $\approx$ 0.3\$)



(**d**) Spouse earnings (1ILS $\approx$ 0.3\$)



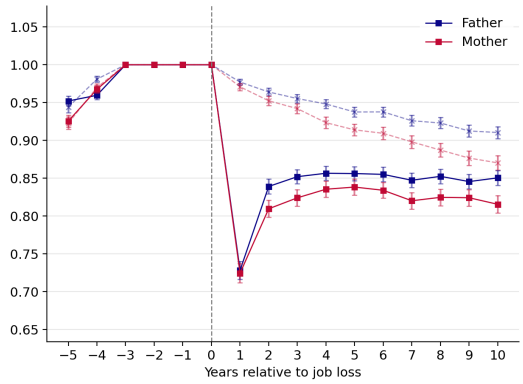
(**e**) Self-employment earnings (1ILS $\approx$ 0.3\$)



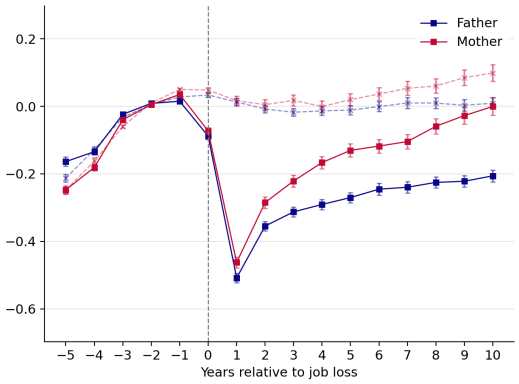
(**f**) Total household earnings (1ILS $\approx$ 0.3\$)

Note: These figures present the impacts of job loss on parents' labor market outcomes, by the number of children in the household that are under 18 at the time of job loss. They depict the raw means of the treatment and control groups. The first three figures show the (a) mean employment status, defined as working and earning above 10,000 ILS (around 3,000 USD) during the calendar year, (b) average annual earnings relative to the average of the three years of earnings before displacement, and (c) average annual earnings in Israeli Shekels. The next three figures present the (d) spouse's annual earnings, (e) earnings from self-employment, and (f) total household income, including both employment and self-employment earnings for the individual and their spouse.

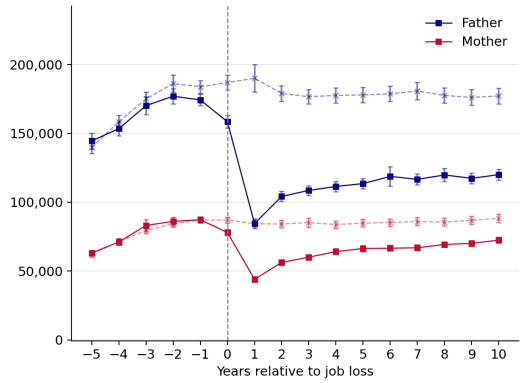
Figure D.5: Effects of job loss on household income and employment by the displaced parent



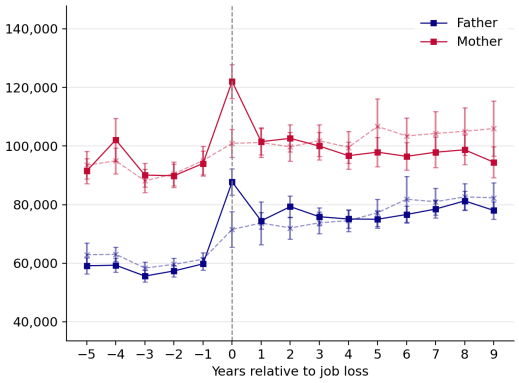
(**a**) Employment status



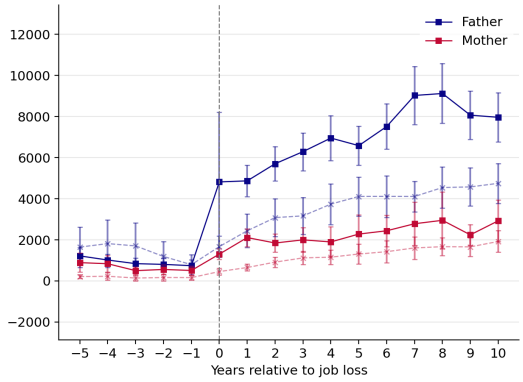
(**b**) Yearly earnings relative to $t < 0$



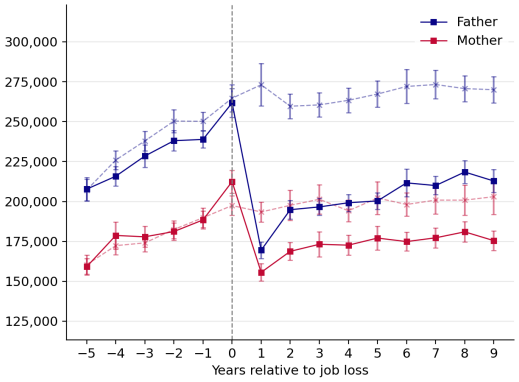
(**c**) Earnings (1ILS $\approx$ 0.3\$)



(**d**) Spouse earnings (1ILS $\approx$ 0.3\$)



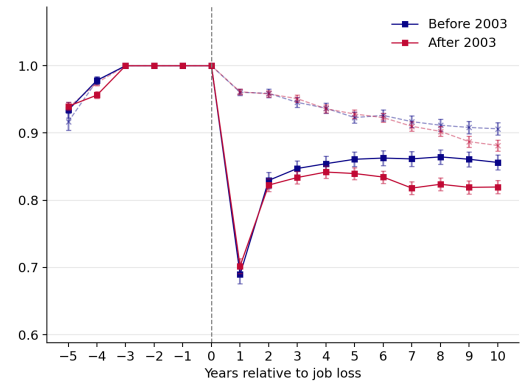
(**e**) Self-employment earnings (1ILS $\approx$ 0.3\$)



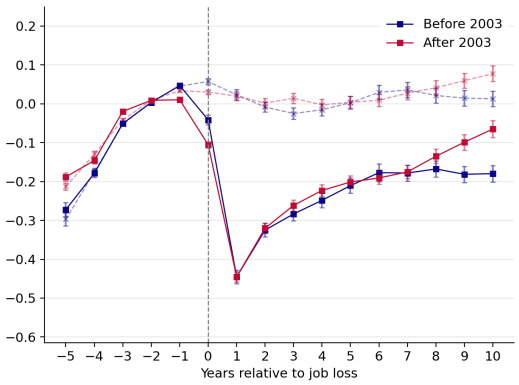
(**f**) Total household earnings (1ILS $\approx$ 0.3\$)

Note: These figures present the impacts of job loss on labor market outcomes, by the gender of the parent who was displaced. They depict the raw means of the treatment and control groups. The first three figures show the (a) mean employment status, defined as working and earning above 10,000 ILS (around 3,000 USD) during the calendar year, (b) average annual earnings relative to the average of the three years of earnings before displacement, and (c) average annual earnings in Israeli Shekels. The next three figures present the (d) spouse's annual earnings, (e) earnings from self-employment, and (f) total household income, including both employment and self-employment earnings for the individual and their spouse.

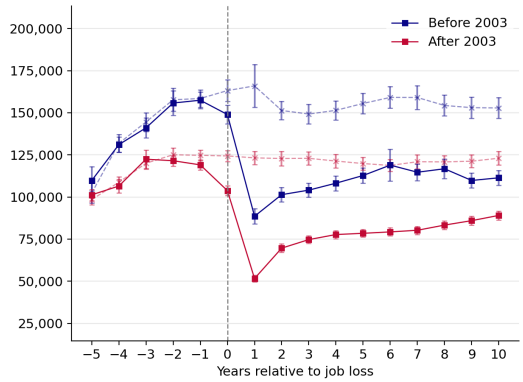
Figure D.6: Effects of job loss on household income and employment before and after the reform



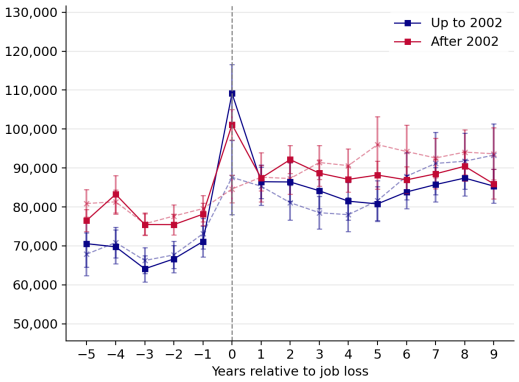
(**a**) Employment status



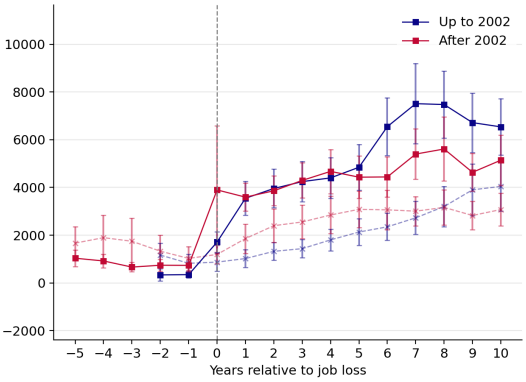
(**b**) Yearly earnings relative to $t < 0$



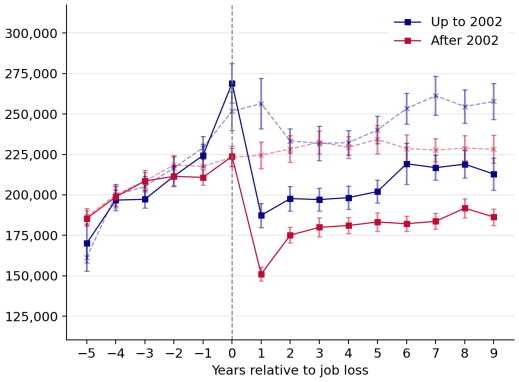
(**c**) Earnings (1ILS $\approx$ 0.3\$)



(**d**) Spouse earnings (1ILS $\approx$ 0.3\$)



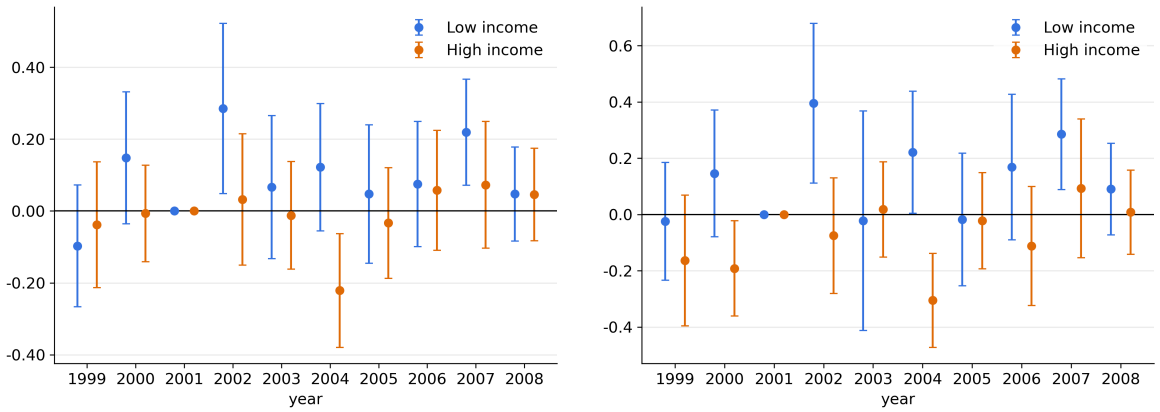
(**e**) Self-employment earnings (1ILS $\approx$ 0.3\$)



(**f**) Total household earnings (1ILS $\approx$ 0.3\$)

Note: These figures present the impacts of job loss on labor market outcomes for parents displaced before and after the reform, i.e., up to the year 2002, and from the year 2003 onwards. They depict the raw means of the treatment and control groups. The first three figures show the (a) mean employment status, defined as working and earning above 10,000 ILS (around 3,000 USD) during the calendar year, (b) average annual earnings relative to the average of the three years of earnings before displacement, and (c) average annual earnings in Israeli Shekels. The next three figures present the (d) spouse's annual earnings, (e) earnings from self-employment, and (f) total household income, including both employment and self-employment earnings for the individual and their spouse.

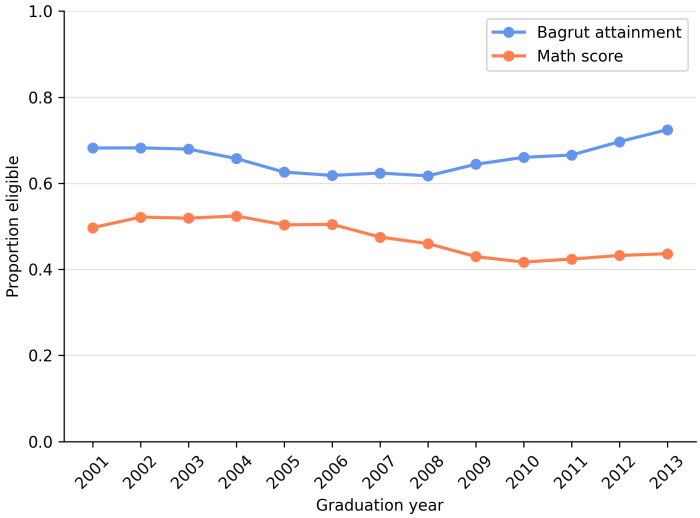
Figure D.7: The job loss penalty on parents interacted with a dummy for large families, by year and by incomes



(**a**) Employment (**b**) Earnings

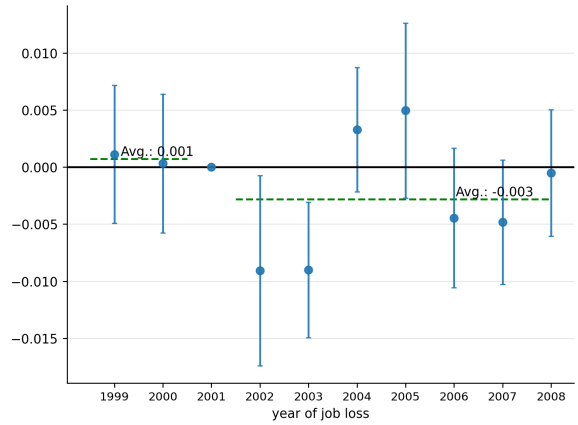
Note: This figure presents the impact of job loss on parents with large families (four or more children) relative to smaller families (up to three children) by the year of job loss and by income, according to Equation 2. Panel (a) presents the effect on employment, defined as earning at least 10,000 ILS a year from work as an employee, and panel (b) presents the effect on yearly earnings relative to the earnings in the three years before job loss. Standard errors are clustered at the firm level.

Figure D.8: Children outcomes by year

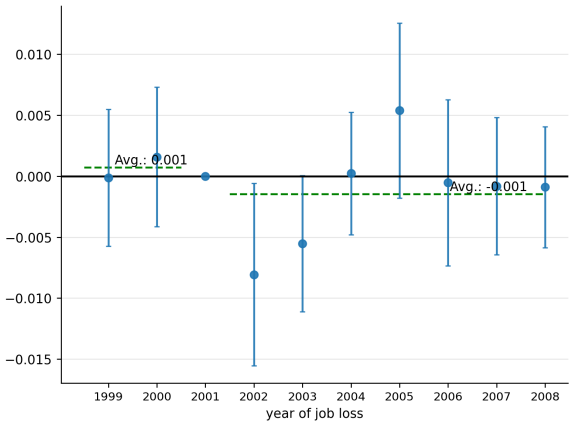


Note: This figure presents the average Bagrut eligibility rates and high-level math exam attainment by year for the entire population.

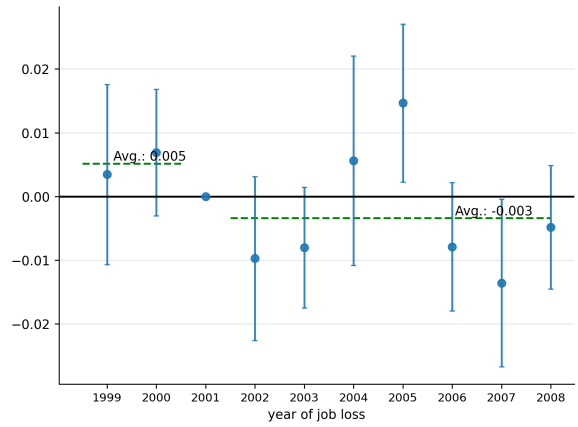
Figure D.9: Event study of job loss penalty on children interacted with the benefits lost after the reform



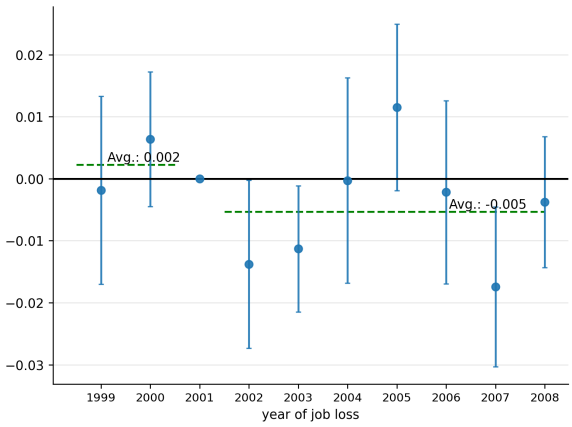
(**a**) Bagrut certificate



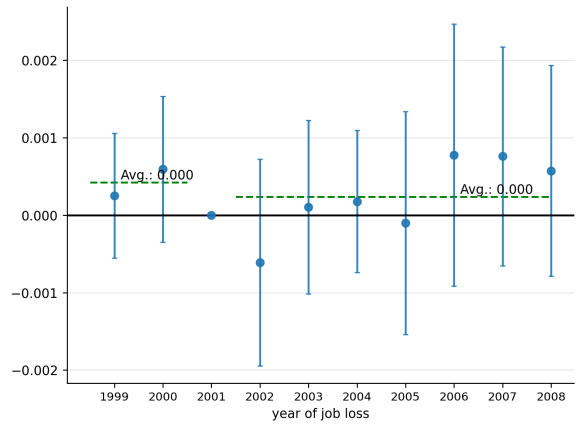
(**b**) Math



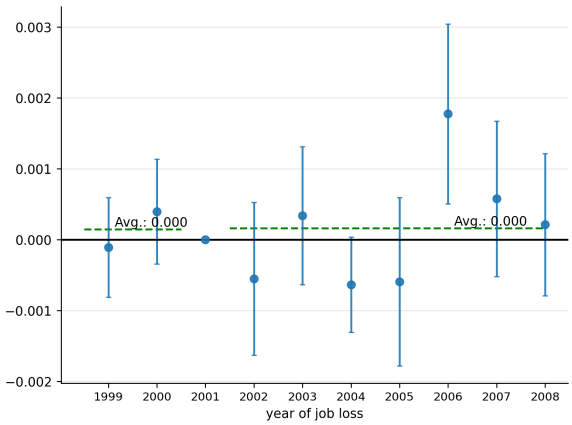
(**c**) Bagrut certificate, low income



(**d**) Math, low income



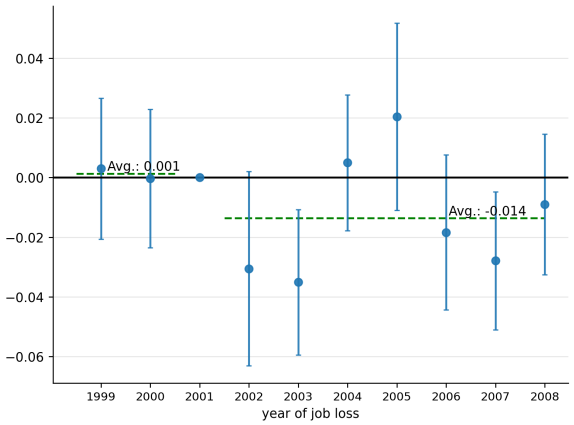
(**e**) Bagrut certificate, high income



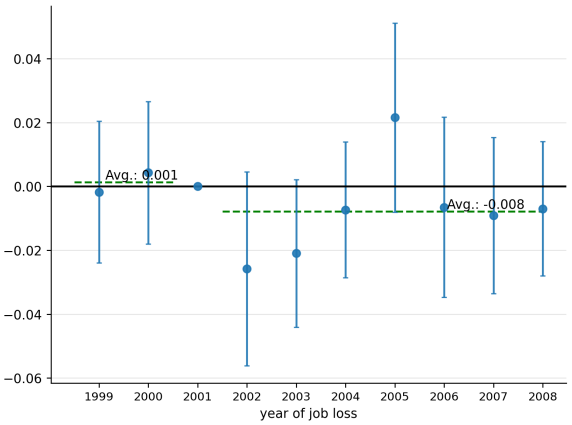
(**f**) Math, high income

Note: This figure shows the impact of job loss on children's outcomes interacted with a linear term for the benefits lost due to the reform for each year of job loss. The potential change in benefits is calculated as the benefits after the reform (2004) minus the benefits before the reform (2001) in thousands of Israeli Shekels in yearly terms. The sample includes children who experienced parental job loss before age 18. Controls include child and parent gender, child's age at job loss, pre-displacement parental earnings, ethnicity, and attendance at a religious school, all interacted with an indicator for displacement occurring after 2002. Standard errors are clustered at the level of the displaced parent.

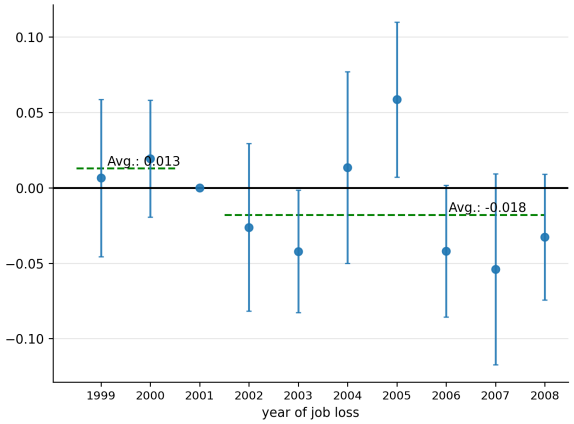
Figure D.10: Event study of job loss penalty interacted with the number of kids



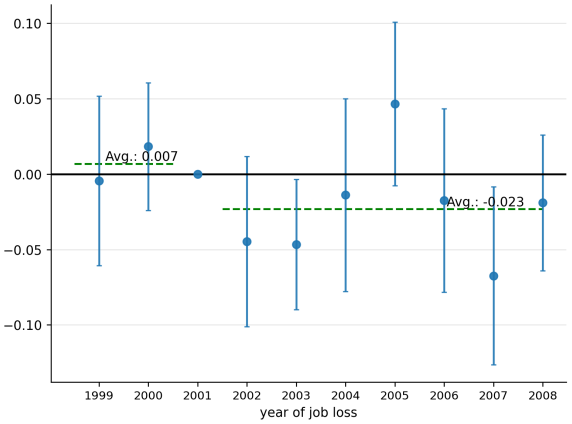
(**a**) Bagrut certificate



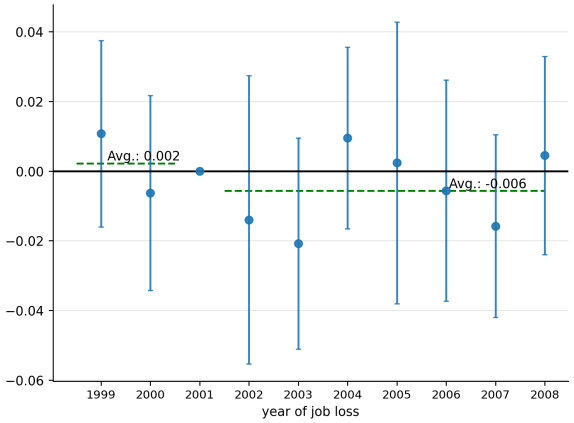
(**b**) Math



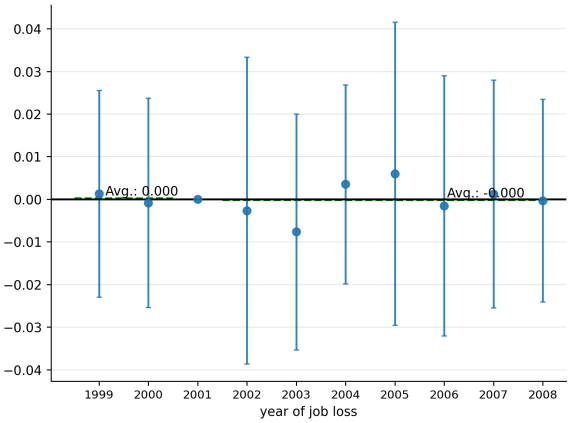
(**c**) Bagrut certificate, low income



(**d**) Math, low income



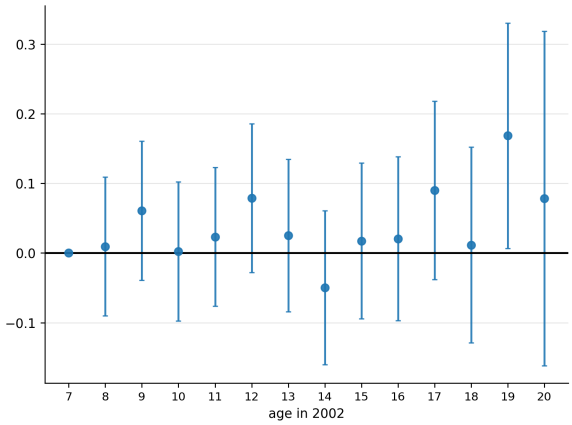
(**e**) Bagrut certificate, high income



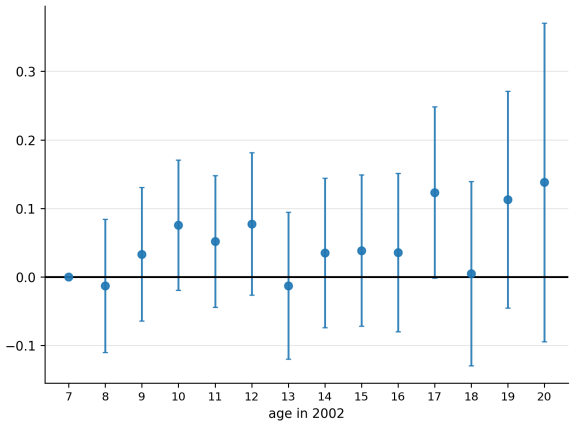
(**f**) Math, high income

Note: This figure shows the impact of job loss interacted with a linear term for the number of children under 18 in the household at the time of job loss and dummies for the year of job loss, with 2001 as the base year, according to Equation 2. The sample includes children who experienced parental job loss before age 18. Controls include child and parent gender, child's age at job loss, pre-displacement parental earnings, ethnicity, and attendance at a religious school, all interacted with an indicator for displacement occurring after 2002. Standard errors are clustered at the level of the displaced parent.

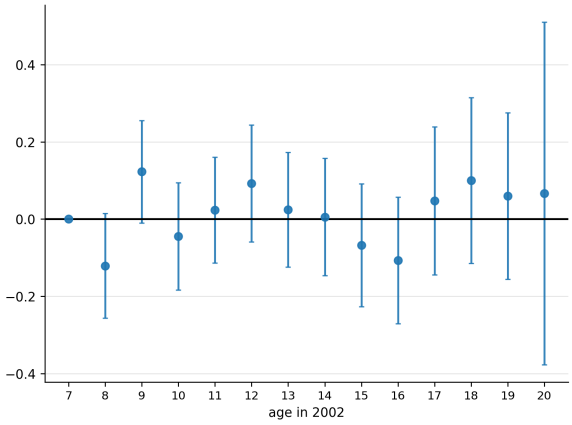
Figure D.11: Event study of job loss penalty *by the age of the child in 2002* interacted a dummy of 3 kids or more



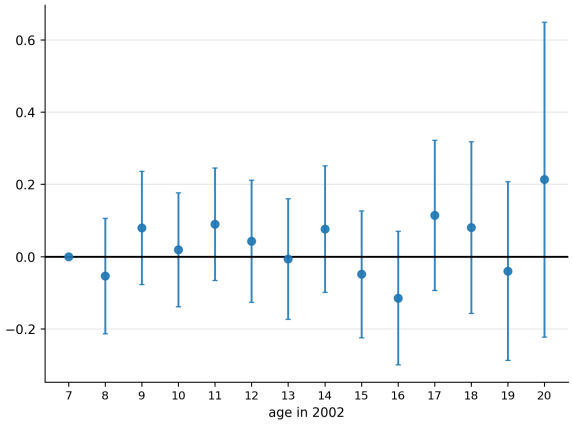
(**a**) Bagrut certificate



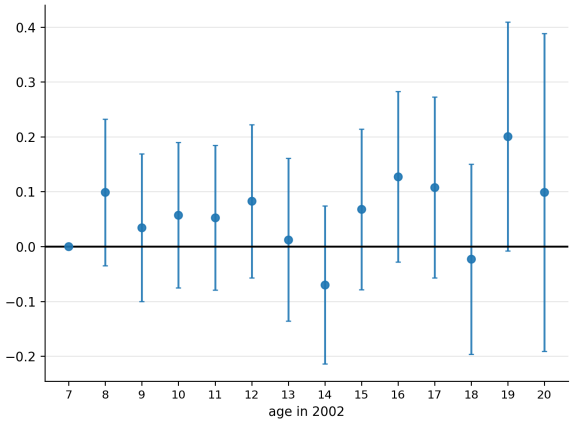
(**b**) Math



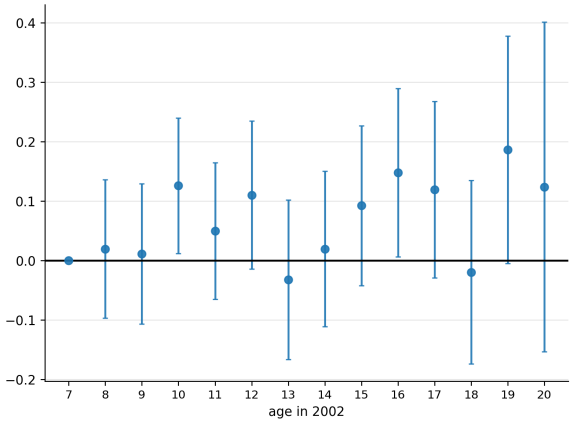
(**c**) Bagrut certificate, low income



(**d**) Math, low income



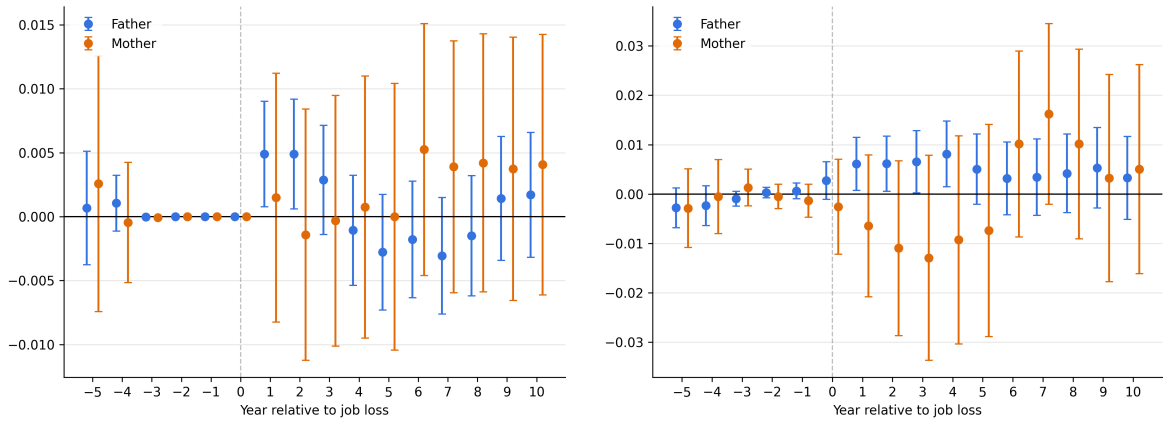
(**e**) Bagrut certificate, high income



(**f**) Math, high income

Note: This figure shows the impact of job loss interacted with a dummy for having three or more children under 18 in the household at the time of job loss and dummies for the age of the child in 2002, with age 7 as the base age, similarly to Equation 2, replacing the year terms with age terms. Controls include child and parent gender, age of the child at the time of job loss, parents' earnings before displacement, ethnicity, and whether the child is going to a religious school, interacted with an indicator that the year of displacement is after 2002. Standard errors are clustered at the displaced parent level.

Figure D.12: The effect of the reform on the impact of job loss on parents' employment and earnings, by parent's gender



(**a**) Employment status, benefits loss

(**b**) Yearly earnings, benefits loss

Note: This figure presents the β_8 coefficient from the triple difference regression described in equation 6 for the effect of the reform on employment and yearly earnings of displaced parents. The impact is estimated as the effect of the loss in child benefits due to the reform, calculated as the benefits after the reform (2004) minus the benefits before the reform (2001) in thousands of Israeli Shekels in yearly terms. Regressions are estimated by the parent, specifically, the father or the mother. Panel (a) presents the effect on employment, defined as earning at least 10,000 ILS a year from work as an employee, and panel (b) presents the effect on yearly earnings relative to the earnings in the three years before job loss. Bars indicate 95% confidence intervals based on standard errors clustered at the parent level.