MONASH University

MONASH BUSINESS SCHOOL

Department of Economics
ISSN number 1441-5429

She who Pays the Piper Calls the Number: Reparations and Gender Differences in Fertility Choice

Discussion Paper no. 2025-16

Moshe Hazan and Shay Tsur

Abstract:

We study how shifting intra-household control over resources affects fertility, exploiting a quasi-natural experiment in Israel where some Holocaust survivors began receiving substantial and unexpected reparations in 1957 and others decades later. Using a triple-difference design with heterogeneity by age, we compare fertility outcomes by timing of reparations, gender of the recipient, and age. Households where only the young female partner received reparations early had 0.25–0.4 fewer children than comparable households where only the male was treated. An event study shows that this effect is driven entirely by post-1957 fertility, suggesting a causal link to increased female resource control

Keywords: Fertility Choice, Intrahouseho

JEL Classification: J13 J16 D13

Moshe Hazan: Department of Economics, Monash University, 900 Dandenong Road, Caulfield East VIC 3145 (email: moshe.hazan@monash.edu); Shay Tsur: Research Department, Bank of Israel, P.O. Box 780, Jerusalem 9100701 (email: shay.tsur@boi.org.il).

She who Pays the Piper Calls the Number: Reparations and Gender Differences in Fertility Choice*

Moshe Hazan
Monash University and CEPR

Shay Tsur
Bank of Israel

September 2025

Abstract

We study how shifting intra-household control over resources affects fertility, exploiting a quasi-natural experiment in Israel where some Holocaust survivors began receiving substantial and unexpected reparations in 1957 and others decades later. Using a triple-difference design with heterogeneity by age, we compare fertility outcomes by timing of reparations, gender of the recipient, and age. Households where only the young female partner received reparations early had 0.25–0.4 fewer children than comparable households where only the male was treated. An event study shows that this effect is driven entirely by post-1957 fertility, suggesting a causal link to increased female resource control.

Keywords: Fertility Choice, Intrahousehold Allocation, Bargaining Power, Reparations, Holocaust.

JEL: J13, J16, D13.

*We thank Sascha Becker, Alma Cohen, Maxime Gravoueille, Pauline Grosjean, Seema Jayachandran, Esteban Klor, Omer Moav, Claudio Labanca, Federico Masera, Solmaz Moslehi, Itzchak Tzachi Raz, Itay Saporta Eksten, Analia Schlosser, Sarit Weisburd, and participants at Monash University, Tel Aviv University, University of New South Wales, the 2023 Annual Meeting of the Israeli Economic Association and the 2024 Australian Gender Economics Workshop for valuable comments and Ofir Golboa for excellent research assistance. The research was conducted in the Research Room of the Central Bureau of Statistics (CBS). We thank Hadas Yafe and David Gordon from CBS for constructing the dataset and for providing technical support, and Dana Haftzadi from the Authority for the Rights of Holocaust Survivors in Israel for providing information and data regarding the compensation provided to Holocaust survivors by the State of Israel. Hazan thanks the Pinhas Sapir Center for Development for financial support. Hazan: Department of Economics, Monash University, 900 Dandenong Road, Caulfield East VIC 3145, Australia. E-mail: moshe.hazan@monash.edu. Tsur: Research Department, Bank of Israel, P.O. Box 780, Jerusalem 9100701, Israel. E-mail: shay.tsur@boi.org.il.

1 Introduction

We examine how increased female bargaining power affects household fertility decisions, exploiting a quasi-natural experiment involving Holocaust reparations paid to survivors in Israel. After World War II, two main compensation schemes were introduced: German reparations under the 1956 Federal Compensation Law (BEG), and Israeli reparations beginning in 1957. German reparations targeted survivors who had lived within Germany's 1937 borders or could demonstrate cultural and linguistic ties to Germany. Israeli reparations compensated those excluded from the German program who had immigrated to Israel by October 1953, partly financed through the collective reparations Israel received from Germany under the 1952 German–Israeli Reparations Agreement, discussed further in Section 2. Both programs involved large and durable transfers: German recipients typically received a lump sum equivalent to Israel's GDP per capita in the mid-1950s and a lifelong monthly stipend equal to about 30% of the average wage at the time, while Israeli recipients received a smaller lump sum (about 25% of GDP per capita) and a lifelong stipend worth around 12% of the average wage in the late 1950s. A third group—primarily survivors who arrived after 1953 and did not meet the German criteria—remained without compensation until the 1990s or later, when Germany established the Section 2 Fund in 1996 and Israel passed further amendments in the early 2000s.[1]

We estimate a triple-difference (DDD) model comparing fertility outcomes before and after treatment, across early (1950s) vs. late (1990s) recipients, and female vs. male recipients. We extend this to a four-way difference-in-differences (DDDD) design by incorporating heterogeneity in the wife's age at treatment. Fertility was significantly lower among households in which the female partner was age 25 or younger in 1957 and received reparations early, relative to comparable households in which the male partner received reparations. Using late recipients—who received reparations only after their reproductive years—as a comparison group allows us to account for health-related confounders from Holocaust exposure. This enhances internal validity by comparing groups with similar wartime experiences but different exposure to income during childbearing years. An event study shows that fertility differentials emerged in the early 1960s and persisted through the 1970s, amounting to a relative reduction of 0.25 to 0.4 children.

Our findings contribute to the literature on intra-household bargaining, which emerged as a critique of Becker's unitary model of household decision-making (Becker, 1974, 1981). Unlike the unitary model, bargaining frameworks predict that outcomes depend not only on total household income but also on who controls it (Manser and Brown, 1980;

[1] In Section 2, we elaborate on the historical background and the various compensation schemes.

McElroy and Horney, 1981; Chiappori, 1988, 1992; Lundberg and Pollak, 1993).[2] Empirical studies show that women allocate resources differently than men, especially in relation to children's goods and health (Hoddinott and Haddad, 1995; Lundberg et al., 1997; Duflo, 2003; Ward-Batts, 2008; Majlesi, 2016).

Early work on non-labor income and fertility found mixed results: Schultz (1990) reports a positive effect of female income in Thailand, while Thomas (1990) finds the opposite in Brazil. More recent studies suggest that increases in female bargaining power tend to reduce fertility (Rasul, 2008; Ashraf et al., 2014, 2020; Hazan et al., 2023). However, these typically capture short-run outcomes, measured only a few years after treatment. By contrast, our study tracks a large and sustained financial transfer over the full reproductive span–an increasingly relevant perspective in light of ongoing debates around income support and pronatalist policies.

Although historically specific, our setting informs the design of unconditional cash transfers (including UBI). Within the survivor population, reparations were non–means-tested and individually paid (a large lump-sum plus a lifelong stipend). In couples with a single recipient, the payment is controlled by that spouse, shifting intra-household decision weights by construction. As Hoynes and Rothstein (2019) note, long-run effects of unconditional transfers are hard to study; while current research emphasises labour-supply effects of UBI (e.g., Jaimovich et al., 2024), our results suggest there may also be unintended demographic consequences.

We do not take a stand on whether absolute resources or relative control is the key margin. Even under per-adult UBI, individually received payments could still reshape bargaining within couples. These mechanisms also interact with child-contingent pronatalist policies that lower the marginal cost of an additional birth (e.g., Milligan, 2005; Cohen et al., 2013). Where unconditional transfers raise women's individually controlled resources, they could attenuate the effectiveness of such pronatalist incentives operating on the same households.

Although the reparations setting offers a strong case for studying intra-household bargaining, potential identification threats warrant discussion. One concern is take-up: some survivors faced moral or practical barriers to claiming reparations. In the early 1950s, Israeli public figures criticized German reparations as a form of forgiveness for

[2] The Women's Equal Rights Law (1951) guaranteed that married women retained full legal competence over their property and guardianship rights. In *Sidis v. The President and Members of the Great Rabbinical Court* (HCJ 202/57, 12 PD 1528, 1958), the Supreme Court applied the statute to overturn a ruling that had granted a husband the rental income from his wife's premarital property, thereby affirming enforcement of these rights in practice (Barak-Erez, 2025).

Nazi crimes. However, as Tovy (2015) notes, this opposition focused mainly on state-level negotiations. After the 1952 agreement between Israel and West Germany was signed, opposition to personal compensation faded, and few survivors ultimately declined payments.

Practical barriers also existed. Less-educated survivors may have found the claims process—especially for German reparations—difficult to navigate. Others may have been deterred by legal costs. These obstacles were mitigated by the United Restitution Organization (URO), which offered expert assistance for minimal fees. According to Katz (2015), URO services were not only cheaper but sometimes more effective than those of private lawyers, who often delayed or mishandled claims.

Our dataset spans nearly three decades, allowing us to study long-run fertility responses. We construct a retrospective panel by merging cross-sectional census data with administrative records. While this enables us to document fertility precisely, limitations in the data prevent us from directly observing certain mechanisms—such as labor supply or time use.

Nonetheless, we can examine several mechanisms proposed in the literature. For example, Doepke and Tertilt (2009) argue that women prefer fewer but more educated children. Survey data show that men tend to prefer more children than women, both in developing (Westoff, 2010) and developed countries (Doepke and Kindermann, 2019). Tsur (2025) finds that reparations improved children's educational outcomes. Extending that analysis, we find suggestive evidence that children born to young female recipients were 10.9 and 10.7 percentage points more likely to complete high-school matriculation and earn an academic degree, respectively. Although these estimates are sizable, they are only marginally significant and should be interpreted cautiously.

Finally, we find no evidence that more educated women reduced fertility by more, as would be expected if rising opportunity costs were the key driver (Galor and Weil, 1996; Iyigun and Walsh, 2007). This does not rule out the role of opportunity cost, but suggests that the effect is not channeled through increased labor supply—possibly instead reflecting preferences for leisure or reduced home production (Rasul, 2008).

We proceed as follows. Section 2 provides historical background on the compensation programs. Section 3 describes the data and sample construction. Section 4 outlines our empirical approach. Section 5 presents the results. Section 6 explores potential mechanisms. Section 7 concludes.

2 Historical and Institutional Background on Holocaust Reparations in Israel

We follow the chronology of Holocaust reparations in Israel as summarized by Dorner (2008), outlining the main institutional changes in order of their introduction. The discussion emphasizes when eligibility for personal compensation was established and the magnitude of benefits, which are central for defining treatment groups in our empirical analysis.

1952: The German–Israeli Reparations Agreement. In September 1952, Israel and West Germany signed the Luxembourg Agreement, a landmark reparations accord. Negotiations had only begun earlier that year, making the agreement the first large-scale commitment by West Germany to provide compensation for Nazi crimes. However, the agreement focused on collective reparations to the Israeli state—primarily to support the resettlement and integration of Holocaust survivors. Personal compensation for individual survivors was not yet part of the legal framework, and public discourse in Israel at the time focused on the moral and political implications of negotiating with Germany, rather than on individual payments.[3]

1953: Initial German law with narrow eligibility. In 1953, West Germany passed an initial compensation law, which raised public hopes for individual payments. However, the law's narrow eligibility criteria and exclusion of many survivors—particularly those living outside Germany or in Israel—led to widespread disappointment. At this point, the rules governing personal reparations remained ambiguous, and most Israeli survivors had little basis for expecting substantial personal compensation.

1956: Expansion of the Federal Compensation Law (BEG). In 1956, West Germany significantly broadened its compensation program through an amendment to the Federal Compensation Law (BEG). The revised law extended eligibility beyond residents of West Germany to include individuals who had lived within Germany's 1937 borders, even if they resided abroad, and also those who could demonstrate belonging to the German linguistic and cultural sphere.[4] While the cultural-affiliation clause was originally designed for ethnic Germans expelled from Eastern Europe, in practice it also enabled many Jewish survivors from Poland, Romania, and Hungary—some of whom were already living

[3] As part of the Luxembourg Agreement, West Germany required Israel to assume responsibility for compensating its own citizens. This clause was not disclosed to the Israeli public and became widely known only after Israeli survivors' applications under the BEG were rejected following the 1956 amendment (Dorner, 2008).

[4] See § 4 BEG, available at https://www.gesetze-im-internet.de/beg/__4.html.

in Israel—to qualify. Eligible individuals received a lump sum equivalent to roughly 100% of Israel's 1956 GDP per capita and a monthly lifelong stipend worth about 30% of the average wage at the time. The deadline for claims was set at December 31, 1969.

1957: Israeli reparations law enacted. In response to pressure from domestic survivor groups, particularly those excluded from or rejected by the German program, Israel passed the Disabled Victims of Nazi Persecution Law (DNP Law) in 1957. This law provided reparations to survivors who had immigrated to Israel by October 1, 1953 but were ineligible for German payments. Benefits were more modest: a lump sum equal to about 25% of 1957 GDP per capita and a monthly lifelong stipend worth roughly 12% of the average wage in the late 1950s. Immigrants from Germany were excluded, as they remained eligible for direct German compensation.

While both the German and Israeli programs required formal applications, take-up among eligible survivors was widespread. Early moral opposition—especially in the early 1950s—focused on the political symbolism of state-to-state negotiations. According to Tovy (2015), resistance to individual compensation faded quickly after the 1952 agreement, and few survivors declined payments on moral grounds. Practical barriers such as legal complexity or cost were mitigated by the United Restitution Organization (URO), which offered expert legal assistance for minimal fees. As Katz (2015) documents, URO services were cheaper and often more effective than private lawyers, who sometimes mishandled claims or caused delays. These supports likely ensured high take-up, including among less-educated or low-income survivors.

Post-1957: A clear delineation. The 1956–1957 period thus marked the moment when eligibility for personal reparations became clearly defined and widely understood. Our empirical strategy treats this as the treatment point: we define the "early" group as those who became eligible in the 1950s and received benefits during their reproductive years. To conservatively ensure fertility decisions were not affected by anticipatory behavior, we restrict the sample to couples married by 1953.

1990s and 2000s: Late expansions. Subsequent changes in the 1990s and 2000s extended reparations to previously excluded groups. In 1996, Germany established the Section 2 Fund for survivors who had endured particularly severe persecution, and in 2007 Israel enacted the Benefits Law to support those excluded from earlier schemes. Following the recommendations of the Dorner Committee, this law was expanded in 2014 to gradually raise benefits to levels similar to the 1957 DNP Law. These later reforms, summarized in Dorner (2008), define the group of "late" recipients in our analysis, who provide a natural comparison for survivors who received reparations during their childbearing years.

3 Data and Summary Statistics

To evaluate the effect of reparations on fertility, we use a dataset assembled by the Israeli Central Bureau of Statistics (CBS) for this study. The core sample comes from the 1995 and 2008 Israeli population censuses, which contain detailed information on 20% of the population in each wave and allow high-precision identification of German reparation recipients. These data are supplemented with administrative records from the Authority for the Rights of Holocaust Survivors, covering the full universe of Israeli recipients. Because only a random subset appears in the long-form census, about 36% of Israeli recipients are observed in the merged data.[5]

While Israeli recipients are directly observed in administrative records, identifying German recipients in the census is indirect: there is no specific question about Germany. Instead, the census asks whether respondents receive compensation from abroad. We identify likely German recipients as individuals who report foreign compensation, were born in Europe, and immigrated to Israel after the Nazi rise to power. This method aligns well with external estimates. In the 1995 census, 94% of individuals who immigrated before 1969, were born before 1946, and reported foreign compensation were born in Europe—mainly Germany, Poland, and Romania. This group includes 7,128 individuals, implying a national total of about 35,640 given the 20% sample—consistent with administrative estimates of 20,000 surviving recipients in 2008. When adjusted for mortality, the two figures align.

We restrict the sample to couples who were married by 1953 and never remarried. These restrictions ensure that marriage occurred before either spouse could anticipate reparations, making early fertility decisions pre-treatment. The remarriage exclusion is important because only women report the number of children ever born (CEB) in the census; this ensures the reported CEB reflects joint fertility rather than prior unions. A limitation of this approach is that we cannot study the effect of reparations on marital stability itself. If reparations shifted bargaining power within couples, they may also have influenced divorce or remarriage decisions (cf. Voena 2015). Our analysis is therefore limited to fertility outcomes in intact marriages.

We further limit the sample to households in which exactly one spouse received reparations—central to our identification strategy. In cases where both spouses received payments, it is unclear how bargaining power would shift. This leaves 3,906 households

[5]We exclude Israeli recipients not observed in the census because we cannot determine whether their spouse received German reparations. Including these cases would prevent us from restricting the analysis to households where exactly one spouse received reparations—a necessary condition for our identification strategy.

out of 7,451 with at least one reparation recipient. These are divided into four groups: those in which either the husband or wife received reparations early (1950s) and those in which either received them late (primarily 1990s or later).

Although the source data are cross-sectional, we construct a household-year panel from 1950 to 1979 using the Israeli Population Registry. By linking census households to birth records, we recover the birth year of each child and compute annual cumulative fertility. This structure supports both an event-study and triple-difference design, measuring fertility dynamically around treatment timing.

A known limitation of the Registry is undercounting of births in the late 1940s and early 1950s. To assess this, we compare CEB reported by women in the census to the number of children matched in the Registry. The two sources align well for post-1955 births but diverge earlier. Our main analysis uses raw Registry data, which offers consistent and complete post-treatment coverage. To test robustness, we construct an alternative pre-1957 fertility measure by subtracting post-1957 births from CEB.[6] We re-estimate our models beginning in 1956 using this corrected measure. Figure 1 shows fertility trends under both raw and corrected measures, separately by cohort. Section 5 shows that results are not driven by early measurement error.

Figure 1 plots cumulative births from 1950 to 1979 for households in which only one spouse received reparations, distinguishing by gender of recipient and timing of payment. Panels (a) and (b) use raw birth data; Panels (c) and (d) use corrected fertility measures. Panels (b) and (d) restrict the sample to households in which the wife was age 25 or younger in 1957, who were most likely to be affected in their fertility decisions.

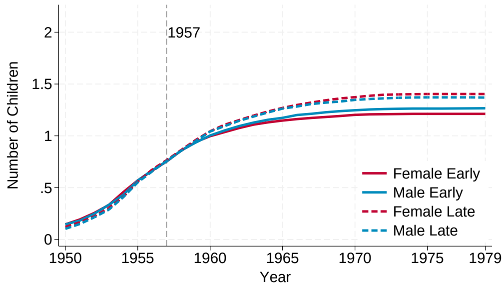
The focus on younger wives is motivated by both historical context and empirical patterns. Although the Holocaust ended in 1945, many survivors remained displaced refugees for several years, delaying marriage and family formation. Women who were already of childbearing age during the war likely made fertility decisions under atypical postwar conditions, whereas younger women—still children or early adolescents when the war ended—were less directly affected. Restricting the sample to wives age 25 or

[6] Let $n_{i,1956}$ denote the number of children woman i had by the end of 1956. We calculate this as

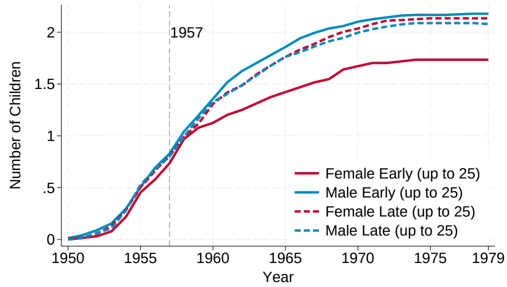
$$n_{i,1956} = n_{i,1995/2008} - \sum_{t=1957}^{1979} \text{birth}_{i,t},$$

where $n_{i,1995/2008}$ is the number of children ever born reported in the 1995 or 2008 census, and $\text{birth}_{i,t}$ is the number of children born in year t according to the Registry. We then reconstruct cumulative fertility for $t \in [1957, 1979]$ as

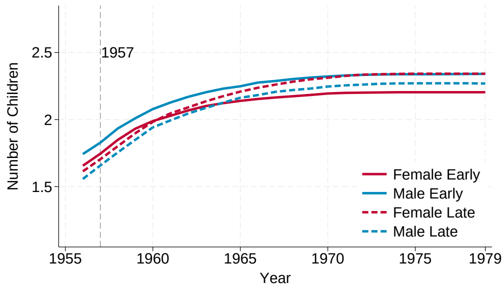
$$n_{i,t} = n_{i,1956} + \sum_{\tau=1957}^{t} \text{birth}_{i,\tau}.$$



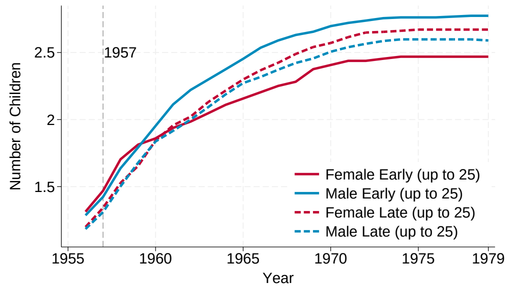
(A) NUMBER OF CHILDREN ALL WOMEN



(B) NUMBER OF CHILDREN - WOMEN UP TO AGE 25 IN 1957



(C) NUMBER OF CHILDREN ALL WOMEN – CORRECTED



(D) NUMBER OF CHILDREN - WOMEN UP TO AGE 25 IN 1957 – CORRECTED

FIGURE 1: COMPARISON OF CHILDREN COUNTS BY YEAR AND COHORT.

Notes: The figures show raw and corrected estimates for all women and for those under 25 years of age in 1957. Corrections account for undercounting of births prior to the mid-1950s in the population registry as discussed above.

younger in 1957 (i.e., 13 or younger in 1945) allows us to examine fertility choices made under more typical postwar circumstances.

Summary statistics support this focus. Among households with younger wives, CEB ranges from 2.47 to 2.77, compared to 2.17 to 2.26 for older wives. Births after 1956 are markedly higher for younger wives: 1.16 to 1.49 vs. 0.43 to 0.47 among older wives. These patterns align with expectations that early-life Holocaust exposure disrupted family formation and suggest that the fertility response to reparations is most pronounced among younger cohorts.

Across all panels, fertility trends are similar before 1957, suggesting strong pre-treatment comparability.[7] After 1957, fertility diverges: households where young wives received early reparations show notably lower completed fertility, especially compared to house-

[7]Formal pre-trend tests from the event study specification are reported below in Figure 2.

holds where the husband received reparations. This pattern appears in both raw and corrected measures.[8]

Although our estimation includes household fixed effects—so identification comes from within-household changes in fertility over time, net of time-invariant differences in fertility preferences or family background—we also assess baseline comparability. Online Appendix Tables 1 and 2 show that households across treatment groups—whether male or female recipient, early or late—are similar in key demographics such as age, marriage duration, education, and origin. Differences in year of immigration reflect design constraints, as early Israeli reparations required arrival by October 1, 1953. These patterns support the credibility of our identification strategy.

Among households with wives age 25 or younger in 1957, observable characteristics are also balanced. Wives' average age is 23.5–23.7; husbands' age is 29–30. Average marriage duration in 1957 is 3.5–4 years. Women completed 9.7–10.3 years of schooling, and men 10.1–10.5, except in households where the wife was married to an early male recipient (9.1).[9]

To provide context, we compare survivor households to non-survivor households married by 1953, with at least one spouse immigrating from Europe by 1972. The average CEB among survivor households is 2.29, compared to 2.36 among non-survivors. As shown in Online Appendix Figure 2, survivor households are more likely to have exactly two children and less likely to have smaller or larger families. With the obvious reservations that apply to Holocaust survivors, their fertility patterns closely resemble those of European-descendant households in Israel, suggesting that our results may not be entirely idiosyncratic to this group.

4 Empirical Strategy

Our identification strategy uses a triple-difference (DDD) design to estimate the effect of increased female control over household resources—via early reparations—on fertility. The variation comes from three sources: (i) whether reparations were received early

[8] Appendix Figure 1 presents a complementary analysis plotting pre- and post-1956 fertility by women's age in 1957, separately by recipient gender and timing, offering a finer-grained check on pre-treatment comparability.

[9] Online Appendix B.1 reports a regression showing a *positive* but statistically insignificant correlation between men's years of schooling and fertility in this group. Appendix E.3 presents results excluding households where the male had fewer than 10 years of schooling. With this sample selection, male years of schooling are more balanced–with between-group gaps no larger than 0.5 years. Results remain similar, alleviating concerns that male education drives the findings.

(1950s) or late (1990s or later), (ii) whether the recipient was the wife or husband, and (iii) whether the year is post-treatment (1957 or later). We estimate household-year regressions from 1950 to 1979, leveraging the fact that exactly one spouse in each household received reparations. All specifications include household fixed effects, year fixed effects, and wife's age fixed effects, absorbing the main effects of treatment variables and life-cycle fertility trends.

Our baseline specification is:

$$\begin{aligned} n_{it} = {} & \beta_1(\text{Female}_i \times \text{Post}_t) + \beta_2(\text{Early}_i \times \text{Post}_t) \\ & + \beta_3(\text{Female}_i \times \text{Early}_i \times \text{Post}_t) + \lambda_t + \gamma_a + \mu_i + \epsilon_{it} \end{aligned} \tag{1}$$

where n_{it} is the cumulative number of children born in household i by year t; Female_i equals 1 if the recipient is the wife; Early_i indicates receipt of reparations in the 1950s; and Post_t equals 1 for years 1957 and later. The coefficient β_3 captures the effect of early reparations received by the wife, relative to the husband.[10]

As discussed in Section 3 and shown in Figure 1, fertility effects are concentrated among households where the wife was 25 or younger in 1957. These women were in the early to middle stages of fertility, with about 1.2–1.3 births by 1957 and substantial fertility remaining. To capture this, we extend the DDD model by interacting treatment with an indicator for being "young" in 1957:

$$\begin{aligned} n_{it} = {} & \beta_1(\text{Female}_i \times \text{Post}_t) + \beta_2(\text{Early}_i \times \text{Post}_t) + \beta_3(\text{Young}_i \times \text{Post}_t) \\ & + \beta_4(\text{Early}_i \times \text{Young}_i \times \text{Post}_t) + \beta_5(\text{Female}_i \times \text{Young}_i \times \text{Post}_t) \\ & + \beta_6(\text{Female}_i \times \text{Early}_i \times \text{Post}_t) + \beta_7(\text{Female}_i \times \text{Early}_i \times \text{Young}_i \times \text{Post}_t) \\ & + \lambda_t + \gamma_a + \mu_i + \epsilon_{it} \end{aligned} \tag{2}$$

where $\text{Young}_i = 1$ if the wife was 25 or younger in 1957. The coefficient β_7 captures the additional effect of early reparations received by young women, while β_6 captures the effect for older women. The sum $\beta_6 + \beta_7$ gives the total effect for young female recipients—our main effect of interest. In Section 5, we report estimates of β_6, β_7, and their sum, and assess robustness to varying the age cutoff from 23 to 29.

To examine dynamics and test for pre-trends, we estimate an event-study version of this model, replacing the post-treatment indicator with a full set of year dummies interacted with treatment group variables. Specifically, we substitute $\beta_6(\text{Female}_i \times \text{Early}_i \times \text{Post}_t)$

[10] The terms Female_i, Early_i, Post_t, and $\text{Female}_i \times \text{Early}_i$ are absorbed by household, year, and age fixed effects and thus omitted from the equation.

and $\beta_7(\text{Female}_i \times \text{Early}_i \times \text{Young}_i \times \text{Post}_t)$ with $\sum_{k \neq 1956} \beta_{6k}(\text{Female}_i \times \text{Early}_i \times D_k)$ and $\sum_{k \neq 1956} \beta_{7k}(\text{Female}_i \times \text{Early}_i \times \text{Young}_i \times D_k)$, where D_k is a dummy for calendar year k. These coefficients trace fertility patterns relative to 1956, the year immediately before treatment.

5 Results

5.1 Baseline Triple Difference Estimates

We begin with the baseline DDD model (Equation (1)), which estimates the effect of early reparations on fertility without accounting for age heterogeneity. The coefficient of interest, β_3, captures the interaction Female $\times$ Early $\times$ Post. As shown in Table 1, Column 1, the estimate is small (0.024 children) and statistically insignificant. This aligns with Figures 1a and 1c, which suggest limited effects unless age at treatment is considered.

5.2 Triple Difference Estimates with Age Heterogeneity

We next estimate Equation (2), which allows the effect of early reparations to vary by the wife's age in 1957. The key coefficients are β_6, capturing the baseline effect for all women, and β_7, which captures the additional effect for younger recipients. Results are reported in Columns 2–8 of Table 1, each corresponding to a different age threshold for defining "young," ranging from 23 to 29.

In Column 2, where the young cutoff is age 23, the estimate on the quadruple interaction term β_7 is –0.325 and statistically indistinguishable from zero. This imprecision likely reflects the small number of households in which the wife was age 23 or younger in 1957. At the age 24 cutoff (Column 3), the coefficient increases in magnitude to –0.475 and is statistically significant at the one percent level. The estimated effect remains large and statistically significant at the one percent level for age 25 (–0.421) and age 26 (–0.360), before attenuating in both magnitude and precision at higher cutoffs. At age 27, the estimate is –0.158 and marginally significant; at age 28 and above, it becomes small and statistically insignificant.

To quantify the overall effect for younger women, we compute the sum $\beta_6 + \beta_7$. At the age 25 cutoff, the total effect is –0.342, statistically significant at the five percent level. Similar patterns emerge for other nearby thresholds: –0.414 at age 24 and –0.268 at age 26, both significant at the five percent level. These results indicate that early reparations significantly reduced fertility among younger women, with the strongest effects concentrated in the mid-20s range.

5.3 Event Study Analysis

To examine the timing and persistence of the fertility response, we estimate an event study specification based on Equation (2). Specifically, we replace the post-treatment indicator with a full set of year dummies, interacted with treatment status. This allows us to trace the evolution of cumulative fertility over time, separately for treated and comparison groups, relative to the year immediately preceding treatment (1956).

Figure 2 plots the estimated coefficients for households in which the wife was age 25 or younger in 1957 (red line) and for all women regardless of age (blue line), with 95 percent confidence intervals. In the pre-treatment period both lines remain close to zero, indicating no differential trends before reparations began. For younger women, the red line turns downward in 1959 and falls steadily through the first half of the 1960s, reaching about –0.5 children by 1965. The effect then flattens, consistent with fertility being largely completed once women reached their mid-30s, as also evident in the raw fertility paths in Figure 1. The magnitude and timing of the effects for younger women mirror the DDD estimates with age heterogeneity, and the sum of the year-specific coefficients ($\beta_{6k} + \beta_{7k}$) is statistically significant at the five percent level in every year from 1961 through 1979.[11] Taken together, the event study shows that reparations reduced completed fertility among younger women by about half a child, with effects that emerged soon after payments began and persisted throughout the reproductive span. By contrast, the blue line for older women remains flat, indicating no fertility response.

5.4 Robustness

To address undercounting of early births in the Population Registry, we construct a corrected fertility measure by subtracting post-1957 births from the total number of children ever born (CEB) reported in the census. We re-estimate the DDD and event study models using this corrected measure, initializing fertility in 1956.

Panel B of Table 1 shows that the estimates closely match those in Panel A. For the age 25 cutoff (Column 4), the coefficient on the quadruple interaction is –0.378, and the total effect for younger women is –0.303, both statistically significant at the five percent level.

[11] Online Appendix Figure 3 plots the sum of these coefficients along with their confidence intervals.

TABLE 1: DDD WITH HETEROGENEOUS AGE EFFECTS

Dep. Var.	Number of Children in year t							
Panel A: Full sample (1950–1979)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young cutoff: Up to	–	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28	Age 29
Female×Early×Young×Post (β_7)		-0.325	-0.475***	-0.421***	-0.360***	-0.158	-0.128	-0.046
		(0.222)	(0.176)	(0.147)	(0.126)	(0.114)	(0.102)	(0.098)
Female×Early×Post (β_6)	0.024	0.030	0.061	0.079	0.092*	0.060	0.061	0.037
	(0.046)	(0.047)	(0.048)	(0.048)	(0.049)	(0.051)	(0.053)	(0.054)
Effect for Young ($\beta_6 + \beta_7$)		-0.295	-0.414**	-0.342**	-0.268**	-0.098	-0.067	-0.009
		(0.217)	(0.169)	(0.139)	(0.116)	(0.102)	(0.088)	(0.081)
Time period	1950–79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79
Mean Dep. Var. 1979	1.30	1.30	1.30	1.30	1.30	1.30	1.30	1.30
N	117,180	117,180	117,180	117,180	117,180	117,180	117,180	117,180
Adjusted R^2	0.845	0.846	0.846	0.846	0.846	0.845	0.845	0.845
Panel B: Subsample (1956–1979)								
Female×Early×Young×Post (β_7)		-0.386**	-0.530***	-0.378***	-0.307***	-0.165*	-0.171**	-0.069
		(0.180)	(0.141)	(0.119)	(0.105)	(0.096)	(0.086)	(0.080)
Female×Early×Post (β_6)	0.024	0.035	0.068*	0.075*	0.086**	0.066	0.079**	0.050
	(0.038)	(0.038)	(0.038)	(0.039)	(0.040)	(0.040)	(0.041)	(0.041)
Effect for Young ($\beta_6 + \beta_7$)		-0.351**	-0.462***	-0.303***	-0.221**	-0.099	-0.092	-0.019
		(0.175)	(0.136)	(0.113)	(0.097)	(0.087)	(0.076)	(0.069)
Time period	1956–79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79
Mean Dep. Var. 1979	2.30	2.30	2.30	2.30	2.30	2.30	2.30	2.30
N	93,744	93,744	93,744	93,744	93,744	93,744	93,744	93,744
Adjusted R^2	0.923	0.923	0.923	0.923	0.923	0.923	0.923	0.923
HH, Year, & Age Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
S.E. clustered at HH level	✓	✓	✓	✓	✓	✓	✓	✓
Number of Households	3,906	3,906	3,906	3,906	3,906	3,906	3,906	3,906

Notes: Panel A reports estimates of the effect of receiving early reparations on the lifetime number of children born to young females. Panel B presents analogous estimates using a corrected measure of the number of children in 1956, constructed from information in the 1995 and 2008 censuses. The rows labeled *Female* × *Early* × *Young* × *Post* and *Female* × *Early* × *Post* correspond to β_7 and β_6 in Eq. (2), respectively. The row labeled *Effect for Young* reports the sum $\beta_6 + \beta_7$. The young age cutoff is measured as of 1957. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects.

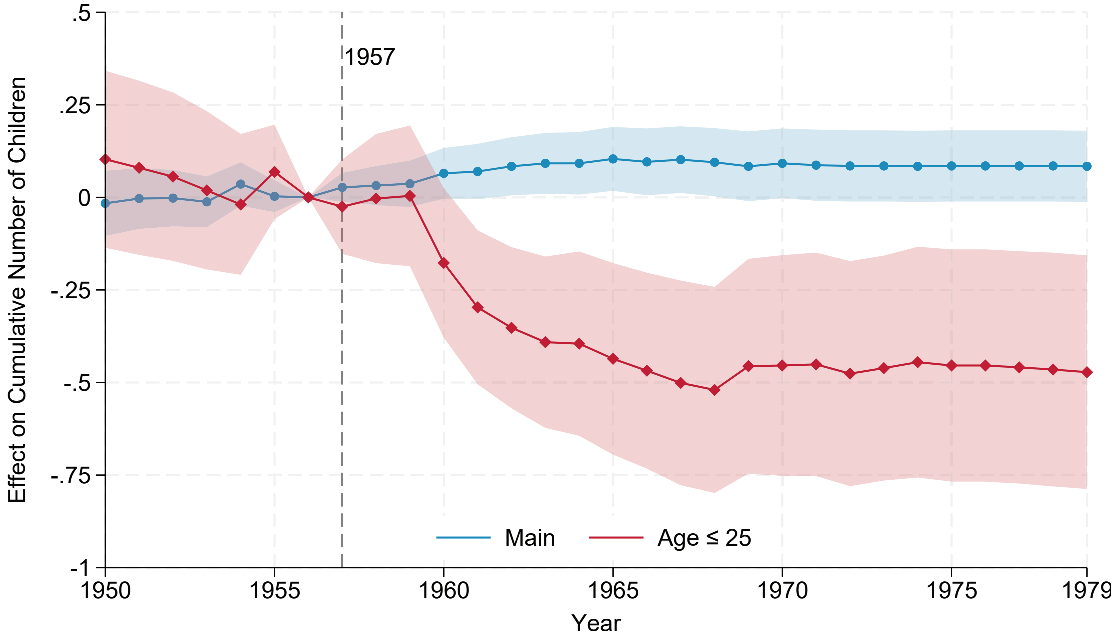


FIGURE 2: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME

Notes: This figure presents event study estimates of the effect of early reparations on the cumulative number of children in the household, relative to the pre-treatment year 1956. The blue line shows estimated effects for households in which the wife was older than 25 in 1957, while the red line shows effects for those in which the wife was 25 or younger in 1957. Shaded areas represent 95% confidence intervals.

Similar patterns hold across other cutoffs, confirming that the main results are not driven by early mismeasurement.

Figure 3 presents the event study based on the corrected measure, beginning in 1956. Fertility among younger women declines from 1960 onward, stabilising around –0.5 children. The estimates closely track those in the main event study, and the sum $\beta_{6k} + \beta_{7k}$ remains statistically significant at the five percent level in every year from 1961 through 1979.[12]

We also assess robustness to the treatment year definition. Varying the cutoff from 1954 to 1959 yields stable estimates.[13] This stability is expected given the stock nature of cumulative fertility.

As an additional check, we re-estimate the DDD and event study models separately within subsamples defined by age in 1957, rather than interacting treatment with age. This allows each specification to be estimated using only households with wives below a given age threshold. Results from this approach, shown in Appendix Section E.2, are consistent with the main findings: estimated effects are large and negative for the youngest women, and fade for older groups.

Finally, as a descriptive complement, Appendix Table 7 reports a cross-sectional analysis of completed fertility. Households in which a young woman received reparations are significantly more likely to end with at most one child. Although effects at higher parities are not statistically significant, the overall pattern is consistent with a shift away from larger families.

5.5 Comparing German and Israeli Reparations

While recipients of German reparations received more generous benefits—both lump-sum and monthly—than recipients of Israeli reparations (Section 2), a natural question is whether fertility responses were correspondingly larger. Appendix Tables 8 and 9 present estimates of the main specification separately by source of reparations. Surprisingly, the estimated effect is larger and more precisely estimated among Israeli recipients than among German recipients.

This discrepancy is likely explained by measurement error in the identification of German recipients. As discussed in Section 3, Israeli recipients are observed directly in administrative data from the Ministry of Finance, whereas German recipients are inferred

[12] Online Appendix Figure 4 plots the sum of these coefficients along with their confidence intervals.
[13] See Online Appendix Table 4.

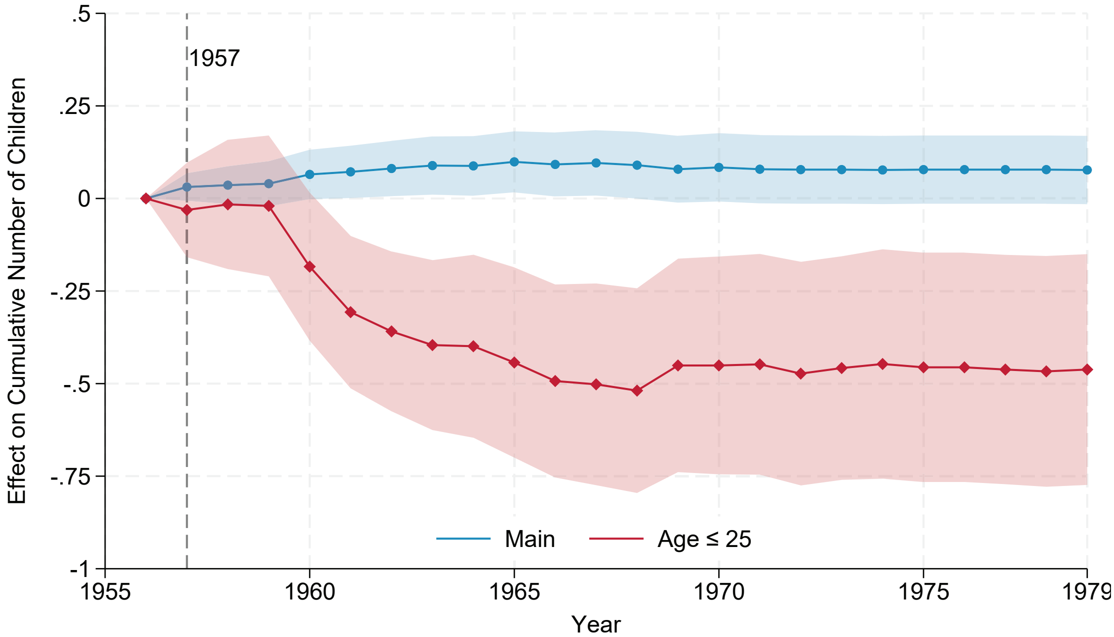


FIGURE 3: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: CORRECTED FERTILITY MEASURE

Notes: This figure presents event study estimates of the effect of early reparations on the cumulative number of children in the household, using the corrected fertility measure to account for underreporting of births prior to 1957. The analysis begins in 1956, one year prior to the treatment. The blue line shows estimated effects for households in which the wife was older than 25 in 1957, while the red line shows effects for those in which the wife was 25 or younger in 1957. Shaded areas represent 95% confidence intervals.

from census reports of foreign pensions and country of origin. This proxy-based assignment likely introduces classical measurement error in treatment status, which attenuates the estimated treatment effect toward zero (Hausman, 2001).

Appendix Section E.5 provides additional detail and discusses the interpretation of these findings.

6 Exploring Potential Mechanisms

To better understand the channels through which early reparations affected fertility, we consider two potential mechanisms: (i) increased investment in child quality, consistent with a quantity–quality tradeoff, and (ii) increased female bargaining power leading to greater labor force participation.

6.1 Quantity–Quality Tradeoff

To test whether reduced fertility reflects greater investment in child quality, we examine adult education outcomes of children born to households where the wife was age 25 or younger in 1957. Each outcome—years of schooling, high school matriculation, and holding an academic degree—is estimated in a separate cross-sectional regression. All specifications control for the child's age in 2010 (and its square), parental age, country of origin, years since immigration, and duration of marriage as of 1957. Standard errors are clustered at the household level. Outcomes are observed when children are between 31 and 61 years old, implying birth years 1949–1979. Thus even the oldest children in our sample were still school-aged when reparations were introduced, so all cohorts could plausibly be affected.[14]

As shown in Table 2, children whose mothers were early recipients are 10.9 and 10.7 percentage points more likely to complete high school matriculation and to earn an academic degree, respectively, relative to children in otherwise similar households where the father was the early recipient or the parents were late recipients. These effects represent increases of 19–27 percent relative to the mean. The estimate for years of schooling is positive but imprecise. While the magnitude of the matriculation and degree effects is substantial, standard errors are large: the matriculation effect is statistically significant at the 10 percent level; the academic degree effect has a p-value of 0.12. We view these results as suggestive but not conclusive evidence that the fertility decline reflected greater

[14]Educational outcomes are from Israel's Education Registry, based on administrative data from the Ministry of Education and higher-education institutions.

TABLE 2: CHILDREN'S EDUCATIONAL OUTCOMES AS ADULTS BY PARENTAL RECIPIENT GROUP

Dep. Var.	(1) Years of Schooling	(2) High School Matriculation	(3) Academic Degree
Female×Early	0.421	0.109*	0.107
	(0.365)	(0.066)	(0.070)
S.E. clustered at HH level	✓	✓	✓
Observations	1,369	1,469	1,469
Number of households	622	639	639
Mean Dep. Var.	14.04	0.572	0.400
R^2	0.131	0.071	0.099

Notes: Dependent variables are: (1) years of schooling completed; (2) an indicator for earning a high school matriculation certificate; and (3) an indicator for holding an academic degree. Each outcome is estimated in a separate regression. Controls include the child's age (in 2010), age squared, and parental age, origin, duration of marriage, and years since immigration (as of 1957). Standard errors clustered at the household level.

investment in child quality.[15] Consistent with this interpretation, Walker et al. (2025) find that female bargaining power raises paternal engagement with children, highlighting the impact of women's resource control on child human capital investments.

6.2 Female Labor Supply

Another possible channel is that women used their increased bargaining power to reduce fertility in favor of labor force participation. In the mid-1950s, labor force participation among married Jewish women in Israel was low: only 16.6 percent of married women aged 14 and over—and just 19.8 percent of those aged 20–24—were in the labor market in June 1954 (Hovne, 1961, Table 21). Although our data do not include retrospective labor supply measures, we observe years of schooling, which proxy the opportunity cost of time.

We restrict attention to households in which the wife was age 25 or younger in 1957. For this group, we first replicate the main effect (–0.341, significant at the 5% level; see Online Appendix Table 5, Column 3). We then estimate a triple-difference model that interacts treatment with indicators for being "educated," defined in three ways: (i) above-median schooling, (ii) top quartile, or (iii) academic degree. In all cases, the coefficient on the

[15] Results are similar when estimated separately for male and female children.

quadruple interaction (Female$\times$Early$\times$Educated$\times$Post) is positive, small, and statistically insignificant. This pattern is consistent with labor supply not being the main channel, though our test is indirect since education is only an imperfect proxy for labor market attachment. Unfortunately, retrospective labor supply measures are not available.[16]

7 Concluding Remarks

This paper examines how increased financial control by women affects fertility, leveraging the quasi-natural experiment of Holocaust reparations paid to survivors in Israel. By comparing fertility outcomes by timing of receipt, recipient gender, and age, we show that young women who received reparations early had significantly lower fertility than comparable households in which the male received reparations. The effect—emerging after 1957 and persisting through the end of the reproductive years—amounts to a reduction of 0.25–0.4 children.

We find suggestive evidence that children in these households attained higher educational outcomes, consistent with a quantity–quality tradeoff. However, the fertility decline is not more pronounced among more educated women, suggesting that labour supply responses are unlikely to be the main channel. Instead, the findings are consistent with a shift in bargaining power that allowed women to reduce home production and family size.

Beyond this historical case, our evidence points to a mechanism: when women's individually controlled resources rise, completed fertility falls. This creates a potential policy tension with per-adult UBI. Unconditional transfers that raise individually controlled resources may dampen the effectiveness of pronatalist, child-contingent programmes that lower the marginal cost of an additional birth. If UBI were to replace or scale back child benefits, the per-child subsidy channel would weaken and the resource-control channel we identify would become more salient; if both operate concurrently, the net effect is theoretically ambiguous and depends on programme design (recipient, timing, size) and on whether absolute or relative resources drive bargaining weights. We view our estimates as mechanism-level evidence that should inform forecasts of fertility under unconditional cash policies.

[16] See Online Appendix Table 3.

References

Ashraf, Nava, Erica Field, Alessandra Voena, and Roberta Ziparo, "Maternal Mortality Risk and Spousal Differences in the Demand for Children," *Working Paper*, 2020.

— , — , and Jean Lee, "Household Bargaining and Excess Fertility: An Experimental Study in Zambia," *American Economic Review*, 2014, *104* (7).

Barak-Erez, Daphne, "Religious Courts as State Courts and the Quest for Gender Equality: Normative Limits, Avoidance and Competition," Jean Monnet Working Paper Jean Monnet Working Paper 2/25, Jean Monnet Center for International and Regional Economic Law & Justice, NYU School of Law, New York, NY 2025.

Becker, Gary S., "A Theory of Social Interactions," *Journal of Political Economy*, 1974, *82* (6), 1063–1093.

— , *A Treatise on the Family*, Cambridge: National Bureau of Economic Research, May 1981.

Chiappori, Pierre-Andre, "Rational Household Labor Supply," *Econometrica*, January 1988, *56* (1), 63–90.

— , "Collective Labor Supply and Welfare," *Journal of Political Economy*, June 1992, *100* (3), 437–467.

Cohen, Alma, Rajeev Dehejia, and Dmitri Romanov, "Do Financial Incentives Affect Fertility," *Review of Economics and Statistics*, 2013, *1* (1), 1–20.

Doepke, Matthias and Fabian Kindermann, "Bargaining over Babies: Theory, Evidence, and Policy Implications," *American Economic Review*, September 2019, *109* (9), 3264–3306.

— and Michèle Tertilt, "Women's Liberation: What's in it for Men?," *The Quarterly Journal of Economics*, 2009, *124* (4), 1541–1591.

Dorner, Dalia, "Commission of Inquiry into Assistance to Holocaust Survivors (In Hebrew)," Technical Report, The State of Israel - The Judicial Authority 2008.

Duflo, Esther, "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa," *The World Bank Economic Review*, 06 2003, *17* (1), 1–25.

Galor, Oded and David N. Weil, "The Gender Gap, Fertility, and Growth," *American Economic Review*, June 1996, *86* (3), 374–387.

Hausman, Jerry, "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left," *Journal of Economic Perspectives*, December 2001, *15* (4), 57–67.

Hazan, Moshe, David Weiss, and Hosny Zoabi, "Women's Liberation and the Demographic Transition," *CEPR Discussion Paper DP16838 v.3*, 2023.

Hoddinott, John and Lawrence Haddad, “Does Female Income Share Influence Household Expenditures? Evidence from Côte D’Ivoire,” *Oxford Bulletin of Economics and Statistics*, 1995, *57* (1), 77–96.

Hovne, Avner, *The Labor Force in Israel*, Jerusalem: The Falk Project for Economic Research in Israel, 1961.

Hoynes, Hilary and Jesse Rothstein, “Universal Basic Income in the United States and Advanced Countries,” *Annual Review of Economics*, 2019, *11*, 929–958.

Iyigun, Murat and Randall Walsh, “Endogenous Gender Power, Household Labor Supply and the Quantity-Quality Tradeoff,” *Journal of Development Economics*, 2007, *82* (1), 138–155.

Jaimovich, Nir, Itay Saporta-Eksten, Ofer Setty, and Yaniv Yedid-Levi, “Universal Basic Income: Inspecting the Mechanisms,” *The Review of Economics and Statistics*, 08 2024, pp. 1–27.

Katz, Yosi, ““Holocaust Profits”: Personal Claims from Germany as an Income Source in the 1950s and 1960s,” *Israel*, 2015, *15*, 137–165. (in Hebrew).

Lundberg, Shelly and Robert A. Pollak, “Separate Spheres Bargaining and the Marriage Market,” *Journal of Political Economy*, 1993, *101* (6), 988–1010.

Lundberg, Shelly J., Robert A. Pollak, and Terence J. Wales, “Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit,” *The Journal of Human Resources*, 1997, *32* (3), 463–480.

Majlesi, Kaveh, “Labor market opportunities and women’s decision making power within households,” *Journal of Development Economics*, 2016, *119*, 34–47.

Manser, Marilyn and Murray Brown, “Marriage and Household Decision-making: A Bargaining Analysis,” *International Economic Review*, 1980, *21*, 31–44.

McElroy, Marjorie B. and Mary Jean Horney, “Nash-Bargained Household Decisions: Toward a Generalization of the Theory of Demand,” *International Economic Review*, 1981, *22*, 333–349.

Milligan, Kevin, “Subsidizing the Stork: New Evidence on Tax Incentives and Fertility,” *Review of Economics and Statistics*, 2005, *87* (3), 539–555.

Rasul, Imran, “Household bargaining over fertility: Theory and evidence from Malaysia,” *Journal of Development Economics*, 2008, *86* (2), 215–241.

Schultz, Paul, “Testing the Neoclassical Model of Family Labor Supply and Fertility,” *The Journal of Human Resources*, 1990, *25* (4), 599–634.

Thomas, Duncan, “Intra-Household Resource Allocation: An Inferential Approach,” *The Journal of Human Resources*, 1990, *25* (4), 635–664.

Tovy, Jacob, *Destruction and Accounting: the State of Israel and the Reparations from Germany 1949-1953 (In Hebrew)*, Tel-Aviv University Press, 2015.

Tsur, Shay, "Rebuilding Through Education: How Holocaust Survivors Benefits Shaped Their Children's Life Outcomes," 2025. Unpublished Manuscript.

Voena, Alessandra, "Yours, Mine, and Ours: Do Divorce Laws Affect the Intertemporal Behavior of Married Couples?," *American Economic Review*, August 2015, *105* (8), 2295–2332.

Walker, Sarah, Pauline Grosjean, Alejandrina Cristia, and Adeline Delavande, "Hefor-She: Bargaining Power, Parental Beliefs, and Parental Speech Investments," 2025. Working paper.

Ward-Batts, Jennifer, "Out of the Wallet and into the Purse: Using Micro Data to Test Income Pooling," *Journal of Human Resources*, 2008, *43* (2), 325–351.

Westoff, Charles F., "Desired Number of Children: 2000-2008," 2010. DHS Comparative Reports No. 25.

Online Appendix for "She who Pays the Piper Calls the Number: Reparations and Gender Differences in Fertility Choice"

This Online Appendix accompanies the paper *"She who Pays the Piper Calls the Number: Reparations and Gender Differences in Fertility Choice."* It compiles additional figures, tables, and robustness checks referenced in the main text.

The appendix is organized as follows. Section A provides a cross-sectional validation of identification by plotting fertility by wife's age in 1957, stratified by timing of receipt (early vs. late) and by recipient gender. Section B reports detailed summary statistics, with a balance check on male schooling and fertility in Section B.1, and a comparison of completed-fertility distributions for survivor vs. non-survivor households in Section B.2. Section C presents event-study estimates of the combined effect for young women, including the version initialized in 1956 using the corrected measure as described in footnote 6 in the main paper. Section D examines heterogeneity by women's education. Section E gathers robustness and sensitivity analyses, including alternative treatment start dates (Section E.1) and age-restricted triple-difference estimates (Section E.2). Finally, we provide descriptive cross-sectional estimates by parity (Section E.4) and compare effects by source of reparations—Israeli vs. German (Section E.5).

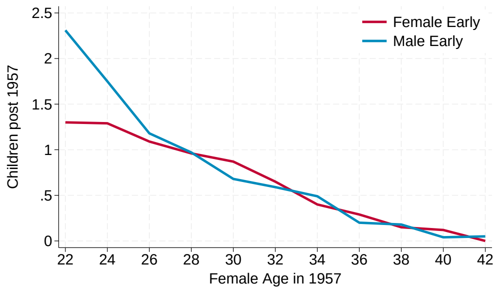
A Fertility by Female Age and Recipient Gender – Cross-Sectional Validation of Identification

To further support our identification strategy, Figure 1 presents cross-sectional fertility profiles by female age in 1957 for both early and late recipient households, separated by recipient gender. Unlike Figure 1 in the main paper, which aggregates fertility for all women or for those under a single age threshold, this figure provides a more granular comparison by displaying fertility at each female age at the time of treatment. This allows for a detailed assessment of pre- and post-treatment fertility patterns across the entire age range.

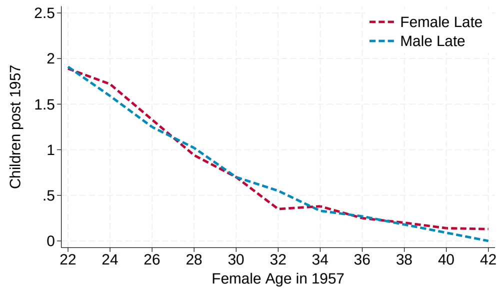
In each panel, the x-axis denotes the woman's age in 1957, regardless of whether the reparations recipient is male or female. This enables a consistent comparison of fertility outcomes across household types.

Panels 1c and 1d display fertility prior to 1957. For both early and late recipient groups, pre-treatment fertility is highly similar between households where the husband or the wife is the recipient, across all ages. This balance supports the parallel trends assumption and suggests that post-treatment differences are not driven by compositional changes or outliers.

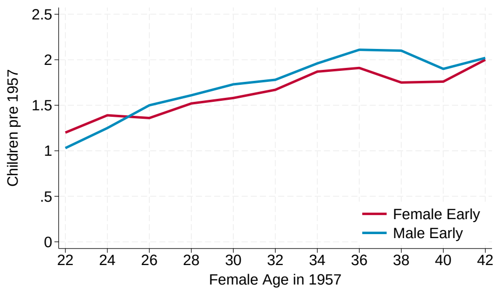
Panels 1a and 1b show post-1956 fertility outcomes for early and late recipient households, respectively. Panel 1a highlights a clear divergence: in Male Early households, fertility is higher, while in Female Early households it is lower. By contrast, Panel 1b shows no difference between Male Late and Female Late households, since the timing of compensation is no longer relevant for fertility. This pattern underscores that the reduction in fertility is specific to cases where young women gained early control over resources.



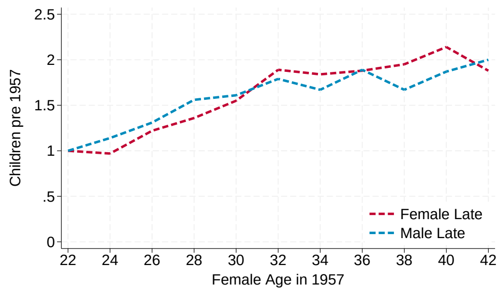
(A) Number of Children Born After 1956, Early Recipients



(B) Number of Children Born After 1956, Late Recipients



(C) Number of Children Born by 1956, Early Recipients (Corrected)



(D) Number of Children Born by 1956, Late Recipients (Corrected)

Figure 1: Pre- and Post-1956 Fertility by Age and Gender of Recipient

Notes: Panels 1a and 1b plot the number of children born after 1956 by age in 1956, separately for households in which only the wife ("Female") or only the husband ("Male") received reparations, split by early and late recipients. Panels 1c and 1d show the cumulative number of children born by 1956 (pre-treatment), using a corrected measure that accounts for undercounting of early births in the registry. All panels distinguish results by the gender of the recipient. Corrections are discussed in the Data section.

B Detailed Summary Statistics

In the main text, we noted that our estimation strategy leverages household fixed effects and relies on variation in the timing, gender, and age of reparations receipt. To support the credibility of this approach, we examined baseline demographic characteristics to assess comparability across groups. As described previously, summary statistics indicate that households in which either spouse received reparations—whether early or late—are broadly similar in terms of age, education, duration of marriage, and country of origin, with differences in year of immigration arising mechanically from eligibility rules.

To provide additional transparency, Appendix Table 1 presents summary statistics for the full sample, while Appendix Table 2 restricts attention to households in which the female partner was age 25 or younger in 1957. Each table reports means (and standard deviations) for key demographic variables by treatment group. This allows readers to verify that treatment and control groups are well balanced on observables, both in the full sample and within the subgroup most relevant for our main results.

TABLE 1: SUMMARY STATISTICS – ALL HHs

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late
Cumulative # of Children (Registry)	1.212	1.404	1.266	1.369
	(1.060)	(1.156)	(1.211)	(1.167)
Children Ever Born (Census)	2.196	2.335	2.338	2.260
	(1.054)	(1.024)	(1.127)	(1.090)
Children Born After 1957	0.540	0.720	0.595	0.703
	(0.731)	(0.907)	(0.884)	(0.902)
Children Born by 1957	1.656	1.614	1.743	1.557
	(0.950)	(0.895)	(0.991)	(0.959)
Male Age in 1957	36.52	34.21	36.13	33.82
	(5.935)	(6.020)	(5.717)	(4.982)
Female Age in 1957	31.72	29.35	31.56	29.84
	(5.025)	(4.665)	(5.741)	(4.890)
Male Years of Schooling	10.80	10.32	10.61	10.34
	(3.625)	(4.462)	(4.393)	(4.334)
Female Years of Schooling	10.09	9.602	10.21	9.882
	(3.351)	(3.623)	(3.667)	(3.739)
Male Years in Israel in 1957	14.08	9.657	9.371	5.066
	(12.39)	(13.34)	(11.80)	(10.10)

Continued on next page

Table 1 – continued from previous page

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late
Female Years in Israel in 1957	9.166	5.750	12.08	8.154
	(8.637)	(9.307)	(14.15)	(13.62)
Duration of Marriage in 1957	8.782	7.853	9.243	7.980
	(3.730)	(3.410)	(4.455)	(3.743)
Male Born in Poland	0.366	0.270	0.302	0.267
	(0.482)	(0.444)	(0.459)	(0.442)
Female Born in Poland	0.300	0.206	0.218	0.154
	(0.459)	(0.404)	(0.413)	(0.362)
Male Born in Asia/Africa	0.0361	0.0453	0.00366	0
	(0.187)	(0.208)	(0.0604)	(0)
Female Born in Asia/Africa	0.00314	0	0.0603	0.0630
	(0.0560)	(0)	(0.238)	(0.243)
Male Born in Romania	0.159	0.264	0.144	0.356
	(0.366)	(0.441)	(0.351)	(0.479)
Female Born in Romania	0.214	0.374	0.165	0.335
	(0.410)	(0.484)	(0.371)	(0.472)
Male Born in Germany	0.0565	0.0433	0.167	0.0362
	(0.231)	(0.204)	(0.373)	(0.187)
Female Born in Germany	0.122	0.0423	0.0786	0.0198
	(0.328)	(0.201)	(0.269)	(0.140)
Male Born in Russia	0.149	0.139	0.115	0.119
	(0.357)	(0.346)	(0.319)	(0.324)
Female Born in Russia	0.118	0.115	0.115	0.160
	(0.323)	(0.319)	(0.319)	(0.366)
Male Born in Other Europe	0.144	0.143	0.253	0.222
	(0.352)	(0.350)	(0.435)	(0.416)
Female Born in Other Europe	0.231	0.263	0.184	0.135
	(0.422)	(0.440)	(0.387)	(0.341)
Male Born in Israel	0.0785	0.0915	0.0110	0
	(0.269)	(0.289)	(0.104)	(0)
Female Born in Israel	0.0126	0	0.157	0.127
	(0.111)	(0)	(0.364)	(0.333)

Observations	637	1,016	1,094	1,159

Notes: Means are reported with standard deviations in parentheses. Sample consists of households with exactly one Holocaust survivor and in which the female partner was age 25 or younger in 1957. "Cumulative # of Children (Registry)" is the total number of children ever born as recorded in the Population Registry, but may undercount births prior to the mid-1950s. "Children Ever Born (Census)" is the lifetime number of children reported by women in the 1995 or 2008 census. "Children Born After 1957" is computed using annual births from the Registry for 1957–1979, as described in footnote 6 in the main paper. "Children Born by 1957" is the difference between the census-reported number of children ever born and the number of children born after 1957, providing a corrected measure for cumulative fertility up to 1957. All other variables are defined in the Data section. See text for definitions of household types.

TABLE 2: SUMMARY STATISTICS – HHS WITH FEMALE AGE UP TO 25

	(1)	(2)	(3)	(4)
Household Type	Female Early	Female Late	Male Early	Male Late
Cumulative # of Children (Registry)	1.734	2.134	2.179	2.079
	(1.027)	(1.207)	(1.282)	(1.201)
Children Ever Born (Census)	2.469	2.662	2.774	2.582
	(0.854)	(1.153)	(1.182)	(0.983)
Children Born After 1957	1.156	1.463	1.488	1.397
	(0.718)	(1.058)	(1.044)	(1.011)
Children Born by 1957	1.312	1.199	1.286	1.184
	(0.833)	(0.862)	(0.910)	(0.794)
Male Age in 1957	29.73	28.96	30.05	29.38
	(4.487)	(3.647)	(3.112)	(3.892)
Female Age in 1957	23.66	23.48	23.65	23.54
	(1.275)	(1.568)	(1.381)	(1.425)
Male Years of Schooling	10.55	10.11	9.14	10.47
	(3.915)	(4.348)	(4.158)	(4.035)
Female Years of Schooling	10.05	9.658	10.32	10.35
	(3.819)	(3.688)	(3.390)	(3.620)
Male Years in Israel in 1957	14.69	12.62	9.863	6.582
	(10.85)	(11.98)	(7.658)	(8.141)
Female Years in Israel in 1957	8.812	6.714	14.49	11.27
	(7.122)	(7.203)	(10.91)	(12.31)
Duration of Marriage in 1957	5.000	4.701	4.655	4.556
	(1.662)	(1.591)	(1.536)	(1.535)
Male Born in Poland	0.297	0.199	0.411	0.268
	(0.460)	(0.400)	(0.493)	(0.444)
Female Born in Poland	0.234	0.212	0.185	0.121
	(0.427)	(0.410)	(0.389)	(0.327)

Continued on next page

Table 2 – continued from previous page

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late
Male Born in Asia/Africa	0.0938	0.117	0	0
	(0.294)	(0.322)	(0)	(0)
Female Born in Asia/Africa	0.0156	0	0.137	0.134
	(0.125)	(0)	(0.345)	(0.341)
Male Born in Romania	0.219	0.277	0.113	0.347
	(0.417)	(0.449)	(0.318)	(0.477)
Female Born in Romania	0.391	0.424	0.119	0.272
	(0.492)	(0.495)	(0.325)	(0.446)
Male Born in Germany	0.0156	0.0433	0.0893	0.0460
	(0.125)	(0.204)	(0.286)	(0.210)
Female Born in Germany	0.0625	0.0173	0.00595	0.00837
	(0.244)	(0.131)	(0.0772)	(0.0913)
Male Born in Russia	0.109	0.0952	0.0893	0.113
	(0.315)	(0.294)	(0.286)	(0.317)
Female Born in Russia	0.0156	0.0606	0.0536	0.105
	(0.125)	(0.239)	(0.226)	(0.307)
Male Born in Other Europe	0.125	0.104	0.292	0.226
	(0.333)	(0.306)	(0.456)	(0.419)
Female Born in Other Europe	0.250	0.286	0.155	0.0753
	(0.436)	(0.453)	(0.363)	(0.264)
Male Born in Israel	0.125	0.165	0.00595	0
	(0.333)	(0.372)	(0.0772)	(0)
Female Born in Israel	0.0312	0	0.310	0.259
	(0.175)	(0)	(0.464)	(0.439)
Observations	64	231	168	239

Notes: Means are reported with standard deviations in parentheses. Sample consists of households with exactly one Holocaust survivor and in which the female partner was age 25 or younger in 1957. "Cumulative # of Children (Registry)" is the total number of children ever born as recorded in the Population Registry, but may undercount births prior to the mid-1950s. "Children Ever Born (Census)" is the lifetime number of children reported by women in the 1995 or 2008 census. "Children Born After 1957" is computed using annual births from the Registry for 1957–1979, as described in footnote 6 in the main paper. "Children Born by 1957" is the difference between the census-reported number of children ever born and the number of children born after 1957, providing a corrected measure for cumulative fertility up to 1957. All other variables are defined in the Data section. See text for definitions of household types.

B.1 Balance Check: Male Years of Schooling and Fertility

As shown in Table 2, for households in which the female partner was age 25 or younger in 1957, average male years of schooling is somewhat lower in the group where the wife was married to an early male recipient (9.14) than in other groups (ranging from 10.11 to 10.55). To assess whether this difference could confound our results, we regress men's years of schooling on the number of children, restricting the analysis to male early recipient households within this age-restricted sample. The estimated coefficient is positive but small (0.018) and statistically insignificant (standard error = 0.038), indicating no evidence that lower male education is associated with larger family size in this group. For completeness, in Section E.3 of this appendix, we report results of our main specifications when we exclude households in which the male partner has fewer than 10 years of schooling. With this sample selection, male years of schooling are more balanced–with between-group gaps no larger than 0.5 years. Our findings remain very similar to the main results, alleviating concerns that differences in male education drive our results.

B.2 Distribution of Completed Fertility: Survivor vs. Non-Survivor Households

To further assess the external validity of our analysis, Figure 2 compares the distribution of completed fertility between survivor households in our analytic sample and a matched group of non-survivor households. The comparison group consists of households with similar characteristics–married by 1953, with at least one spouse who immigrated to Israel from Europe by 1972–but in which neither spouse was a Holocaust survivor. As shown, survivor households are more likely to have exactly two children and less likely to have either very small families (zero or one child) or larger families (three, or four or more children). This pattern supports the representativeness of the survivor sample with respect to completed fertility and household composition.

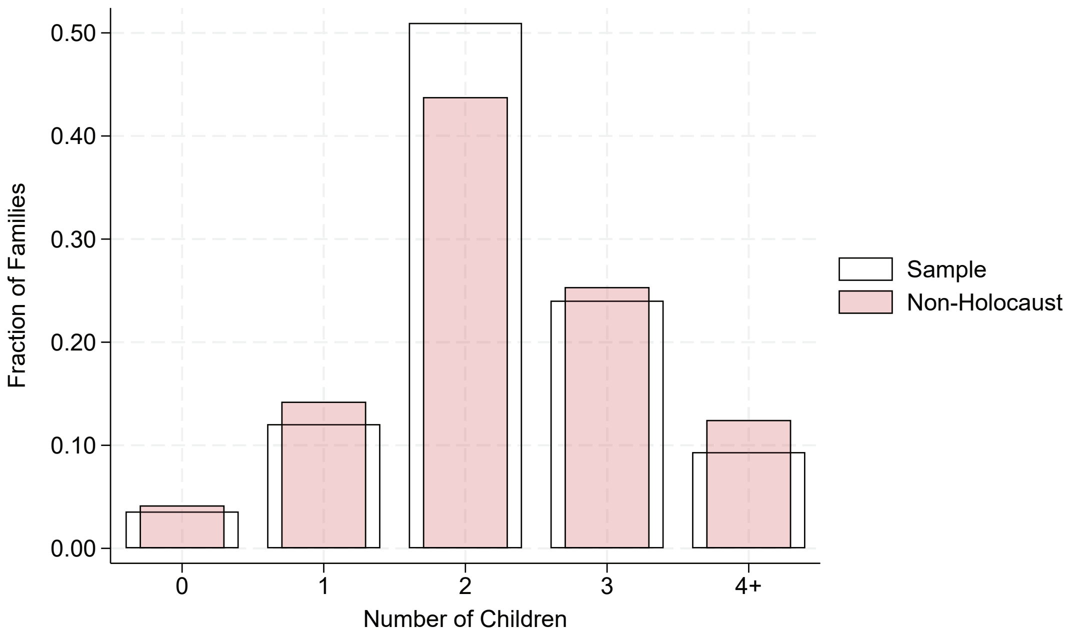


FIGURE 2: DISTRIBUTION OF COMPLETED FERTILITY: SURVIVOR AND NON-SURVIVOR HOUSEHOLDS

Notes: The figure displays the distribution of completed fertility (number of children ever born) for all survivor households in the our sample (3,906 observations) and for a comparison group of non-survivor households (married by 1953, at least one spouse immigrated to Israel from Europe by 1972, neither spouse a Holocaust survivor). Survivor households are more likely to have exactly two children, and less likely to have either very small families (zero or one child) or larger families (three, or four or more children).

C Event Study: Combined Effect for Young Women

Figure 3 presents the event study estimates for the total effect on cumulative fertility among women up to age 25 at the time of reparations. Specifically, for each year, we plot the sum of the coefficients on Female × Early × D_k and Female × Early × Young × D_k (as shown in Figure 2 of the main text), but here we also report joint confidence intervals for this sum. This provides a direct statistical assessment of the effect for the relevant subgroup, rather than relying on visual summation of separate lines in the main event study figure. As discussed in the main text, these estimates show a significant and sustained decline in fertility for young women following early reparations, with the confidence intervals in this figure confirming that the effect is statistically significant from the early 1960s onward.

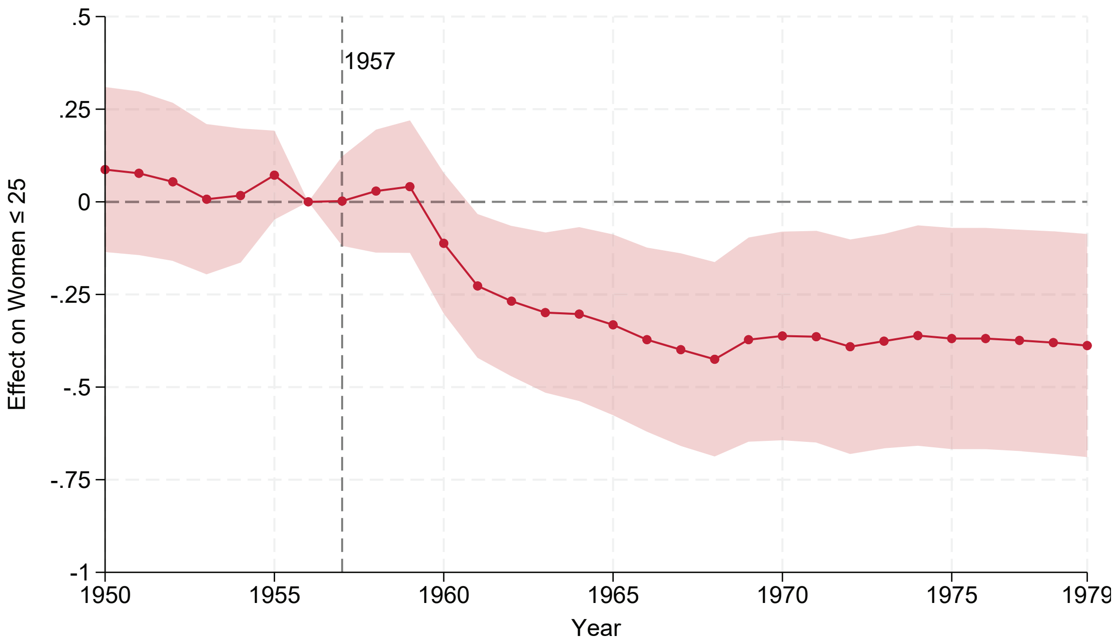


Figure 3: Impact of Early Reparations on Cumulative Fertility Over Time

Notes: This figure presents event study estimates of the total effect of early reparations on the cumulative number of children among women who were 25 or younger in 1957. For each year, we plot the sum of the relevant coefficients from the main specification Female × Early × D_k and Female × Early × Young × D_k, along with joint 95% confidence intervals. This provides a direct assessment of statistical significance for the combined effect in this subgroup, complementing the event study shown in Figure 2 of the main text.

Figure 3 in the main paper presents event study estimates of the effect of early reparations on cumulative fertility, using the corrected fertility measure and starting the analysis in 1956 to address undercounting of early births. That figure shows separate event-study coefficients for younger and older women, each with their own confidence intervals. The discussion in the main text notes that, for younger women, the sum of the relevant coefficients ($\beta_{6k} + \beta_{7k}$) remains statistically significant at the 5% level for each year from 1961 onward.

For completeness, Figure 4 directly plots the annual sum of these coefficients for women up to age 25, along with their joint standard errors. This presentation provides a visual assessment of the total effect and its significance, without requiring the reader to visually sum two separate lines. The results confirm that the reduction in cumulative fertility for young women is large, persistent, and statistically significant throughout the post-treatment period, further strengthening the robustness and clarity of our main findings.

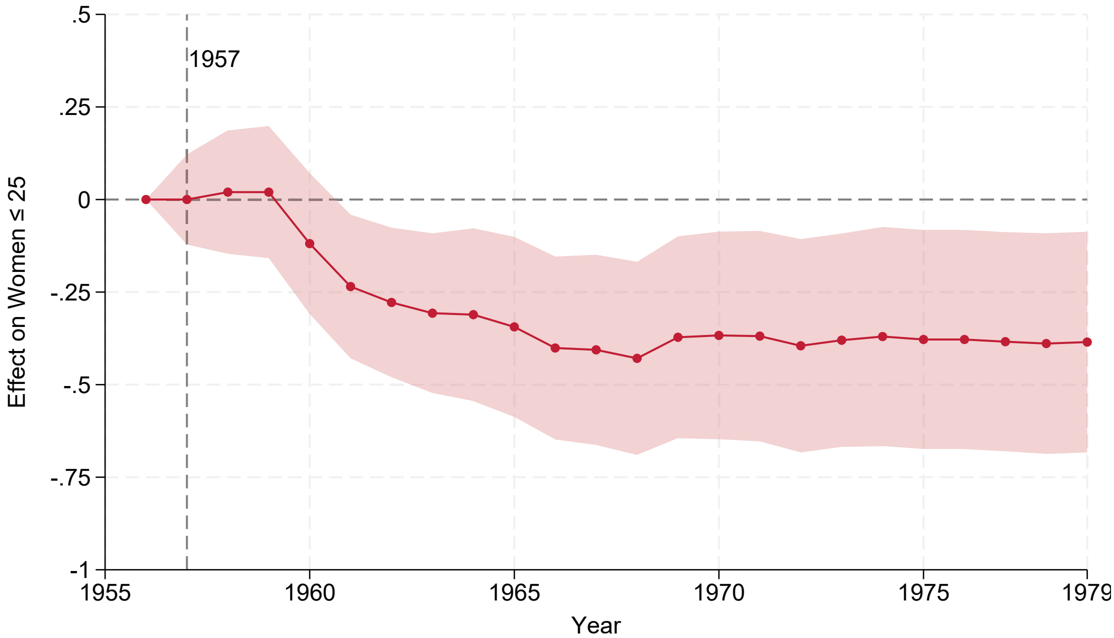


FIGURE 4: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME

Notes: This figure presents event study estimates of the total effect of early reparations on the cumulative number of children among women who were 25 or younger in 1957. The analysis begins in 1956, with cumulative fertility initialized using the corrected number of children ever born by 1956 (see Appendix Section F). For each year, we plot the sum of the relevant coefficients from the main specification (*Female* $\times$ *Early* $\times$ D_k and *Female* $\times$ *Early* $\times$ *Young* $\times$ D_k), along with joint 95% confidence intervals. This provides a direct assessment of statistical significance for the combined effect in this subgroup, complementing the event study shown in Figure 3 of the main text.

D Heterogeneity by Women's Education

Table 3 reports results from estimating the triple-difference model as in Equation (1) of the main paper, but with the "Young" indicator replaced by an indicator for "Educated" status, and the sample restricted to households where the wife was age 25 or younger in 1957. Women are classified as educated according to three alternative definitions: (i) above-median years of schooling (median=10 years of schooling), (ii) top quartile of years of schooling (cutoff=12 years of schooling), and (iii) holding an academic degree (8.5% of women).

In all specifications the main effect of early reparations on fertility for young women is negative and statistically significant in columns 1, 3 and 4. The interaction terms for educated women are positive, small in magnitude, and statistically insignificant across all definitions. These results indicate that the reduction in fertility associated with early reparations was not concentrated among more educated women. This finding does not support the hypothesis that increased female labor supply (due to higher opportunity cost among the more educated) was the main mechanism behind the observed fertility decline.

Table 3: Triple-Difference with Heterogeneous Education Effects

	Cumulative Fertility			
	(1)	(2)	(3)	(4)
Definition of Educated	Baseline	Above Median	Top Quartile	Academic Degree
Female×Early×Post	-0.341^{**}	-0.352	-0.410^{**}	-0.352^{**}
	(0.139)	(0.233)	(0.175)	(0.140)
Female×Early×Post× Educated		0.010	0.184	0.106
		(0.290)	(0.285)	(0.739)
HH, Year, Age FE	✓	✓	✓	✓
S.E. cluster at HH	✓	✓	✓	✓
Time period	1950-1979	1950-1979	1950-1979	1950-1979
Households	702	702	702	702
Observations	21,060	21,060	21,060	21,060
R^2	0.812	0.813	0.813	0.813

Notes: Each column reports results from estimating the triple-difference model in Equation (1) of the main paper, restricting the sample to households where the wife was age 25 or younger in 1957. Column 1 reports the baseline specification with no education interaction (this estimate also appears in Table 5 below). Columns 2–4 introduce an indicator for "Educated" status, defined as (2) above-median years of schooling (cutoff=10 years of schooling), (3) top quartile of schooling (cutoff=12 years of schooling), and (4) having an academic degree (8.5% of women). In these columns, the "Young" indicator in Equation (1) is replaced by "Educated." The outcome is cumulative fertility. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects within the relevant age range.

E Robustness and Sensitivity Analyses

E.1 Sensitivity with respect to Year of Treatment

Table 4 reports results from the same specification as Table 1, column (4), where "Young" is defined as women up to age 25 in 1957. In this table, we vary the definition of the treatment year from 1954 to 1959 to account for any possible uncertainty about when expectations of personal reparations might have begun, in light of the Septemner 1952 collective reparations agreement between Germany and Israel. Although there is little evidence that survivors anticipated personal compensation that early, this robustness check addresses any concerns about earlier expectation effects. Notably, the results in column (4) of this table–where the treatment year is set to 1957–are identical to those in column (4) of the main results table, confirming that this is the baseline specification. Across all columns, the estimated effects are highly consistent, as expected given that the outcome variable (cumulative fertility) is not sensitive to small shifts in the timing of births.

TABLE 4: SENSITIVITY: VARYING THE START YEAR OF EXPOSURE TO REPARATIONS

Dep. Var.	Number of children in year t					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1954	1955	1956	1957	1958	1959
Female×Early×Young×Post	-0.396^{**}	-0.392^{**}	-0.413^{***}	-0.421^{***}	-0.428^{***}	-0.442^{***}
	(0.155)	(0.155)	(0.151)	(0.147)	(0.144)	(0.140)
Female×Early×Post	0.082	0.074	0.076	0.079	0.078^{*}	0.077^{*}
	(0.052)	(0.051)	(0.050)	(0.048)	(0.047)	(0.046)
Effect for Young	-0.314^{**}	-0.318^{**}	-0.337^{**}	-0.342^{**}	-0.350^{**}	-0.365^{***}
	(0.146)	(0.146)	(0.143)	(0.139)	(0.136)	(0.132)
HH, Year, Age FE	✓	✓	✓	✓	✓	✓
S.E. cluster at HH	✓	✓	✓	✓	✓	✓
Time period	1950-1979	1950-1979	1950-1979	1950-1979	1950-1979	1950-1979
# of Households	3,906	3,906	3,906	3,906	3,906	3,906
N	117,180	117,180	117,180	117,180	117,180	117,180
R^2	0.851	0.851	0.851	0.851	0.851	0.851

Notes: Each column reports results from the baseline regression where the definition of "treatment year" (the first year households are considered exposed to reparations) is varied from 1954 to 1959, as indicated. The outcome is the number of children in year t. "Young" is defined as female age up to 25 in 1957. The row labeled Effect for Young is the sum of the coefficients on Female×Early×Young×Post and Female×Early×Post. Standard errors (in parentheses) are clustered at the household level. All specifications include household, year, and age fixed effects.

E.2 Age-Restricted Triple-Difference Estimates

As a robustness check, we replicate our main analysis by estimating the triple-difference model in Equation (1) of the main paper on subsamples defined by each age cutoff. For each cutoff, we restrict the sample to households in which the wife was at most that age in 1957, and apply the same specification, including fixed effects for household, year, and wife's age within the relevant range.

Table 5 reports results for age cutoffs ranging from 23 to 29, paralleling the main analysis. The coefficient on Female × Early × Post is negative and statistically significant for the youngest groups, with the effect attenuating as the age cutoff increases.

TABLE 5: TRIPLE-DIFFERENCE ESTIMATES FOR AGE-RESTRICTED SAMPLES (WIFE'S AGE IN 1957)

	Cumulative Fertility						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample age cutoff	Up to 23	Up to 24	Up to 25	Up to 26	Up to 27	Up to 28	Up to 29
Female×Early×Post	−0.276	-0.411^{**}	-0.341^{**}	-0.263^{**}	−0.094	−0.062	−0.003
	(0.214)	(0.169)	(0.139)	(0.116)	(0.102)	(0.088)	(0.082)
Female×Post	−0.193	−0.015	0.023	0.016	−0.021	−0.034	−0.042
	(0.120)	(0.096)	(0.077)	(0.067)	(0.059)	(0.052)	(0.048)
Early×Post	−0.031	0.078	0.085	0.021	−0.014	−0.013	−0.028
	(0.144)	(0.112)	(0.088)	(0.077)	(0.066)	(0.058)	(0.053)
HH, Year, Age FE	✓	✓	✓	✓	✓	✓	✓
S.E. cluster at HH	✓	✓	✓	✓	✓	✓	✓
Time period	1950-1979	1950-1979	1950-1979	1950-1979	1950-1979	1950-1979	1950-1979
Ave. Age in '79	44.1	44.8	45.6	46.2	46.8	47.4	48
Households	293	469	702	943	1202	1508	1769
Observations	8,790	14,070	21,060	28,290	36,060	45,240	53,070
R^2	0.816	0.810	0.812	0.811	0.812	0.811	0.814

Notes: Each column reports results from estimating the triple-difference model in Equation (1) of the main paper on households where the wife was up to the indicated age (measured in 1957). The outcome is cumulative fertility. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects within the relevant age range.

For further illustration, Figures 5–11 present event study plots for each age-restricted sample, where we estimate the event study equivalent of Equation (1) of the main paper. Across all specifications, the reduction in cumulative fertility is most pronounced for the youngest age groups and diminishes as the age cutoff rises, consistent with our main findings. These results further confirm that our conclusions are robust to alternative modeling approaches and sample definitions.

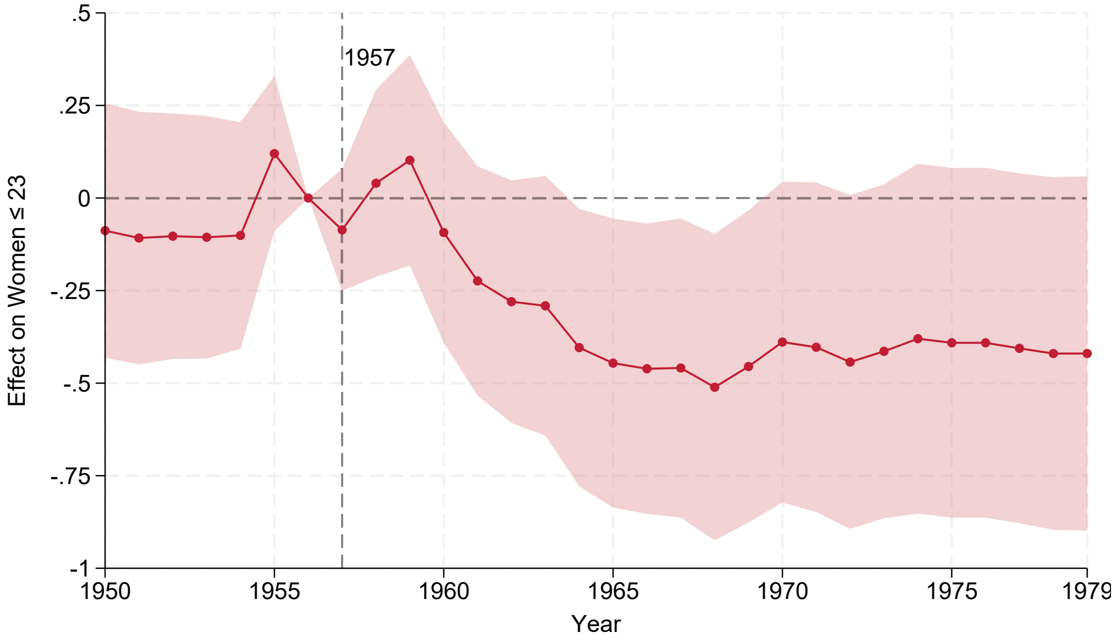


FIGURE 5: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 23 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 23 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

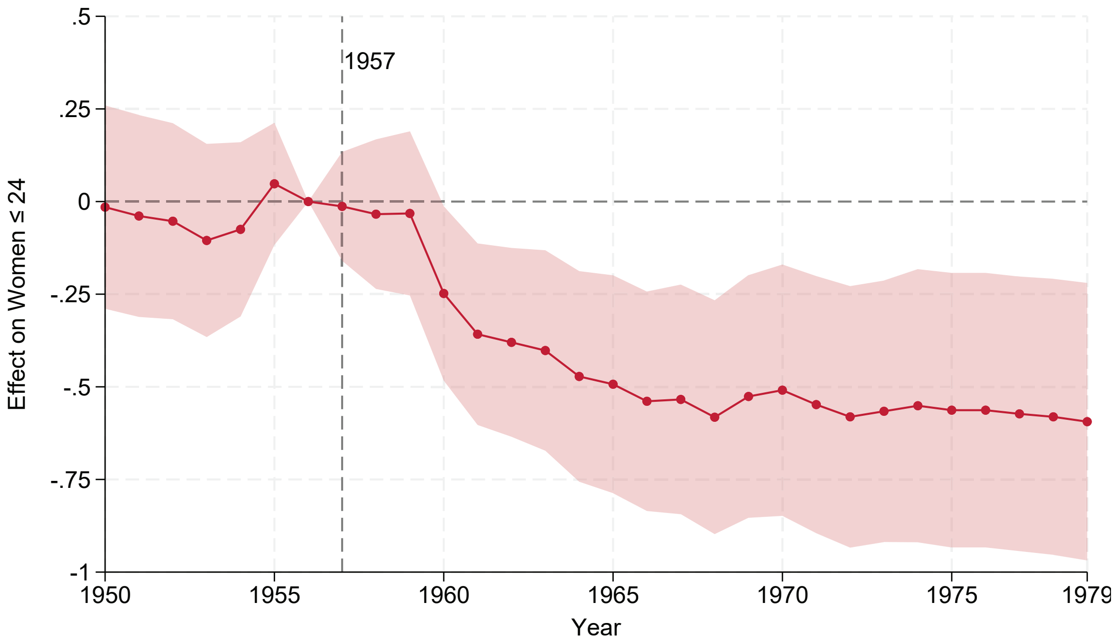


FIGURE 6: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 24 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 24 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

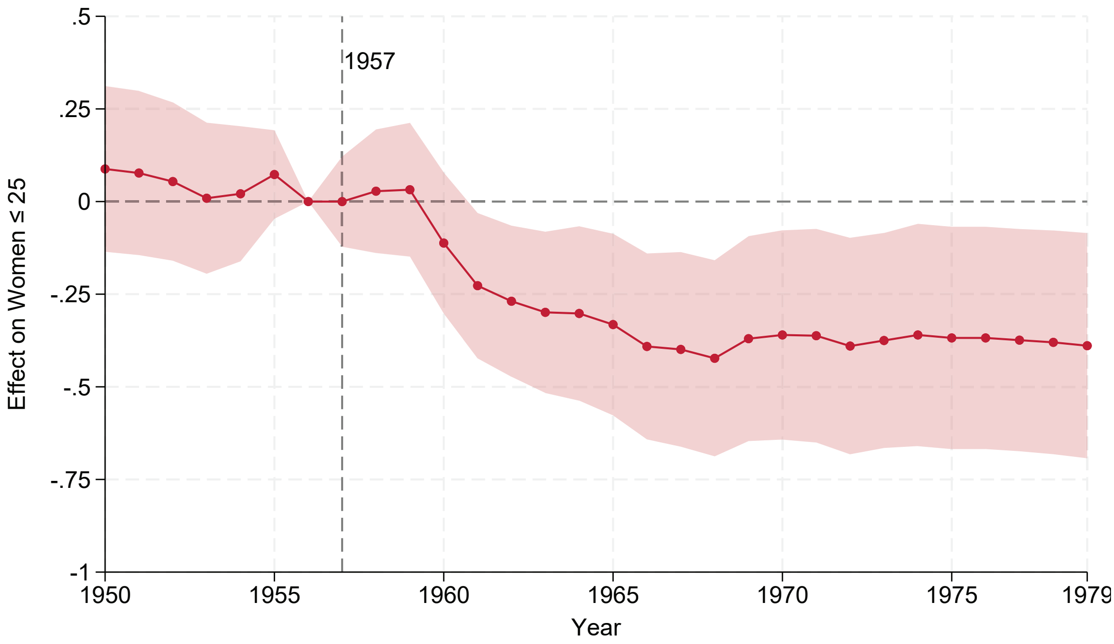


FIGURE 7: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 25 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 25 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

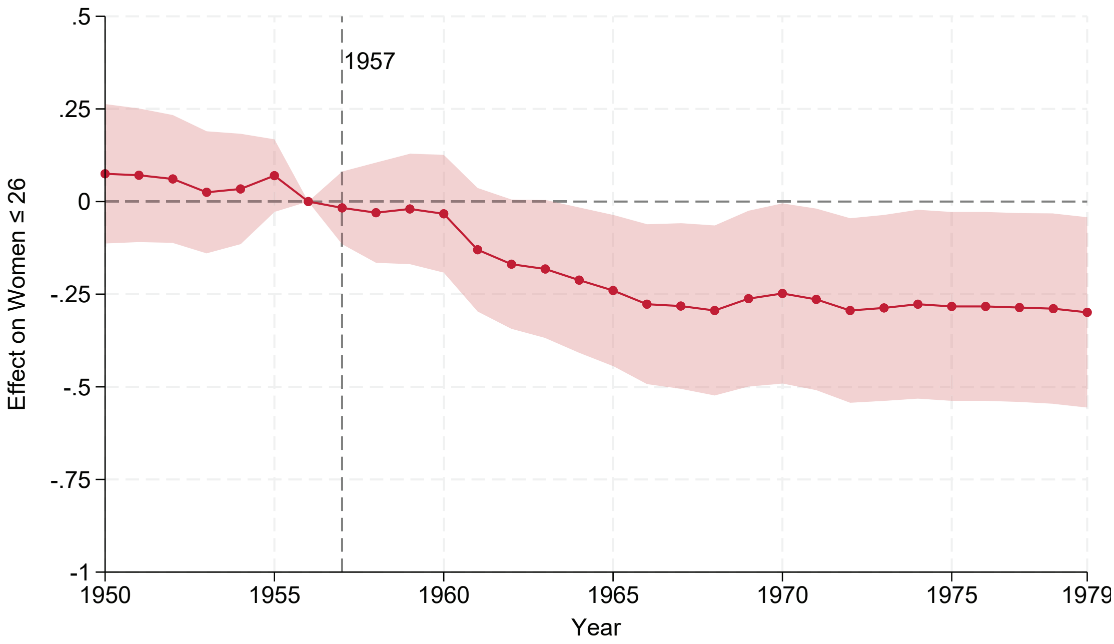


FIGURE 8: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 26 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 26 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

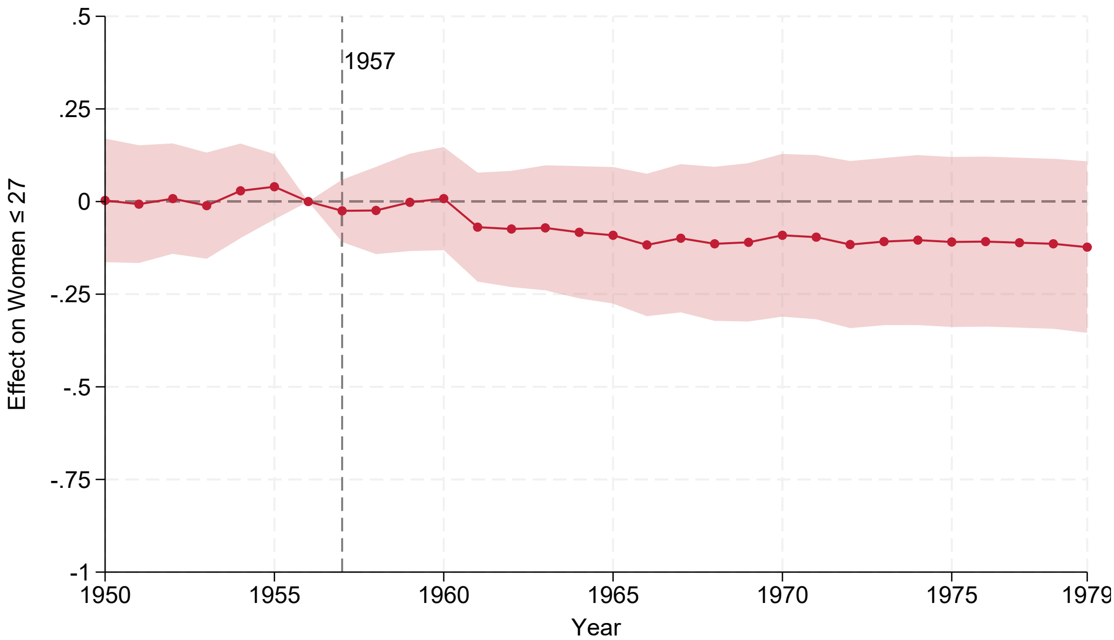


FIGURE 9: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 27 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 27 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

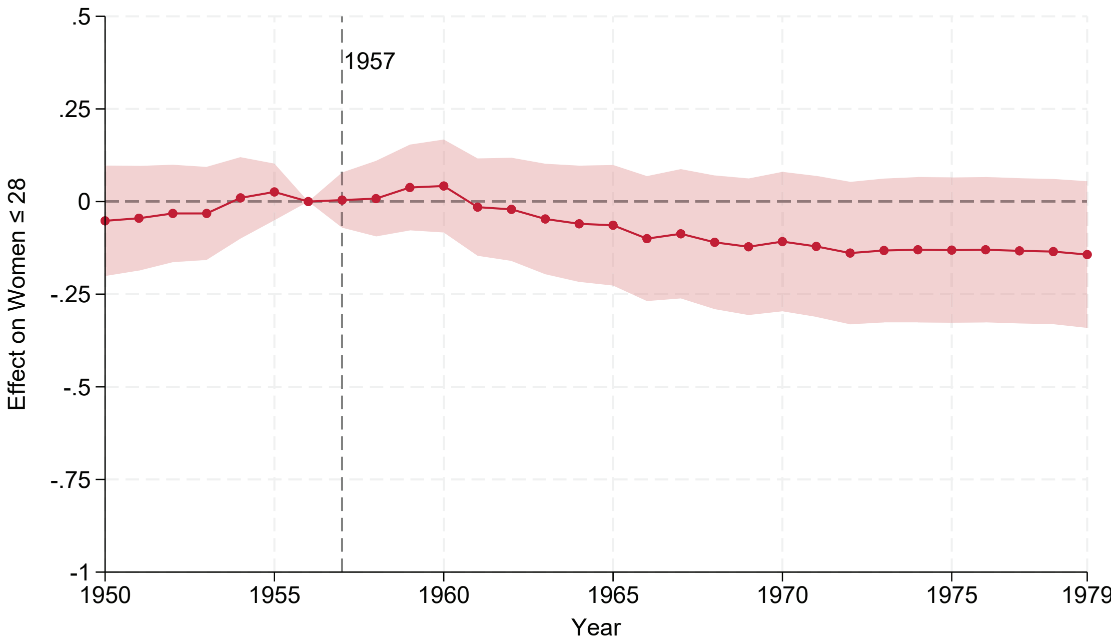


FIGURE 10: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 28 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 28 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

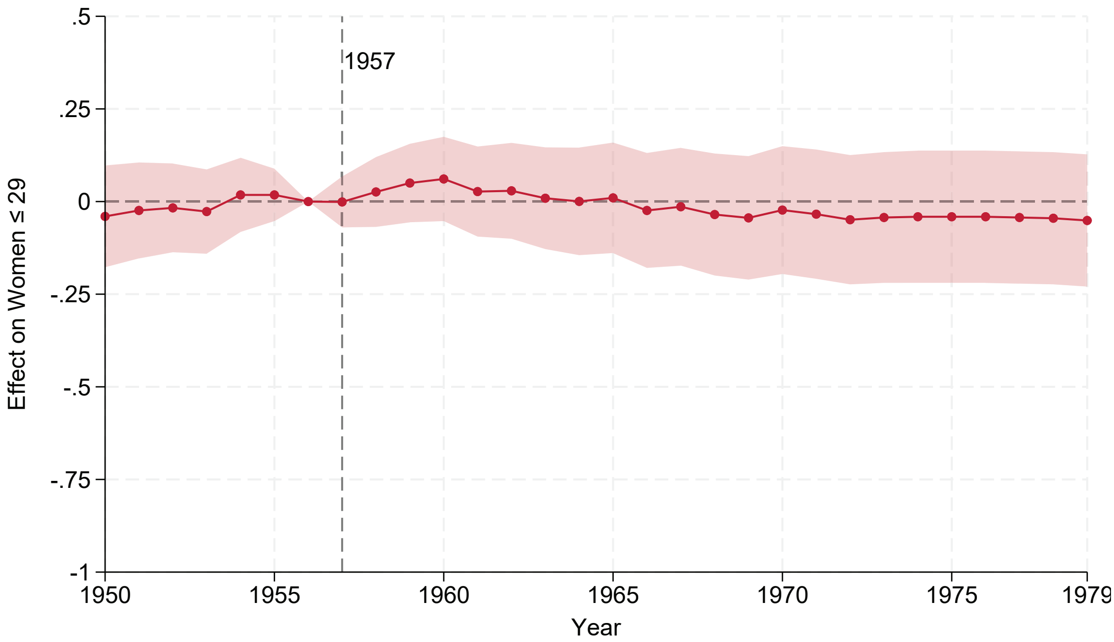


FIGURE 11: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 29 IN 1957

Notes: This event study is estimated on the subsample of households in which the wife was up to age 29 in 1957. The outcome is the cumulative number of children. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level. All specifications include household, year, and age fixed effects within the relevant age range.

E.3 Sensitivity to Excluding Low Male Years of Schooling

In this subsection, we re-estimate the main specification limiting the sample to households in which the male partner completed at least 10 years of schooling. This restriction addresses the observed imbalance in male educational attainment across household types—particularly the lower average years of schooling among male early recipients—by excluding households with very low male education. For households with a female partner age 25 or younger in 1957, the average male years of schooling increases to 12.51, 12.81, 12.40, and 12.90 for the "Female Early," "Female Late," "Male Early," and "Male Late" groups, respectively. As shown in Table 6, the results are very similar to the main results presented in 1, indicating that our findings are robust to excluding households with low male educational attainment.

TABLE 6: DDD WITH HETEROGENEOUS AGE EFFECTS – EXCLUDING HOUSEHOLDS WITH LOW MALE EDUCATION

Dep. Var.	Number of Children in year t							
Panel A: Full sample (1950–1979)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young cutoff: Up to	–	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28	Age 29
Female×Early×Young×Post		-0.586**	-0.719***	-0.465**	-0.332**	-0.136	-0.111	-0.054
		(0.292)	(0.226)	(0.188)	(0.160)	(0.144)	(0.130)	(0.125)
Female×Early×Post	0.005	0.022	0.069	0.071	0.078	0.048	0.047	0.037
	(0.060)	(0.061)	(0.062)	(0.063)	(0.065)	(0.067)	(0.071)	(0.072)
Effect for Young		-0.564**	-0.650***	-0.394**	-0.254*	-0.088	-0.064	-0.017
		(0.286)	(0.217)	(0.177)	(0.146)	(0.128)	(0.109)	(0.102)
Time period	1950–79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79
Mean Dep. Var. 1979	1.363	1.363	1.363	1.363	1.363	1.363	1.363	1.363
N	70,980	70,980	70,980	70,980	70,980	70,980	70,980	70,980
Adjusted R^2	0.850	0.851	0.851	0.850	0.850	0.850	0.850	0.850
Panel B: Subsample (1956–1979)								
Female×Early×Young×Post		-0.448*	-0.633***	-0.384**	-0.249*	-0.161	-0.151	-0.060
		(0.248)	(0.186)	(0.154)	(0.136)	(0.123)	(0.110)	(0.103)
Female×Early×Post	-0.005	0.010	0.053	0.051	0.052	0.043	0.050	0.027
	(0.049)	(0.050)	(0.050)	(0.051)	(0.052)	(0.053)	(0.054)	(0.054)
Effect for Young		-0.438*	-0.580***	-0.333**	-0.197	-0.118	-0.101	-0.033
		(0.242)	(0.179)	(0.146)	(0.125)	(0.111)	(0.096)	(0.088)
Time period	1956–79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79
Mean Dep. Var. 1979	2.275	2.275	2.275	2.275	2.275	2.275	2.275	2.275
N	56,784	56,784	56,784	56,784	56,784	56,784	56,784	56,784
Adjusted R^2	0.921	0.921	0.921	0.921	0.921	0.921	0.921	0.921
HH, Year, & Age Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
S.E. clustered at HH level	✓	✓	✓	✓	✓	✓	✓	✓
Number of Households	2,366	2,366	2,366	2,366	2,366	2,366	2,366	2,366

Notes: The sample is restricted to households in which the male partner completed at least 10 years of schooling. Panel A reports estimates of the effect of receiving early reparations on the lifetime number of children born to young females. Panel B presents analogous estimates using a corrected measure of the number of children in 1956, constructed from information in the 1995 and 2008 censuses. The rows labeled *Effect for Young* report the sum of the coefficients on *Female* × *Early* × *Young* × *Post* and *Female* × *Early* × *Post*. The young age cutoff is measured as of 1957. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects.

E.4 Descriptive Evidence on Completed Fertility Distributions

To complement the panel-based results presented in the main paper, we examine how treatment status is associated with completed fertility outcomes. Table 7 reports cross-sectional estimates from a linear probability model, using the original cross-sectional census data before reshaping it into a household-year panel.

The sample is the same as in the main analysis: couples married by 1954 in which exactly one spouse received reparations, either in the 1950s ("early") or in the 1990s or later ("late"). The dependent variables are indicators for falling into three completed fertility categories: no children, at most one child, and two or three children. The triple interaction term (Female $\times$ Early $\times$ Young) captures households in which the wife was age 25 or younger in 1957 and was the early recipient.

The results show that such households were significantly more likely to end up with at most one child, consistent with our main finding of reduced fertility when young women received reparations. While the coefficients for childlessness and for having two or three children are not statistically significant, the overall pattern supports the demographic relevance of the fertility reduction documented in the panel estimates.

Table 7: The Effect of Reparations by Parity

Dep. Variable:	CEB=0	CEB≤1	2≤CEB≤3
	(1)	(2)	(3)
Female×Early×Young	0.054	0.154*	-0.123
	(0.078)	(0.083)	(0.084)
Female×Early	-0.015	0.002	0.009
	(0.030)	(0.032)	(0.033)
Female	0.005	-0.001	0.002
	(0.021)	(0.023)	(0.023)
Early	0.031	-0.023	0.012
	(0.034)	(0.036)	(0.037)
Controls	Yes	Yes	Yes
Observations	3,906	3,906	3,906
R-squared	0.184	0.203	0.156

Notes: Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Each column reports results from a linear probability model estimating the effect of treatment on indicators for parity categories. $CEB = 0$ is an indicator for households with no children; $CEB \leq 1$ equals 1 if the household has at most one child; and $2 \leq CEB \leq 3$ equals 1 if the household has either two or three children. Controls include duration of marriage in 1957, as well as demographic controls for both spouses: age, years of schooling, years since immigration as of 1957, and dummies for country of origin.

E.5 Comparing Effects by Source of Reparations

In this section, we compare fertility responses by the source of reparations—German versus Israeli. As discussed in Section 2 of the main paper, recipients of German reparations received substantially more generous benefits than those compensated under the Israeli program. If the mechanism operates through increased resource control by women, one might expect larger fertility effects among German recipients.

To explore this, we replicate our main DDDD specification (Equation (2) in the main paper) separately for households in which the recipient received reparations from Israel versus Germany. Appendix Tables 8 and 9 report the results. Each table follows the same format as Table 1 in the main text, with columns corresponding to increasing thresholds for the "young" indicator based on age in 1957.

The results reveal that the estimated fertility response is substantially larger and more precisely estimated among Israeli recipients. For instance, with a young cutoff of age 25, the estimated total effect ("Effect for Young") are -0.592 and -0.415 in the Israeli sample (Table 8, Panels A and B, respectively), compared to -0.121 and -0.204 in the German sample (Table 9, Panels A and B, respectively). Moreover, the Israeli estimates are consistently significant across a wider range of age thresholds.

This discrepancy is plausibly explained by measurement error in the identification of German recipients. As discussed in Section 3 of the main paper, Israeli recipients are observed directly via administrative records from the Ministry of Finance, whereas German recipients are inferred using census reports of foreign rents or pensions and European country of origin. This proxy-based assignment likely results in nontrivial misclassification of treatment status, introducing classical measurement error. Such misclassification biases the estimated treatment effect toward zero, consistent with standard attenuation bias (Hausman, 2001).

Taken together, these findings suggest that while the true fertility effect among German recipients may be similar—or even larger—it is likely obscured by greater measurement error in treatment assignment.

TABLE 8: DID PANEL: AGE IN '57 INTERACTIONS - CUMULATIVE FERTILITY – ISRAELI REPARATIONS RECIPIENTS

Dep. Var.	Number of Children in year t							
Panel A: Full sample (1950–1979)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young cutoff: Up to	–	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28	Age 29
Female×Early×Young×Post		-0.553*	-0.683***	-0.677***	-0.506***	-0.276*	-0.145	0.019
		(0.283)	(0.236)	(0.198)	(0.168)	(0.153)	(0.143)	(0.140)
Female×Early×Post	-0.010	0.013	0.051	0.085	0.093	0.054	0.028	-0.035
	(0.069)	(0.070)	(0.071)	(0.073)	(0.075)	(0.078)	(0.082)	(0.084)
Effect for Young		-0.540**	-0.632***	-0.592***	-0.413***	-0.222*	-0.117	-0.016
		(0.274)	(0.225)	(0.184)	(0.151)	(0.132)	(0.117)	(0.112)
Time period	1950–79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79
Mean Dep. Var. 1979	1.384	1.384	1.384	1.384	1.384	1.384	1.384	1.384
Observations	84,420	84,420	84,420	84,420	84,420	84,420	84,420	84,420
R-squared	0.843	0.844	0.844	0.844	0.844	0.843	0.843	0.843
Panel B: Subsample (1956–1979)								
Female×Early×Young×Post		-0.496**	-0.661***	-0.544***	-0.470***	-0.268**	-0.207*	-0.076
		(0.227)	(0.185)	(0.159)	(0.137)	(0.125)	(0.116)	(0.111)
Female×Early×Post	0.048	0.076	0.115**	0.129**	0.154***	0.117**	0.119*	0.072
	(0.054)	(0.055)	(0.055)	(0.057)	(0.058)	(0.059)	(0.061)	(0.063)
Effect for Young		-0.420*	-0.546***	-0.415***	-0.316**	-0.151	-0.088	-0.004
		(0.220)	(0.176)	(0.148)	(0.124)	(0.110)	(0.098)	(0.091)
Time period	1956–79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79
Mean Dep. Var. 1979	2.300	2.300	2.300	2.300	2.300	2.300	2.300	2.300
Observations	67,536	67,536	67,536	67,536	67,536	67,536	67,536	67,536
R-squared	0.917	0.917	0.917	0.917	0.917	0.917	0.917	0.917
HH, Year, & Age Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
S.E. clustered at HH level	✓	✓	✓	✓	✓	✓	✓	✓
Number of Households	2,814	2,814	2,814	2,814	2,814	2,814	2,814	2,814

Notes: Panel A reports estimates of the effect of receiving early reparations from Israel on the lifetime number of children born to young females. Panel B presents analogous estimates using a corrected measure of the number of children in 1956, constructed from information in the 1995 and 2008 censuses. The rows labeled *Effect for Young* report the sum of the coefficients on *Female* × *Early* × *Young* × *Post* and *Female* × *Early* × *Post*. The young age cutoff is measured as of 1957. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects.

TABLE 9: DID PANEL: AGE IN '57 INTERACTIONS - CUMULATIVE FERTILITY – GERMAN REPARATIONS RECIPIENTS

Dep. Var.	Number of Children in year t							
Panel A: Full sample (1950–1979)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young cutoff: Up to	–	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28	Age 29
Female×Early×Young×Post		-0.077	-0.268	-0.189	-0.206	-0.035	-0.082	-0.053
		(0.281)	(0.215)	(0.181)	(0.157)	(0.142)	(0.125)	(0.116)
Female×Early×Post	0.040	0.034	0.059	0.068	0.080	0.050	0.061	0.055
	(0.052)	(0.053)	(0.054)	(0.054)	(0.055)	(0.056)	(0.058)	(0.059)
Effect for Young		-0.043	-0.209	-0.121	-0.126	0.015	-0.021	0.002
		(0.276)	(0.208)	(0.172)	(0.147)	(0.130)	(0.111)	(0.100)
Time period	1950–79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79	1950-79
Mean Dep. Var. 1979	1.313	1.313	1.313	1.313	1.313	1.313	1.313	1.313
Observations	98,010	98,010	98,010	98,010	98,010	98,010	98,010	98,010
R-squared	0.853	0.854	0.854	0.853	0.854	0.853	0.853	0.853
Panel B: Subsample (1956–1979)								
Female×Early×Young×Post		-0.264	-0.411**	-0.245*	-0.173	-0.080	-0.140	-0.057
		(0.227)	(0.170)	(0.146)	(0.131)	(0.122)	(0.107)	(0.097)
Female×Early×Post	0.006	0.010	0.039	0.041	0.046	0.030	0.050	0.027
	(0.043)	(0.043)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)
Effect for Young		-0.254	-0.372**	-0.204	-0.127	-0.050	-0.090	-0.030
		(0.220)	(0.176)	(0.148)	(0.124)	(0.110)	(0.098)	(0.091)
Time period	1956–79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79	1956-79
Mean Dep. Var. 1979	2.277	2.277	2.277	2.277	2.277	2.277	2.277	2.277
Observations	78,408	78,408	78,408	78,408	78,408	78,408	78,408	78,408
R-squared	0.926	0.926	0.926	0.926	0.926	0.926	0.926	0.926
HH, Year, & Age Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
S.E. clustered at HH level	✓	✓	✓	✓	✓	✓	✓	✓
Number of Households	3,267	3,267	3,267	3,267	3,267	3,267	3,267	3,267

Notes: Panel A reports estimates of the effect of receiving early reparations from Germany on the lifetime number of children born to young females. Panel B presents analogous estimates using a corrected measure of the number of children in 1956, constructed from information in the 1995 and 2008 censuses. The rows labeled *Effect for Young* report the sum of the coefficients on *Female* × *Early* × *Young* × *Post* and *Female* × *Early* × *Post*. The young age cutoff is measured as of 1957. Standard errors, clustered at the household level, are reported in parentheses. All specifications include household, year, and age fixed effects.

References

Hausman, Jerry, "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left," *Journal of Economic Perspectives*, December 2001, *15* (4), 57–67.