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

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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, permanent and unexpected reparations in 1957 and others only decades later, after their childbearing years. Using a triple-difference design with heterogeneity by the wife's age, we compare fertility outcomes by timing of reparations, recipient's gender, and age. Households in which the wife was age 25 or younger in 1957 and received reparations early had 0.25–0.4 fewer children than the corresponding male-recipient comparison, after netting out gender differences among late-recipient households. An event study tracking households across the full reproductive span shows that the gap emerged only after 1957 and stabilized at roughly this magnitude by the late 1960s. A complementary within-early-cohort design compares female-only-early to dual-early households among young wives, ruling out selection into early female receipt. The estimate is similar, indicating that the operative margin is the relative shift in spousal control rather than the wife's receipt per se. Suggestive evidence shows higher educational attainment among the affected children, consistent with a quantity–quality reallocation. The findings speak to the design of individually paid unconditional transfers and child-contingent pronatalist policies.

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

JEL: J13, J16, D13.

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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: Survivors compensated directly by Germany 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 enacted later programs and expansions in 2007 and 2014.

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 the corresponding male-recipient comparison, after netting out gender differences among late-recipient households. Because late recipients received reparations only after our 1950–1979 fertility panel and after the relevant reproductive years, they are effectively untreated within the analysis window. They therefore provide a survivor comparison group rather than a group that becomes treated during the panel. Event-study estimates show that the differential emerged only after 1957 and stabilized at roughly 0.25–0.4 fewer children by the late 1960s.

To address concerns that these patterns reflect differences between early and late female recipients, we also implement a complementary comparison within the early-recipient cohort. We restrict the sample to households in which the wife was age 25 or younger in 1957 and was herself an early recipient, and compare female-only early households to households in which both spouses received early reparations. This comparison holds fixed the wife's young early-recipient status, and therefore mitigates the concern that the estimated effect is driven by differences between young female recipients who did and did not qualify for early reparations. The two groups follow similar fertility paths before 1957, but diverge thereafter, with lower fertility in female-only early households. The estimated post-1957 decline is similar in magnitude to the baseline estimates.

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; McElroy and Horney, 1981; Chiappori, 1988, 1992; Lundberg and Pollak, 1993). Relatedly, gender identity norms may make relative resources within marriage salient for household behavior: Bertrand et al. (2015) show that outcomes such as labor supply, marital stability, and home production respond to whether the wife earns more than the husband.[1] 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 was assigned to that spouse, potentially shifting intra-household decision weights. As Hoynes and Rothstein (2019) note, long-run effects of unconditional transfers are hard to study; while current research emphasizes labor-supply effects of UBI (e.g., Jaimovich et al., 2024), our results suggest there may also be unintended de-

[1] Israel's 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).

mographic consequences.

The relevant policy margin is not only whether transfers raise total household resources, but also whether they change the allocation of resources across spouses. Child-contingent pronatalist policies lower the marginal cost of an additional birth (e.g., Milligan, 2005; Cohen et al., 2013). Individually paid unconditional transfers, by contrast, raise adult resources independently of fertility. When such transfers increase women's control over resources relative to men's, they may attenuate the effect of child-contingent pronatalist incentives operating on the same households. Our female-only versus dual-early comparison sharpens this point. In both groups the wife received early reparations; what differs is whether the husband also did. The lower fertility of female-only early households therefore suggests that the operative margin is not the wife's receipt per se, but how the transfer changes the distribution of control within the household. The implications for per-adult UBI thus depend on whether payments are effectively pooled, and on the extent to which equal individual payments alter relative control within couples.

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 Nazi crimes. However, as Tovy (2015) notes, this opposition focused mainly on direct 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. In theoretical models of fertility choice, households jointly choose the number of children and investments in each child, and greater female bargaining power may move households toward a lower-quantity/higher-quality bundle (Doepke and Tertilt, 2009). Survey evidence documents gender differences and within-couple disagreement over fertility preferences; in many settings women are less inclined than men to want an additional child, although the pattern varies across countries (Westoff, 2010; Doepke and Kindermann, 2019). Tsur (2025) finds that reparations improved children's educational outcomes. Extending that analysis, we find suggestive evidence that the same bargaining shock was associated with higher child educational attainment. Interpreted through the lens of theoretical fertility models, this pattern is consistent with a movement toward a lower-quantity/higher-quality bundle. It should not be read as an estimate of the causal effect of family size on child outcomes, since treatment affects both fertility and the composition of children observed.

Finally, we find no robust evidence that more educated women reduced fertility by more, as might be expected if higher labor market opportunity costs were the key driver (Galor and Weil, 1996; Iyigun and Walsh, 2007). We interpret this evidence cautiously, since education is an imperfect proxy for labor-market opportunities in this cohort, higher-education cells are small, schooling among survivors may itself reflect selection, and we do not observe retrospective labor supply or time use. The result therefore does not rule out an opportunity-cost channel more broadly.

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. Official negotiations had begun earlier that year and proceeded on two parallel tracks.

The Israeli government negotiated collective reparations intended to finance the resettlement and integration of Holocaust survivors in Israel. In parallel, the Claims Conference—an umbrella body of Jewish organizations established to pursue material claims against Germany—negotiated protocols concerning Jewish victims' personal and material claims.

The resulting Luxembourg framework therefore linked the state-to-state agreement to an individual-compensation track. West Germany agreed to pay collective reparations to Israel, while the Claims Conference protocols created a fund for relief, rehabilitation, and resettlement of Jewish victims, in principle those living outside Israel, and provided for consultation over the German legislation that would implement individual compensation. For our setting, the central consequence is that the agreement and the accompanying legal framework left many Israeli survivor-citizens outside direct German personal compensation, while Germany's subsequent compensation legislation preserved eligibility for specific groups, especially those with residence or cultural ties to Germany.[2]

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. Thus, although the Luxembourg framework included a personal-compensation track, actual eligibility under German law—especially for survivors living in Israel—remained narrow and 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.[3] 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 in Israel—to qualify. For the main disability-compensation component, claimants also had to document health damage, with eligibility requiring recognized physical or emotional

[2] As part of the Luxembourg Agreement, West Germany required Israel to assume responsibility for compensating many of 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).

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

disability of at least 25 percent. 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. Like the German program, the Israeli program was structured as a disability-compensation scheme: claimants had to obtain recognition of physical or emotional disability related to Nazi persecution, and benefits varied with the assessed disability rate. At the same time, the disability requirement should not be interpreted as necessarily limiting eligibility to a narrow group of severely disabled survivors. The Dorner Committee report indicates that, in later Israeli administrative practice, disability recognition was applied broadly, especially for mental-health claims. Benefits under the Israeli program were more modest than under the German program: recipients received 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. We restrict the sample to couples married by 1953 so that marriage formation and initial fertility decisions preceded the point at which personal eligibility became clearly defined.

This restriction reduces, but does not by itself eliminate, the scope for anticipation.

1990s onward: Late expansions. Subsequent changes in the 1990s and 2000s extended reparations to previously excluded groups under different eligibility criteria. In 1996, Germany established the Section 2 Fund for survivors who had endured severe persecution, such as confinement in camps or ghettos or hiding under difficult conditions for specified minimum periods. In 2007, Israel enacted the Benefits Law to support survivors who did not meet the Section 2 criteria or had difficulty documenting the required duration of persecution. Under this later Israeli law, proof of even one day under difficult conditions, such as in a ghetto or hiding, was sufficient. The Dorner Committee's 2008 report reviewed the situation of survivors covered by these later Israeli arrangements and recommended increasing benefits. Following these recommendations, the Benefits Law was expanded in 2014 to gradually raise benefits to levels similar to the 1957 DNP Law. In our analysis, recipients covered by the Section 2 Fund, the Benefits Law, and subsequent related expansions are classified as "late" recipients because these programs began decades after the relevant reproductive years.

Because these later programs began after the end of our 1950–1979 fertility panel and after the relevant reproductive years, late recipients are effectively untreated within the analysis window. They therefore serve as a survivor comparison group rather than as a group that becomes treated during the panel. Anticipation of late receipt during the 1970s cannot be tested directly, but it is unlikely given that the Section 2 Fund and the Israeli Benefits Law were established only decades later.

This institutional history implies that early and late recipients were not separated by a single rule. Early receipt depended on a combination of origin or cultural affiliation, immigration date, exclusion from alternative programs, and recognized disability. Late receipt reflected later expansions that covered previously excluded survivors under different documentation requirements. For this reason, we do not rely on early-versus-late status alone as a source of quasi-random variation. Instead, our empirical strategy compares early and late recipients within gender and age groups, examines pre-treatment fertility dynamics, and, most importantly, uses a complementary within-early-cohort comparison that holds fixed the wife's young early-recipient status. This comparison directly addresses the concern that the main results reflect selection into early receipt among young female survivors.

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 us to identify likely 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 the census component consists of two 20% long-form census samples, a recipient who was alive and census-eligible in the relevant census waves had about a 36% probability of appearing in the merged data.[4]

While recipients of Israeli reparations are directly observed in administrative records, identifying recipients of German reparations in the census is indirect: there is no specific question about compensation from 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.[5]

We restrict the sample to couples who were married by 1953 and are observed in the census as still married to the same spouse. These restrictions ensure that marriage occurred before either spouse could anticipate reparations, making early fertility decisions pre-treatment. The same-spouse restriction 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.

A related concern is that the same-spouse restriction could induce differential sample

[4] 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.

[5] The two figures align when adjusted for mortality between 1995 and 2008.

selection if the probability of leaving the original marriage differs by recipient gender or timing. We can assess this concern only partially. For individuals who were married by 1953, the census identifies whether they are still in the same marriage, currently divorced, or currently married to a different spouse. However, for individuals who are no longer married to the same spouse, we do not observe the former spouse's reparations status; for male recipients, we also cannot classify the former wife by age in 1957. We therefore examine an individual-level diagnostic: among recipients who were married by 1953, we calculate the share who are either currently divorced or currently married to a different spouse in the 1995/2008 census. This measure does not include individuals who became widowed and did not remarry.

These diagnostics do not indicate statistically significant differences in the share no longer observed in the original marriage across early and late recipients, although they cannot rule out all forms of selection into the same-spouse sample. Among female recipients of all ages, 12.3 percent of early recipients and 12.9 percent of late recipients are either divorced or married to a different spouse in the census; the t-statistic for the difference is 0.48. Among female recipients who were age 25 or younger in 1957, the corresponding shares are 7.6 and 9.9 percent, with a t-statistic of 0.81. Among male recipients of all ages, the shares are 9.8 and 9.4 percent, with a t-statistic of 0.23. The remaining concern is not differential exclusion per se, but whether the excluded marriages would have changed the fertility comparison. Because young female late recipients are slightly more likely to be excluded on this margin, same-spouse selection would bias us toward a negative estimate only if the excluded young female late-recipient marriages had lower post-1957 fertility than the young female late-recipient marriages that remain in the sample, relative to the analogous selection pattern among male-recipient households. This diagnostic cannot rule out such selection, especially because we do not observe the former spouse's reparations status, the age of former wives for male recipients, or widowhood for those who did not remarry. Nevertheless, the differences in exclusion rates are small in levels and statistically imprecise.

For the main analysis, we further limit the sample to households in which exactly one spouse received reparations. This restriction is central to our baseline identification strategy, which compares households in which the transfer was assigned to the wife with households in which it was assigned to the husband. In cases where both spouses received payments, the direction and magnitude of the within-household shift in resource control is less clear for this comparison. This leaves 3,906 households out of 7,451 with at least one reparation recipient. These are divided into four groups: households in which either the husband or wife received reparations early (1950s) and households in which either received them late (primarily 1990s or later).

For a complementary analysis in Section 5.4, we relax this restriction in one targeted way. We retain households in which the wife was age 25 or younger in 1957 and was an early recipient, and compare households in which the wife was the only early recipient to households in which both spouses were early recipients. This comparison holds fixed the wife's young early-recipient status, and therefore removes the direct early-versus-late comparison among young female recipients that motivates the selection concern. Online Appendix Table 3 reports summary statistics for this complementary sample, and Online Appendix Figure 5 plots the corresponding raw cumulative fertility paths.

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.

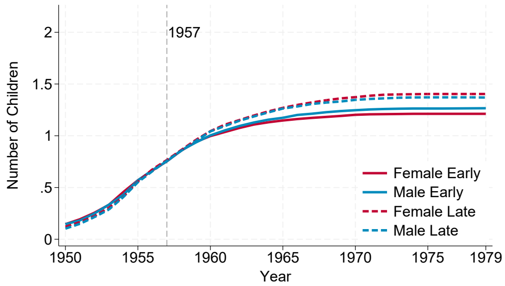
A known limitation of the Registry is undercounting of births in the late 1940s and early 1950s. To assess this, we compare CEB reported by women in the census to the number of children matched in the Registry. The two sources align well for post-1955 births but diverge earlier. Our main analysis uses raw Registry data, which offers consistent and complete post-treatment coverage. Since the undercount is concentrated before treatment, the main concern is measurement of pre-treatment cumulative fertility rather than post-treatment births. Missed births before the start of the panel enter as household-specific level differences and are absorbed by household fixed effects. Missed births during 1950–1955, however, can affect the within-household cumulative-fertility path in the pre-treatment years and are therefore not fully addressed by fixed effects alone. We therefore use a corrected measure initialized in 1956 as a direct robustness check that the estimates are not driven by early measurement error. To construct this measure, we subtract births in 1957–1979 from CEB.[6] In practice, births after 1979 are negligible for this sample: by 1979 even the youngest wives in the age-restricted sample were in their

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

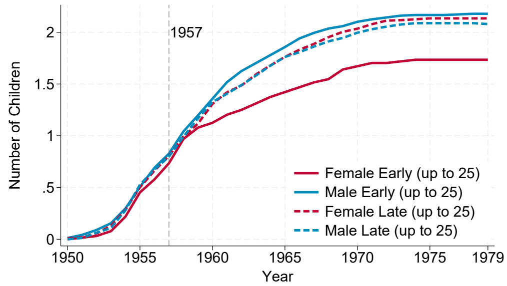
$$n_{i,1956} = n_{i,1995/2008} - \sum_{t=1957}^{1979} \text{birth}_{i,t},$$

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

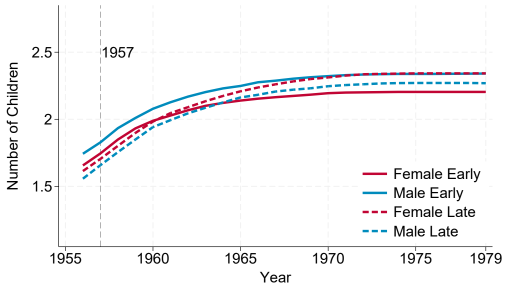
$$n_{i,t} = n_{i,1956} + \sum_{\tau=1957}^{t} \text{birth}_{i,\tau}.$$



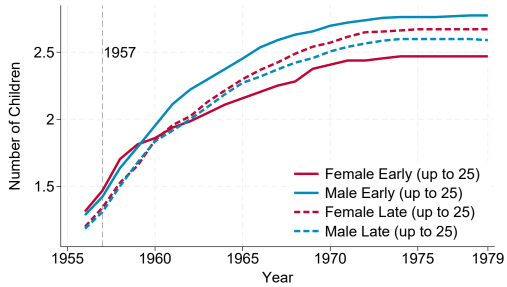
(A) NUMBER OF CHILDREN ALL WOMEN



(B) NUMBER OF CHILDREN - WOMEN AGE 25 OR YOUNGER IN 1957



(C) NUMBER OF CHILDREN ALL WOMEN – CORRECTED



(D) NUMBER OF CHILDREN - WOMEN AGE 25 OR YOUNGER IN 1957 – CORRECTED

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

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

mid-40s, and the fertility paths are flat by the mid-1970s. 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; these women were most likely to have remaining fertility decisions affected by reparations.

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 wartime and postwar conditions, whereas younger women—still children or early adolescents when the war ended—were less likely to have made marriage and fertility decisions during that period. Restricting the sample to wives age 25 or younger in 1957 (i.e., 13 or younger in 1945) allows us to examine fertility choices made under more typical postwar circumstances.

The age cutoff is also consistent with the fertility schedule of the relevant population at the time. Among Israeli women of European and American origin in the 1960s, age-specific fertility rates were highest at ages 20–24 and 25–29 and fell sharply after age 30. Across 1960–1968, roughly three-quarters of age-specific fertility occurred before age 30, while less than 10 percent occurred after age 35 (Goldscheider and Friedlander, 1981). This implies that women who were 25 or younger in 1957 were still entering the main childbearing ages during the post-treatment period, whereas older women had substantially less remaining scope for completed fertility to respond.

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 0.52 among older wives. These patterns confirm that younger wives had substantially more fertility remaining after 1956, making them the group in which any fertility response to reparations is most likely to appear.

Across all panels, fertility trends are similar before 1957, providing reassuring evidence of pre-treatment similarity in cumulative fertility.[7] After 1957, fertility diverges: households where young wives received early reparations show notably lower completed fertility, especially compared to households 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. Given that fertility behavior and the timing of fertility decline varied across European regions during this period (Spolaore and Wacziarg, 2022), we pay particular attention to country of origin. Online Appendix Tables 1 and 2 show that households are similar in age and

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

[8] Online 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.

marriage duration, while some differences remain in origin, schooling, and especially years in Israel. Differences in year of immigration partly reflect design constraints, as early Israeli reparations required arrival by October 1, 1953, but they also motivate the event-study evidence and the complementary within-early-cohort comparison below.

Among households with wives age 25 or younger in 1957, observable characteristics are generally balanced, with one exception in male schooling. Wives' average age is 23.5–23.7; husbands' age is 29–30. Average marriage duration in 1957 is 4.5–5 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 reparation-recipient households to non-recipient European-origin households married by 1953, with at least one spouse immigrating from Europe by 1972. The average CEB among recipient households is 2.29, compared to 2.36 among non-recipient comparison households. As shown in Online Appendix Figure 2, reparation-recipient households have similar average completed fertility, but their parity distribution is more concentrated at two children and less dispersed at the tails. This comparison suggests that the sample is not an outlier in average fertility, while cautioning against interpreting the estimates as fully representative of all European-origin households in Israel.

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 (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.

[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. Online Appendix Section E.4 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.

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 terms λ_t, γ_a, and μ_i denote calendar-year fixed effects, wife's-age fixed effects, and household fixed effects, respectively, where a is the wife's age in year t. 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. The main effects of Female_i, Early_i, Young_i, and their time-invariant interactions are absorbed by household fixed effects; identification comes from their interactions with Post_t or, in the event-study specification below, with year indicators.

The fixed effects are central to the interpretation of these estimates. Identification does not come from cross-sectional differences in completed fertility across households, but from within-household changes in cumulative fertility over time, compared across recipient gender, timing of receipt, and wife age. Household fixed effects absorb time-

[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.

invariant differences across households, including fertility preferences, family background, country-of-origin norms, baseline health differences, and fixed consequences of Holocaust exposure. Year fixed effects absorb aggregate changes in fertility, while wife's-age fixed effects absorb common life-cycle fertility patterns.

These fixed effects do not, by themselves, rule out gender-specific post-1957 fertility changes among young wife-recipient households that are related to health, assimilation, immigration timing, or origin-specific fertility transitions. For such a factor to explain the estimates, however, it would have to differentially affect the post-1957 fertility path of households in which the wife was young and received early reparations, relative to the corresponding male-recipient and late-recipient groups. The event-study specification and the within-early-cohort comparison below examine this implication directly.

To examine dynamics and test for pre-trends in the two treatment effects of interest, we estimate a dynamic version of Equation 2. Relative to Equation 2, the lower-order interactions with Post_t are retained in their original post-treatment form, while the two interactions corresponding to β_6 and β_7 are allowed to vary flexibly by calendar year. Specifically, we substitute $\beta_6(\text{Female}_i \times \text{Early}_i \times \text{Post}_t)$ 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. The coefficients β_{6k} trace the year-specific female-early effect for households not classified as young, while β_{7k} trace the additional differential effect for households in which the wife was young in 1957. The total year-specific effect for young wives is therefore $\beta_{6k} + \beta_{7k}$. All event-study coefficients are normalized relative to 1956, the year immediately before treatment.

Finally, we also estimate a complementary event-study specification within the early-recipient cohort. The sample is restricted to households in which the wife was age 25 or younger in 1957 and was an early recipient. We compare households in which the wife was the only early recipient to households in which both spouses were early recipients. Let FemaleOnly_i equal one for the former group and zero for the latter. We estimate:

$$n_{it} = \sum_{k \neq 1956} \theta_k(\text{FemaleOnly}_i \times D_k) + \lambda_t + \gamma_a + \mu_i + \epsilon_{it}, \tag{3}$$

where 1956 is again the omitted year. Equation (3) is the event-study analogue of a simple two-group difference-in-differences. It compares female-only early households with dual-early households, normalizing the difference in 1956 to zero. Since FemaleOnly_i is time-invariant, its main effect is absorbed by household fixed effects; the coefficients θ_k are identified from interactions of FemaleOnly_i with year indicators and trace the fertility path of female-only early households relative to dual-early households.

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, the effect for wives not classified as young, and β_7, the additional effect for young wives. The total effect for young wives is $\beta_6 + \beta_7$, which we report directly. 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 is no longer statistically significant at conventional levels; 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.

Finally, Table 1 reports the coefficient on Female $\times$ Young $\times$ Post (β_5). Since this term is not interacted with Early, it captures whether young female-recipient households in the late-recipient group experienced a differential post-1957 fertility shift. The estimates are small in magnitude and statistically insignificant across all age cutoffs, providing no evidence of a coincident fertility change among young female recipients who did not receive reparations during their childbearing years.

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 two components of the event-study specification. The blue line shows the year-specific analogue of β_6, the female-early effect for households not classified as young. The red line shows the year-specific analogue of β_7, the additional differential effect for households in which the wife was age 25 or younger in 1957. Thus, the total effect for young wives is the sum $\beta_{6k} + \beta_{7k}$, which is plotted directly in Online Appendix Figure 3 with joint confidence intervals. The additional young-wife component turns negative after 1957 and declines through the first half of the 1960s. The combined young-wife effect becomes statistically significant from 1961 onward and stabilizes at roughly 0.4 fewer children, consistent with the DDD estimates in Table 1.

5.4 A Complementary Within-Early-Cohort Comparison: Female-Only versus Dual-Early Households

We next examine the complementary within-early-cohort comparison described in Section 4. This exercise helps address the concern that the baseline estimates reflect differences between young female recipients who did and did not qualify for early reparations. In this comparison, all wives were age 25 or younger in 1957 and all were early recipients; the comparison is between households in which the wife was the only early recipient and households in which both spouses were early recipients. Online Appendix Figure 5 plots the corresponding raw trends, and Online Appendix Table 3 reports summary statistics for the two groups.

The table shows that the two groups are similar along several dimensions most directly related to the identification concerns raised above: wives' average age in 1957 is nearly identical, as are average years in Israel by 1957 and duration of marriage, and the shares of wives born in Poland and Romania are also very similar. Corrected pre-1957 fertility is

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×Young×Post (β_5)		-0.170	0.021	0.071	0.068	0.019	-0.001	-0.017
		(0.124)	(0.101)	(0.084)	(0.074)	(0.068)	(0.063)	(0.060)
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×Young×Post (β_5)		-0.143	0.072	0.084	0.095	0.043	0.033	0.006
		(0.110)	(0.089)	(0.073)	(0.065)	(0.059)	(0.054)	(0.051)
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: Column 1 estimates Equation (1); columns 2–8 estimate Equation (2). The coefficient on *Female×Early×Post* corresponds to β_3 in Equation (1) and to β_6 in Equation (2); for compactness it is reported in a single row. Panel A reports estimates using Registry cumulative fertility through year t. 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*, *Female×Young×Post*, and *Female×Early×Post* correspond to β_7, β_5, 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.

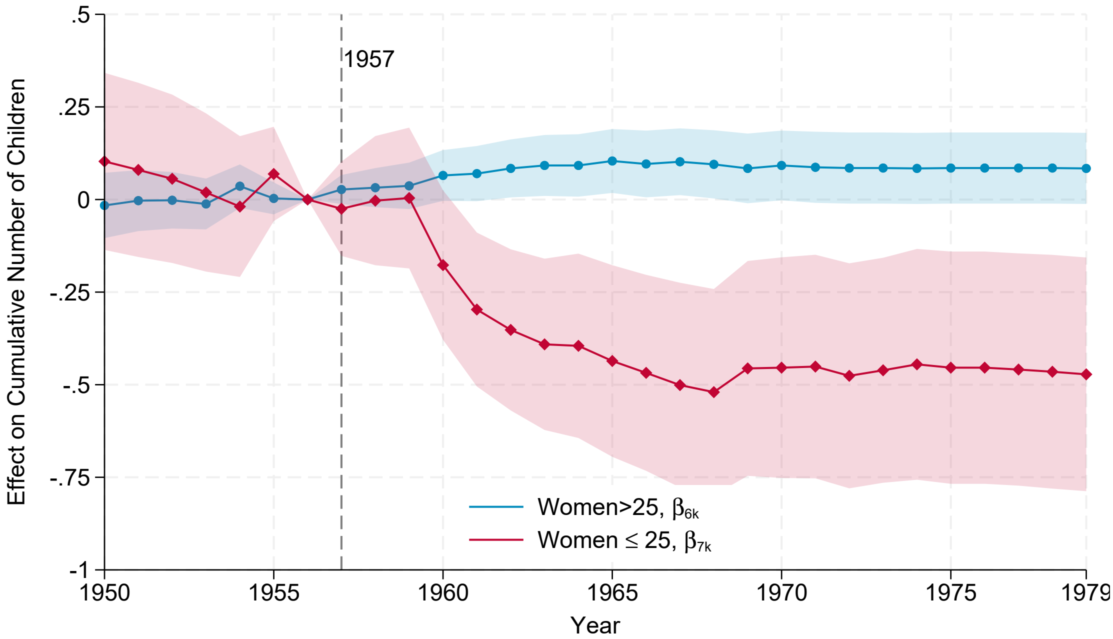


FIGURE 2: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME

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 the year-specific analogue of β_6 in Equation (2), the female-early effect for households not classified as young. The red line shows the year-specific analogue of β_7, the additional differential effect for households in which the wife was 25 or younger in 1957. The total effect for young women is therefore given by the sum of the blue and red coefficients; this sum, together with its joint confidence interval, is reported in the Online Appendix. Shaded areas represent 95% confidence intervals.

not lower in female-only early households; if anything, it is slightly higher than in dual-early households. At the same time, the groups differ in total reparations received, and some observable characteristics also differ, including both spouses' schooling and some husband origin variables. We therefore view this comparison as a complementary design that mitigates concerns that the baseline estimates reflect selection into early female receipt.

Figure 3 reports the event-study estimates from Equation (3). The estimates are close to zero prior to 1957, with no evidence of differential pre-trends. After 1957, the coefficients become negative and decline through the 1960s. By 1968, the point estimate is -0.459, and it remains between -0.436 and -0.459 through 1979, consistent with a persistent gap in completed fertility. The corresponding difference-in-differences estimate, replacing the event-time indicators with a post-1957 indicator, is -0.342 with a standard error of 0.124. Using the corrected fertility measure described in Section 3, the estimate is -0.282 with a standard error of 0.108.

Although the magnitude is close to the baseline DDD estimate, the two exercises use different counterfactuals. The baseline DDD compares female- and male-recipient households while using late recipients to net out gender-specific differences unrelated to receipt during childbearing years. The within-early comparison instead conditions on the wife being a young early recipient and compares households in which she was the sole early recipient to households in which both spouses received early reparations. It therefore addresses a different selection concern—selection into early receipt among young female survivors—but introduces different limitations, including small cell sizes, greater total transfers in dual-early households, and differences in some husband characteristics. We interpret the similarity in estimates as complementary evidence rather than as an independent replication of the same design.

5.5 Heterogeneity by Women's Education

Online Appendix Table 4 examines whether the fertility response among young wives varies with women's education. Column 1 reports the baseline young-wife specification. Columns 2–4 restrict to the same young-wife sample but replace the age interaction with an education interaction; in these columns, Female $\times$ Early $\times$ Post is the effect for the lower-education group, and the education interaction captures the additional effect among more educated women. Women are classified as educated according to three alternative definitions: (i) above-median years of schooling (median=10), (ii) top quartile of years of schooling (cutoff=12), and (iii) holding an academic degree (8.5% of women).

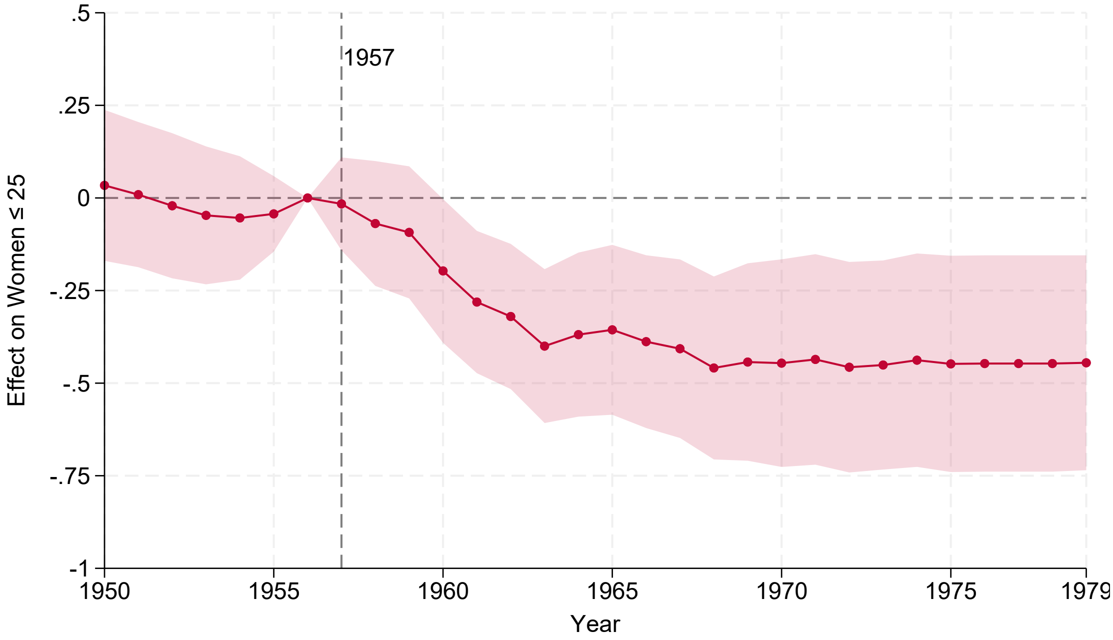


FIGURE 3: FEMALE-ONLY EARLY VERSUS DUAL-EARLY HOUSEHOLDS (WIVES AGE $\leq$ 25 IN 1957)

Notes: This figure presents event study estimates of the effect of being a female-only early household (wife an early recipient, husband not) on the cumulative number of children in the household, relative to the pre-treatment year 1956. The sample is restricted to households in which the wife was 25 or younger in 1957. The omitted group consists of households in which both spouses are early recipients. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the household level.

The point estimates do not reveal a robust education gradient. In particular, the broad median split produces a negative but imprecise estimate, while narrower high-education definitions yield larger point estimates but rely on smaller cells. The interaction terms for educated women are positive, small in magnitude, and statistically insignificant across all definitions. We therefore interpret the education results as showing no clear evidence that the fertility response was concentrated among women with higher measured schooling.

5.6 Robustness

5.6.1 Undercounting of Early Births in the Population Registry

To address undercounting of early births in the Population Registry, we construct a corrected fertility measure by subtracting births in 1957–1979 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 one percent level. Similar patterns hold across other cutoffs, confirming that the main results are not driven by early mismeasurement.

Figure 4 presents the event study based on the corrected measure, beginning in 1956. Fertility among younger women declines from 1960 onward, with the combined young-wife effect stabilizing at roughly the same magnitude as in the main event study. The sum $\beta_{6k} + \beta_{7k}$ remains statistically significant at the five percent level in every year from 1961 through 1979.[11]

5.6.2 Additional Robustness and Complementary Evidence

Treatment-year definition. We also assess robustness to the treatment-year definition. Varying the cutoff from 1954 to 1959 yields stable estimates.[12] This stability is expected given the stock nature of cumulative fertility. Since exact household payment dates are unavailable, we interpret this exercise as a robustness check on the treatment-year definition, not as evidence on the precise timing of individual payments.

[11] Online Appendix Figure 4 plots the sum of these coefficients along with their confidence intervals.
[12] See Online Appendix Table 5.

Age-restricted specifications. 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. Online Appendix Section E.3 reports this age-restricted implementation: Online Appendix Table 8 reports the corresponding DDD estimates, and Online Appendix Figures 6–12 plot the event-study estimates. The pattern is consistent with the main findings: estimated effects are large and negative for the youngest women, and fade for older groups.

Taken together, these robustness checks show that the main pattern is not sensitive to the treatment-year definition or to estimating the model within age-restricted subsamples.

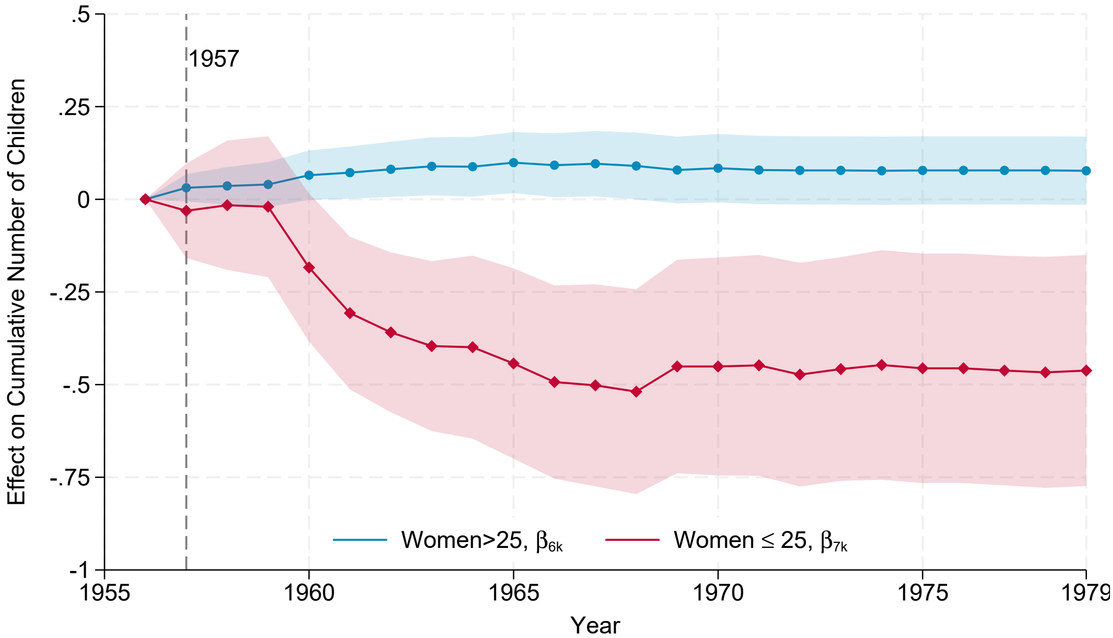


FIGURE 4: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: CORRECTED FERTILITY MEASURE

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 plots β_{6k}, the female-early effect for households in which the wife was older than 25 in 1957. The red line plots β_{7k}, the additional differential effect for households in which the wife was age 25 or younger in 1957. The total effect for households in which the wife was age 25 or younger in 1957 is $\beta_{6k} + \beta_{7k}$ and in plotted in Online Appendix Figure 3. Shaded areas represent 95% confidence intervals.

5.6.3 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. Online Appendix Tables 6 and 7 present estimates of the main specification separately by source of early reparations for the treated spouse. The late-recipient comparison group is the same survivor comparison group used in the baseline specification and is not source-specific in the same way; the Israeli- and German-source samples should therefore not be interpreted as mutually exclusive partitions of the baseline sample. The source-specific estimates point to a much clearer fertility response among Israeli-source early recipients: the Israeli-source effects are large and statistically significant for the central young-wife cutoffs, whereas the German-source estimates are smaller and generally not statistically significant.

One plausible explanation is measurement error in the proxy for German reparations receipt. As discussed in Section 3, Israeli recipients are observed directly in administrative data from the Ministry of Finance, whereas German recipients are inferred from census reports of foreign pensions and country of origin. This proxy-based assignment likely introduces misclassification in treatment status, which can attenuate the estimated treatment effect toward zero (Hausman, 2001). Source-specific differences in eligible populations and benefit rules may also contribute, so these estimates should be interpreted as suggestive.

Online Appendix Section E.2 provides additional detail and discusses the interpretation of these findings.

6 Child Education and the Quantity–Quality Margin

We next examine whether the fertility response is accompanied by changes in children's human-capital outcomes. In theoretical models of fertility choice, households jointly choose the number of children and investments in each child; a shift in intra-household bargaining power may therefore move households toward a different quantity–quality bundle. Our empirical exercise should be interpreted in this joint-choice sense. It is not an estimate of the causal effect of an additional sibling on a given child's education, since treatment affects fertility and therefore the set of children born. Instead, we ask whether the same treatment group that reduced fertility also had children with higher educational attainment.

We examine this mechanism using adult educational 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.[13]

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). All specifications also include indicators for female recipient and early recipient; these coefficients are omitted for compactness. Standard errors are clustered at the household level.

Table 2 provides suggestive, but not conclusive, evidence on children's educational outcomes. The matriculation estimate is statistically significant at the 10 percent level, while the academic-degree estimate is not conventionally significant ($p = 0.12$). Because the regressions include indicators for female recipient and early recipient, the coefficient on $Female \times Early$ is a cross-sectional difference-in-differences: it compares the female-versus-male recipient gap among early-recipient households to the corresponding gender gap among late-recipient households, conditional on controls. The corresponding point estimates are economically large—10.9 and 10.7 percentage points for matricu-

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

lation and academic degree, respectively. The estimate for years of schooling is positive but imprecise. Because these regressions condition on the set of children born, and treatment itself affects fertility, the estimates cannot by themselves establish a quantity–quality tradeoff in the sibling-effect sense. We interpret them as consistent with a shift toward a lower-quantity/higher-quality bundle, rather than as direct evidence on the causal effect of sibship size.[14] 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.

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 relative to the corresponding male-recipient comparison, after netting out gender differences among late-recipient households. The DDD estimates imply a reduction of roughly 0.25–0.4 children among households in which the wife was young enough to have substantial remaining fertility. Event-study estimates show that the gap emerged only after reparations began and persisted through the end of the reproductive period, with year-specific estimates temporarily approaching one-half child.

We find suggestive evidence that the same bargaining shock was associated with higher child educational attainment, consistent with a shift toward a lower-quantity/higher-quality bundle in the joint-choice sense of theoretical fertility models. This result should not be read as an estimate of the causal effect of family size on child outcomes, since treatment affects both fertility and the composition of children observed. The fertility decline is not more pronounced among more educated women, providing no clear evidence that the response was concentrated among women whose labor-market opportunity costs were likely to be higher. Since schooling is an imperfect proxy for labor-market opportunities in this cohort and we do not observe retrospective labor supply, we interpret this as an indirect check on opportunity-cost explanations rather than as a direct test of labor-market responses.

Beyond this historical case, our evidence points to a policy-relevant mechanism: fertil-

[14] Estimating the same specifications separately for sons and daughters yields similar coefficients on $Female \times Early$; the differences by child sex are not statistically significant. Thus, we find no evidence that the educational response differed for sons versus daughters.

ity responds not only to total household resources, but also to their allocation across spouses. This creates a potential tension between individually paid unconditional transfers, including per-adult UBI, and child-contingent pronatalist policies. Pronatalist policies lower the marginal cost of an additional birth, while unconditional transfers raise individual resources independently of fertility. When such transfers increase women's individually controlled resources relative to men's, they may attenuate the effect of pronatalist incentives operating on the same households.

Our female-only versus dual-early comparison sharpens this point: the fertility response is largest where the wife's receipt is not matched by the husband's, and the countervailing force on pronatalist incentives is muted when both spouses receive early payments. The operative margin is therefore not the wife's receipt per se but the relative shift in control across spouses. Our findings do not imply that all forms of UBI would reduce fertility; rather, the demographic effects of unconditional transfers depend on whether payments accrue to one or both spouses and on whether they are effectively pooled or instead alter individual control within couples.

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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, including a balance check on male schooling and fertility in Section B.1 and a comparison of completed-fertility distributions for reparation-recipient and non-reparation-recipient 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 described in footnote 6 of the main paper. Section D examines heterogeneity by women's education. Section E gathers robustness checks and complementary analyses. Section E.1 considers alternative treatment start dates. Section E.2 compares effects by source of early reparations, Israeli vs. German. Section E.3 reports age-restricted triple-difference estimates and corresponding event-study figures. Section E.4 examines robustness to excluding households in which the male partner has fewer than 10 years of schooling.

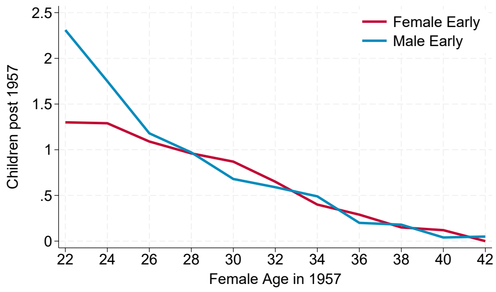
A Fertility by Female Age and Recipient Gender – Cross-Sectional Validation of Identification

Figure 1 plots pre- and post-1956 fertility by wife's age in 1957, separately by recipient gender and timing of receipt. This age-by-age view complements the aggregate trends in Figure 1 of the main text and shows whether the post-treatment pattern is concentrated among women with substantial remaining fertility.

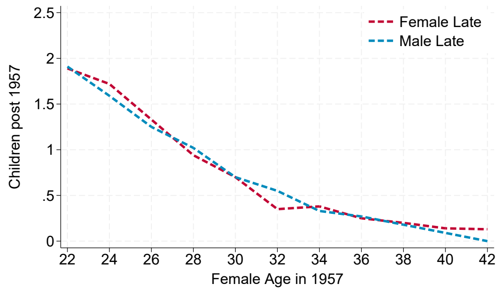
In each panel, the x-axis denotes the woman's age in 1957, regardless of whether the reparations recipient is male or female. This enables a consistent comparison of fertility outcomes across household types.

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.

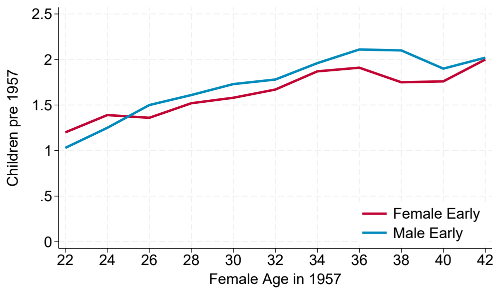
Panels 1a and 1b show post-1956 fertility outcomes for early and late recipient households, respectively. Panel 1a highlights a clear divergence: in Male Early households, fertility is higher, while in Female Early households it is lower. By contrast, Panel 1b shows no difference between Male Late and Female Late households; late payments occurred after the fertility panel and after the relevant reproductive years, so late recipients are effectively untreated during the analysis window. This pattern underscores that the reduction in fertility is specific to cases where young women gained early control over resources.



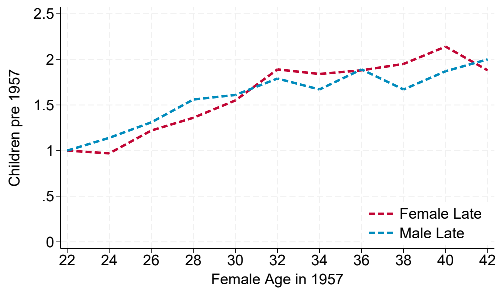
(A) Number of Children Born After 1956, Early Recipients



(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 1957, 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. The tables below show reassuring similarity in age and duration of marriage, while also documenting remaining differences in origin, schooling, and years in Israel. These differences are important context for the event-study evidence and the complementary within-early-cohort comparison in the main text.

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. Appendix Table 3 further focuses on this younger-wife sample and compares households in which only the wife was an early recipient to households in which both spouses were early recipients. Each table reports means (and standard deviations) for key demographic variables by treatment group. This allows readers to assess balance on observables in the full sample, in the subgroup most relevant for our main results, and in the complementary within-early-cohort comparison.

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 in 1957–1979	0.540	0.720	0.595	0.703
	(0.731)	(0.907)	(0.884)	(0.902)
Children Born by 1956	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)

Continued on next page

Table 1 – continued from previous page

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late
Male Years in Israel in 1957	14.08	9.657	9.371	5.066
	(12.39)	(13.34)	(11.80)	(10.10)
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

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Table 1 – continued from previous page

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late

Notes: Means are reported with standard deviations in parentheses. Sample consists of households with exactly one reparation recipient. "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 in 1957–1979" is computed using annual births from the Registry for 1957–1979, as described in footnote 6 in the main paper. "Children Born by 1956" is the difference between the census-reported number of children ever born and the number of children born in 1957–1979, providing a corrected measure for cumulative fertility up to 1956. 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

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) 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 in 1957–1979	1.156	1.463	1.488	1.397
	(0.718)	(1.058)	(1.044)	(1.011)
Children Born by 1956	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)

Continued on next page

Table 2 – continued from previous page

Household Type	(1) Female Early	(2) Female Late	(3) Male Early	(4) Male Late
Female Born in Poland	0.234	0.212	0.185	0.121
	(0.427)	(0.410)	(0.389)	(0.327)
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 reparation recipient 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 in 1957–1979" is computed using annual births from the Registry for 1957–1979, as described in footnote 6 in the main paper. "Children Born by 1956" is the difference between the census-reported number of children ever born and the number of children born in 1957–1979, providing a corrected measure for cumulative fertility up to 1956. All other variables are defined in the Data section. See text for definitions of household types.

TABLE 3: SUMMARY STATISTICS – FEMALE-ONLY EARLY AND DUAL-EARLY HOUSEHOLDS

Household Type	(1) Female-only Early	(2) Dual-Early
Cumulative # of Children (Registry)	1.734	2.253
	(1.027)	(1.091)
Children Ever Born (Census)	2.469	2.615
	(0.854)	(0.928)
Children Born in 1957–1979	1.156	1.549
	(0.718)	(1.128)
Children Born by 1956	1.312	1.066
	(0.833)	(0.696)
Male Age in 1957	29.73	29.67
	(4.487)	(3.293)
Female Age in 1957	23.66	23.58
	(1.275)	(1.423)
Male Years of Schooling	10.55	9.022
	(3.915)	(3.712)
Female Years of Schooling	10.05	8.593
	(3.819)	(3.902)
Male Years in Israel in 1957	14.69	9.692
	(10.85)	(2.542)
Female Years in Israel in 1957	8.812	8.396
	(7.122)	(2.476)
Duration of Marriage in 1957	5.000	4.901
	(1.662)	(1.820)
Male Born in Poland	0.297	0.341
	(0.460)	(0.477)
Female Born in Poland	0.234	0.242
	(0.427)	(0.431)
Male Born in Asia/Africa	0.0938	0.0110
	(0.294)	(0.105)
Female Born in Asia/Africa	0.0156	0
	(0.125)	(0)
Male Born in Romania	0.219	0.231
	(0.417)	(0.424)
Female Born in Romania	0.391	0.385
	(0.492)	(0.489)
Male Born in Germany	0.0156	0.0220
	(0.125)	(0.147)

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Table 3 – continued from previous page

Household Type	(1) Female-only Early	(2) Dual-Early
Female Born in Germany	0.0625	0.0110
	(0.244)	(0.105)
Male Born in Russia	0.109	0.0549
	(0.315)	(0.229)
Female Born in Russia	0.0156	0.0330
	(0.125)	(0.180)
Male Born in Other Europe	0.125	0.341
	(0.333)	(0.477)
Female Born in Other Europe	0.250	0.319
	(0.436)	(0.469)
Male Born in Israel	0.125	0
	(0.333)	(0)
Female Born in Israel	0.0312	0.0110
	(0.175)	(0.105)
Observations	64	91

Notes: Means are reported with standard deviations in parentheses. Sample consists of households in which the female partner was age 25 or younger in 1957 and was an early reparations recipient. "Female-only Early" households are those in which the wife was the only early recipient; "Dual-Early" households are those in which both spouses were early recipients. "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 in 1957–1979" is computed using annual births from the Registry for 1957–1979, as described in footnote 6 in the main paper. "Children Born by 1956" is the difference between the census-reported number of children ever born and the number of children born in 1957–1979, providing a corrected measure for cumulative fertility up to 1956. All other variables are defined in the Data section.

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.4 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: Reparation-Recipient vs. Non-Reparation-Recipient Households

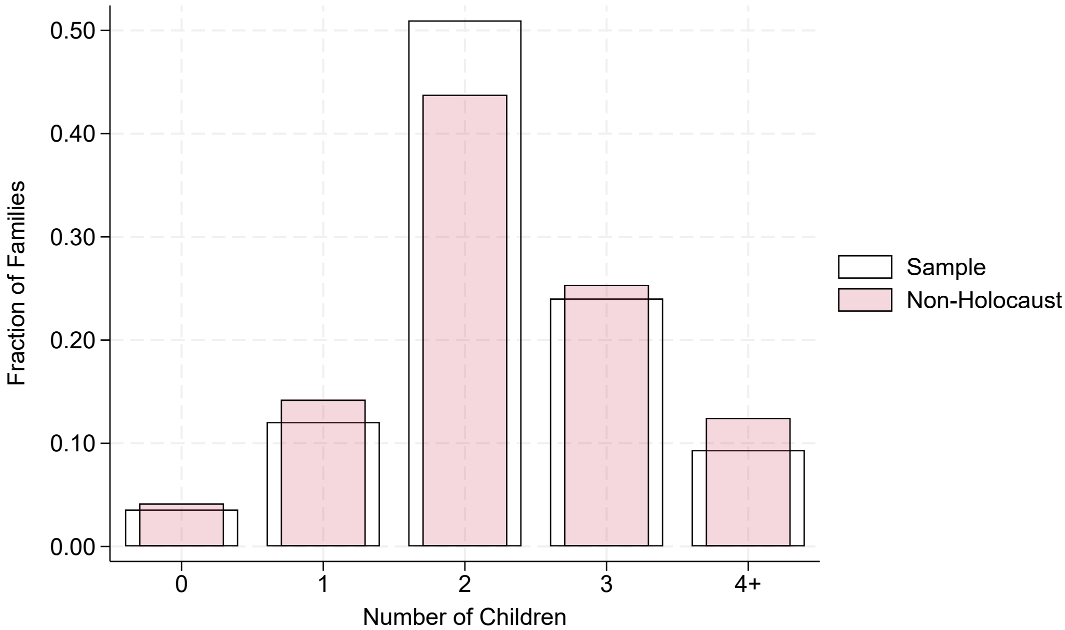

FIGURE 2: DISTRIBUTION OF COMPLETED FERTILITY: REPARATION-RECIPIENT VS. NON-REPARATION-RECIPIENT HOUSEHOLDS

Notes: The figure displays the distribution of completed fertility (number of children ever born) for all reparation-recipient households in our sample (3,906 observations) and for a comparison group of non-reparation-recipient households (married by 1953, at least one spouse immigrated to Israel from Europe by 1972, neither spouse a reparation-recipient). Reparation-recipient 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).

Figure 2 provides useful context on the external validity of the analytic sample. Reparation-recipient households have completed fertility close to that of the comparison group on average, but their parity distribution is more concentrated at two children and less dispersed at the tails. This suggests that the sample is not an outlier in average fertility, but it also cautions against interpreting the estimates as fully representative of all European-origin households in Israel.

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 $\beta_{6k} + \beta_{7k}$, where β_{6k} is the year-specific analogue of Female $\times$ Early and β_{7k} is the year-specific analogue of Female $\times$ Early $\times$ Young in Figure 2 of the main text. 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 4 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.

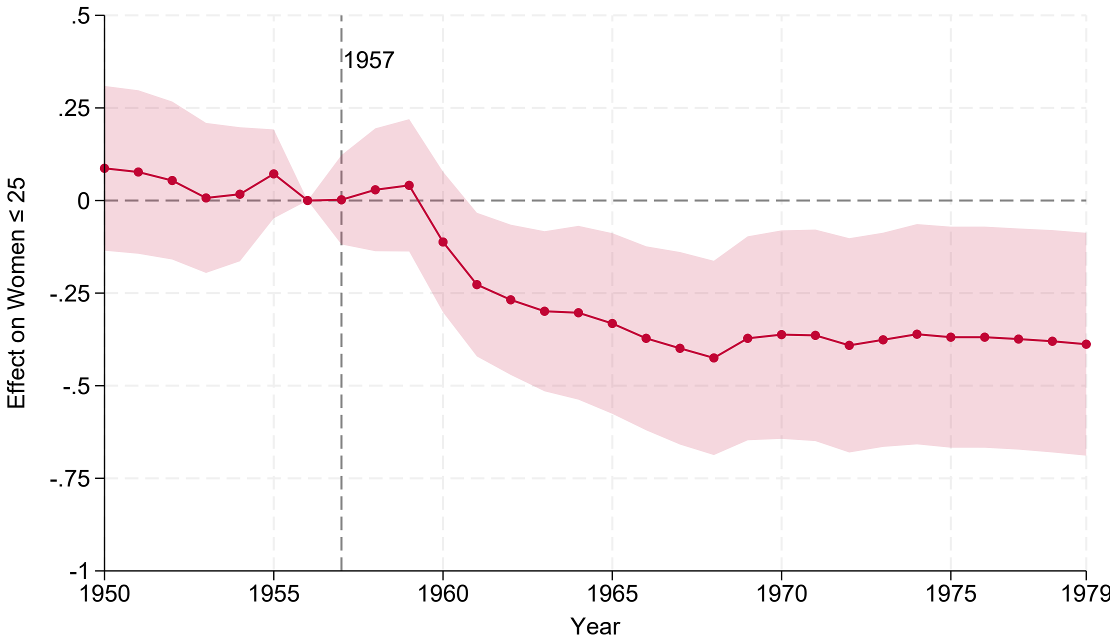

FIGURE 3: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME

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 $\beta_{6k} + \beta_{7k}$ from the main specification, 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.

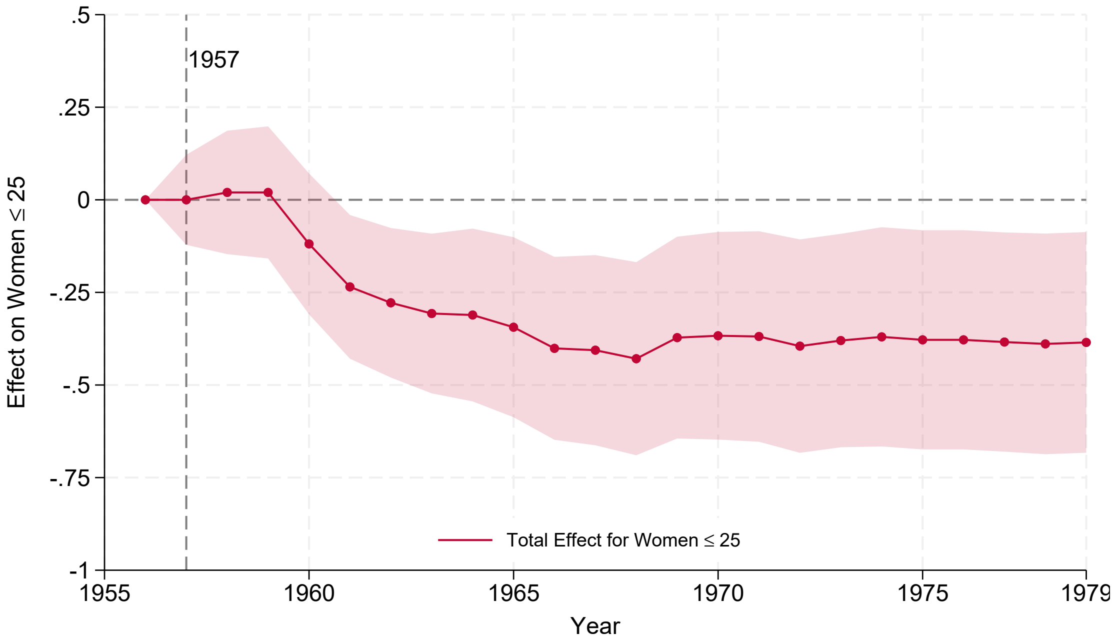

FIGURE 4: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: CORRECTED FERTILITY MEASURE

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 Section 3 of the main paper). For each year, we plot the sum $\beta_{6k} + \beta_{7k}$ from the corrected-measure specification, 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 4 of the main text.

C.1 Female-Only Early versus Dual-Early Households

This section complements the early-cohort analysis in the main text (Section 5.4) by presenting the corresponding raw trends.

Figure 5 plots the raw evolution of cumulative fertility for the two early-recipient household types used in the early-cohort robustness exercise. The sample is restricted to households in which the wife was 25 or younger in 1957.

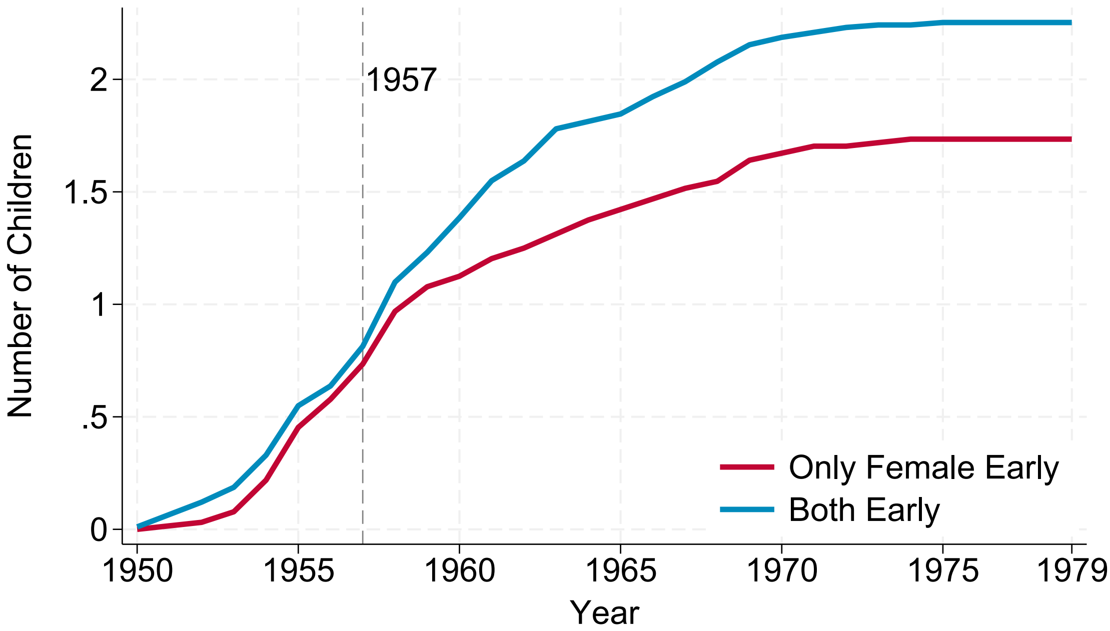

FIGURE 5: CUMULATIVE FERTILITY: FEMALE-ONLY EARLY VS DUAL-EARLY (WIVES AGE $\leq$ 25 IN 1957)

Notes: This figure plots the average cumulative number of children in the household over time for households in which the wife was 25 or younger in 1957. The red line corresponds to households in which only the wife is an early recipient, while the blue line corresponds to households in which both spouses are early recipients.

D Heterogeneity by Women's Education

Table 4 reports results from estimating a quadruple-difference specification analogous to Equation (2) of the main paper, but with education (rather than age) as the heterogeneity dimension: we replace the "Young" indicator with an indicator for "Educated" status and include the full set of interactions with $Post_t$ implied by that specification (Educated × Post, Early × Educated × Post, Female × Educated × Post, and Female × Early × Educated × Post). The sample is restricted to households in which 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), (ii) top quartile of years of schooling (cutoff=12), and (iii) holding an academic degree (8.5% of women).

TABLE 4: EDUCATION HETEROGENEITY IN THE YOUNG-WIFE SAMPLE

	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: Column 1 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 (this estimate also appears in Table 8 below). Columns 2–4 use the same young-wife sample and replace the "Young" indicator in the fully interacted specification with 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 Columns 2–4, Female × Early × Post is the effect for the lower-education group, while Female × Early × Post × Educated is the additional effect for the higher-education group. 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 5 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 possible uncertainty about when expectations of personal reparations might have begun, in light of the September 1952 collective reparations agreement between Germany and Israel. Although there is little evidence that survivors anticipated personal compensation that early, this sensitivity check addresses 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 5: 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 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.

In this subsection, "Israeli" and "German" refer to the source of early reparations for the treated spouse. The late-recipient comparison group is the same survivor comparison group used in the baseline specification and is not source-specific in the same way. Thus, the Israeli- and German-source samples should not be interpreted as mutually exclusive partitions of the baseline sample. To explore source-specific patterns, we replicate our main DDDD specification (Equation (2) in the main paper) separately by source of early reparations for the treated spouse. Appendix Tables 6 and 7 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 estimated fertility response is substantially larger and more precisely estimated among Israeli-source early recipients. For instance, with a young cutoff of age 25, the estimated total effects ("Effect for Young") are -0.592 and -0.415 in the Israeli-source sample (Table 6, Panels A and B, respectively), compared to -0.121 and -0.204 in the German-source sample (Table 7, Panels A and B, respectively). Moreover, the Israeli-source estimates are consistently significant across a wider range of age thresholds.

One plausible explanation is measurement error in the proxy for German reparations receipt. 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. Such misclassification can bias the estimated treatment effect toward zero, consistent with standard attenuation bias (Hausman, 2001). Source-specific differences in eligible populations and benefit rules may also contribute, so these estimates should be interpreted as suggestive.

TABLE 6: DDDD WITH AGE INTERACTIONS – SOURCE OF EARLY REPARATIONS: ISRAEL

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 using Registry cumulative fertility through year t, limiting early-recipient households to those in which the treated spouse received early reparations from Israel. 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 late-recipient comparison group is the same survivor comparison group used in the baseline specification and is not source-specific in the same way. 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 7: DDDD WITH AGE INTERACTIONS – SOURCE OF EARLY REPARATIONS: GERMANY

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 using Registry cumulative fertility through year t, limiting early-recipient households to those in which the treated spouse received early reparations from Germany. 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 late-recipient comparison group is the same survivor comparison group used in the baseline specification and is not source-specific in the same way. 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.3 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 8 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 8: 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 6–12 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.

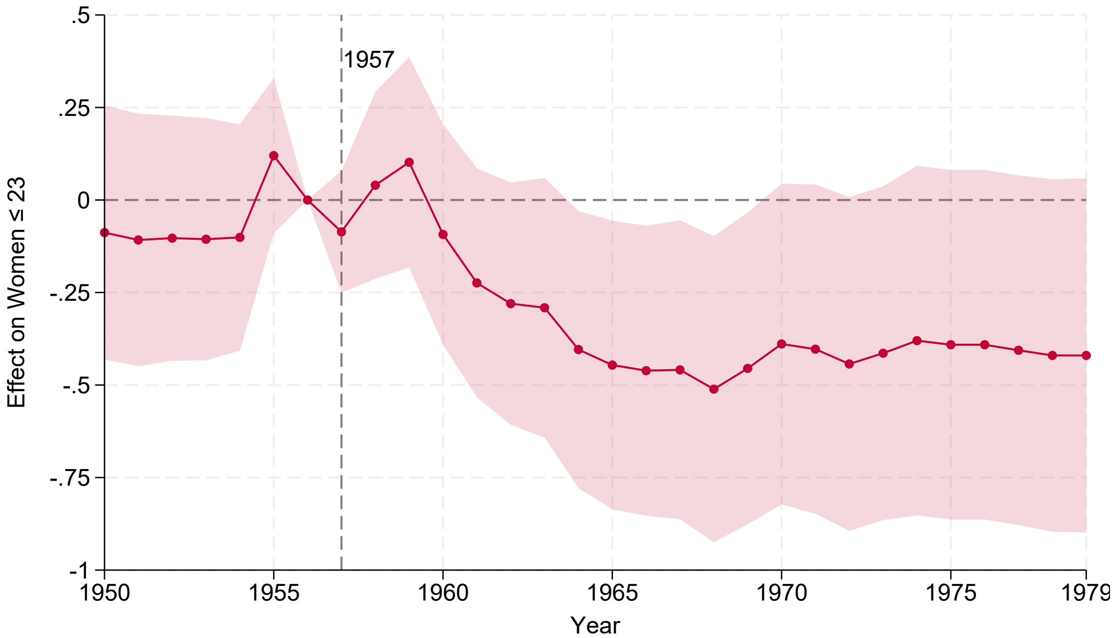

FIGURE 6: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 23 IN 1957

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.

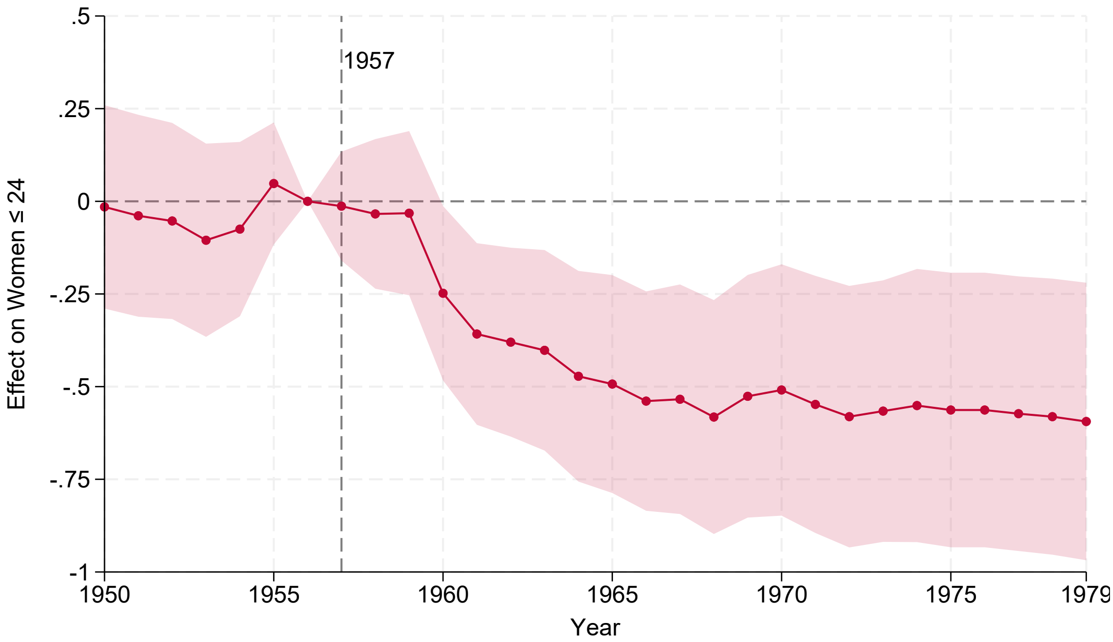

FIGURE 7: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 24 IN 1957

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.

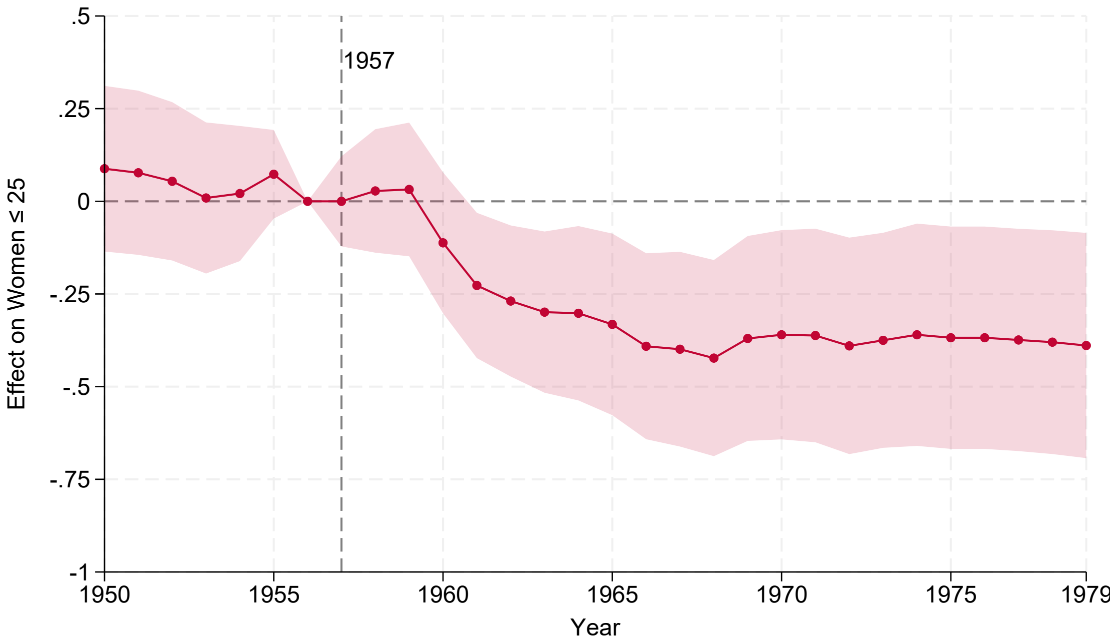

FIGURE 8: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 25 IN 1957

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.

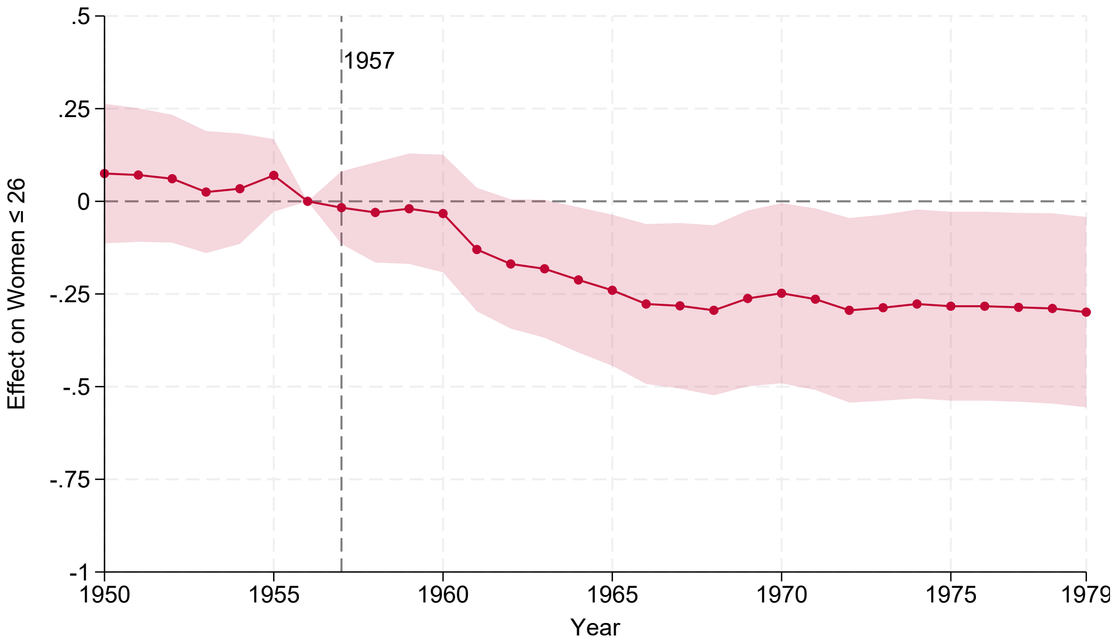

FIGURE 9: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 26 IN 1957

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.

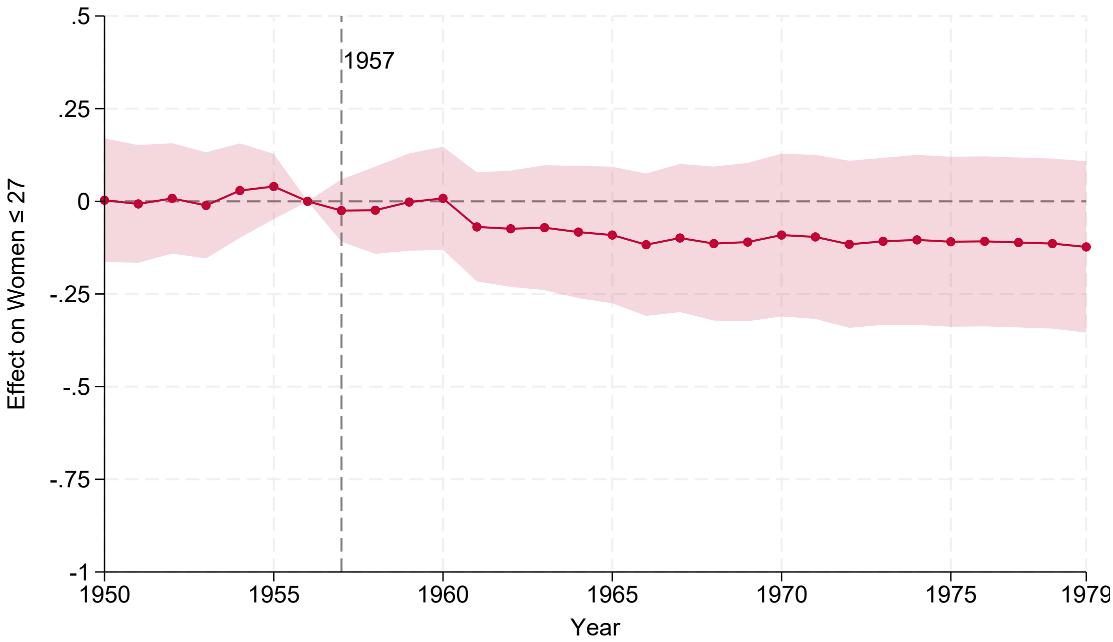

FIGURE 10: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 27 IN 1957

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.

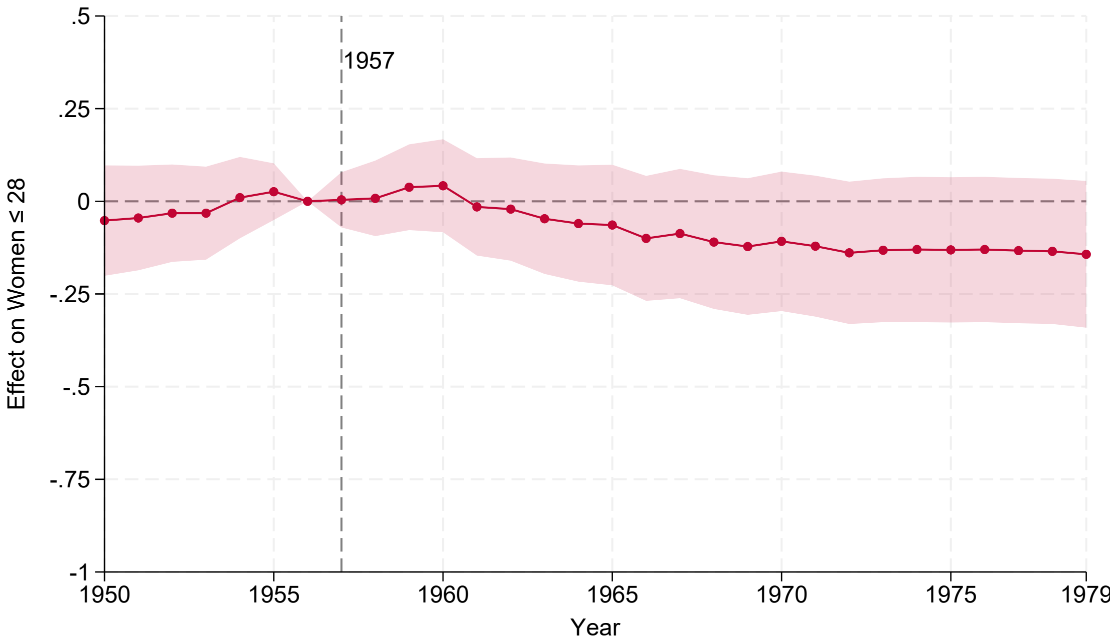

FIGURE 11: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 28 IN 1957

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.

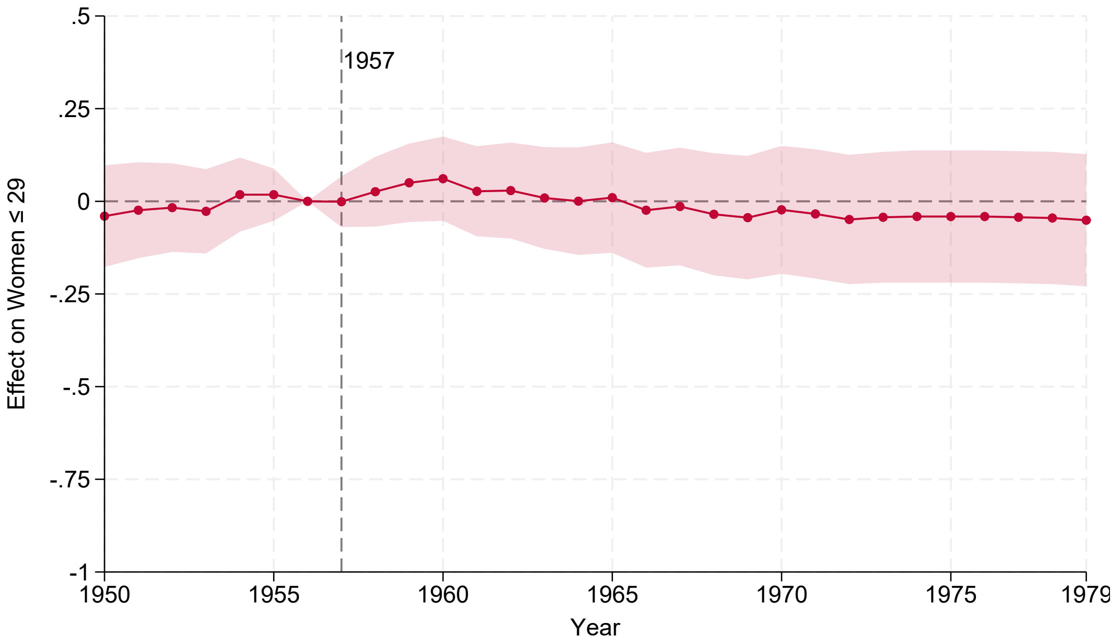

FIGURE 12: IMPACT OF EARLY REPARATIONS ON CUMULATIVE FERTILITY OVER TIME: WOMEN UP TO AGE 29 IN 1957

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.4 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 9, the results are very similar to the main results presented in Table 1, indicating that our findings are robust to excluding households with low male educational attainment.

TABLE 9: 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 using Registry cumulative fertility through year t. 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.