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Dora L. Costa
Matthew E. Kahn

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ABSTRACT

We show how fertility delays compound across generations to raise age at first grandmotherhood. Under the 1960 first-birth schedule median grandmotherhood came at 44. Under the 2024 schedule it is 57, cutting the healthy years with grandchildren from 28 to 15. Longer life does not make up the loss of years when the grandmother is healthy enough to supply money and care. Delay is most pronounced among the college-educated, with reproductive-control access accounting for over half the fall in grandmotherhood by age 55 among White college graduates. When a daughter times her first birth to maximize her own payoff, the loss her choice imposes on her mother is external to her calculation. We call this the Rotten Adult Kid Problem. Delay raises both the price of a grandmother's care and wealth per grandchild.

Dora L. Costa
University of California, Los Angeles
Department of Economics
and NBER
costa@econ.ucla.edu

Matthew E. Kahn
University of Southern California
Department of Economics
and NBER
kahnme@usc.edu

An online appendix is available at http://www.nber.org/data-appendix/w35773

In 1960 43% of all first-time mothers in the United States were age 20 to 24. By 2023 only 26% of first-time mothers were in that age group (Julian and Manning, 2025). Published statistics suggest that the consequence for grandparenthood has already arrived. Between 2014 and 2022 the prevalence of grandparenthood among adults age 50 or older fell from 58 to 50% (Westrick-Payne, 2024).

This paper shows that delayed grandmotherhood is an important but largely overlooked consequence of the long-run decline in fertility and the postponement of first births. Prior research has documented the timing, duration, and educational gradients of grandparent roles (Margolis and Wright, 2017; Margolis and Verdery, 2019) and the role of grandparental childcare in facilitating young parents' labor supply (Compton and Pollak, 2014), but has devoted little attention to the causes of delayed grandmotherhood or to its consequences for older adults' activities. We emphasize a mechanism this work leaves implicit: postponement compounds across generations, and its effects are most visible among college-educated women—the group that delays the most and is least likely to be grandmothers in 2022 (Westrick-Payne, 2024). We call this amplification compound delay.

Delayed grandparenthood has received little attention because most U.S. datasets are organized around households rather than lineages: fertility surveys end at the transition to parenthood, and surveys of older Americans rarely link to adult children's fertility. No single source answers all of our questions, so we combine several. We document compound delay in three different datasets and show that the number of years a grandmother can expect to share with her grandchild has fallen. Rising longevity added years of grandmotherhood for most of the twentieth century.[1] We find that it no longer does. A woman would need to stay healthy to age 86, rather than the current 77, to recover the years compound delay has taken. The added years of life also come at the wrong ages. Delay removes years with a grandchild when she is healthy enough to supply care and money. Longer life returns them after eighty.

Fertility decline has been attributed to the rising opportunity cost of women's time, to culture, and to incomplete contracts within the family. Two such contracting failures, one generation apart, frame this paper. Goldin (2026) argues that fertility decline reflects incomplete contracting *within* young households. Some women rationally postpone childbearing or remain childless because partners cannot credibly commit ex ante to sharing future childcare. We document its mirror one generation up. Compton and Pollak (2026) treat grandchildren as family public goods and apply Coasian reasoning to the resulting conflict: because transaction costs within families are usually low, grandparents can bargain with their adult children to induce them to have children or to have them sooner, offering child care or a down payment on a house, rather than cash, which families find an inappropriate currency. We ask what an intergenerational bargain is worth. When adult children time their fertility on their own private costs and benefits, the value grandchildren generate for their parents is external to that choice. We call this externality the Rotten Adult Kid Problem.

[1] Margolis and Verdery (2019) simulate White women born between the 1880s and 1960 and find the mean length of grandparenthood rising from 27.4 to 34.5 years, because the mean age at death rose 9.7 years while the mean age at grandparenthood rose only 2.7. They study cohorts whose first grandmotherhood is complete. We study the first-birth rates now facing women who have not yet had grandchildren.

The Rotten Adult Kid externality operates through the timing of the birth and through the resources a grandmother provides after the birth. We calculate that a complete contract over timing would buy only about five weeks of earlier childbearing because the daughter's career return dwarfs what a year with a grandchild is worth to her mother. The money and care a grandmother supplies is worth four to thirteen thousand dollars per lineage, against the fifty dollars she would pay for five weeks of earlier childbearing. We find that the care a grandmother gives is worth up to twice the money she sends. The family conflict is over childcare, and its price rises as grandmothers' labor market and leisure opportunities improve.

Two developments accompany compound delay. One is changes across cohorts in women's career expectations, college enrollment, and professional-school graduation rates documented by Goldin (2006). The second is the reproductive-control revolution. Legal access to the pill and to abortion lowered young women's first-birth rates and raised their schooling and careers, principally on the college margin (Bailey, 2006; Goldin and Katz, 2002; Guldi, 2008; Myers, 2017). We show that the reproductive-control revolution reverberates one generation up. A woman who never had access to either technology becomes a grandmother later because her daughters did. This *echo* is larger than the direct effect on the women who gained access. Like the delay itself it is largest among the college-educated. Section 7 identifies it from state-of-birth variation in legal access.

Compound delay redistributes resources across grandchildren. Fewer grandchildren, arriving later, raise the wealth available per grandchild because a grandmother's wealth is divided among her grandchildren. We find that resources per grandchild rose 55 percent for the college-educated between 1995–2006 and 2008–2022 while *falling* for every other education group, widening a gap that was already large. Part of this is the extensive margin. A fifth of college-educated women's wealth sits with women who have no grandchildren.

We document that a grandmother supplies the same amount of care on a day with a grandchild whether or not she holds a job or has a college degree. She gives up market work if she holds a job, television if she does not.

1 Economic Framework

1.1 Compound delay

Becoming a grandmother requires two successive first births. If mothers and daughters each have their first child at age 32, grandmotherhood begins around age 64. If both have their first child at age 22, grandmotherhood starts at age 44. The twenty-year gap between age 44 and 64 is compound delay.

Mothers and daughters will not necessarily have the same first-birth distributions. Intergenerational persistence in fertility behavior amplifies compound delay if each mother who postpones her first birth has a daughter who also postpones. Treating successive generations as statistically independent will understate compound delay.

We focus on the female line because the costs of a birth are largely borne by women. Motherhood carries an earnings penalty (Miller, 2011). Grandmothers provide more childcare than grandfathers (see Section 8.1), and give more care to their daughters' than their sons' children.[2][3]

1.2 Existence and interaction value

The economic significance of delayed grandmotherhood depends on how grandchildren enter older adults' utility function. Grandchildren may enter through *existence value*. Grandmothers derive satisfaction from the continuation of the family line and from knowing that descendants will survive them. They will not substantially alter their labor supply, volunteering, or other activities upon becoming a grandmother. Alternatively, grandchildren may enter the utility function through *interaction value*. Grandmothers enjoy spending time with grandchildren, transmitting family history, and providing care. Delayed fertility costs interaction value by compressing the window during which grandmothers and grandchildren overlap in healthy and active years. Let U denote utility, G age at grandmotherhood, T_h the end of the grandmother's healthy years, D her age at death, and τ_t her time with grandchildren at age t. Existence value is v, and $u(\tau_t)$ is the flow of interaction value. We then write

$$U \;=\; v \cdot \mathbf{1}\{G \leq D\} \;+\; \int_{G}^{T_h} u(\tau_t)\, dt. \tag{1}$$

The first term (existence value) jumps at the grandchild's arrival. The grandmother receives interaction value (the second term) only in the years between G and T_h, the healthy-overlap window that delayed fertility compresses. Existence value depends on whether the family line continues. It thus jumps at the first grandchild and is unchanged by any additional grandchild.[4] Interaction value depends on the time a grandmother spends with grandchildren, not on how many there are. We therefore study age at first grandmotherhood rather than completed grandchild count.

A fifty-year-old does not know when, or whether, her first grandchild will arrive. Her expected interaction value depends on the whole distribution of G, because each year of delay removes a year from the window $[G, T_h]$. We therefore report $P(G \leq A)$ over the *whole* age range in Section 4.

Existence value predicts that becoming a grandmother does not change the allocation of time. Interaction value predicts reallocation toward family care, financed out of other uses of time. Direct evidence on $u(\tau_t)$ is scarce, because it requires knowing how the hours feel rather than where they go. Section 8.1 finds that hours are reallocated, financed from work and leisure.

College education raises the price of a grandmother's time. Grandmothers have traditionally been caregivers. Today, highly educated grandmothers face a wider menu of alternatives—continued

[2]Using the Health and Retirement Study we estimated that within the same family a mother has a 9.2 percentage point higher probability of supplying substantial grandchild care to a daughter compared to a son upon a first birth. In contrast, money transfers do not depend on the sex of the child who has a first birth (Appendix J).

[3]We do not model grandfathers and partners separately. They enter the household budget constraint and the bargaining problem implicitly.

[4]A grandmother might treat later grandchildren as further insurance that the line continues, in which case existence value rises in their number. Nothing below turns on this. Existence value enters through $\mathbf{1}\{G \leq D\}$, which does not depend on the hours she spends with a grandchild.

employment, geographic mobility, travel, leisure, and volunteering. A day spent with a grandchild has to be financed by reductions in attractive alternative activities.[5]

1.3 The Rotten Adult Kid Problem

Family bargains are enforceable when the players are few, information is roughly symmetric, and family ties are strong enough to sustain informal agreements (Pollak, 1985; Compton and Pollak, 2026). Fertility timing is different. A daughter cannot promise a date for her first birth. A mother cannot promise grandchild care, because her health may fail and nothing obliges her to give it. Nor can she promise financial transfers at the birth of a child. Nor can she condition her later-life transfers on her daughter's fertility choices.[6] A mother could instead pay for a birth this year, needing no promise about later years, but families find such transactions repugnant (Roth, 2007). We thus get the intergenerational externalities we call the *Rotten Adult Kid Problem*.

We formalize the Rotten Adult Kid Problem as follows. Let an adult daughter choose her age at first birth a to maximize her own payoff $u(a)$, which rises with delay while she builds her career and then falls. Suppose her mother derives $V(a)$ from grandmotherhood, decreasing in a because later grandchildren arrive when she is less healthy. The daughter chooses a^* satisfying $u'(a^*) = 0$ rather than the family optimum a^{**} satisfying

$$u'(a^{**}) + V'(a^{**}) = 0. \tag{2}$$

Because $V' < 0$, the daughter delays past the family optimum, $a^* > a^{**}$. The wedge $-V'(a^*)$ is the uninternalized cost her timing imposes on her mother.[7] A daughter who values her mother's welfare internalizes part of V'. The wedge $-V'(a^*)$ is thus an upper bound on the uninternalized cost and $a^* - a^{**}$ an upper bound on the correction.

There are two externalities to delaying past the family optimum. The first is a *timing* externality, and stronger outside options for the grandmother make it *smaller*. Alternatives cushion V against delay, flattening V'. The second is a *provision* externality that operates after the grandchild arrives. Under the same limited commitment, adult children treat grandparental childcare as a free input rather than valuing it at the grandmother's foregone alternatives. This externality *rises* with the grandmother's outside options. Rising opportunities for older women therefore soften the family's conflict over daughters' delay while sharpening conflict over who cares for grandchildren once they arrive.

[5] We cannot observe the developmental return to grandchild care and may thus understate its value.

[6] Bernheim, Shleifer, and Summers (1985) model bequests as conditioned on children's behavior, but punishing children who bear later is not credible because it would harm the grandchildren. McGarry (1999) finds little evidence that bequests are conditioned on behavior. In the HRS, inter vivos transfers go disproportionately to less well-off children while bequests are divided equally.

[7] Becker's Rotten Kid Theorem (Becker, 1974) internalizes such an externality when utility is transferable and a benevolent parent moves last. Neither condition fits our setting. Transferable utility is restrictive (Bergstrom, 1989), and a mother cannot condition her transfers on her daughter's timing.

1.4 From the motherhood penalty to the grandmotherhood penalty

A first birth imposes a large and persistent earnings cost on mothers but not on fathers (Kleven, Landais, and Søgaard, 2019; Kleven, Landais, and Leite-Mariante, 2025). Postponement is the second-best response of a woman who cannot contract with her partner over who will absorb that cost. Delaying a first birth raises college graduates' career earnings by about five percent a year, a return exhausted by the early thirties (Miller, 2011). It will be largest among college-educated women because their earnings profile is steepest.

Anticipation makes delay cheaper for both generations. A younger woman postpones her first birth after reading her earnings profile. Her mother anticipates her daughter's delay and builds a life that does not depend on grandchildren, thus raising the price of an hour of grandchild care later. This price is the shadow cost of care to the daughter, and its increase leads her to delay. Each woman is optimizing. The inefficiency is that the daughter does not internalize V'. Foreseeing the delay, the mother arranges her life so as to mind it less, lowering $-V'$ and the cost of the daughter's delay.[8]

Women who anticipate their daughter's delay should work longer. The labor force participation of college graduates at ages 59–63 rose between the 1934–36 and 1955–57 birth cohorts while their number of grandchildren at those ages fell from 3.0 to 1.9. Among women without a college degree the number of grandchildren fell from 5.5 to 4.8 (Appendix E). We cannot determine whether working longer is a response to fewer grandchildren or the reverse because a woman's career, her own age at first birth and her daughter's are chosen together. Participation is already declining before the first grandchild arrives. Event-study estimates of that arrival fail their pre-trend tests, as predicted by anticipation (Appendix E.2).

1.5 Family size and childlessness

Falling family size affects grandmotherhood in two ways. It shifts grandmotherhood later, and it raises the chance that a woman never becomes a grandmother at all. A woman's age at her first grandchild is the minimum of her age at her children's first births. Falling family size removes children from that minimum. If her n children have first births at ages distributed $F_1, \ldots, F_n$, then her probability of grandmotherhood is

$$P(G \leq g) \;=\; 1 - \prod_{k=1}^{n} \big[1 - F_k(g - a_k)\big], \tag{3}$$

where a_k is her age at child k's birth. The probability is increasing in n at *fixed* timing behavior. Each additional child is another draw from the distribution and another insurance policy against a child who never has children. With several children no single one controls whether she becomes a grandmother. With one, there is a hold-up and $-V'$ rests wholly on the only child's decision.

[8] We do not estimate a structural model. Instead, we take first-birth schedules as primitives and use equation (2) as an organizing device.

1.6 Empirical Implications

Our framework yields six empirical implications. First, grandmotherhood shifts later by roughly the sum of the two generations' delays, and thus by more than the delay in either generation's own first births. Treating the generations as statistically independent understates the shift in grandmotherhood (Sections 3 and 4). Second, the delay is concentrated among the college-educated because their return to postponement is greatest (Sections 4 and 5). Third, falling family size shifts grandmotherhood later and raises the chance that a woman never becomes a grandmother at all. The marginal cost of one fewer child rises as family size falls. Section 4 evaluates both margins using estimated first-birth schedules for synthetic cohorts. Fourth, postponements in daughters' first births echo one generation up, delaying their mothers' grandmotherhood. We can thus assess the impact on grandmotherhood of the reproductive-control revolution experienced by daughters (Section 7). Fifth, grandmothers will reallocate time toward family care and away from market work and leisure if they care about interaction value rather than existence value. At the same time, a grandmother's rising outside options raise the price of that care and concentrate dynastic wealth on fewer, later grandchildren (Sections 8.1 and 8.2). Finally, the framework can price the two externalities against each other. Section 9 calibrates the wedge $-V'$, the uninternalized cost the daughter's timing imposes on her mother, against the daughter's return to delay. We ask whether the daughter's return to postponing exceeds her mother's loss.

2 Data

We measure the delay in grandmotherhood by combining three demographic sources—National Vital Statistics Reports, the Census and American Community Survey, and the Health and Retirement Study—together with an external estimate of the healthy years grandmothers and grandchildren overlap. To understand what caused the delay we return to the Census and ACS, merged with the legal history of access to contraception and abortion for unmarried women. We use the American Time Use Survey and the Health and Retirement Study to measure the time and money that flow to a grandchild. We use those flows to price the years removed by delay in Section 9.

The **National Vital Statistics Reports** are complete and subject to neither recall nor survival bias—valuable because noise compounds when we combine two first-birth distributions. We use age-specific first-birth rates for 1960, 1980, 2000, and 2024, and first-birth counts for 2007 and 2024. Appendix A provides details about construction and our treatment of the right-censoring inherent in single-year natality data. A drawback to the data is that education first appeared on birth certificates only in 1969, and then only for a sample of states.

We use pooled **1940–2020 Census and American Community Survey** samples through IPUMS to document heterogeneity in race and education, creating synthetic cohort cells by education and race at single-year-of-age resolution. White and Black race categories include Hispanic women because the Census samples do not support a consistent Hispanic-origin classification across the years. We use the age of the eldest co-resident child to recover a cohort's first-birth timing.

We limit our sample to women between ages 28 and 40, which is late enough that their education is essentially complete and early enough that their first child is still at home. This yields 7,426,031 records across the 1940–2020 censuses and ACS.

We observe realized grandmotherhood using the **Health and Retirement Study** (HRS, 1992–2022), through the RAND Longitudinal and Family Files, but at the cost of barely reaching the daughters' cohorts and only through their mothers. The data follow birth cohorts for thirty years. We learn in each wave if a woman observed between ages 50 and 85 has become a grandmother and we can count how many grandchildren she has. Unlike the census data, we can observe grandchildren not living with the grandmother. We use waves 3 and 5–16 of the 1992–2022 panel.[9] We code a woman as a grandmother in any wave in which her children report a positive number of children of their own, and limit our analytical sample to women observed between ages 50 and 85. This age interval covers the transition to grandmotherhood.

Our HRS sample includes women who have no children and hence no grandchildren (childless non-grandmothers). A woman contributes an observation at every age at which she is interviewed because grandchildren are counted at each wave. Our estimation sample comprises 119,004 person-wave observations on 20,511 women; roughly four-fifths (82 percent) of the person-waves are grandmother-waves, and the mean number of grandchildren is 5.4. (See Appendix D for HRS coverage and limits and details about the construction of the grandchild count.)

We calculate the healthy years a grandmother can expect to spend with a grandchild using disability-free life expectancy for women aged 60–90 in 2001 from Solé-Auró, Beltrán-Sánchez, and Crimmins (2015), who define disability as difficulty with any of four activities of daily living.

We investigate the role of the *reproductive-control revolution* in delayed grandmotherhood using the Census and ACS samples merged with measures of confidential legal access to reproductive-control technologies, that is, access without a parental-consent requirement.[10] Our measure is the share of ages 18–20 at which a woman's *state of birth* allowed young unmarried women to consent to either the oral contraceptive or an abortion, built from the state-by-year legal coding of Myers (2017). We create a second measure where we rebuild the pill component under the alternative coding of Bailey et al. (2011). Approved by the FDA in 1960, the pill initially faced legal barriers for young unmarried women. State liberalization during the late 1960s and early 1970s expanded access to both technologies. By the mid-1970s every state had extended confidential access before age 21. We merge on state of birth crossed with year of birth. The legal coding covers 1960–1979, and the consent regime becomes ambiguous after *Danforth* (1976). We therefore restrict the estimation sample to cohorts born by 1958 and observed at ages 24–26, young enough to bear on the first births the reform is meant to shift and available in the 1960, 1970, and 1980 censuses. We restrict the sample to White and Black women, leaving us with 492,794 women.[11]

[9] Waves 1 and 2 collect only minor-child counts; wave 4's child-level counts are selected on the outcome (Appendix D.1).

[10] We estimate the reproductive-control design only in the Census and ACS; the HRS lacks the relevant cohorts, cell sizes, and statistical power (Appendix H).

[11] We exclude other-race women because of sample size limitations. Pooling them with either group would mix populations whose access laws and migration patterns differ.

We use two data sources to study grandmotherhood in terms of price and income effects. We use the 2005–2024 **American Time Use Survey** (ATUS) to understand how grandparenthood reshapes the allocation of time. The ATUS provides nationally representative time-diary data for a single 24-hour period. It measures realized engagement on the diary day. It does not directly identify grandparents. Our samples therefore include all individuals. We create an indicator equal to one if a grandchild was present during any diary activity, taken from the "who" records. We construct seven time-use categories (market work, volunteering, religious activities, socializing, television, non-TV leisure, and childcare) from the activity records. In addition, we measure all supervisory (secondary) care of non-own children. We pool the 2005–2024 waves, excluding 2020 because collection was disrupted by the pandemic. We restrict the samples to women aged 55–74 and report men in the same age range in the appendix. Appendix I describes variable definitions, activity codes, and weighting.

We return to the HRS to establish how wealth per grandchild has changed. The HRS reports household net worth including all homes, which we deflate to 2022 dollars using the CPI-U. We compare the wealth of women aged 65–79 in 1995–2006 with that of women in the same age group in 2008–2022. We use an older age group than in the rest of our HRS work because wealth is near its lifetime peak at these ages. We also document how a grandchild's arrival affects inter vivos transfers, comparing a new-parent child to his or her still-childless siblings in a within-sibship event study.See Appendix L for details.

3 Period Evidence (Vital Statistics)

We establish how changes in period first-birth schedules lead to delayed grandmotherhood, using the 1960, 1980, 2000 and 2024 schedules. We begin by assuming that both generations face the same period first-birth schedule. Our benchmark estimate of median age at first grandmotherhood comes from taking a single year's distribution of age at first birth and convolving it with itself. This yields the implied distribution of age at first grandmotherhood for a hypothetical cohort. We then relax that assumption, pairing a mother's schedule with a later daughter's schedule.Section 4 substitutes cohort for period schedules.

Consider the following example to fix intuition about what our convolution does. A woman who has her first child at 24, whose daughter in turn has her first child at 26, becomes a grandmother at 50. In reality, the two ages at first birth vary, and the mean tells us nothing about the spread of ages at grandmotherhood. If first births were equally likely at 22 and 26 in each generation with a mean of 24 in both, grandmotherhood would fall at 44, 48, or 52, not simply at 48. The 44 and the 52 pull the distribution wider than doubling the mean suggests.

We formalize our example below. Let $p(a)$ denote the probability that a first birth occurs at age a under a given fertility schedule, with cumulative distribution $F(a) = \sum_{x \le a} p(x)$; we write p_m, F_m for the mother's schedule and p_d, F_d for the daughter's. If A_m and A_d denote the mother's and daughter's ages at first birth, then age at first grandmotherhood, G, is

$$G = A_m + A_d. \tag{4}$$

We then sum over every possible pair, giving the implied cumulative distribution of age at first grandmotherhood as

$$P(G \le g) = P(A_m + A_d \le g) = \sum_a p_m(a)\, F_d(g-a), \tag{5}$$

the discrete convolution of the mother's and daughter's first-birth distributions.[12]

We construct empirical distributions of age at first birth from age-specific live first-birth rates in published U.S. National Vital Statistics Reports. Because the data are by five year age groups, we fit a parametric first-birth schedule. Details are in Appendix A, where we also present benchmarks assuming a uniform age within each bin.

We benchmark age at first grandmotherhood for a hypothetical cohort using two simplifying assumptions. First, each observed period Vital Statistics distribution represents the fertility schedule faced by an entire generation. Second, ages at first birth are independent across generations, conditional on the maternal and daughter schedules. Positive correlation in age at first birth between mothers and daughters would amplify compound delay.

Our benchmarks are conservative because we study the timing of *entry* into grandmotherhood and ignore the number and spacing of grandchildren. When a woman has more than one child, she becomes a grandmother through the child who bears first. Her age at grandmotherhood is therefore

$$G = \min_{1 \le j \le K} \left(A_m^{(j)} + A_d^{(j)} \right), \tag{6}$$

where $A_m^{(j)}$ is her age at the birth of her j-th child, $A_d^{(j)}$ is that child's own first-birth age, and K her number of children. The convolution in equation (4) is the single-child case $K = 1$. That convolution bounds the true distribution from one side because a minimum age at first grandmotherhood cannot be larger than the age at first grandmotherhood through any child. We can thus write

$$P(G \le g) \;\ge\; \sum_a p_m(a)\, F_d(g-a). \tag{7}$$

The single-child case convolution *understates* the cumulative probability of grandmotherhood at every age. The gap between the convolution and the true age at first grandmotherhood increases in completed parity. The understatement is largest for high-parity, earlier-childbearing women because early childbearers tend to have more children. Appendix B evaluates equation (6) directly, using completed parity from IPUMS. It decomposes the shift from the 1960 to the 2024 schedule

[12] We work with hypothetical cohorts rather than linked families. The second generation is itself a schedule of women's first births, routing grandmotherhood through daughters. Because men's first births are later, a male first-birth schedule would show more delay.

into the two generations' timing. A grandmother's own postponement accounts for 53 to 56 percent of the shift.

Table 1 summarizes the resulting demographic benchmarks for our hypothetical cohorts. The first four rows, where the mother and daughter distributions are from the same year, represent historical and contemporary steady states in which successive generations share the same fertility distribution. The rows where the mother and daughter distributions differ represent transitions where mothers experienced earlier fertility while their daughters postpone first births to substantially older ages. Each row answers a counterfactual: what would the age distribution of grandmotherhood be if a hypothetical cohort faced the stated age-specific first-birth rates at every age? A hypothetical cohort can differ from any real cohort, and real cohorts are still postponing fertility. The final row therefore adds a forecast: it shifts each generation's schedule one year older, bounding how much further grandmotherhood would be delayed if postponement continues.

The resulting trend in age at first grandmotherhood exceeds the trend in first births that produces it. Table 1 shows that delayed fertility substantially postpones grandmotherhood. The first four rows indicate that median age at first grandmotherhood rises from about 44 in 1960 to 47 in 1980 to 50 in 2000 and 57 in 2024. Roughly half of the total 1960 to 2024 shift (about 7 years of the 13) occurs between the 2000 and 2024 fertility regimes, so much of the compression is recent. The pattern at the 75th and 25th percentiles is similar, with a total increase between 1960 and 2024 of about 11 and 16 years at the 25 and 75th percentiles, respectively.

The transition rows of Table 1 show that today's delay in grandmotherhood reflects the fertility behavior of the younger generation. Even when mothers follow the relatively early fertility schedules observed in 1960 or 1980, daughters following the 2024 fertility schedule delay the median age at grandmotherhood by 5 years using the 1980 distribution for mothers and by 6.5 years using the 1960 distribution for mothers. The final row of Table 1 shows that if the age at first birth continues to rise, the median age at first grandmotherhood will reach 59.

Figure 1 illustrates how the acceleration of delayed first childbearing over the most recent two decades affects compound delay. Relatively modest changes in first-birth timing translate into much larger shifts in the timing of grandparenthood. We use the period distribution of age at first birth from the Vital Statistics natality records for 2007 and 2024. Panel A1 plots the first-birth cumulative distribution. The median rose from about 24.7 to 27.9 between 2007 and 2024, a shift of just over three years. Panel A2 plots the implied distribution of age at first grandmotherhood, obtained by convolving each year's first-birth distribution with itself. The median rises from about 50.7 to 56.1, a shift of five and a half years.[13] The probability of having become a grandmother by age 60 falls from 83 percent in 2007 to 67 percent in 2024.

We have constructed age at first grandmotherhood for hypothetical cohorts by convolving period rates. Section 4 replaces period rates with synthetic cohort first-birth rates, reconstructed from census data. Section 5 examines realized grandmotherhood.

[13] The figure uses first-birth counts, which embed the female population's age structure. Table 1 uses age-specific rates. Appendix A reconciles the 2024 medians.

4 Synthetic Cohort Evidence (IPUMS Census)

We previously constructed hypothetical grandmotherhood distributions using period first-birth schedules. We now replace those period rates with synthetic cohort first-birth rates for each education and race group and investigate our framework's implication that grandmotherhood is later and less likely among the college-educated. We use the IPUMS Census and American Community Survey to construct census cells large enough to estimate first-birth schedules separately by education and race for every single birth-year cohort.

We reach five conclusions. First, the probability of grandmotherhood by age 55 has declined across cohorts. Second, the probability is far lower for college-educated women than for non-college women. Third, the divide is by education rather than race. College-educated women of both races reach grandmotherhood at nearly the same, low rate. Non-college women of both races become grandmothers at nearly the same, high rate. Fourth, falling family size delays grandmotherhood. A missing second child increases delay far more than a missing third child. Fifth, intergenerational persistence in fertility timing compounds the delay among the college-educated.

We calculate age at first grandmotherhood by building a synthetic two-generation measure that mirrors the convolution of the previous section. A key difference is that we allow the daughter's first-birth distribution to depend on her mother's education and race group. Let $p_{c,s}(a)$ denote the probability that a woman's first birth occurs at age a for birth cohort c and group s, where s indexes the interaction of race and education. Let $F_{c,s}(a) = \sum_{x \leq a} p_{c,s}(x)$ denote the corresponding cumulative distribution function. A woman in cohort c and group s who has her first birth at age a has a first daughter in cohort $c + a$. We assume that the daughter inherits her mother's group s, and has her own first birth according to the distribution $p_{c+a,\,s}$. A shift in the composition of the daughter generation can therefore be misread as a change in timing.[14] Convolving the two generations' first-birth distributions yields a probability of age at first grandmotherhood ($G = A_m + A_d$) of

$$P(G \leq g) = \sum_a p_{c,s}(a)\, F_{c+a,\,s}(g - a). \tag{8}$$

Equation (8) contains an intergenerational correlation because the daughter's cohort $c + a$ is endogenous to her mother's timing and her schedule is that of her mother's group. We measure that correlation in Section 4.2.

We construct both age-at-first-birth schedules from repeated cross-sections.[15] We date a first birth from the age of a woman's eldest co-resident child. This is informative only if no older child has already left home. We verify the assumption against the question on children ever born, which is available only for 1980 and 1990: earlier censuses asked it of ever-married women alone, which would select the schedule on marriage, and later ones dropped it. For cohorts born after about 1950

[14] Group inheritance is imperfect. A daughter's education also reflects parental marital sorting, which changed substantially across these cohorts (Chiappori et al., 2025). Appendix Table C2 quantifies how much the assumption matters.

[15] Appendix C describes the retrospective measurement, changing Census fertility questions, and resulting sample restrictions.

the upper tail of the mother's own first-birth distribution is therefore no longer pinned, and for cohorts born after the mid-1950s the daughters are too recent to appear in our samples at all. For the cohorts we cannot observe directly, we therefore use a parametric fit, and we flag every cohort whose grandmotherhood probability draws materially on it. Unflagged cohorts run from 1929 to 1949 for White college women, through 1951 for Black college women, and into the early 1960s for the non-college groups. College women's schedules censor earlier because their later childbearing pushes their daughters further into the censored region.

Figure 2 illustrates our first two findings, namely the lower and declining probability of grandmotherhood by age 55 for college-educated relative to non-college-educated women and an educational, not a racial, divide. It plots the probability of grandmotherhood by age 55 against the mother's birth cohort, separately for college-educated and non-college White and Black women. The college-educated women of both races sit near the bottom, at roughly a 30 percent probability of grandmotherhood by 55, while the non-college women of both races sit near the top, around 60 percent. The two college lines track one another, as do the two non-college lines. Therefore education, not race, orders the groups. The probability of grandmotherhood is drifting downward across cohorts for both college and non-college women. The drift among college women is greater among the most recently observed cohorts.

4.1 The family-size margin

What is the grandchild penalty if a woman has fewer children? Equation (3) predicts that a mother with fewer children becomes a grandmother later even if none of her children changes when she bears. Moving a mother from two children to one lowers her probability of grandmotherhood by age 55 by 11 to 15 percentage points, depending on group, and from three to two by 4 to 5 points, so giving up the second child costs about three times as much as giving up the third.[16] The two-to-one penalty exceeds the entire 10-point decline White college-educated mothers recorded between the 1929 and 1949 cohorts. The margin matters most for the college-educated, whose births are latest: moving from two children to one delays their first-quartile age of grandmotherhood by 2.6 years for White women and 3.1 for Black women.

Childlessness at age 50 is U-shaped, falling across the cohorts that came closest to universal motherhood and returning to roughly its early twentieth-century level, and is about twice as high among the college-educated throughout (Appendix Table D6).

4.2 Intergenerational persistence

How much of compound delay among the college-educated is intergenerational persistence rather than the mechanical product of two late-fertility schedules? Equation (8) answers this directly, because it assigns the daughter her mother's education–race group. Appendix Table C2 reports

[16] The calculation assumes 2.5-year spacing, daughters who inherit their mother's education–race group, and conditionally independent sisters' first-birth timing. The last makes these upper bounds: Appendix D.5 finds a within-sibship correlation of about 0.22.

the difference between $P(G \leq 55)$ for a mother–daughter pair of the same group and a pair where the daughter is drawn from the population at large.[17] For college-educated women the effect is negative and sizeable, twelve to fifteen points for White women and about ten for Black women. For the non-college groups it is small and positive. By treating the two generations as independent we therefore understate the delay in grandmotherhood among the college-educated by roughly that amount.

We can estimate within-group transmission net of group inheritance in the HRS. A year of maternal delay transmits roughly a fifth to two-fifths of a year to the daughter (Appendix Table D5), and its interaction with the daughter's birth cohort is significant in every specification.[18] Daughters are tracking their mothers more closely over time, while the correlation among *sisters* is 0.22 and flat across maternal cohorts.

5 Realized Cohort Evidence (HRS)

The two preceding sections inferred the age distribution of grandmotherhood. The Health and Retirement Study reports it. We observe it for cohorts old enough to have entered the panel, but cannot see the recent cohorts whose timing is shifting the most.

5.1 Results

Panel A of Table 2 describes changes across cohorts in the realized probability of being a grandmother at various ages and shows that the delay in grandmotherhood is a change in timing rather than eventual incidence for the observed cohorts.[19] The decline at age 55 is monotone and large. Seventy-two percent of the 1940–1944 cohorts were grandmothers by 55, against 53 percent of the 1960–1964 cohorts—a fall of nearly twenty percentage points in twenty birth years. Grandmotherhood by age 60 is roughly 83 percent across the 1935–1939 through 1945–1949 cohorts and then falls abruptly to 65 percent beginning with the 1950–1954 cohorts. The decline shrinks as the age at which we measure grandmotherhood rises, from about twenty points at 55 to fifteen at 60, ten at 65, and three at 70. Women not yet grandmothers in their late fifties catch up by their late sixties.

Panel B documents that the gap in grandparenthood by education is enormous but longstanding. At age 55, 79 percent of non-college women born in 1940–1944 were grandmothers against 39 percent of the college-educated. By the 1960–1964 cohorts the figures are 64 and 27 percent. Both groups postpone substantially, by 15 points and 12 points respectively. The gap narrows only slightly, from forty points to thirty-seven.

[17] This captures persistence through inheritance of the mother's education–race group. We test the convolution's conditional-independence assumption of no within-group transmission by estimating the sister correlation in Appendix D.5.

[18] Transmission is no stronger among the college-educated: the interaction with maternal education is insignificant throughout.

[19] The prevalence of grandmotherhood among women of cohort c observed at age a is the realized $P(G \leq a \mid c)$. We previously estimated this quantity using the convolutions.

Panel B also shows that the gap by race has widened. Grandmotherhood at 55 falls from 72 to 44 percent for White women across these cohorts, but only from 79 to 69 for Black women. The seven-point head start Black women held in the 1940–1944 cohorts widens to twenty-five points. This gap is unconditional on education. In contrast, Section 4 finds few racial differences among women within education groups.[20]

Compound delay is only now becoming visible in the HRS because the panel first observes women at age 50. More than half of the women in eight of the nine cohort bands were already grandmothers by age 50. Women born in the 1930s and 1940s had their first births around age 21, as did their daughters. Their median age at first grandmotherhood was therefore in their mid-forties, before they entered the panel. The HRS cannot recover a median that falls below its entry age of 50. Only the 1955–1959 band reaches a fifty percent grandmotherhood rate at an age we observe (52.3). When we compare the vital-statistics, census, and HRS estimates we need to do so where they overlap, that is, at $P(G \leq A)$ using common reference ages.

Our estimate of compound delay is biased downward because realized grandmotherhood is that of survivors. The bias is larger at the oldest ages and for the oldest cohorts. Mortality selection retains the college-educated, who are least likely to be grandmothers. It thus depresses the measured probability of grandmotherhood in the oldest cohorts and flattens the measured decline across cohorts. Differential attrition out of the survey works in the same direction. We adjust for differential non-response by applying the HRS person weights. We cannot control for mortality selection.

5.2 Not yet a grandmother

The share of women not yet grandmothers by age 55 rose from 28 to 47 percent between the 1940–1944 and 1960–1964 cohorts, and from 61 to 73 percent among college graduates (Table 2). Because most of these women became grandmothers eventually, we measure the probability of grandmotherhood by a given age rather than completed lifetime grandmotherhood.

5.3 Within-mother evidence of compound delay

Is delay transmitted from mother to child, or does the education gap in grandmotherhood simply reflect the smaller families of college-educated women? We disentangle the two by comparing the births of adult siblings.[21] We estimate a within-mother discrete-time hazard of each adult child's first birth. Our identification comes from siblings of the same mother who reach childbearing ages in different birth cohorts. Details are in Appendix D.4.

[20] The rise in the White share holding a college degree accounts for about a quarter of the decline in White grandmotherhood at 55. (A Kitagawa decomposition splits the total change of -0.275 into a composition component of -0.065, or 23.8 percent. The remainder is postponement within education groups. Across these cohorts the White college share roughly doubled from 18 to 35 percent, while the Black share rose from 14 to 20 percent. We do not report the same decomposition for Black women because the endpoint college cells contain only 12 and 43 women.)

[21] We examine all children to obtain a larger sample size. Appendix D.4 shows that there is little difference in transmission between sons and daughters.

The education gap in grandmotherhood is handed down. College families do not simply have fewer children. Within a sibship, children born into later cohorts postpone more. The steepening is larger for college-educated mothers, with a cohort interaction between -0.0008 and -0.0028 (Appendix Table D2). The effect is economically large. Among college-educated mothers, the share of children who are parents by age 30 falls by ten to twenty percentage points between siblings born ten years apart. That is between a fifth and a half of the roughly forty-point aggregate educational gap in grandmotherhood by age 55. Appendix D.4 also shows that the children of Black mothers postpone *less*.

5.4 Agreement across data sources

Three different data sources and estimators show that grandmotherhood is arriving later across cohorts, with the largest delay among the college-educated. But at the same time life expectancy, including healthy life expectancy, has been rising. The next section investigates whether women have been spending fewer healthy years as grandmothers.

6 Healthy Overlap

Our estimated distribution of grandmotherhood ages determines not only when grandmotherhood begins but how many of a woman's remaining healthy years it occupies. The years a grandmother and a grandchild share are the only years in which she can interact with her grandchild and supply time, care, or help with the grandchild's development. We measure the health-overlap window by computing, from the grandmotherhood distribution F_G, the expected overlap

$$\text{OVERLAP}(T_h) \;=\; E\big[\max(T_h - G,\, 0)\big] \;=\; \sum_g f_G(g)\, \max(T_h - g,\, 0),$$

that is, the expected number of years a woman spends as a grandmother before a window endpoint T_h. We can compute $\text{OVERLAP}(T_h)$ from either period or cohort schedules because G is the sum of two generations' ages at first birth.[22] The period schedules of Section 3, applied to both generations at once, give the aggregate change between the 1960 and 2024 fertility regimes. The cohort-specific mother and daughter schedules of Section 4 give the estimates by education, race, and cohort band in Table 3.

The measure is unconditional when we compute it on the cohort schedules. Women who never become grandmothers contribute zero. The measure thus aggregates the extensive margin (whether grandmotherhood arrives by T_h) and the intensive margin (how early within the window it arrives) into a single population average.

[22] We use the same Coale–McNeil densities as Table 1. We truncate at age 50 and renormalize (see Appendix A for details). The choice of a truncated Coale–McNeil over a uniform density matters most when fertility is late. Under the 1960 schedule the truncated fit and a uniform-within-bin reading of the same rates give 27.9 and 27.8 healthy years; under the 2024 schedule they give 15.0 and 16.7.

The period schedules are fitted densities of age at first birth. They thus contain no childlessness and every woman becomes a grandmother. We are averaging over grandmothers alone when we compute overlap on the period schedules. Both the period and the cohort calculations measure the same thing about timing, but only the cohort calculations also reflect whether grandmotherhood arrives at all.

We impose an endpoint at a common healthy age. Fixing the endpoint holds survival constant, so a change in overlap is attributable to timing alone. (Section 6.1 asks how much longevity would have to rise to offset the delay.)[23] We consider several endpoints, including those specific to education and race.

Once both generations face the same postponed schedule, observed fertility postponement reduces healthy overlap by roughly thirteen years, about 46 percent of the overlap a grandmother expected under the 1960 fertility schedule. Comparing the two first-birth schedules shows the change directly. Under the 1960 schedule the median age at (matrilineal) first grandmotherhood is 43.6, and a woman who becomes a grandmother can expect 27.9 years of overlap before age 72.5. Under the 2024 schedule the median is 56.9 and the expectation is 15.0 years. Each schedule here is applied to both generations, as in Section 3.

Table 3 illustrates differences in healthy years overlap from fertility postponement by education and race. It reports the synthetic-cohort version by decadal birth band and demographic group at two common endpoints: age 85, which counts all the overlap years a woman is likely to live to see (Panel A), and age 72.5, which counts only the years she can expect to be free of disability (Panel B). Panel C repeats Panel B at each group's own measured healthy window.

Three findings in Table 3 stand out. First, the education gradient is large and stable. Non-college White women born in the 1930s could expect 20.0 years of healthy overlap against 11.4 for their college-educated counterparts, a penalty of 8.6 years. The Black penalty is slightly larger, 9.5 years in the same band. This penalty compounds delay in both generations, because the daughters of college-educated mothers both disproportionately postpone first births and obtain college educations. Second, within every group, healthy overlap peaks in the 1920s and 1930s bands and declines thereafter. Figure 2 shows the same cohort turning point in the probability of grandmotherhood by age 55. Third, a comparison of Panels A and B shows that delay moves overlapping life years beyond the healthy window. For non-college White women born in the 1940s, 27.8 total overlap years contain only 18.7 healthy ones. The unhealthy share grows mechanically as G rises.

The education penalty shrinks when we account for documented differences in longevity and health by education and race. Panel C of Table 3 repeats Panel B using disability-free life expectancy for women in 2001 when they were age 60–90, as estimated by Solé-Auró, Beltrán-Sánchez, and Crimmins (2015). Table F2 repeats the exercise using each group's own *total* remaining life

[23]Existing measures do not isolate the effect of fertility timing on healthy overlap. Cohort measures combine mortality improvement with delayed fertility (Margolis and Verdery, 2019). Period health-expectancy measures average over the whole population, including those who never become grandparents (Margolis and Wright, 2017). Our levels are consistent with both; differences reflect our matrilineal definition and fixed endpoint.

expectancy in place of its disability-free years. The penalty falls from 8.5 to 7.0 healthy years for White women in the 1930s band, and from 9.5 to 7.9 for Black women. Roughly four-fifths of the education penalty survives because the measured education gradient in the healthy window of 2.5 years cannot offset the 13–15 year difference in median age at grandmotherhood by education. The adjustment also all but closes the overlap advantage of Black non-college women over their White non-college counterparts, which in the 1930s band falls from 1.3 healthy years at the common endpoint to 0.3 years once each group is evaluated at its own shorter window.[24] Table F3 repeats Panels A and B at a total endpoint of age 80 and shows that nothing depends on the choice within the measured age range.

Delay removes 46 percent of a woman's healthy overlap years but only 36 percent of the grandchild care those years would have carried (Appendix G). The years it removes are those in which the grandchild is oldest. The 1960 schedule spans grandchild ages 0 to 28 and the 2024 schedule 0 to 16. Infancy survives in both, and only the top of the age range is lost. Care is concentrated in the years the two schedules share. Of the grandchild care a 45-year-old woman will ever supply, 15 percent lies past age 72. The ATUS records care by the woman's age, not the grandchild's, and an eighty-year-old's grandchildren are typically adults. If care falls with her age mainly because her grandchildren are growing up, then average care by age understates the provision of childcare by a woman who becomes a grandmother late in life. Appendix G weighs this against the opposite bias, that the average is measured on all women rather than on grandmothers alone.

We measured the grandmother's window; a couple's shared window is shorter. Husbands are typically older than their wives and face higher age-specific mortality, so the years in which both are alive fall well short of the wife's own remaining years. Compton and Pollak (2021) calculate that a wife of sixty married to a husband of sixty-two could expect 24.4 further years herself but only 17.7 with her husband also alive. Delayed grandmotherhood leaves an even shorter shared window and some couples with no shared grandparenthood at all.

6.1 Does longer life offset the delay?

We now ask how much longer a woman would have to stay healthy to recover the years taken by postponement. Under the 1960 schedule she could expect 27.9 healthy years with her grandchildren; under the 2024 schedule she can expect 15.0. We solved for the healthy-window endpoint T_h^* that restores 27.9 healthy years under the 2024 schedule to obtain age 85.9, or 13.4 years more than our fixed healthy-window endpoint of age 72.5 (Table 4).[25]

No measure of healthy longevity is near age 85.9. Disability-free life expectancy places the healthy window endpoint at 74.2 in 1970 and 76.5 in 2010 (Crimmins, Zhang, and Saito, 2016).[26]

[24] Our results are consistent with Margolis and Verdery (2019), who find that the length of grandparenthood is the dimension on which racial convergence has not occurred.

[25] The requirement exceeds the 13-year gap in overlap between the 1960 and 2024 schedules because an added year of health buys a full year of overlap only for women who already are grandmothers, that is, $\partial \text{OVERLAP}/\partial T_h = F_G(T_h) < 1$.

[26] Definitions of disability vary across studies, explaining why our healthy endpoint of 72.5 differs from 76.5. The latter still falls 9.4 years short of 85.9.

Total life expectancy at 65 implies a mean age at death of 80.8 in 1960 and 85.8 in 2024. Restoring the 1960 overlap would thus require a woman to stay healthy to an age the average woman surviving to 65 does not reach in any state of health.

Longevity has slowed as postponement has accelerated (see Figure 3). Life expectancy at 65 rose 3.1 years in the thirty years between 1960 and 1990 but has increased by only 1.9 years in the thirty-four years between 1990 and 2024. Since 2000 life expectancy has risen 1.8 years while the median age at first grandmotherhood has increased by 6.7.

Longer life makes a year of the daughter's delay *more* costly. The cost depends on how many women are inside the healthy window. A year of delay costs a year of overlap only for a woman whose first grandchild would otherwise have arrived before T_h. A further year is costless for a woman whose first grandchild arrives after T_h. Because a woman inside the healthy window loses one year and a woman outside of it loses none, the marginal cost (years of overlap lost per year of delay) is $F_G(T_h)$, that is, the share of women whose first grandchild arrives before T_h. This share has fallen from 0.999 under the 1960 schedule to 0.991 in 1980, 0.970 in 2000, and 0.919 in 2024 (Table 4). Eight percent of women now have their first grandchild after T_h, whereas almost none did in 1960. Each further year of the daughter's delay pushes another 1.1 to 1.5 percent of women past T_h. But as T_h rises, more women enjoy healthy overlap with a grandchild. At $T_h = 80$, the 2024 first-birth schedule yields an $F_G(T_h)$ of 0.979 rather than 0.919. Each of the women brought into the healthy-overlap window loses a year when her daughter delays.

7 The Echo of Reproductive-Control

We now test whether the reproductive-control revolution of the late 1960s and early 1970s, which gave young unmarried women legal access to the pill and to abortion, can explain later grandmotherhood. Reproductive control affects both the first births of women who gained access and the first births of their daughters. We call the second its *echo*. The echo does not require the grandmother to have used the pill, sought an abortion, or even to have had access to either. A woman born in 1929 reached age 21 a decade before the FDA approved the pill and more than two decades before *Roe*; her daughters, born in the 1950s, were fully exposed. The reproductive-control revolution moved daughters' first births, and that shift propagates through equation (8) into their mothers' age at grandmotherhood.

Early legal access to the pill lowered the probability of an early first birth and raised women's schooling, labor-force participation, and career investment, principally among women on the college margin (Bailey, 2006; Goldin and Katz, 2002). Which reproductive technology did the work is contested. The statutes granting young women access to the pill often granted access to abortion at the same time. Myers (2017), recoding the legal environment, argues that once the two are separated abortion access moved teen births and marriages. Guldi (2008) finds that minors' abortion access lowered white birthrates. Bailey, Guldi, and Hershbein (2013) reply that the coding errors identified in this literature cannot account for its findings. We report both codings. The main text uses Myers

(2017) and includes abortion, and Appendix H uses Bailey et al. (2011) and omits it.

7.1 Measuring Fertility Control

The same statutes often legalized both the pill and abortion. Our exposure measure is therefore access to either the pill or abortion for young, unmarried women. Our causal claims rest on that combined exposure. Identification comes from differences across states of birth within a cohort and across cohorts within a state. Let S_{sc} denote the share of ages 18–20 at which women of birth cohort c born in state s could lawfully consent, while young and unmarried, to either the pill or an abortion, under the legal coding of Myers (2017). Let B_i equal one if woman i, observed at age 24, 25, or 26 in a census year from 1960 to 1980, has a co-resident own child—an accurate proxy for a first birth by age 25 at those ages. Writing $g(i)$ for her group (White college, White non-college, and Black non-college), $s(i)$ for her state of birth, $c(i)$ for her birth cohort, and $a(i)$ for her single year of age, we estimate

$$B_i = \sum_g \beta_g \left(S_{s(i)c(i)} \times \mathbb{1}[g(i) = g]\right) + \sum_g \gamma_g \, \mathbb{1}[g(i) = g] + \mu_{s(i)} + \lambda_{c(i)} + \phi_{a(i)} + \varepsilon_i, \qquad (9)$$

where μ_s are state-of-birth fixed effects, λ_c birth-cohort fixed effects, and ϕ_a single-year-of-age indicators. β_g is the change in the probability of a first birth by 25 when group g moves from no confidential access to full access. We exclude Black college women from equation (9) because there are too few of them at these ages to form a separate group. They enter when we do not group by education. We cluster standard errors on state of birth, the 50 states and the District of Columbia.

Recall that our sample consists of women born by 1958 and observed in the 1960, 1970, and 1980 censuses. They are in three cohort bands—born 1933–36, 1943–46, and 1953–56. The pill's legalization separates the first two bands from the last; the consent statutes and legal abortion arrive between the last two bands and within the last. We estimate equation (9) four further ways: 1) with state-of-birth-specific linear cohort trends; 2) restricted to women whose census state of residence equals their state of birth; 3) with the policy controls of Myers (2017); and 4) with the union rebuilt under the alternative pill coding of Bailey et al. (2011).[27]

We then separate our combined measure of reproductive control into its components. We replace the single exposure with three mutually exclusive exposures at ages 18 to 20: 1) the share of those ages during which the pill was legal and a woman could consent to it; 2) the share during which it was legal but she could not; and 3) the share during which abortion was legal in her state on any terms. We interact each exposure with the woman's race–education group and enter them jointly. We use the resulting estimates to ask what the grandmotherhood distribution would have looked like had the legal regime never changed.

[27] The Myers controls are exposure to pre-*Roe* abortion-reform statutes, state equal-pay laws, state laws against racial discrimination, and no-fault divorce.

7.2 Reproductive control and first births by age 25

Panel A of Table 5 reports equation (9), showing that access to reproductive control widened the educational gradient in early motherhood. Moving from no confidential access to full access lowers the probability of a first birth by age 25 by 5.4 percentage points among White, college-educated women. The corresponding estimate is essentially zero (-0.000) for White non-college women. Our estimate for White college-educated women is essentially unchanged by including state-specific cohort trends (-0.053), adding the Myers policy controls (-0.054), restricting to non-migrants (-0.050), and using the Bailey coding (-0.052).

Panel B shows that when we separate our combined measure of reproductive control into its pill and abortion components, the magnitudes of the pill coefficients are large for White college-educated women and the abortion coefficient for White non-college women. For White college women the pill coefficient is -0.086 where the pill was legal and a woman could consent and -0.082 where the pill was merely legal. Both coefficients strengthen slightly under state trends.[28] In contrast, the abortion coefficient is only -0.034 and does not survive state trends. For White non-college women the pattern reverses. The pill terms are zero and the abortion coefficient is -0.047 (-0.030 with state trends).However, while the difference in pill coefficients across education groups is statistically significant ($p < 0.001$), the difference in the abortion coefficient is not ($p = 0.40$).[29]

The estimate for Black women runs opposite to the White estimate under every measure, coding, and specification. The coefficient on our reproductive control variable is a statistically significant $+0.149$ for Black non-college women and a statistically significant $+0.157$ when the education groups are pooled within race.[30] Our legal-access measure is an intention to treat and Black women probably were less treated. The access laws of the late 1960s arrived alongside changes in family-planning funding, welfare, and enforcement that bore differently on Black women. A state-by-cohort design cannot separate the legal regime from these changes.[31]

7.3 The no-reproductive-control counterfactual

We now ask how much of measured compound delay is accounted for by reproductive control in the 1929 and 1949 cohorts. We remove the estimated effect of the legal regime from every first-birth schedule and recompute the convolution (Appendix H). By removing reproductive control completely, we obtain an upper bound on its contribution to delayed grandmotherhood.

[28] Where the pill was legal but a woman could not consent, the coefficient is already -0.082, so minors'-consent statutes added little for the college-educated.

[29] We report results in Appendix H where we additionally divide the abortion term into where a woman could consent and where she could not. We cannot adjudicate between Goldin and Katz (2002) and Myers (2017). Only 4 percent of the variation in abortion-consent exposure survives the state and cohort fixed effects. The pill-consent and abortion-consent exposures correlate at 0.78 across state-of-birth $\times$ cohort cells.

[30] The positive and highly statistically significant coefficient we obtain when we pool by race implies that changes in education composition cannot explain the result. Access may have led some women to complete college and thus changed who is in the education groups. Pooling the groups removes this channel.

[31] Our interpretation is consistent with Guldi (2008), who finds that minors' legal access reduced birth rates among White women but finds no corresponding effect for non-White women.

Panel C of Table 5 shows that without access to reproductive control, a White college-educated mother from the 1949 cohort would have had a 0.340 probability of being a grandmother by age 55 rather than 0.231, a 10.9 percentage point increase. The corresponding increase for White non-college mothers would have been 1.2 points. Access to reproductive control widened the educational gap in early grandmotherhood by about 10 points. Early grandmotherhood fell by 0.097 across the 1929–1949 college cohorts. Access to reproductive control accounts for 56 percent of that decline.

The echo dominates in both cohorts. For the 1949 college cohort the daughters account for a 59 percent share of compound delay (6.4 of the 10.9 points). The daughter channel is the entire effect for the 1929 cohort, whose members never had access at any age. Appendix H runs a narrower counterfactual that switches off early pill access alone using Bailey et al. (2011)'s coding. It returns a smaller echo, but for the 1949 cohort the daughters' share of it is 89 percent rather than 59. Grandmotherhood moved when the grandchild's mother could control her own fertility.[32]

8 Price and Income Effects

The rising outside options for women that reinforce delay have both price and income effects, pulling in opposite directions. They increase the price of an hour of grandchild care. They also make grandmothers wealthier with fewer grandchildren, raising the resources per grandchild. We first examine the price effect in the allocation of time (Section 8.1), then the income effect in the resources per grandchild (Section 8.2), and then the transfer of money and time after the arrival of a grandchild (Section 8.3).

8.1 The price effect: time use

An hour with a grandchild has to be taken from something else. We compare time use on days with and without grandchild contact for all women aged 55–74, regardless of their grandmother status (which we cannot observe). We identify grandchild contact from the diary as a day on which a grandchild is present during any recorded activity. For respondent-day i and activity category k, let Y_{ik} be the outcome—minutes per day in Panel A, an indicator for any time in the activity in Panel B—and let C_i equal one if a grandchild was present during any diary activity. Our seven categories are childcare, market work, volunteering, religious activities, socializing, television, and non-TV leisure, with childcare pre-specified as the primary outcome. We estimate separately for each category and on both margins, the probability that the activity occurs and the minutes spent in it,

$$Y_{ik} = \theta_k C_i + \mathbf{Z}_i' \boldsymbol{\pi}_k + \mu_{y(i)} + \nu_{m(i)} + \rho_{d(i)} + \kappa_k \, Holiday_i + u_{ik}, \tag{10}$$

where $\mathbf{Z}_i$ contains age, an indicator for a bachelor's degree or higher, race, Hispanic origin, and marital status, and $\mu_{y(i)}$, $\nu_{m(i)}$, and $\rho_{d(i)}$ are fixed effects for survey year, calendar month, and

[32] The counterfactual moves only the probability of a first birth by age 25, leaving the rest of each schedule unchanged. It also uses access at ages 18–20 alone. We estimate the counterfactual only for cohorts born before 1950. For later cohorts, the mother also had access to reproductive control.

day of week. Estimates are weighted by the ATUS final person weight, and standard errors are computed from the survey's 160 successive-difference replicate weights. The coefficient of interest is θ_k, the difference in time use between contact and non-contact days for otherwise-similar women on otherwise-similar days. The estimates describe how time use differs on contact days; they do not identify a causal effect.[33]

We estimate the composition of a contact day, not the contact rate, that is, the share of days on which a grandchild appears at all. Compound delay sets both the contact rate and the composition of the contact day. Grandchild contact on any given day is rare (recall that the denominator is all women aged 55–74). Only 4.8 percent of women aged 55–74 have a grandchild present during any diary activity. The rate is lower for men (2.4 percent). The rate is lowest where fertility delays are largest. Because the denominator is all women rather than all grandmothers, these rates mostly track how few college-educated women are grandmothers by these ages. The contact rate is 2.0 percent for college-educated women against 6.0 percent for the less educated (Appendix Table I1).

Table 6 documents the composition of a woman's day when she has contact with a grandchild. It shows that she reallocates her time away from paid work and passive leisure. Active childcare more than quadruples, from 12 to 54 minutes ($= 12 + 42$), and the probability of any childcare rises 43 points. This is partly definitional—a grandmother caring for a grandchild registers both the contact and the childcare. Market work falls by 31 minutes and 6 points of participation. Television falls by 22 minutes and non-TV leisure by 9, with no change in the probability of either. Minutes of volunteering, religious time, and socializing are unchanged, but the probability of any socializing rises 5 points, consistent with a grandchild visit classified as social contact and displacing other social time. The reductions in work, television, and leisure survive multiple testing across the seven outcomes, as do the changes in work and socializing participation.[34]

The reallocation of a grandmother's time on a contact day is not an artifact of *when* contact occurs in the week. Contact days are disproportionately on weekends and holidays, when market work is already low. All specifications include fixed effects for survey year, month, day of week, and holiday. The estimates thus compare contact and non-contact days within the same kind of day (Appendix I).

Women who see a grandchild differ from those who do not in employment, retirement, and proximity to an adult child. We do not control for these choices because they are not exogenous. Section 1.4 treats them as choices made in anticipation of a daughter's timing. One of these choices, hours of work, is a dependent variable in Table 6. We find that proximity is a response to a child's first birth. Within a sibship, a child's first birth raises the probability that her mother lives within ten miles of her child by about five percentage points, largely because of co-residence (Appendix J).

[33] Our estimate of the fall in market work is likely overstated. Contact days are selected: grandchildren may visit when the grandmother's time is already relatively cheap, and the ATUS does not identify non-contacting grandmothers, so we cannot correct the selection. The estimates are descriptive rather than causal.

[34] Westfall–Young q-values of < 0.001, 0.003, and 0.036 for the three minute reductions, and < 0.001 and 0.008 for the two participation margins.

We report three additional results in Appendix I. First, neither education nor labor-force status changes how much childcare a grandmother provides on a contact day. Employed and retired grandmothers give up the same amount of their day. What changes is what they give up. An employed grandmother finances contact out of market work and leaves television alone. A retired grandmother cuts television by close to a fifth of her ordinary viewing.[35] Second, grandfathers aged 55–74 cut market work by about as much as grandmothers do, though the reduction is significant only nominally. They do not cut television or leisure. Third, adding supervisory care of non-own children sharply raises measured childcare on contact days. Because supervisory care is a secondary activity, other activities remain unchanged.

Grandchild care does not come out of eldercare. Elder care takes 10.1 minutes on a contact day and 10.0 minutes on other days. Delay changes the kind of care a woman is doing at a given age. The college-educated postpone most. At ages 55–74 they are thus less likely to have a grandchild to care for and more likely to have a parent who needs care. College-educated women born in the 1950s provide a hundred hours of grandchild care over two years less often than others, 28 percent against 34, and give as much to a parent more often, 7.4 percent against 5.7.[36]

The patterns we uncover favor interaction value over existence value. Existence value predicts that a grandmother's time use on a contact day would resemble her time use on any other day, and it does not.

8.2 The income effect: resources per grandchild

We documented how contact with a grandchild changes a grandmother's time allocation. We now examine how wealth per grandchild has changed. We compare household net worth (deflated to 2022 dollars) for women aged 65–79 between an early period (1995–2006) and a late one (2008–2022). We choose this age range both because wealth is near its lifetime peak at these ages and because the panel cannot yet observe late-period cohorts at older ages. Wealth per grandchild rises when a woman has more to give or fewer grandchildren to give it to. Both channels favor the college-educated, who postponed most and whose net worth also grew fastest (Section 5). We find that wealth accumulation dominates.[37]

8.2.1 The statistic

Let $R = E[W]/E[N]$, where W is a woman's household net worth and N her number of grandchildren. R is thus aggregate real wealth held by women in the grandparent generation divided by aggregate grandchildren, the dollars standing behind a randomly chosen grandchild. Equation (11)

[35] A retired grandmother has almost no market work to cut because she averages under nine minutes of market work on a day without a grandchild.

[36] All numbers are means. The differences between the college-educated and others are statistically significant: -0.0765 (s.e. 0.0145, $p < 0.0001$) for grandchild care and $+0.0169$ (s.e. 0.0061, $p = 0.006$) for parent care, clustered on the woman. Minutes of elder care are from ATUS. The provision of 100 or more hours of care is from the HRS, using all women aged 55–74 in waves 5–16.

[37] We do not measure inheritance at death, which primarily passes to children rather than directly to grandchildren.

decomposes $\ln R$ into three terms: the wealth of the grandparent generation, the number of children they had, and the number of children those children had.

$$\ln R \;=\; \underbrace{\ln E[W]}_{\text{wealth}} \;-\; \underbrace{\ln E[K]}_{\text{first-generation fertility}} \;-\; \underbrace{\ln\bigl(E[N]/E[K]\bigr)}_{\text{second-generation fertility}}\;, \tag{11}$$

where K is the number of adult children and $E[N]/E[K]$ is grandchildren per adult child.K and N are measured from realized child and grandchild counts. The fertility terms enter negatively. A group whose children arrive in smaller numbers has more wealth per grandchild. Appendix K provides details on the calculation of the equation terms.

We compute R two ways. Our preferred statistic is unconditional, calculated over all women aged 65–79, because wealth held by a woman who is not yet a grandmother is still wealth in the grandparent generation. It overstates wealth per grandchild, because at these ages most women without grandchildren will never have any. The conditional statistic, computed over grandmothers only, understates it and selects on the paper's own dependent variable. Grandmotherhood by these ages is falling fastest for the college-educated, so the conditional statistic for the late-period college group would be computed using only the women whose daughters did *not* delay. We lead with the unconditional statistic and report both.

The two statistics are linked exactly. Total grandchildren is the same under both, because non-grandmothers contribute none, so

$$R_{\text{all}} \;=\; R_{\text{grandma}}\big/s_W, \qquad \text{hence} \qquad \Delta\ln R_{\text{all}} \;=\; \underbrace{\Delta\ln R_{\text{grandma}}}_{\text{intensive}}\;\underbrace{-\,\Delta\ln s_W}_{\text{extensive}}, \tag{12}$$

where s_W is the share of the age group's wealth held by grandmothers. The extensive term is large precisely when women who would be wealthy grandmothers are not grandmothers yet.[38]

We compare women aged 65–79 in 1995–2006 with women the same age in 2008–2022. We cannot follow cohorts. Grandchildren accumulate with age. Age and cohort are therefore not separately identified when some cohorts are observed only early and others only late.[39]

8.2.2 Results

Table 7 reports R by education, showing that the college-educated fared exceptionally well. A level advantage is unsurprising—college graduates marry one another, and household net worth counts both careers. The change in R requires explanation. Dollars per grandchild *fell* for every education group except college graduates: down 5 percent for women without high school, 9 percent for high-school graduates, and 13 percent for those with some college, against a rise of 55 percent for college

[38] This is a wealth-weighted extensive margin, not the grandmother rate itself. If p is the grandmother share, then $s_W = p\,E[W \mid G]/E[W]$, so $\ln s_W = \ln p + \ln\bigl(E[W \mid G]/E[W]\bigr)$. The term therefore changes with both grandmother prevalence and the covariance between wealth and grandmother status.

[39] Women born in 1915–1919 are observed only early, at mean age 77.4; women born in 1955–1959 only late, at mean age 65.7. Mean age in the 1995–2006 and 2008–2022 comparison groups is 71.5 and 71.7.

graduates. The aggregate rose 40 percent because composition changes were dramatic.[40]

Panel B, which decomposes the change in wealth per grandchild through equation (12), shows increases in both the intensive (fewer grandchildren and more wealth among grandmothers) and extensive (fewer grandmothers) margins for college-educated women. For the non-college-educated the intensive margin decreased while the extensive margin increased modestly. For the college-educated the intensive margin accounts for 69 percent (= 0.303/0.436) of the increase in wealth per grandchild, leaving a substantial 31 percent to the extensive margin. The share of college-educated women's wealth held by grandmothers fell from 0.924 to 0.809, so nearly a fifth ($= 1 - 0.809$) of the wealth of college-educated women aged 65–79 sits with women who have no grandchild to leave it to.[41] The extensive margin accounts for about 22 percent (= 0.118/0.534) of the college–high-school divergence, and operates almost entirely through the college-educated. It reflects both compound delay and grandchildlessness that will prove permanent, which these data cannot separate.[42]

We have investigated differences in resources by race and ethnicity. Proportionally the gap narrowed: dollars per grandchild rose 43 percent for White non-Hispanic women against 82 percent for Black and 76 percent for Hispanic women, so the White-to-Black ratio fell from 7.6 to 6.0. In dollars the gap widened, from about \$102,000 to \$140,000. Every group of grandmothers in Table 7 except for the college-educated has less wealth standing behind each grandchild than her counterpart did twenty years earlier.

8.2.3 Sources of the education gap in resources per grandchild

Equation (11) decomposes the college–non-college gap in resources per grandchild into wealth, first-generation fertility, and second-generation fertility. Wealth explains the largest share. College-educated women's net worth grew 0.26 to 0.31 log points faster than any comparison group's, which is about two-thirds of the gap in every contrast. First-generation fertility adds 0.07 to 0.13, since college graduates had fewer adult children to begin with and the difference widened. Second-generation fertility accounts for the rest, never as much as a sixth.[43] Wealth accumulation, not fertility compounding, drives the gap (Appendix K).

8.2.4 Inequality in dynastic resources

Concentration of dynastic resources is occurring on two margins. The first is extensive: the share of college-educated women aged 65–79 with no grandchildren rose from 9.4 to 16.3 percent. The

[40] College graduates rose from 13.6 to 23.5 percent of women aged 65–79 and from 29 to 50 percent of their wealth. Holding the early grandchild-population composition fixed, aggregate wealth per grandchild would have risen only 9 rather than 40 percent. Thus roughly three-quarters of the aggregate increase reflects changing composition.

[41] Most will not. Among women with no grandchildren at ages 65–69, only about one-quarter become grandmothers over the next decade, and roughly three-fifths reach age 79 alive and still without a grandchild (Appendix Table K5).

[42] The education divergence is not driven by the 2008 financial crisis: excluding 2008–2012 increases it from 0.532 to 0.611 log points (Appendix K, Table K3).

[43] The small second-generation term partly reflects the observation window: women aged 65–79 in 1995–2022 were born in 1916–1957, so few had daughters exposed to the largest fertility delays. Its importance should rise as the 1950s and 1960s cohorts, whose grandmotherhood by 55 fell sharply, enter these ages.

second is the gap between groups. Geometric-mean wealth per grandchild runs $34,000 for non-college women against $148,000 for the college-educated in the early period, and $36,000 against $194,000 in the late period, so the ratio rises from 4.3 to 5.4 with the non-college figure flat in real terms. Changes in the dispersion of grandchild counts are not an important third margin. Among college-educated women the count component explains about three percent of the increase in dispersion; among non-college women, counts became less dispersed (Appendix K).[44]

8.3 Do resources follow the grandchild? Transfers at the first birth

We examine whether a grandmother gives an adult child who recently had a baby more money than she gives to her other children. Compton and Pollak (2026) expect an intergenerational bargain to run through transfers of time and money rather than cash inducements. The Health and Retirement Study records financial help to each adult child separately, so we can compare siblings in the same family interviewed at the same time (Appendix L). A grandmother becomes 2.4 percentage points more likely to give money to the child who has just had a baby than to that child's siblings, remaining more likely to do so for a decade, but the sums are modest. The jump in the first full wave after the birth annualizes to $202 (95 percent confidence interval $39 to $365) in 2022 dollars, while at the same event time the probability of supplying a hundred or more hours of grandchild care rises 28 percentage points. College and non-college grandmothers give alike at the birth. An education gradient in transfers appears only five or more waves later. Resources per grandchild therefore diverge because of differences in accumulated wealth, not because grandmothers respond differently to a birth.

9 Valuing a Family Bargain

A daughter's delayed fertility costs her mother healthy overlap. Once a grandchild arrives, the grandmother provides childcare, which the daughter treats as free. If there are gains to trade, why is there no bargain between mother and daughter?

We argue that early in her career the daughter's return to delay in each year is larger than her mother's yearly provision of money and care. We estimate that a complete contract would move first births by only weeks. A mother thus cannot outbid the career return that produced the delay in the first place. To obtain more childcare, the daughter's yearly payment to her mother for grandchild care would need to make up for her mother's lost opportunities. Families would find such a payment for care repugnant (Roth, 2007). Compton and Pollak (2026) therefore expect intergenerational bargains to be struck in child care and down payments rather than in cash.

We compare the gains for each generation in the noncooperative outcome against the gains from cooperation on the timing and childcare provision margins. We use an illustrative, partial-

[44] Net worth is observed while women are alive, so this is potential rather than realized inheritance. Because non-positive net worth cannot enter the log, the dispersion calculations omit the poorest 3,875 of 44,756 grandmothers and therefore understate inequality.

equilibrium calibration built from observed flows and hold family behavioral responses fixed (Appendix M). In the language of family bargaining (Lundberg and Pollak, 1996), we measure a surplus relative to the parties' threat points. The disagreement outcome is the daughter's privately optimal age at first birth, a^*.[45]

Table 8 summarizes the calibration results. The first block estimates the cost that one year of delay imposes on the mother. It is a money-and-care flow that is also the annual value of the childcare provision margin. The second block estimates the daughter's return to that year of delay. The third takes the ratio of the quantities in the first two blocks. Let a^{**} be the age that maximizes the two generations' joint payoff, $-V'(a^*)$ what a year of the daughter's delay costs her mother, and $|u''|$ how fast the daughter's own marginal return to waiting declines. The quantity $a^* - a^{**} = -V'/|u''|$ measures how much earlier a daughter would bear a first child if she took her mother's loss into account. We call it a first-order correction because it is a local approximation to the complete solution of the cooperative problem.

The ratio measures the time it takes for the daughter's marginal return to delay to fall by an amount equal to her mother's loss. Her marginal return to waiting one more year declines by $|u''|$ each year and reaches zero at a^*. Had she counted her mother's loss she would have stopped when a further year cost her mother $-V'$, which is $-V'/|u''|$ years before a^*.

The mother loses a year of healthy overlap each year the daughter delays. She loses it when she is oldest, at the end of the healthy-overlap window. The first block of Table 8 prices that year of lost healthy overlap based on the money and grandchild care grandmothers provide ten or more years after the birth. The grandmother provides \$331 a year in money. The chance she is providing a hundred hours or more of care increases by 18 percentage points, which we value at its market price. The total price is \$460 to \$1,003 a year.[46] Discounting back to the daughter's decision at a^*, we obtain a loss of $-V'(a^*)$ priced at roughly \$230 to \$1,100 for each year of delay.

We calculate the gain from cooperation as follows. A daughter's marginal return to delay falls by three to ten thousand dollars with each additional year of waiting. This is the $|u''|$ of the table's second block.[47] The gain from cooperation on the timing margin is the mother's loss divided by the decline in the daughter's marginal return, $-V'/|u''| = a^* - a^{**}$, which is measured in years. It is between one week and four and a half months of earlier childbearing, centered on about five weeks. The extra overlap those weeks buy is worth about \$46 per lineage, valued at the total price of a lost year of healthy overlap (\$460 to \$1,003). Pairing the largest loss to the mother with the slowest decline in her daughter's return, the combination most favorable to the mother's case, yields \$410 per lineage.

[45] Because the calculation holds behavioral responses fixed, we recover no preference parameters and make no welfare claim. We also do not need a sharing rule. A bargain could capture the surplus regardless of how it is divided.

[46] These figures differ from those in Section 8.3 because the time horizon is different. That section reports the first full post-birth wave, when money transfers are smaller and caregiving is larger.

[47] Miller (2011) finds that delaying motherhood raises college graduates' career wages by 5 percent a year and does nothing for other women. The return to delay is exhausted by a woman's early thirties, close to the a^* of 31 in our calibration.

The money and care a grandmother provides dwarfs the gain of \$46 (or at most \$410) per lineage that five weeks of earlier childbearing is worth. That flow recurs in every year of a 14.5-year window and is the flow in the first block of Table 8. Its present value is \$4,300–\$13,400 per lineage. The value of grandchild care rises with a grandmother's outside options, making our calculation a lower bound.[48] Care dominates the flow. It is worth up to twice the money she sends (Appendix L).

Both the timing and childcare provision figures are averages and thus conceal types. Some parents are untroubled by the absence of a grandchild. For others it is a lasting private grief. Averaging understates the loss for families who value a grandchild most.

10 Conclusion

As educational attainment rises and first births continue to move later, the typical first-time grandmother will be a college graduate in her sixties. Conditional on a day of grandchild contact, she supplies as much active care as a woman without a degree. What changes is the number of healthy years during which such contact can occur.

Delayed fertility shortens the years in which grandparents can invest in the next generation's skills and pass on their norms and identity (Bisin and Verdier, 2011). Falling and delayed fertility also thins the broader kin networks of cousins, aunts, uncles, and community elders through which norms, identities, and local knowledge historically have been transmitted (Bernheim and Bagwell, 1988). Accounts of declining civic engagement usually emphasize the forces that pull individuals away from communities—television, suburbanization, digital media. Our findings suggest a complementary mechanism: delayed and reduced fertility gradually thins the kin networks through which these interactions occur.

Delayed grandparenthood also bears on the intergenerational transmission of human capital. A large literature on the technology of skill formation (Cunha and Heckman, 2007) emphasizes that family investments during childhood determine the persistence of economic advantage across generations, and it has focused on parents. As fertility is delayed across successive generations, grandparents spend fewer healthy years overlapping with grandchildren, reducing opportunities for childcare, mentoring, cultural transmission, and other forms of informal investment. Fewer grandchildren means more time and money for each. But the cost of delay depends on the role a grandmother plays in a grandchild's development. If the returns are largest in early childhood, delay takes 17 percent of her healthy years with a grandchild under ten. If the returns run through adolescence and early adulthood, when attitudes are most malleable (Krosnick and Alwin, 1989), delay takes 60 percent of her healthy years with a grandchild between 14 and 24.

[48] We value childcare at its replacement cost, not its opportunity cost. Employed grandmothers give up market work on contact days (see Section 8.1). Their opportunity cost may exceed the childcare wage.

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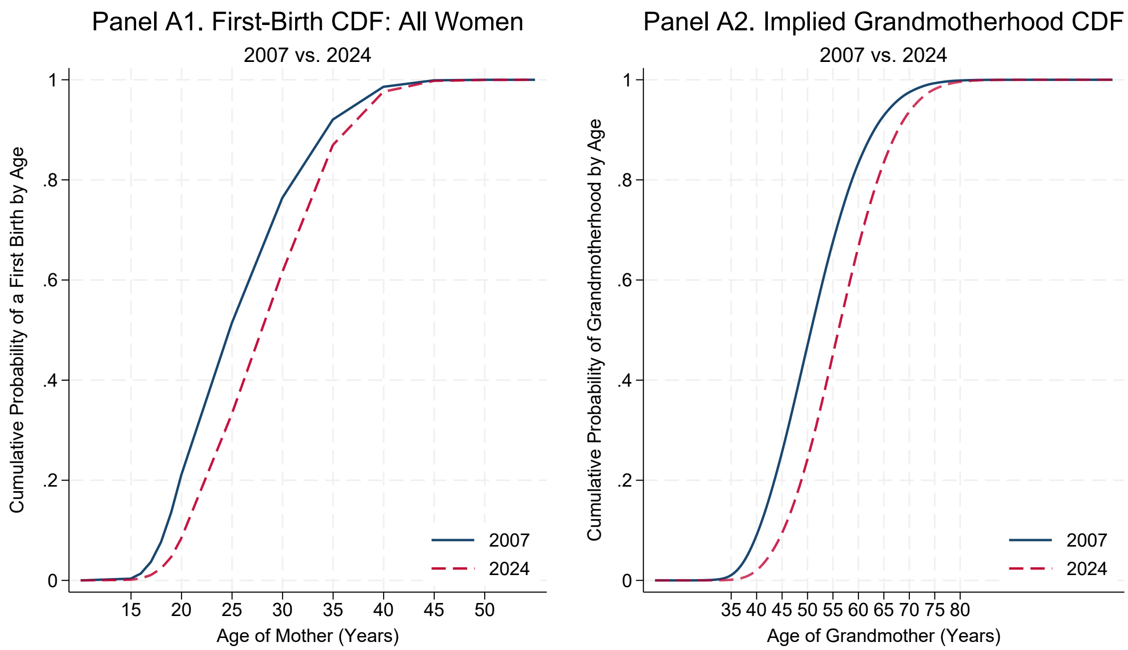
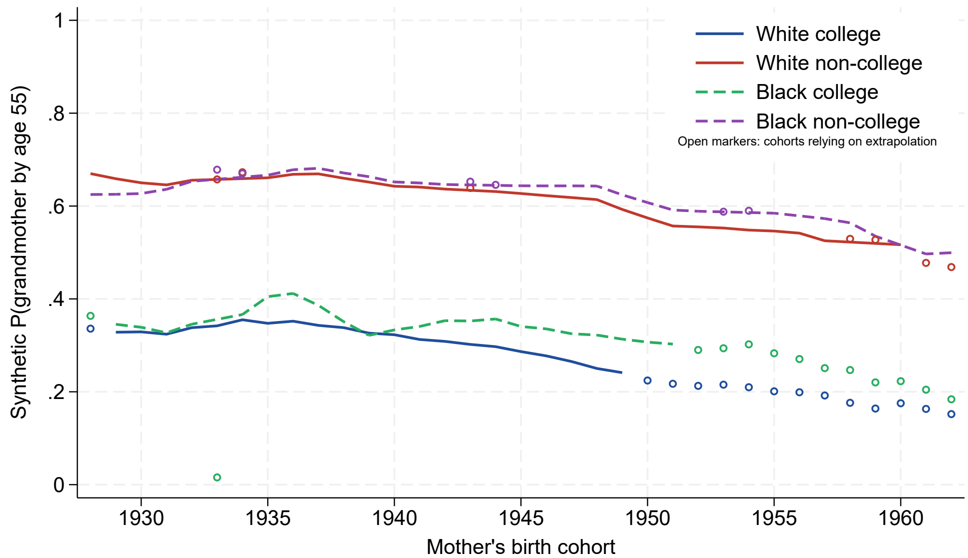
Figure 1: Recent changes in first-birth and implied grandmotherhood CDFs for all women, 2007–2024.



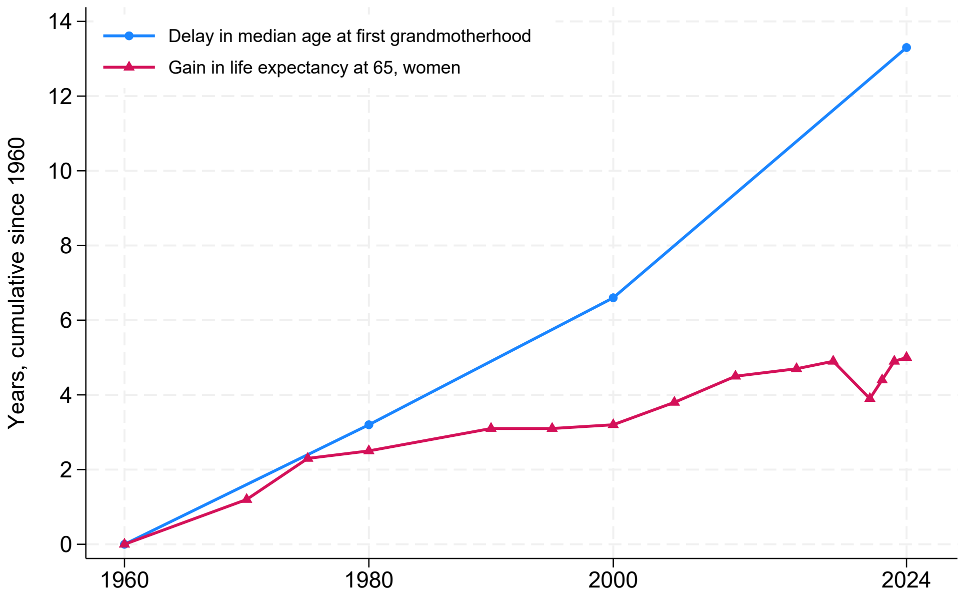
Notes. Panel A1 plots the cumulative distribution of age at first birth; Panel A2 plots the implied cumulative distribution of age at first grandmotherhood, obtained by convolving each year's first-birth distribution with itself (equation (5)). Source: authors' calculations from National Vital Statistics Reports.

Figure 2: Synthetic probability of grandmotherhood by age 55, by mother's birth cohort, for college-educated and non-college White and Black women.



Notes. Solid lines are three-cohort moving averages through cohorts whose estimates do not rely on extrapolation. Open circles mark cohorts whose estimates do and are excluded from the averages. Extrapolation binds for recent years. Children ever born, which anchors the upper tail of a mother's own first-birth schedule, was asked of all women in 1980 and 1990 and not afterwards. In addition, the most recent cohorts' daughters have not yet appeared in the data. The later childbearing of the college-educated pushes more of the calculation into the censored region, which is why those lines end sooner. A line running near an open circle interpolates across that skipped cohort between its reliable neighbors. Heavily extrapolated cohorts need not cluster in time, because the extent of extrapolation depends on which censuses happen to observe each cohort's mothers and daughters. Black college cells are small and that series is correspondingly noisier. Source: authors' calculations from the 1940–2020 Census and American Community Survey.

Figure 3: Postponement and longevity since 1960



Notes. Both series are cumulative years since 1960. The delay is the median age at first grandmotherhood implied by each year's first-birth schedule, from Table 1. The longevity series is remaining life expectancy at 65 for U.S. women. The two move together until the mid-1970s and separate thereafter. The fall after 2019 and the recovery through 2024 are the pandemic; the 2024 value stands 0.1 years above 2018. The vertical distance between them shows how far the gain in total life expectancy falls short of the delay, 8.3 years by 2024. Section 6.1 states the shortfall in disability-free terms instead.

Table 1: Demographic Benchmarks for Age at First Grandmotherhood

Scenario	Mother regime	Daughter regime	Age at first grandmotherhood: 25th	Median	75th
1960 distribution remained constant	1960	1960	40.3	43.6	47.7
1980 distribution remained constant	1980	1980	42.0	46.8	52.7
2000 distribution remained constant	2000	2000	44.1	50.1	57.5
2024 distribution remained constant	2024	2024	50.8	56.9	64.1
1960 distribution becomes the 1980	1960	1980	41.1	45.1	50.2
1960 distribution becomes the 2000	1960	2000	42.0	46.7	52.7
1960 distribution becomes the 2024	1960	2024	45.3	50.1	56.1
1980 distribution becomes the 2000	1980	2000	43.0	48.4	55.1
1980 distribution becomes the 2024	1980	2024	46.3	51.8	58.5
2000 distribution becomes the 2024	2000	2024	47.4	53.5	60.8
2024, postponement-projected (+1 yr/gen.)	2024^{+}	2024^{+}	52.7	58.8	65.9

Notes. Age at first birth is modeled with the Coale–McNeil schedule (Coale and McNeil, 1972) and the two generations' continuous densities are convolved directly using equation (5). Fitted parameters and a uniform-within-bin robustness check are in Appendix Table A3. The final row shifts each generation's schedule older by one year as a continued-postponement bound. Every percentile therefore moves by exactly two years. *Source:* age-specific first-birth rates from National Vital Statistics Reports (Appendix Table A1); grandmotherhood distribution constructed by the authors.

Table 2: Realized Grandmotherhood by Birth Cohort: HRS Women, 1992–2022

Panel A. $P(G \leq A)$, all women

Birth cohort	By age 55	By age 60	By age 65	By age 70
1930–1934	—	—	0.858	0.888
			(0.022)	(0.012)
1935–1939	—	0.830	0.905	0.907
		(0.020)	(0.009)	(0.011)
1940–1944	0.722	0.835	0.859	0.914
	(0.024)	(0.014)	(0.014)	(0.015)
1945–1949	0.682	0.816	0.833	0.861
	(0.022)	(0.015)	(0.016)	(0.020)
1950–1954	0.642	0.654	0.757	—
	(0.025)	(0.021)	(0.026)	
1955–1959	0.569	0.678	—	—
	(0.021)	(0.027)		
1960–1964	0.527	—	—	—
	(0.034)			

Panel B. $P(G \leq 55)$, by group

Birth cohort	No college	College+	White NH	Black NH
1940–1944	0.793	0.390	0.720	0.788
1945–1949	0.783	0.345	0.651	0.862
1950–1954	0.741	0.364	0.605	0.832
1955–1959	0.701	0.275	0.521	0.741
1960–1964	0.640	0.274	0.444	0.693

Notes. Realized $P(G \leq A)$ is the weighted prevalence of having at least one grandchild among women of the given birth cohort observed at that exact age. Sample: female HRS respondents, waves 3 and 5–16 (1992–2022). A cell is reported only when the entire five-year cohort band reaches the age within the panel window and at least 50 women are observed, so that the cohort gradient does not reflect the panel's endpoint; "—" means the panel cannot yet observe the cell. Panel A standard errors are linearized survey standard errors; Panel B omits them because the subgroup cells are descriptive and several are thin. NH denotes non-Hispanic. Hispanic women, reported in Appendix Table D1, track the Black profile. All estimates use HRS person weights. *Source:* authors' calculations from the RAND HRS Longitudinal and Family Files.

Table 3: Expected overlap and healthy overlap years, by birth cohort and group

Birth-cohort band	All women	White, college	White, non-college	Black, college	Black, non-college
Panel A. Expected overlap years by age 85					
1900–09	24.0	–	25.4	–	18.3
1910–19	25.1	–	26.6	–	21.1
1920–29	27.7	19.4	29.5	16.9	27.0
1930–39	27.9	20.3	29.5	19.9	30.6
1940–49	26.0	19.2	27.8	19.4	29.4
Panel B. Expected healthy overlap years by age 72.5					
1900–09	16.2	–	17.1	–	12.9
1910–19	16.7	–	17.8	–	14.9
1920–29	18.5	10.8	20.0	10.0	18.9
1930–39	18.5	11.4	20.0	11.8	21.3
1940–49	17.0	10.5	18.7	11.4	20.5
Panel C. Expected healthy overlap years, group-specific healthy windows					
Window endpoint (age)	–	77.5	75.0	76.5	73.7
1900–09	–	–	18.8	–	13.4
1910–19	–	–	19.6	–	15.5
1920–29	–	14.2	21.9	12.2	19.7
1930–39	–	14.9	21.9	14.3	22.2
1940–49	–	13.9	20.5	13.9	21.4

Notes: Expected overlap is $E[\max(T_h - G, 0)]$, unconditional on becoming a grandmother (women who never become grandmothers contribute 0 years). Each cell is the expected number of years a woman in the given birth cohort and group spends as a (matrilineal, first-grandchild) grandmother before the panel's window endpoint. The common endpoints are age 85 (Panel A) and disability-free age 72.5 (Panel B). With a common endpoint, cross-group differences reflect fertility timing alone. Panel C uses each group's own disability-free life expectancy at age 60 from Solé-Auró, Beltrán-Sánchez, and Crimmins (2015), where a disability is a difficulty with any of four activities of daily living and college is defined as twelve or more years of education. Cohorts enter only if they satisfy the reliability restriction of Figure 2 (no material reliance on Coale–McNeil extrapolation of daughter schedules) and daughters' fertility is fully observed. Cells with fewer than five qualifying cohorts are suppressed. Within-band averages are unweighted. Extrapolation shares are in Appendix Table F1, and group-specific total-window variants in Appendix Tables F2 and F3. Source: authors' calculations using synthetic cohorts from IPUMS Census/ACS and Solé-Auró, Beltrán-Sánchez, and Crimmins (2015).

Table 4: What restoring the 1960 overlap would require, and what longevity delivered

Required	
Healthy-window endpoint restoring 27.9 overlap years	85.9
Delivered	
Disability-free endpoint, 1970	74.2
Disability-free endpoint, 2010	76.5
Age 65 plus remaining life expectancy, 1960	80.8
Age 65 plus remaining life expectancy, 2024	85.8
Overlap lost per year of the daughter's delay, $F_G(T_h)$	
1960 schedule, at $T_h = 72.5$	0.999
1980 schedule, at $T_h = 72.5$	0.991
2000 schedule, at $T_h = 72.5$	0.970
2024 schedule, at $T_h = 72.5$	0.919
2024 schedule, at $T_h = 80$	0.979

Notes. All entries are ages except the last block. The required endpoint solves $\text{OVERLAP}_{2024}(T_h^*) = \text{OVERLAP}_{1960}(72.5)$ on the fitted schedules behind Table 1, truncated at age 50, giving $T_h^* = 85.9$. The disability-free endpoints are age 65 plus the disability-free share of remaining expectancy at 65, from Crimmins, Zhang, and Saito (2016), Table 3, which reports 1970 through 2010. The two rows below them are age 65 plus total remaining expectancy, so they are the age the average woman surviving to 65 reaches in any state of health, not a healthy window; they bound the disability-free endpoint from above. Life expectancy at 65 for women is from NCHS, *Health, United States, 2019*, Table 4 for 1950 to 2018, and NCHS Data Briefs 492, 521 and 548 for 2021 to 2024. Disability definitions differ. The 72.5 used in Table 3 comes from an activity-of-daily-living definition at ages 60 to 90. The movement across the endpoints is comparable, not the levels. (The levels favor the comparison because crediting 76.5 rather than our own 72.5 is more generous to longevity.) $F_G(T_h)$ is both the rate at which overlap rises with the endpoint ($\partial\,\text{OVERLAP}/\partial T_h$) and the overlap lost per year of the daughter's delay. Both sides of the comparison are period constructs.

Table 5: Reproductive-Control and the Probability of a First Birth by Age 25, and Its Echo in Grandmotherhood

	(1) Baseline	(2) State cohort trends	(3) Non-migrants	(4) Bailey et al. coding
Panel A. First stage: confidential access to the pill or abortion (union)				
White, college	-0.054^{***}	-0.053^{***}	-0.050^{***}	-0.052^{**}
	(0.012)	(0.013)	(0.013)	(0.020)
White, non-college	-0.000	-0.002	-0.001	-0.001
	(0.010)	(0.010)	(0.013)	(0.019)
Black, non-college	$+0.149^{***}$	$+0.132^{***}$	$+0.139^{***}$	$+0.151^{***}$
	(0.012)	(0.011)	(0.014)	(0.019)
Pooled by race, no education split				
White	-0.013	–	–	–
	(0.013)			
Black	$+0.157^{***}$	–	–	–
	(0.014)			
Panel B. Allocating the union between technologies (Myers coding and controls)				
	White college	White non-college	Black non-college	
Pill, could consent	-0.086^{***}	$+0.008$	$+0.093^{***}$	
	(0.019)	(0.014)	(0.019)	
Pill, legal only	-0.082^{***}	$+0.005$	$+0.073^{***}$	
	(0.016)	(0.012)	(0.013)	
Abortion (any legal)	-0.034^{**}	-0.047^{***}	$+0.051^{***}$	
	(0.017)	(0.012)	(0.018)	
Panel C. The echo, following Myers: implied P(grandmother by 55), White women				
	1929 cohort	1949 cohort		
College-educated				
Actual $P(G \leq 55)$	0.329	0.231		
No-control counterfactual	0.383	0.340		
Difference (pp)	5.48	10.90		
via her own exposure	0.00	4.58		
via her daughters' exposure	5.48	6.42		
Non-college				
Actual $P(G \leq 55)$	0.661	0.609		
No-control counterfactual	0.665	0.621		
Difference (pp)	0.41	1.20		
via her own exposure	0.00	0.00		
via her daughters' exposure	0.41	1.20		

Notes. Panel A. The dependent variable is a co-resident own child among White and Black women aged 24–26 in the 1960–1980 censuses (cohorts born by 1958) Each row reports that group's own slope, recovered as the sum of the main effect and the group interaction. White non-college is the base category of the interaction and its slope is the main effect. No group is thus omitted from the reported coefficients. The three-group rows contain 487,491 observations (326,729 in column 3). The pooled rows contain 492,794 observations. Exposure is the share of ages 18–20 at which a woman's state of birth allowed young unmarried women to consent to the pill or an abortion (Myers, 2017), interacted with group, as in equation (9). Controls are state-of-birth and birth-cohort fixed effects and single-year age indicators; standard errors are clustered on state of birth (51 clusters). Column 2 adds state-specific linear cohort trends, column 3 restricts to non-migrants, and column 4 uses the Bailey et al. (2011) pill coding. The Black coefficient is robustly positive across specifications.
Panel B. Three mutually exclusive exposures entered jointly under the Myers coding with her policy controls. Abortion includes both legality and consent because the legal-without-consent cell varies within only six states.
Panel C. Applies the Panel B coefficients to each cohort's exposure and recomputes the convolution of equation (8), shifting births along the age axis while holding completed motherhood fixed. The generation-channel rows shift one generation at a time and need not sum exactly. The counterfactual is computed for White women only. Details on the full construction and on the treatment of the non-college truncation are in Appendix H, which also reports the Bailey-coding variant (Appendix Table H1) and a decomposition separating pill and abortion access by whether a minor could consent (Appendix Table H2).

Table 6: Time Use by Grandchild Contact on the Diary Day: Women Aged 55–74 (ATUS 2005–2024)

	Childcare	Work	Volunteer	Religious	Social	TV	Leisure
Panel A: Minutes per day							
Grandchild contact	41.80***	−31.01***	−0.34	1.07	0.75	−22.05***	−8.88***
	(2.96)	(6.24)	(1.49)	(1.57)	(2.51)	(5.04)	(2.89)
Westfall–Young q	<0.001	<0.001	0.934	0.835	0.934	0.003	0.036
Mean, no contact	12.19	127.26	11.28	13.06	41.20	191.70	82.60
Panel B: Participation (any time)							
Grandchild contact	0.426***	−0.060***	−0.005	−0.007	0.050***	−0.009	−0.013
	(0.015)	(0.014)	(0.007)	(0.012)	(0.016)	(0.012)	(0.015)
Westfall–Young q	<0.001	<0.001	0.874	0.874	0.008	0.874	0.874
Mean, no contact	0.10	0.31	0.08	0.14	0.38	0.83	0.60
Observations				33,935			

Notes: The sample consists of ATUS respondents who are women aged 55–74 in the pooled 2005–2024 surveys. We exclude 2020 (N = 33,935). Each column regresses the outcome—minutes per day (Panel A) or an indicator for any time in the activity (Panel B)—on an indicator for grandchild presence during any diary activity, controlling for age, education, race, Hispanic origin, and marital status, with fixed effects for survey year, calendar month, day of week, and holiday. Estimates are weighted by the ATUS final person weight. Standard errors (in parentheses) use the survey's successive-difference replicate weights. "Mean, no contact" is the weighted outcome mean among respondent-days without grandchild contact. Westfall–Young q-values adjust for family-wise error across the seven activity categories using 2,000 bootstrap replications. The childcare coefficient is in part definitional and is reported as a descriptive fact. Supervisory (secondary) childcare estimates appear in Appendix I. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Source: authors' calculations using IPUMS ATUS, 2005–2024.

Table 7: Resources per Grandchild by Education: HRS Women Aged 65–79

Panel A. $R = E[W]/E[N]$, dollars standing behind a grandchild				
	All women 65–79		Among grandmothers	
	1995–2006	2008–2022	1995–2006	2008–2022
< High school	24,544	23,214	23,598	21,394
	(1,826)	(1,988)	(1,719)	(1,835)
HS/GED	85,752	77,726	81,783	73,056
	(5,409)	(4,994)	(5,112)	(4,990)
Some college	152,414	132,794	139,320	121,212
	(8,544)	(7,742)	(7,600)	(6,802)
College+	266,942	412,674	246,565	333,857
	(15,833)	(23,685)	(14,962)	(17,163)
All women	95,394	133,142	89,195	115,485
	(4,837)	(6,228)	(4,271)	(5,527)

Panel B. Decomposition of $\Delta \ln R$, all women 65–79				
	$\Delta \ln R_{\text{all}}$	Intensive	Extensive	s_W: early → late
< High school	−0.056	−0.098	+0.042	0.961 → 0.922
HS/GED	−0.098	−0.113	+0.015	0.954 → 0.940
Some college	−0.138	−0.139	+0.001	0.914 → 0.913
College+	+0.436	+0.303	+0.133	0.924 → 0.809
All women	+0.333	+0.258	+0.075	0.935 → 0.867

Notes. $R = E[W]/E[N]$, where N is a woman's number of grandchildren, is aggregate real household net worth divided by aggregate grandchildren: the dollars standing behind a randomly chosen grandchild. It is *not* the average grandmother's own ratio $E[W/N]$, which is dominated by grandmothers with one grandchild and does not decompose. Wealth is total household net worth including all homes, in 2022 dollars (CPI-U). Sample: women aged 65–79, HRS waves 3 and 5–16. The "all women" columns include non-grandmothers, who contribute wealth but zero grandchildren. Panel B decomposes $\Delta \ln R_{\text{all}} = \Delta \ln R_{\text{grandma}} - \Delta \ln s_W$. It is the identity of equation (12). Bootstrapped differences-in-differences are in Appendix Table K1. *Source:* authors' calculations from the RAND HRS Longitudinal and Family Files.

Table 8: Pricing the Two Margins: Dollars and Weeks

What a year of delay costs the mother	
Healthy overlap forgone	1 year, at the end of a 14.5-year window
Flow that year carries (money + care)	\$460–\$1,003 per year
Her loss, $-V'(a^*)$	\$227–\$1,089 per year of delay
What a year of delay is worth to the daughter	
Decline in her marginal return, $\lvert u''\rvert$	\$2,900–\$10,000 per year, per year
Implied first-order correction under the calibration	
$a^* - a^{**} = -V'/\lvert u''\rvert$	**1 week to 4.5 months**
centered on	about 5 weeks
The flow the correction is priced against	
PV of money + care over the overlap	\$4,300–\$13,400 per lineage

Notes. All amounts are in 2022 dollars. The mother's loss $-V'(a^*)$ is the money-and-care flow of the marginal overlap year, discounted back to the daughter's decision. The daughter's $|u''|$ is the rate at which her own return to delay declines, from Miller (2011). Each range spans the calibration's alternative discount rates, hours assumptions, and return constructions. Appendix Table M1 records every input, its source, and the choice behind each range, and Appendix M sets out what the exercise does and does not identify. *Source:* authors' calculations.