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IMPACT OF ADULTS' COLLEGE COMPLETION ON THEIR PARENTS' MORTALITY: EVIDENCE FROM THE 1993 KOREAN HIGHER EDUCATION REFORM

Ah-Reum Lee
William H. Dow
Jacqueline M. Torres

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ABSTRACT

A large literature examines the economic and health returns to college education. We extend this literature by studying the upward intergenerational effects of children's education on parental mortality. We exploit Korea's 1993 Higher Education Reform, which we estimate raised college completion by 13 percentage points among eldest children in the 1974–1983 birth cohorts relative to the prior 10-year cohort. Using data from the Korean Longitudinal Study of Aging, reduced-form estimates indicate a 27% (s.e.=10%) reduction in mortality among parents whose oldest child was in the reform-affected cohort. Because the reform affected college completion but not other education levels or quality, we use it as an instrumental variable (IV) for child college completion. IV estimates indicate a 20% (s.e.=7%) reduction in parental mortality for children induced to complete college by the reform. These parents received greater financial support and contact from their children and had lower smoking and higher physical activity, although these mechanisms explain only a small share of the mortality benefit. Effects were larger for parents of daughters in reduced-form analyses, although IV estimates did not differ significantly by child gender. Future research should further investigate the mechanisms through which children's college education affects parental mortality.

Ah-Reum Lee
University of Southern California
USC Dornsife College of Letters, Arts and Sciences
Center for Economic and Social Research
ahreuml@usc.edu

William H. Dow
University of California, Berkeley
School of Public Health
and NBER
wdow@berkeley.edu

Jacqueline M. Torres
University of California, San Francisco
School of Medicine
Epidemiology & Biostatistics
Jacqueline.Torres@ucsf.edu

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Abstract

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Keywords: Education, Mortality, Upward spillover, South Korea

JEL classification: I12; I26

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1. Introduction

Education is one of the most powerful predictors of health and longevity. Individuals with higher educational attainment generally experience better health outcomes and live longer than their less-educated peers, a pattern observed across countries and over time (Clark & Royer, 2013; Lleras-Muney, 2005; Lleras-Muney, 2022a, 2022b; Meara et al., 2008). This relationship is thought to operate through multiple channels, including improved health knowledge, increased economic stability, and better access to healthcare (Cutler & Lleras-Muney, 2010; Cutler et al., 2011).

Emerging research extends these insights beyond the individual, suggesting that education may also produce intergenerational health benefits. Building on extended human capital models (Grossman, 1972, 2015; Jacobson, 2000), which conceptualize health as jointly produced within families through shared resources and time, recent studies propose that children's education may influence parental health via upward transfers of health information, caregiving, and material support (De Neve & Harling, 2017; Ma, 2019; Zhang et al., 2022). A growing body of evidence from diverse global settings supports this idea: studies from China (Cui et al., 2021), Mexico (Yahirun et al., 2017), South Africa (De Neve & Harling, 2017), Tanzania (De Neve & Fink, 2018), the United States (Friedman & Mare, 2014), Sweden (Lundborg & Majlesi, 2018), and across multiple European countries (Sabater et al., 2020) find that parents with highly educated children tend to have lower mortality risks. Other studies link children's education to better physical functioning (Zimmer, 2002; Yahirun et al., 2016), improved cognition (Ma, 2019), and better mental health (Yahirun et al., 2020; Gutierrez et al., 2024) among older parents. These findings point to positive "upward" spillover effects of educational attainment within families (De Neve & Kawachi, 2017).

Despite this growing literature, most evidence to date is based on observational studies, leaving open questions about causality. A few studies have leveraged quasi-experimental variation from national education reforms to examine the causal impact of offspring education on parental survival. For instance, leveraging a 1974 Tanzanian compulsory schooling reform, De Neve and Fink (2018) demonstrated that children's education positively influenced parental survival, with sons' education having a particularly notable effect on mothers' survival. This finding partially aligns with studies by Lundborg and Majlesi (2018) and Cui et al. (2021), which leveraged compulsory schooling reforms from Sweden and China, respectively. Both studies found a protective overall effect of increased offspring schooling on parents' survival, with a more pronounced impact of daughters' education. However, to date, quasi-experimental studies have been limited to national reforms that have increased mandatory schooling minimums at the lower end of the educational distribution (e.g., increases from lower to upper secondary schooling). Despite high internal validity, studies leveraging these reforms may be of limited generalizability in contexts where higher (e.g., college) education is common and most relevant for informing current policy debates.

This paper contributes to the limited evidence on the health effects of children's higher education on parental survival by providing causal estimates from South Korea – a rapidly aging society where family remains a key source of eldercare in the absence of comprehensive public support. While the introduction of the National Pension Program (NPP) in 1988 marked a crucial step toward establishing a safety net for retirees, many older adults have not had sufficient time to contribute to the system, resulting in inadequate pension benefits (Organisation for Economic Co-operation and Development, 2011). With limited public wealth transfers and social protection, South Korea continues to rely heavily on intergenerational support structures, where

older adults frequently depend on their children for financial and emotional assistance (Kim, 2010; Lee & Mason, 2012). This reliance makes children's education a potentially powerful lever for addressing health disparities.

Furthermore, South Korea offers a compelling case for a natural experiment, having experienced a dramatic rise in higher educational attainment (Choi and Kim, 2015)**.** A key driver of this expansion was the 1993 higher education reform, a pivotal policy shift in which the government eased college admission restrictions by eliminating fixed quotas per institution, significantly broadening access to college (see Section 3 for institutional background). This reform led to a striking 45-percentage-point increase in college enrollment rates between 1993 and 2004, fundamentally reshaping the nation's educational landscape, workforce, and societal structures (Chae & Hong, 2009; Shin, 2015). Prior research has leveraged this reform to identify the causal effects of college completion on women's fertility (Sohn & Lee, 2019) and labor market outcomes (Choi, 2015). We hypothesize that the reform may have also generated intergenerational spillover effects on parental health, enhancing not only the education of younger generations but also the overall health and well-being of older generations.

We first present reduced-form evidence showing a large effect of the reform on parent mortality: a remarkable 27% reduction in mortality for parents whose oldest child was born in the 1974–1983 cohort first affected by this college reform. This pattern is statistically significantly stronger for daughters than for sons, with reform exposure leading to a 30% reduction in parental mortality for daughters and a 23% reduction for sons. We then present evidence indicating that the reform operated almost exclusively via increased college completion, which allows us to use an instrumental variables strategy exploiting variation in college completion induced by the 1993 higher education reform to estimate the causal effect of

children's college completion on parental survival in South Korea. Specifically, we apply an instrumental-variable hazard model using the two-stage residual inclusion (2SRI) method, which is well-suited for nonlinear models such as time-to-event (survival) outcomes (Tchetgen Tchetgen et al., 2015; Wooldridge, 2015; Terza et al., 2008; O'Malley et al., 2011).

Our instrumental-variables (IV) estimates indicate a 20 percent reduction in parents' mortality risk among those whose children completed college, compared with those whose children did not. Two-stage residual inclusion (2SRI) estimates are similar to the corresponding observational Cox models, with overlapping confidence intervals. While we do not find evidence of statistically significant heterogeneity by parent or child gender in the IV models, and wide confidence intervals preclude definitive conclusions, the magnitude of the longevity benefit of offspring college completion appears larger for fathers than for mothers and more pronounced in the case of daughters' (vs. sons') college completion. Prior studies from China, South Africa, and Sweden report significant heterogeneity by parent or child gender (Cui et al., 2021; De Neve & Fink, 2018; Lundborg & Majlesi, 2018), although direct comparisons with these studies are challenging given the variation in exposures and empirical strategies. Notably though, we do have stronger findings showing that the reform increased girls' education substantially more than boys, with commensurately larger reduced-form mortality benefits for the parents of girls. Additionally, although observational studies in Europe (e.g., Sabater et al., 2020) have suggested that the survival benefits of offspring education may be more pronounced among socioeconomically disadvantaged parents, our reduced-form estimates point instead to somewhat larger protective effects for urban and higher-educated parents, with formal interaction tests indicating significant heterogeneity across those dimensions. However, the corresponding instrumental variables estimates do not detect significant heterogeneity across these groups,

suggesting that any subgroup patterns associated with policy exposure do not translate into statistically significant heterogeneity in the estimated causal effect of college completion.

Supplementary analyses suggest that these benefits may operate in part through changes in parental health behaviors (e.g., smoking, physical activity), while we found no significant effects on age-related chronic health conditions. For these intermediate outcomes, the returns to offspring college completion seem to be particularly concentrated among older fathers. Offspring college completion also positively impacts some aspects of intergenerational contact and financial support (e.g., frequency of in-person meetings and contact, financial support). However, children's college completion continues to have a strong, albeit slightly attenuated, effect on parental mortality even after controlling for measures of health behaviors and intergenerational support. These results may reflect incomplete capture of the underlying mechanisms. For instance, health-literacy spillovers and care-coordination functions, as well as prevention/treatment-directed financial transfers, are imperfectly measured by our available behavior and support variables, leaving residual variation attributable to offspring education.

This study provides the first-ever quasi-experimental evidence of the impact of offspring college education on older parents' late-life health outcomes. Our findings underscore the broader social benefits of educational attainment, extending beyond individual outcomes to influence family health and well-being. As higher education becomes increasingly accessible, these results underscore the importance of expanding educational opportunities to enhance health equity and longevity across generations. Further research is needed to explore additional pathways linking children's education to parental health outcomes.

The remainder of the paper is organized as follows. Section 2 outlines the theoretical background. Section 3 describes the institutional context of the 1993 South Korean higher

education reform. Section 4 details the data, sample, and empirical strategy. Section 5 reports the results. Section 6 discusses the findings. Section 7 concludes.

2. Theoretical Background

The relationship between education and health has long been conceptualized within the human capital framework (Grossman, 1972, 2015; Galama et al., 2018), where education enhances individuals' efficiency in producing health. In this view, educational attainment increases lifetime earnings and improves decision-making capacity, thereby shaping health-related behaviors and outcomes. College completion in particular can raise productivity in both labor and health domains, leading to improved wages, more informed health choices, and better long-term health. These effects are mediated not only through higher income (Becker, 2007; Cowan, 2011), but also through changes in preferences and social environments, such as exposure to less risky peer groups (Katz et al., 2001). However, education may also have unintended consequences – for instance, increased exposure to health-harming behaviors that provide short-term gratification, such as alcohol or tobacco use (Galama et al., 2018).

Extending this model, researchers have theorized the family as a unit of joint health production (Jacobson, 2000; Arrondel and Masson, 2006), wherein education can have spillover effects across generations. Parents may invest in their children's education both altruistically and as a form of old-age insurance (Barro and Becker, 1989; Cox and Stark, 1996, 1998). Conversely, adult children may invest in their parents' well-being, deriving utility from doing so or responding to implicit familial obligations (e.g., caring for aging parents after receiving support themselves) (Cigno, 1993, 2000).

Better-educated children are particularly well-positioned to support their parents' health due to their enhanced capacity to allocate both market (e.g., financial resources) and non-market inputs (e.g., health information, coordination of care). As such, adult children with higher education are more effective at navigating healthcare systems, promoting preventive behaviors, and managing chronic illness on behalf of their parents.

Three main mechanisms are often cited as pathways through which children's education may influence parental health. First, higher educational attainment increases children's earning potential, enabling them to provide financial support for their parents' healthcare needs. Second, educated children may offer more time and caregiving assistance (for instance, maintaining more frequent parent–child interaction and offering in-kind support), either due to more flexible work arrangements or a stronger sense of responsibility, although time constraints and geographic distance can limit their ability to provide direct care (Choi et al., 2020; Lee et al., 2025). Third, education enhances health literacy and decision-making capacity (Choi et al., 2025), allowing children to share relevant health information, advocate for appropriate treatments, and model or promote healthier behaviors within the family.

Ultimately, the extent to which children shape parental health outcomes likely depends on a combination of economic, relational, and informational pathways, and their ability to modify health behaviors, support treatment adherence, and buffer the consequences of health shocks. Therefore, we assess whether controlling for these intermediate outcomes attenuates the estimated association: (1) financial transfers from children to parents, capturing economic support; (2) the quality of intergenerational relationships, proxied by frequency of contact and meetings; and (3) parental health behaviors, including smoking, drinking, and physical activity. By examining the links between children's educational attainment and each of these

mechanisms, we assess the extent to which they help explain observed mortality differences across groups.

Additionally, the magnitude and likelihood of upward transfers may vary by parental socioeconomic status. Theoretical models grounded in altruism (Barro and Becker, 1989) and family constitutions (Cigno, 1993) suggest that support is more likely to flow toward parents with fewer resources, particularly in contexts where public safety nets are limited. In contrast, models based on exchange or strategic bequests predict greater support to wealthier parents who can offer something in return, such as future inheritance, housing, or financial guarantees (e.g., co-signing) (Bernheim et al., 1985). To assess these theoretical predictions, we stratify our analyses by parental gender and SES. Lifelong socioeconomic disadvantage is proxied by parental schooling and total wealth (education, household assets), geographic disparities in health infrastructure are captured by place of residence (urban–rural residence), and structural caregiving norms and health risks are reflected by parental sex (gender).

3. The 1993 Higher Education Reform

The 1993 higher education reform in South Korea, initiated by President Kim Young-Sam, marked a strategic shift from a quota-based, government-regulated system to one emphasizing deregulation and institutional autonomy (Choi, 1996; Lee and Sohn, 1994). Central to this shift were the 'Plan for the Promotion of College Enrollment Liberalization' and the '5·31 Education Reform Plan,' which facilitated a significant expansion in the higher education sector (Lee, 2000). These reforms allowed new universities to operate under relaxed standards, resulting in a dramatic increase in enrollment rates by over 25 percentage points between 1993 and 1997 (Choi and Lee, 2017). Additionally, the adoption of the 'University Establishment Standard Principle'

supported the growth of specialized colleges, underscoring the government's commitment to diversifying educational opportunities.

A particularly striking outcome of these reforms was the significant rise in college enrollment rates among female students. The percentage of women pursuing higher education in South Korea climbed from 22% in 1980 to 33% in 1990, and following the 1990s reforms, surged to 68% by 2000, more than doubling within a decade (Korean Educational Statistics Service (n.d.)). This rapid growth highlights the critical role of the 1993 reform in promoting gender equity in access to higher education and expanding opportunities for women across South Korea. For further details about the institutional background for the reform, see **eAppendix 1**.

4. Data and Empirical Strategy

4.1.Data

This study draws on data from the Korean Longitudinal Study of Aging (KLoSA), a nationally representative longitudinal survey targeting individuals aged 45 and above (born in 1961 or earlier) in South Korea. The KLoSA uses a multistage stratified cluster sampling method based on 15 geographic areas and dwelling types across the country. The baseline wave in 2006 included 10,254 participants and follow-up interviews have been conducted biennially since the baseline. The KLoSA database is open to the public, and anonymized data can be downloaded from the Korea Employment Information Service website (https://survey.keis.or.kr/eng/klosa/klosa01.jsp).

4.2. Analytic Sample

For the purposes of our analysis, we focused on analyzing the mortality of KLoSA respondents who were initially surveyed in Wave 1 (2006) with follow-up through Wave 8 (2020) (n = 10,254). We implemented several inclusion and exclusion criteria to refine our sample. First, we excluded individuals who did not have at least one adult child (aged 25 or older) born between 1964 and 1983 – covering the 10 birth cohorts directly affected by the 1993 higher education reform in South Korea (1974–1983) and the 10 adjacent birth cohorts outside the reform window (1964–1973) (n = 3,391). We chose the 10-year comparison window as our primary specification to balance cohort comparability and statistical power: narrower windows reduced precision and limited subgroup analyses, whereas wider windows increased the potential for unrelated cohort differences (see **Section 4.7**, Robustness Checks, for details). We also excluded observations with missing data on key outcomes or covariates from the analysis (n = 330). The Little's MCAR (Missing Completely at Random) test indicates that our data were not missing completely at random. However, multiple imputation using chained equations produced results similar to the complete-case analysis (see **eAppendix 2**) (Azur et al., 2011). We therefore proceeded with reporting on the complete-case approach. After applying these criteria, the final analytic sample consisted of 6,533 respondents (n = 3,722 women and 2,811 men). The selection criteria for this analytic sample are illustrated in detail in **eFigure 1**.

4.3.Measures

Adult Child Education. In families with multiple children, measuring and categorizing educational attainment can be approached in different ways. For this study, we primarily focus on the educational achievement of an "index" child, which we define in our primary analyses as the oldest child. This approach is consistent with established methodologies in the literature, as

outlined in our supplementary materials (**eAppendix 3**). Using the oldest child as the index aligns with prior research, simplifying the analysis of intergenerational effects while providing a reliable point of reference.

Our primary measure of educational attainment is college completion, coded as a binary variable (1 = oldest child completed college, 0 = otherwise). We focus on college completion (vs. enrollment, the direct outcome of the reform) because, in KLoSA, adult children's education is recorded only as the highest completed degree; information on college enrollment or partial college is not available for children. However, the enrollment-completion gap during this period was small (approximately 1–2 percentage points) (Department of Education, 1997), implying that enrollment gains closely tracked degree completion. To address concerns that any reform-related mortality changes could reflect pathways other than degree completion, we first present the reduced-form association between oldest-child reform exposure and parental mortality, followed by the corresponding instrumental-variable estimates.

Additionally, because educational attainment may not be fully represented by the oldest child alone, we conduct supplementary analyses that redefine the index child as the highest-educated child and, separately, construct a family-level measure capturing the proportion of children who completed college. These specifications assess whether results are sensitive to alternative ways of summarizing children's education and whether parental outcomes vary with the concentration of college-educated children within the family.

Instrumental Variable. We utilize the implementation of the 1993 South Korean higher education reform as an instrumental variable predicting college completion. The reform, which broadened higher education access, affected individuals born from 1974 onwards (see **Figure 1**).

For our analysis, the instrumental variable is coded as '1' if the respondent's index child was born between 1974 and 1983 (thereby affected by the reform and part of the treatment group) and '0' if born between 1964 and 1973 (part of the control group). While we use 10-year birth cohort intervals to construct the IV in the main analysis, we also test alternative cohort ranges (3, 5, and 15 years) in the supplementary analyses. As previously noted, we adopt a 10-year interval as our primary specification because it provides sufficient statistical power to conduct subgroup and heterogeneity analyses, but we evaluate robustness to alternative intervals.

Outcome measures. The primary dependent variable in this study is parental all-cause mortality, specifically the risk of death among parents. In KLoSA, date of death information was obtained from next of kin interviews. We use parental age (measured in years) as the time variable, with parental death due to any cause serving as the event of interest. The risk of death is tracked from the age at which the respondent was first interviewed in 2006, and respondents are followed until their last interview in 2020 or until their death, whichever occurs first. Respondents are censored during the follow-up period if they are lost to follow-up, if their index child dies during the study, if they emigrate or move into non-community dwelling units (such as skilled nursing facilities).

Covariates. We control for a broad range of demographic, socioeconomic, and family-level characteristics. Specifically, we include respondent (parental) demographic factors such as age (in years) and its quadratic term, gender (male/female), marital status (married/partnered versus separated/divorced/widowed/single), and birth cohort (Japanese colonization cohort (1914–1939), Korean war/industrialization cohort (1940–1954), and Baby boomer cohort (1955–1964)).

Socioeconomic factors include respondents' early-life SES (based on their parents' educational attainment), respondents' own educational attainment (in years), household income, household assets, urban or rural residence, and region (17 provinces and special cities).

Additionally, family-level characteristics are considered, including the number of household members, number of living children, children's gender composition (the percentage of living children who are male), and whether the respondent's parents are still alive, and respondent age at first childbirth to account for potential confounding due to variations in parental characteristics. Additional details on the measurement of these confounders are provided in **eAppendix 4**.

Mechanisms. We consider three types of mechanisms via which parents' mortality might be affected by college completion: (1) intergenerational support from the index child to parents, including frequency of contact and financial assistance, (2) parents' health behaviors, such as smoking, drinking, and physical activity, and (3) parents' chronic health conditions, including self-reported physician diagnoses of high blood pressure, diabetes, cancer or malignant tumors (excluding minor skin cancer), chronic lung disease, liver disease, heart-related disease, cerebrovascular disease, psychiatric problems, and arthritis. Further details on the measurement of these mechanisms are also provided in **eAppendix 4**. (We note that parents could also be affected by changing child time allocation during the years in which the children are in college, but that is a temporary period at younger parental ages when the parents typically have fewer health needs, and well before the time period of our survey observation, so we do not consider such effects.)

In the supplementary analysis, we assess the direct impact of children's education on these intermediate outcomes to shed light on possible pathways through which higher educational attainment among children may influence their parents' survival. Additionally, we estimate models that control for these measures to explore whether, and to what extent, the impact of children's education on parental survival may be partly explained by these pathways.

4.4.Empirical Strategy

We employ a Cox proportional hazards (PH) model, one of the most widely used statistical tools for analyzing censored survival outcomes (Cox 1972; 1975). It accommodates dynamic selection by allowing the baseline hazard (the underlying risk of the event occurring) to vary over time. For example, as individuals age, the risk of death typically increases, and the Cox model controls for this by letting the baseline hazard change naturally based on the data. The model also addresses censoring by incorporating available data on individuals whose survival times are only partially observed, ensuring their information contributes to the overall analysis and improving the accuracy of the results.

While the Cox model can incorporate observed characteristics to control for confounding factors, the estimated hazard ratio (HR) may be biased due to unmeasured confounding when important confounders are not fully observed or measured. In our case, unmeasured confounding, such as differences in parental health status in early life, genetic predispositions, or unobserved socioeconomic factors, can bias the estimation of the impact of offspring education on parental mortality.

To address these challenges, we employ an instrumental variable (IV) approach (Angrist et al., 1996) within the Cox model, utilizing a two-stage residual inclusion (2SRI) framework.

This approach corrects for potential biases due to unmeasured confounding by leveraging an external source of variation – specifically, the 1993 higher education reform (i.e., instrument) – that is strongly associated with the endogenous variable (offspring education) but independent of unobserved factors influencing the outcome (parental mortality).

The 2SRI method (Tchetgen Tchetgen et al., 2015) is analogous to two-stage least squares (2SLS) in linear models (Hausman, 1978), particularly in how it addresses endogeneity by isolating exogenous variation in the endogenous regressor from unmeasured confounders. While there is ongoing debate about the relative performance of 2SLS and 2SRI, 2SRI is widely regarded as having theoretical and practical advantages in survival models (Terza et al., 2008; O'Malley et al., 2011), making it the preferred approach for our analysis.

We estimate the following logit model in the first stage:

$$log\left(\frac{\left(P(X=1|W,Z)\right)}{(1\ -\ P(X\ =\ 1\ |W,Z)}\right)\ =\ \alpha_0 +\ \alpha_w W\ +\ \alpha_z Z + e^{2SRI}$$

where X represents the index child's college completion (treatment), W denotes the instrument (exposure to the reform), and Z includes covariates such as parental demographics, socioeconomic characteristics, and household factors. The residual (e^{2SRI}) captures unobserved variation in X not explained by W and Z.

The second stage controls for potential endogeneity by including the residual from the first stage as a control function. We estimate the treatment effect (β_X) using a Cox PH model as follows:

$$h(t\mid X,Z,e^{2SRI})\ =\ h_0(t)\ exp(\beta_X X\ +\beta_Z Z\ +\beta_e e^{2SRI})$$

Here, $h(t\mid X,Z,e^{2SRI})$ is the hazard function conditional on the treatment, covariates, and residual. The baseline hazard function, $h_0(t)$, is unspecified (i.e., non-parametric). β_X represents

the treatment effect of interest measured in hazard ratio (HR), β_Z denotes the coefficients for covariates, and β_e captures the effect of the residual, controlling for unmeasured confounders.

While the Cox model makes no assumptions about the form of the baseline hazard function (non-parametric part of the model), it assumes a parametric form for the effect of predictors on the hazard; that is, covariates influence the hazard through a log-linear relationship (parametric part of the model).

A key assumption of the Cox model is the proportional hazards assumption, which posits that hazard ratios between groups remain constant over time. To ensure the validity of the Cox model, we assess this assumption using Schoenfeld residuals (Schoenfeld, 1982), which allow us to evaluate whether the relationship between covariates and the hazard changes over time. Further details regarding the proportional hazards assumption and its validation process are provided in **eTable 1**.

In our supplementary analyses, we employed a two-stage least squares (2SLS) framework using survival status by 2020 to assess changes in absolute mortality. This approach allows us to better compare our estimates with previous studies on this topic that have utilized 2SLS methodologies.

4.5.Validity of the IV Analysis

To ensure that the 1993 higher education reform is a valid instrument, we relied on the key identifying assumptions, including the predictive power of our instrument (the education reform) affecting the endogenous variable (education of the adult child), the exclusion restriction (the reform is correlated with parental survival only through its effect on the adult child's schooling),

and independence (there are no unmeasured confounders between the reform and parental well-being).

Predictive Power. To assess the predictive power of the instrument (exposure to the reform), we estimate its influence on children's college completion, testing if the partial F-statistic for the instrument exceeds the conventional threshold of 10. We find that it does, regardless of the choice of the index child, thus demonstrating a strong relationship between the instrument and the treatment (Partial F = 62 when the oldest child is the index). Furthermore, prior studies have confirmed that the 1993 higher education reform significantly increased access to higher education for cohorts born after 1974, as evidenced by analyses of Census data (Sohn and Lee, 2019). This provides additional support for the relevance of our chosen instrument.

Exclusion Restriction. Although we have no additional instruments that could provide over-identification tests of the exclusion restriction, several factors support our assumption that the reform instrument only affects parental mortality through its impact on children's college completion.

First, the 1993 higher education reform primarily increased college enrollment and completion, with no significant changes in the rates of other levels of educational attainment (e.g., high school completion or graduate education) during our study period, as shown in the supplementary table (**eTable 2**) using KLoSA data. We cannot examine partial college attainment since our data only include highest level of education completed, but as indicated above, it was rare in this period to have attended but not completed college. This suggests that

the instrument is unlikely to affect parents' outcomes through shifts in other levels of education, thereby reducing concerns about alternative educational pathways.

Second, because our primary specification used the oldest child to define both reform exposure and educational attainment, the exclusion restriction may be violated if the 1993 reform also increased schooling among younger siblings in families whose oldest child belonged to the pre-reform cohorts. In such cases, parents in the control group could still have benefited from reform-induced educational gains among their younger children, thereby reducing the contrast between exposed and unexposed families and thus biasing our results. Although restricting the analysis to respondents with only one child would have addressed this concern most directly, too few such respondents were available for IV/2SRI estimation (803 respondents). To assess this possibility, we conducted sensitivity analyses that excluded mixed-exposure families, defined as families with multiple children in which the oldest child was unexposed but at least one younger sibling was reform-exposed (see Section 4.7, Robustness Checks). The results remained protective and were modestly larger in magnitude, suggesting that any resulting bias likely attenuated the main estimates without altering the substantive conclusions.

Third, although increased financial burden from higher education spending could in principle represent a pathway linking the reform to parental well-being, this concern appears limited in our setting. Available evidence suggests that, although education-related household financial burden in Korea rose gradually during the 1990s, these increases were driven primarily by private education expenditures rather than by sharp increases in regular education costs, including college tuition (Kim et al., 2012; Organisation for Economic Co-operation and Development, 2019). For instance, from 1990 to 2010, real private education expenditures (e.g., private tutoring and hagwon) increased by an average of 5.5% annually, whereas real

expenditures on regular education (e.g., tuition and other school fees) declined by 0.3% annually (Kim et al., 2012). These education expenditure increases were relatively small to plausibly have large parental mortality effects, and to the extent that they differentially affected treated families, would be expected to bias results slightly down.

Fourth, although the rapid expansion of higher education during the 1990s raises potential concerns about declines in institutional quality, which could present an alternative pathway for underestimating effects on parental health, prior research indicates that newly established institutions were generally comparable to older ones across key indicators such as student-faculty ratios, library resources, and investment per student (Choi, 2015; Ahn, 2011).

Taken together, these findings support the credibility of the exclusion restriction – that is, the 1993 higher education reform appears to have influenced parental mortality primarily through its effect on children's college completion. While we find no empirical evidence of alternative pathways, such as changes in other educational levels, educational quality, parental financial strain, or labor market competition, the possibility of unobserved channels cannot be entirely ruled out.

Independence. The independence assumption requires that the instrument be unrelated to unobserved confounders that jointly influence the treatment (college completion) and the outcome (parental mortality). While this assumption cannot be directly verified, its plausibility is supported by the cohort-based policy shift created by the 1993 education reform, which applied to children born in 1974 or later. South Korea experienced broader socioeconomic changes during the 1990s, including economic liberalization and the 1997 financial crisis. However, these shocks were national in scope and unlikely to have affected parents differently based on whether

a child was born just before versus just after the eligibility cutoff. For instance, prior evidence suggests that Korean college graduates entering the labor market during the 1997–1998 crisis experienced worse early employment and earnings outcomes, with more persistent effects among men than women (Choi et al., 2020). However, these penalties appear concentrated among narrower graduation cohorts around the crisis, rather than implying a sharp discontinuity at our reform cutoff. If anything, such shocks likely introduced within-group heterogeneity among reform-exposed cohorts and may have attenuated our estimates by weakening the returns to college for some early exposed cohorts. We therefore view late-1990s macroeconomic shocks as a source of possible within-treatment heterogeneity, but a less compelling explanation for a discontinuous violation of the IV independence assumption at the reform cutoff itself.

Finally, an assumption specific to the survival context is that the censoring process must be independent of event times and unmeasured covariates. This assumption can be partially validated by observable characteristics. However, our descriptive statistics show significant differences between censored (e.g., lost to follow-up) and uncensored individuals on observed variables (**eTable 3**). To address this, we employ inverse probability weighting (IPW), which controls for differences in the likelihood of being censored by weighting on these observed characteristics that predict censoring. By incorporating IPW, the analysis reduces potential bias and ensures that the retained sample better reflects the original cohort. Results were highly similar with and without IPW (**eTable 4**), suggesting that differential censoring is unlikely to be driving the main findings.

4.6.Heterogeneity Analysis

After performing the above procedures on the entire sample, we evaluated whether the effects of children's education on parental outcomes varied by characteristics of both parents and children, including gender, place of residence, parental education, and household assets. We considered these subgroup analyses secondary/exploratory and therefore did not adjust inference for multiple testing. Therefore, these results should be viewed as suggestive.

We begin by stratifying the data (i.e. for both first and second stage estimation) based on the gender of both parents and children, recognizing the traditional preference for sons, as well as shifting gender dynamics in educational achievement and caregiving responsibilities in Korean society. These gendered norms may influence the level of intergenerational support, which in turn could impact the relationship between children's education and parental outcomes.

Beyond gender, we also explore differences across several key sociodemographic characteristics of the respondents. Specifically, we examine whether the impact of children's educational attainment on parental mortality varies by where the parents live (urban vs. rural), their own educational background (less than secondary education versus secondary education or higher), and the level of household assets (below median vs. above median) to identify which groups of parents benefit most from their children’s educational achievements.

4.7.Robustness Checks

First, we explore alternative instrument specifications to ensure the robustness of our findings. This includes using the highest-educated child as the index child instead of the oldest child and alternatively specifying the endogenous regressor as the proportion of children who completed college among all adult children. These alternative specifications allow us to assess the broader impact of education across the family, rather than focusing on a single child.

Second, there is inherent imbalance between treatment and control groups – most notably in terms of respondent (parental) age, which is closely related to mortality risk. KLoSA respondents have lived through significant historical events, such as Japanese colonization (1910–1945) and the Korean War (1950–1953), which influenced survival dynamics. Treatment and control groups were meaningfully different in their exposure to these events given birth year imbalances across the two groups.

To mitigate the potential impacts of treatment group imbalance, we re-estimated our models with varying cohort boundaries (i.e., 3-, 5-, 10-, and 15-year intervals). While we expected that estimates with these more conservative boundaries (i.e., with 3 and 5-year birth cohort bounds) would impose better compositional balance across treatment groups, we also expected that estimates would be underpowered given sample size reductions. We therefore focused on comparing the magnitude and direction of point estimates relative to our primary results. In addition, we address potential birth cohort effects stemming from these historical experiences. We stratify the analysis by birth cohort (Japanese colonization cohort (1914–1939), Korean war/industrialization cohort (1940–1954), Baby boomer cohort (1955–1964) and test if the results are consistent across cohorts.

Third, to evaluate whether results were primarily driven by age-related imbalances across our treatment and control groups, we extended analyses to evaluate the relationship between adult child college completion and a wide range of age-related chronic health conditions, which were also considered previously as potential mechanisms. Specifically, if unobserved cohort effects or age imbalances were driving both children's education and parental mortality, we would expect those same factors to also impact chronic conditions as well. The absence of an effect of children's education on chronic health conditions, or even mixed evidence across a

range of chronic health conditions, would reduce concerns that unobserved cohort effects or inherent age imbalances are responsible for the observed relationship between children's education and parental mortality.

Fourth, to assess the possibility that younger siblings' reform exposure could blur the contrast between treated and comparison groups, we conducted sensitivity analyses that distinguished respondents in fully exposed, partially exposed, and fully unexposed families. Respondents were classified as partially exposed if they had multiple children and the oldest child was unexposed but at least one younger sibling was reform-exposed (n = 1,427). Among the remaining respondents with multiple children, those whose children were all unexposed to the reform were classified as fully unexposed (n = 2,197), whereas those whose children were all exposed, including the oldest child, were classified as fully exposed (n = 1,446). Respondents with only one child were excluded from these robustness analyses because partial exposure is only defined among respondents with multiple children (n = 803). We then estimated models comparing fully exposed respondents with fully unexposed respondents and, separately, partially exposed respondents with fully unexposed respondents to evaluate whether partial exposure among younger siblings may have weakened the exposure contrast in the main specification.

Finally, our dataset does not allow for a placebo test using fictitious treatment cohorts because it lacks sufficient coverage of respondents completely unaffected by the reform (via their adult children). However, we conducted a placebo test using similar birth cohorts based on an external dataset from a country where no comparable higher education reform occurred**.** Specifically, we use data from the U.S. Health and Retirement Study (HRS) to examine whether a similar relationship between offspring education and parental mortality is observed in a context where no broad educational expansion policy was implemented during the same period. If we

find no significant relationship in the U.S. setting, this would further support the validity of our instrument by suggesting that the observed effect in South Korea is driven by the 1993 higher education reform rather than unmeasured trends in aging populations (e.g., inherent age-related variation in treatment groups and mortality). Additionally, we aimed to implement this test in countries more comparable to South Korea (e.g., Japan, Taiwan). However, longitudinal surveys with nationally representative samples that track deaths over our study window were not available or lack consistent mortality follow-up, precluding comparable analyses.

5. Results

5.1.Descriptive Statistics

Descriptive statistics are summarized in **Table 1**; 32% of the sample is in the treatment group (younger cohort) and 68% in the control group (older cohort). The respondents in the sample have an average age of 62 years; respondents in the treatment group are notably younger, with an average age of 54 years (SD: 4.7), compared to 66 years (SD: 6.6) in the control group. The treatment group also has higher proportions of married or partnered individuals (92% vs. 77%), more college graduates (12% vs. 6%), and more urban residents (82% vs. 71%). Additionally, the treatment group reports higher average household incomes (1.4 vs. 0.9 million KRW). Household composition differs as well: the treatment group has larger households on average (3.2 vs. 2.7 residents) but fewer children (2.3 vs. 3.7 children) compared to the control group.

5.2.Reduced-Form Associations between Reform Exposure and Parental Mortality

We first present reduced-form evidence on whether exposure to Korea's 1993 Higher Education Reform is associated with parental mortality. **Figure 2** visualizes cohort-specific hazard ratios from a Cox model that includes baseline covariates and single-year indicators for the oldest

child's birth year. The pattern shows a downward shift in parental mortality risk beginning around the 1974 cutoff, with cohorts born after 1974 generally exhibiting lower hazards than adjacent pre-1974 cohorts. The same pattern is also present in stratified figures by parent and child gender. In particular, the decline appears for both fathers and mothers, although the post-1974 reduction is more pronounced and sustained among fathers. A similar pattern is observed when stratifying by the gender of the oldest child: mortality hazards decline after the cutoff in both groups, with clearer separation and a more sustained drop among parents of daughters than among parents of sons. Consistent with these patterns, a reduced-form Cox model using a single reform-cohort indicator (1974–1983 vs. 1964–1973) indicates roughly a 27 percent reduction in parental mortality (Hazard Ratio [HR]: 0.726, 95% Confidence Interval [CI]: 0.559, 0.944; see **Table 2** for reduced-form estimates). The association attenuates with covariate adjustment, from a 71% reduction in the unadjusted model (HR: 0.291, 95% CI: 0.243, 0.349) to a 40% reduction after adjustment for respondent age alone (HR: 0.603, 95% CI: 0.477, 0.764), suggesting that age composition explains an important share of the crude difference. Further adjustment for demographic and family background characteristics yields a 29% reduction (HR: 0.706, 95% CI: 0.543, 0.918), and the fully adjusted model indicates a 27% reduction (HR: 0.726, 95% CI: 0.559, 0.944).

Additionally, the Kaplan-Meier survival curves presented in **Figure 3** illustrate mortality differences by treatment status. In the unadjusted estimates, parents of children in the reform cohort consistently exhibit higher survival probabilities than those in the control group, with survival rates of approximately 90% versus 70% by the end of the analysis period. After covariate adjustment, the survival gap between the two groups narrows but remains evident, suggesting that while some differences reflect pre-existing variation (e.g., in respondents' own

socio-economic status), exposure to the reform may still have an independent positive effect on parental survival.

5.3.Impact of Child's College Education on Parental Mortality

The first-stage results demonstrate the predictive strength of the 1993 education reform as an instrumental variable in determining adult children's college completion (**Table 3**). The results indicate that exposure to the reform increases the likelihood of college completion among KLoSA respondents' oldest children by 12.6 percentage points (pp) ($\beta = 0.126$, 95% CI: 0.095, 0.157). The instrument's strength is further confirmed by a high partial F-statistic of 62, which supports the reform's relevance and reliability in this analysis.

When disaggregating the data by the gender of the child, the education impact of the reform remains significant but varies in magnitude. For sons, exposure to the reform increases the probability of college completion by 8pp ($\beta = 0.085$, 95% CI: 0.041, 0.128), while for daughters, the effect is stronger, with a 17pp increase ($\beta = 0.171$, 95% CI: 0.128, 0.215).

Before turning to IV estimates, we note that the observational Cox model shows a strong association between oldest-child college completion and lower parental mortality (HR = 0.785, 95% CI: 0.687, 0.897), implying about a 21 percent lower mortality hazard for parents of college-educated oldest children (**eTable 5**). Our preferred two-stage residual inclusion (2SRI) Cox model, which instruments college completion with reform exposure, yields a similar estimate: parents whose oldest child completed college experience roughly a 20 percent lower mortality hazard than those whose oldest child did not complete college (HR = 0.802, 95% CI: 0.700, 0.919) (**Table 4**), suggesting that residual selection bias may be limited in this setting.

5.4.Heterogeneity Analysis

We first examined heterogeneity in the reduced-form association between reform exposure and parental mortality (**Table 5**). The reduced-form estimates show protective effects across all subgroups, with some variation in magnitude. For fathers, oldest-child reform exposure is associated with a 30% reduction in parental mortality risk (HR = 0.701, 95% CI: 0.540, 0.909), compared with a 24% reduction for mothers (HR = 0.756, 95% CI: 0.622, 0.918). Similarly, the estimated reduction is larger for daughters (HR = 0.701, 95% CI: 0.557, 0.882) than for sons (HR = 0.773, 95% CI: 0.638, 0.937). Protective associations are also observed across socioeconomic subgroups, with stronger estimates for urban parents (HR = 0.679, 95% CI: 0.527, 0.874) than for rural parents (HR = 0.799, 95% CI: 0.681, 0.937), and for parents with at least a high school education (HR = 0.690, 95% CI: 0.556, 0.856) than for those with less than high school education (HR = 0.782, 95% CI: 0.645, 0.948). By contrast, the estimated reductions are very similar across household asset groups (HR = 0.748, 95% CI: 0.608, 0.920 for the below-median asset group; HR = 0.752, 95% CI: 0.582, 0.971 for the above-median asset group).

Formal interaction tests for the reduced-form models provide some evidence of heterogeneity across subgroups (**eTable 7**). The interaction terms indicate significantly stronger protective effects for daughters than sons, for urban than rural parents, and for parents with at least a high school education than for those with less schooling. However, there is no evidence of heterogeneity by parent gender or household assets.

We next examined heterogeneity in the 2SRI estimates (**Figure 4**; see **eTable 5** and **eTable 6** for detailed estimates). Overall, the 2SRI estimates are broadly similar to the reduced-form results in both direction and general magnitude. However, the evidence for heterogeneity is

weaker, as formal interaction tests do not detect statistically significant heterogeneity across subgroups.

For fathers, having the oldest child complete college reduces the mortality hazard by 23% (HR = 0.772, 95% CI: 0.643, 0.926), compared with a 16% reduction for mothers (HR = 0.838, 95% CI: 0.720, 0.976). Likewise, college completion among oldest daughters corresponds to a 29% reduction in parental mortality risk (HR = 0.714, 95% CI: 0.577, 0.882), compared with a 19% reduction for oldest sons (HR = 0.809, 95% CI: 0.663, 0.987). In both cases, the 95% confidence intervals overlap substantially, and the formal interaction tests do not indicate heterogeneity by parent or child gender (eTable 7), although modest differences cannot be ruled out.

A similar pattern is observed across socioeconomic subgroups. For parents living in rural areas, a child's college completion reduces the mortality hazard by 19% (HR = 0.805, 95% CI: 0.684, 0.949), compared with a 24% reduction for urban parents (HR = 0.763, 95% CI: 0.590, 0.985). The estimated reductions are very similar across household asset groups, at 21% for parents in the below-median asset group (HR = 0.789, 95% CI: 0.652, 0.954) and 20% for those in the above-median asset group (HR = 0.803, 95% CI: 0.669, 0.964). By parental education, the estimated reduction is 16% among parents with less than high school education (HR = 0.841, 95% CI: 0.711, 0.995) and 21% among those with high school education or higher (HR = 0.794, 95% CI: 0.671, 0.939). However, formal interaction tests for the 2SRI models do not detect statistically significant heterogeneity across these socioeconomic subgroups (**eTable 7**).

5.5.Examining Potential Mechanisms

Children's college completion is estimated to reduce the likelihood of parental smoking by 4pp, with stronger effects observed for fathers (7pp reduction for fathers; 2pp reduction for mothers), increase the probability of engaging in regular exercise (more than once a week) by 12pp, and produce no significant change in drinking habits (**Figure 5**; see **eTable 8**, **eTable 9** for detailed estimates).

Additionally, we find that adult children's college completion increases in-person meetings between the oldest child and older parents by 4pp, increases contact frequency by 5pp, and increases financial support from adult children to parents by 7pp, with mothers benefiting slightly more (**Figure 6**; see **eTable 10**, **eTable 11** for detailed estimates).

After adjusting for these intermediate outcomes, the estimated mortality reduction attenuates only slightly, from about 20% reduction (HR = 0.802, 95% CI: 0.700, 0.919) to about 18% reduction (HR = 0.824, 95% CI: 0.717, 0.947), with similarly modest attenuation by parent and child gender (**eTable 12**, **eTable 13**).

5.6.Robustness Tests

Alternative Instrument Specifications. When examining the impact of children's college completion on parental outcomes, both the oldest child and the highest-educated child show similarly strong effects in reducing parental mortality (**eTable 14**, **eTable 15**). Additionally, the reduction in parental mortality remains significant when using the proportion of all adult children who completed college as the main regressor (**eTable 16**).

Testing Birth Cohort Effects. In models stratified by birth cohort, hazard ratios for parental mortality show an approximately 17–22% reduction across all groups, including those born during the Japanese colonization period (1914–1939), the Korean War/Industrialization period

(1940–1954) (**eTable 17**). This suggests a significant reduction in mortality risk regardless of the historical context. Furthermore, introducing cohort fixed effects yields nearly identical results, indicating that unobserved cohort-specific factors do not substantially influence this relationship (**eTable 18**).

Testing Different Cohort Boundaries. When testing different cohort boundaries for children, the results remain robust across 3-year, 5-year, 10-year, and 15-year intervals. Exposure to the 1993 education reform consistently shows a significant positive effect on college completion rates across all specifications, with the strongest effects observed using the 5-year birth cohort intervals (**eTable 19**).

Testing Alternative Household Exposure Classifications. As an additional robustness check, we examined whether the main estimates were sensitive to households in which the oldest child was unexposed but one or more younger siblings were reform-exposed (**eTable 20, eTable 21**). These analyses were restricted to respondents with multiple children, because respondents with only one child cannot be partially exposed by construction (n = 803 excluded). Compared with the main specification, which estimated a 20% reduction in parental mortality, the estimated reduction was larger when the comparison was restricted to fully exposed versus fully unexposed respondents (26%) and when partially exposed respondents were compared with fully unexposed respondents (27%). However, in both cases, the confidence intervals overlapped with that of the main specification, suggesting limited evidence of a meaningful difference in magnitude. Overall, these findings suggest that the main specification may be somewhat attenuated when

respondents with younger-sibling exposure are included in the comparison group, although part of the difference may also reflect the restriction to respondents with multiple children.

Impact on other age-related outcomes. We find no significant effects of offspring education on the incidence or prevalence of nine different age-related chronic health diagnoses (e.g., hypertension, diabetes) (**eTable 22**). Point estimates are close to zero across outcomes, except for small, marginal effects on psychiatric problems, which are less straightforward to interpret as age-related given that mental health trajectories are more variable across the life course. This pattern suggests that the survival gains from children's higher education may emerge mainly in parents' later life, potentially through improved disease management (e.g., better health information, care coordination, and medication adherence) that reduce case-fatality rather than via prevention of the onset of chronic disease. Furthermore, it makes cohort or period confounding less plausible: secular improvements in medical technology, diagnostic practices, or population health would be expected to reduce both chronic-condition incidence and diagnosed prevalence over the same period, which we do not observe. Moreover, if we expected the impact of adult child college completion on parental mortality to be driven solely by age imbalances in treatment and control groups, we would expect similar effects across the full range of age-related disease outcomes. These results reduce our concerns that our primary estimates of interest simply reflect parental aging.

Placebo tests. The placebo test results indicate that belonging to a birth cohort comparable to the treatment group in our analysis – but in the U.S. context, where no similar reform occurred – has no measurable impact on parental mortality. The first-stage regression shows a very weak

association between adult child birth cohort membership and college completion rate (β = -0.031, 95% CI: -0.382, 0.319, F = 0.03), as expected due to the absence of a similar reform in the U.S. Likewise, the second-stage results indicate no impact of increased adult child college completion on parental mortality (HR = 1.021, 95% CI: 0.775, 1.343, F = 0.03) (**eTable 23**).

Robustness across Model Specifications. Our preferred 2SRI specification with a second-stage Cox model yields a hazard ratio of 0.802 (95% CI: 0.700, 0.919), indicating roughly a 20 percent lower mortality hazard among parents whose adult child completed college. Consistent with this, the conventional 2SLS model using survival status by 2020 estimates a 5.5pp reduction in mortality by 2020 (β = -0.055, 95% CI: -0.077, -0.033), which is equivalent to about a 23% proportional reduction relative to the 23.7% mortality rate in the reform-exposed group. Both estimates are statistically significant and directionally consistent, reinforcing the robustness of the main result across relative and absolute outcome scales, with small differences plausibly attributable to the outcome definition and the samples used (**eTable 24**).

6. Discussion

Leveraging a population-based cohort study from South Korea and a 1993 higher education reform with dramatic effects on college entry and completion as a quasi-experiment, we assessed the impact of adult children's college attainment on parental survival. Our findings indicate a significant reduction in parental mortality risk due to children's education. We also explored the possibility of heterogenous treatment effects, but confidence intervals are too wide for conclusive findings on heterogeneity. Supplementary analyses suggest effects on parental health behaviors – particularly among fathers – as well as some aspects of intergenerational contact and support, but

no effects on doctor-diagnosed chronic conditions. However, the estimated effect of children's college completion on parental mortality remains strong, even after crudely controlling for these (endogenous) potential mechanisms.

Our research contributes to the growing body of evidence showing that increases in children's education have beneficial impacts on parental mortality. While direct comparisons with previous studies are difficult due to methodological differences, our results align well with existing observational studies in countries like the U.S., Sweden, Europe, and Mexico (Friedman & Mare, 2014; Torssander, 2014; Sabater et al., 2020; Yahirun et al., 2017). These studies found that parents benefit substantially when their children achieve higher education, with lower mortality rates reported across these populations. By using a quasi-experimental design, we also expand on previous studies from diverse contexts, including Tanzania, Sweden, and China (De Neve & Fink, 2018; Lundborg & Majlesi, 2018; Cui et al., 2021) that evaluated increases in schooling at the lower end of attainment (primary/secondary) induced by national reforms, such as compulsory-schooling laws.

Altruistic intergenerational transfer models predict that upward support is more likely to flow toward parents with fewer resources, particularly where public safety nets are limited (Barro & Becker, 1989; Cigno, 1993). These considerations motivate potential heterogeneity by parent gender in South Korea, where mothers have historically had lower socioeconomic resources on average (Stansbury et al., 2024). Existing quasi-experimental evidence also points to cross-context variation, with larger estimated benefits for mothers in Tanzania (De Neve & Fink, 2018) and for fathers in Sweden and China (Lundborg & Majlesi, 2018; Cui et al., 2021). In our data, however, we do not find statistically significant differences by parent gender, though modest differences remain possible given the wide confidence intervals.

Additionally, we find no strong evidence to support altruistic intergenerational transfer models across other domains. The effect of children's education on parental survival appears more pronounced for parents in urban areas or those with higher education, contrary to the predictions of such models, although the 95% confidence intervals overlap and formal interaction tests for the 2SRI models do not provide conclusive evidence of heterogeneity. A plausible explanation for larger effects for urban parents is selective migration: highly educated children often move to cities for work, weakening day-to-day ties to rural parents (e.g., emotional support and frequent contact) in ways our data may not capture. Higher-educated parents may also be better positioned to translate children's schooling into survival gains through greater health literacy, care access, and financial slack that amplify information and support from educated children.

Our findings are suggestive of larger mortality reductions associated with daughters' educational achievements than sons'. However, the confidence intervals overlap substantially in both the reduced-form and 2SRI estimates (e.g., about 68% of the sons' CI and 72% of the daughters' CI in the 2SRI estimates), while formal interaction tests provide mixed evidence: the reduced-form interaction suggests a significantly weaker effect for sons relative to daughters, but the corresponding 2SRI interaction estimate, while directionally similar, is not statistically significant. These results suggest that any differences across child gender in downstream mortality effects are likely more modest than the first-stage and reduced-form contrasts alone might imply. This pattern may also partly reflect limited precision, especially given the weaker first stage for sons, which makes the IV estimates for sons less precise. This contrasts with recent findings from Mexico (Gutierrez et al., 2024) where sons' education had a greater impact on improving parents' psychosocial well-being. Differences across settings may reflect variation in gender norms and family roles in later-life support, including which children provide caregiving and health-related

assistance when parents experience health shocks. Although son preference historically shaped intergenerational expectations in Korea, daughters' role in supporting older parents may have become more prominent as daughters' socioeconomic positions have improved and social norms have shifted. Prior work also documents that daughters often respond more to parents' health needs than sons (Bott et al., 2017; Haberkern et al., 2015; Zeng et al., 2016), suggesting a plausible channel for cross-context differences. Future work with more precise estimates and direct measures of caregiving and transfers will be important for clarifying whether and why intergenerational effects vary by child gender.

Our results also suggest that adult child college completion influences some, but not all, of the potential mechanisms that we explored for how children's education benefits parental mortality. Specifically, parents whose adult child completed college are less likely to smoke and more likely to engage in regular exercise, though no significant changes were observed in drinking habits. While adult child college education also increases the frequency of parent-child contact and parental receipt of material support, we find only limited evidence that these channels explain the mortality reduction. That is, even after crudely accounting for these potential mechanisms in our model, the estimated impact of children's college completion remains robust, with only a slight attenuation. This aligns with Ma et al. (2021), who found no evidence that financial transfers mediated the relationship between adult child schooling and cognitive performance in the Mexican context.

This limited attenuation suggests that important mechanisms may remain unmeasured. One possibility is that the benefits of children's education depend less on the quantity of support than on its quality and on broader resources not captured in our data. While our measures record contact and material assistance, they do not reflect how timely, effective, or consequential that support is

for healthcare management. A college-educated child may provide more targeted advice, recognize warning signs, communicate more effectively with clinicians, and help parents navigate providers, referrals, and follow-up care. These informational and organizational forms of support may be especially relevant for mortality, even if they are not captured well by simple measures of parent–child contact. Children's education may also improve parents' economic stability in ways not captured by direct transfers, for example by reducing financial strain or helping families absorb medical and caregiving shocks.

It is possible that parental behavioral changes may extend beyond the relatively coarse indicators available in our data. Although we observe reduced smoking and more frequent exercise, college-educated children may also influence other health-related behaviors that are difficult to measure well, such as medication adherence, follow-up after diagnosis, and the timing of care-seeking when symptoms emerge. They may also reinforce disease management routines on an ongoing basis, helping parents sustain healthier behaviors rather than merely initiate them. Such unmeasured changes in everyday health management could plausibly accumulate over time and contribute meaningfully to lower mortality risk. However, evaluating this mechanism requires further work, likely relying on other data sources (e.g., medical claims or health records).

This study has several limitations. First, we conducted an instrumental variable analysis within the framework of a natural experiment, specifically focusing on the 1993 higher education reform in South Korea. Consequently, our findings are specific to the local average treatment effect for families whose children's college completion was shifted by the reform. This limits the broader applicability of our results, including to those whose children would have completed college regardless of the presence of the reform, those in other birth cohorts, or other geographic regions.

Second, significant differences in parental characteristics before the college expansion – such as education levels, rural versus urban residence, and family size – were likely shaped by broader socioeconomic shifts, including rapid industrialization. These factors may have influenced children's college completion rates, raising the possibility that the observed effects were not solely due to the education reform. To account for these factors, we adjusted for pre-expansion covariates and conducted multiple sensitivity tests. Our findings remained robust across birth cohorts and cohort boundaries, suggesting that unobserved generational factors or cohort selection effects do not drive the results. However, the possibility remains that unmeasured factors, such as broader social or economic changes coinciding with the reform and not directly observed, may have contributed to the observed effects. While our robustness checks help mitigate these concerns, fully disentangling the impact of educational expansion from other concurrent shifts remains a challenge.

Third, because KLoSA records only children's highest completed degree, we cannot distinguish children who enrolled in college but did not complete it from those who never enrolled. Although this group was likely small, given an estimated enrollment–completion gap of only about 1–2 percentage points during this period (Department of Education, 1997), its effects on parents may have differed from those of children who completed college in either direction: partial college may have imposed educational costs without the full return associated with graduation, but it may also have provided some skills or labor-market advantages relative to high school completion alone. This limitation introduces some ambiguity but is unlikely to drive the main findings given the small enrollment-completion gap.

Fourth, the rapid expansion of higher education in South Korea may itself have changed the returns to college for the reform-exposed cohorts. Some children induced into college by the

reform may have entered a more crowded or less stable labor market than earlier cohorts, so the payoff to college may have differed across cohorts (Choi 2015; Choi 2005). This possibility suggests that the effects of children's college completion on parents may be heterogeneous across cohorts and may not fully generalize to other periods or settings in which the returns to college differ.

Lastly, the rapid expansion of higher education in South Korea raises concerns about potential disparities in the quality of education provided by newly established institutions compared to older, more established ones. While some empirical data suggest that new institutions may not lag significantly behind established ones, there may be other quality aspects that are harder to quantify for higher education. Future studies should explore the potential quality disparities among educational institutions established during the rapid expansion and assess their long-term impact on the outcomes of both students and their families.

7. Conclusion

Our study highlights the intergenerational benefits of higher education, showing that children's college attainment can positively affect parental longevity. Leveraging South Korea's rapid educational expansion in the 1990s, we provide robust evidence that improved access to higher education contributes to better health outcomes across generations. These findings have significant implications for policies aimed at expanding educational access, particularly in societies where higher education plays a key role in social mobility. As more individuals gain access to college, ensuring that underprivileged families can also benefit from these health advantages is essential. By demonstrating how education influences family well-being beyond individual returns, this study reinforces the broader societal value of investing in higher

education. Future research should explore whether similar effects are observed in other countries with varying education policies and economic conditions.

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Table 1. Descriptive statistics by treatment status

	Index child treatment status (oldest child)			
	Control Group	Treatment Group	Total	Test
N	4,432 (67.8%)	2,101 (32.2%)	6,533 (100.0%)	
Age (in years)	66.1 (6.6)	53.9 (4.7)	62.2 (8.3)	<0.001
Gender				
Female	2,613 (59.0%)	1,109 (52.8%)	3,722 (57.0%)	<0.001
Marital Status				
Separated/divorced/widowed/single	1,017 (22.9%)	176 (8.4%)	1,193 (18.3%)	<0.001
Married/Partnered	3,415 (77.1%)	1,925 (91.6%)	5,340 (81.7%)	
Education				
1.No education(illiterate)	374 (8.4%)	14 (0.7%)	388 (5.9%)	<0.001
2.No education(reading)	654 (14.8%)	40 (1.9%)	694 (10.6%)	
3.Elementary school	1,679 (37.9%)	419 (19.9%)	2,098 (32.1%)	
4.Middle school	748 (16.9%)	511 (24.3%)	1,259 (19.3%)	
5.High school	692 (15.6%)	839 (39.9%)	1,531 (23.4%)	
6.Two-year college	31 (0.7%)	31 (1.5%)	62 (0.9%)	
7.College grad	237 (5.3%)	218 (10.4%)	455 (7.0%)	
8.Post college (Master)	14 (0.3%)	24 (1.1%)	38 (0.6%)	
9.Post college (PhD)	3 (0.1%)	5 (0.2%)	8 (0.1%)	
Urban Locality				
Urban	3,151 (71.1%)	1,717 (81.7%)	4,868 (74.5%)	<0.001
Rural	1,281 (28.9%)	384 (18.3%)	1,665 (25.5%)	
Household Income (unit: 10 million KRW)	0.1 (0.2)	0.1 (0.2)	0.1 (0.2)	<0.001
Number of people in household	2.7 (1.3)	3.2 (1.2)	2.9 (1.3)	<0.001
Number of living children	3.7 (1.4)	2.3 (0.8)	3.2 (1.4)	<0.001
Event (respondent death)	29.7%	11.1%	23.7%	<0.001

Notes: 1) Models include respondents whose adult children (aged 25 or above) were reported to be in either the reform-affected or prior birth cohorts. Treatment group = respondents whose oldest child falls in the first 10 cohorts affected by the 1993 higher education reform (birth years 1974–1983); Control group = preceding 10 cohorts (1964–1973). Korean Longitudinal Study of Aging (KLOSA), 2006–2020 v2. 2) Between-group comparisons indicate differences for all variables ($p < 0.001$). Continuous variables tested with t-tests; categorical variables with χ^2 tests.

Table 2. Hazard ratios and 95% confidence intervals from reduced-form Cox proportional hazards models: Association between oldest child's reform exposure and respondent mortality

	M1. Unadjusted		M2. Adjusted (respondent age only)		M3. Adjusted (respondent age + demographic and family background characteristics)		M4. Adjusted (full covariates)	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.291*** (0.027)	(0.243 0.349)	0.603*** (0.073)	(0.477 0.764)	0.706*** (0.095)	(0.543 0.918)	0.726** (0.097)	(0.559 0.944)
Observations	5,880		5,880		5,880		5,880	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Model 1 is unadjusted; Model 2 adjusts for respondent age; Model 3 adjusts for respondent age plus the key demographic and family background characteristics, including respondent gender, educational attainment, parental education, and number of living children; and Model 4 further controls for marital status, urbanicity, household income, household assets, children's gender composition, whether the respondent's father or mother is alive, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. Statistical significance: *** p<0.001, ** p<0.01, * p<0.05.

Table 3. First-stage estimates from logistic regression models: Association between exposure to the 1993 higher education reform and oldest child's college completion, overall and by child gender

	Oldest Child			
	All	**Sons**	**Daughters**	**Interaction (Daughters – Sons)**
Exposure to 1993 education reform	0.126***	0.085***	0.171***	0.075***
	(0.095 0.157)	(0.041 0.128)	(0.128 0.215)	(0.027 0.122)
Observations	6,525	3,443	3,082	6,525
F	62	14	57	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) First-stage models are estimated using logistic regression; coefficients are reported as average marginal effects (AMEs). 3) The interaction column reports the difference in AMEs between daughters and sons from a pooled model including a treatment-by-child-gender interaction. 4) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 5) Robust standard errors clustered at the household level. 5) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 6) Kleibergen-Paap Wald F-tests are used for F-statistics. *** p<0.001, ** p<0.01, * p<0.05.

Table 4. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards model with an instrumental variable: Impact of oldest child's college completion on respondent mortality

	HR	95% CI
College completion	0.802**	(0.700 0.919)
	(0.071)	
Respondent characteristics		
Age	1.221**	(1.057 1.411)
	(0.090)	
Age squared	0.999	(0.982 1.033)
	(0.001)	
Age at first child birth	1.007	(0.982 1.033)
	(0.013)	
Female	0.514***	(0.441 0.600)
	(0.040)	
Years of Education	0.974**	(0.957 0.990)
	(0.008)	
Married or partnered	0.792**	(0.670 0.937)
	(0.068)	
Rural residence	0.846**	(0.726 0.986)
	(0.066)	
Household income (standardized)	0.963	(0.865 1.072)
	(0.053)	
Household assets (standardized)	1.005	(0.907 1.113)
	(0.052)	
Number of household members	0.997	(0.951 1.045)
	(0.024)	
Number of living children	0.993	(0.944 1.045)
	(0.026)	
Proportion of sons	0.999	(0.997 1.001)
	(0.001)	
Respondent's mother alive	0.941	(0.768 1.152)
	(0.097)	
Respondent's mother education	0.935	(0.851 1.027)
	(0.045)	
Regional fixed effects	Included	
Observations	5,873	
Kleibergen-Paap Wald F-statistic	62	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Robust standard errors are clustered at the household level. For 2SRI estimates, 95% confidence intervals are based on a bootstrap with 1,000 replications. 3) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. Statistical significance: *** p<0.001, ** p<0.01, * p<0.05.

Table 5. Hazard ratios and 95% confidence intervals from reduced-form Cox proportional hazards models, stratified by parent gender, child gender, and parental socioeconomic status: Association between oldest child's reform exposure and respondent mortality

Subsample	Fathers		Mothers	
	HR	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.701** (0.093)	(0.540 0.909)	0.756** (0.075)	(0.622 0.918)
Observations	2,523		3,357	
Subsample	**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.773** (0.076)	(0.638 0.937)	0.701** (0.082)	(0.557 0.882)
Observations	3,093		2,787	
Subsample	**Rural**		**Urban**	
	HR	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.799** (0.065)	(0.681 0.937)	0.679** (0.088)	(0.527 0.874)
Observations	1,595		4,285	
Subsample	**Below median**		**Above median**	
	HR	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.748** (0.079)	(0.608 0.920)	0.752* (0.098)	(0.582 0.971)
Observations	3,691		2,189	
Subsample	**Less than high school education**		**High school or above**	
	HR	**95% CI**	**HR**	**95% CI**
Oldest child's exposure to the reform (1=Children born 1974–1983, 0=Children born 1964–1973)	0.782** (0.077)	(0.645 0.948)	0.690*** (0.076)	(0.556 0.856)
Observations	1,872		4,008	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Adjusted models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental education, sample cohorts, and regional fixed effects. In stratified analyses, the stratifying variable (parent gender, oldest child gender, urbanicity, household assets, or education) was not additionally adjusted for. 3) Robust standard errors clustered at the household level. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. Statistical significance: *** p<0.001, ** p<0.01, * p<0.05.

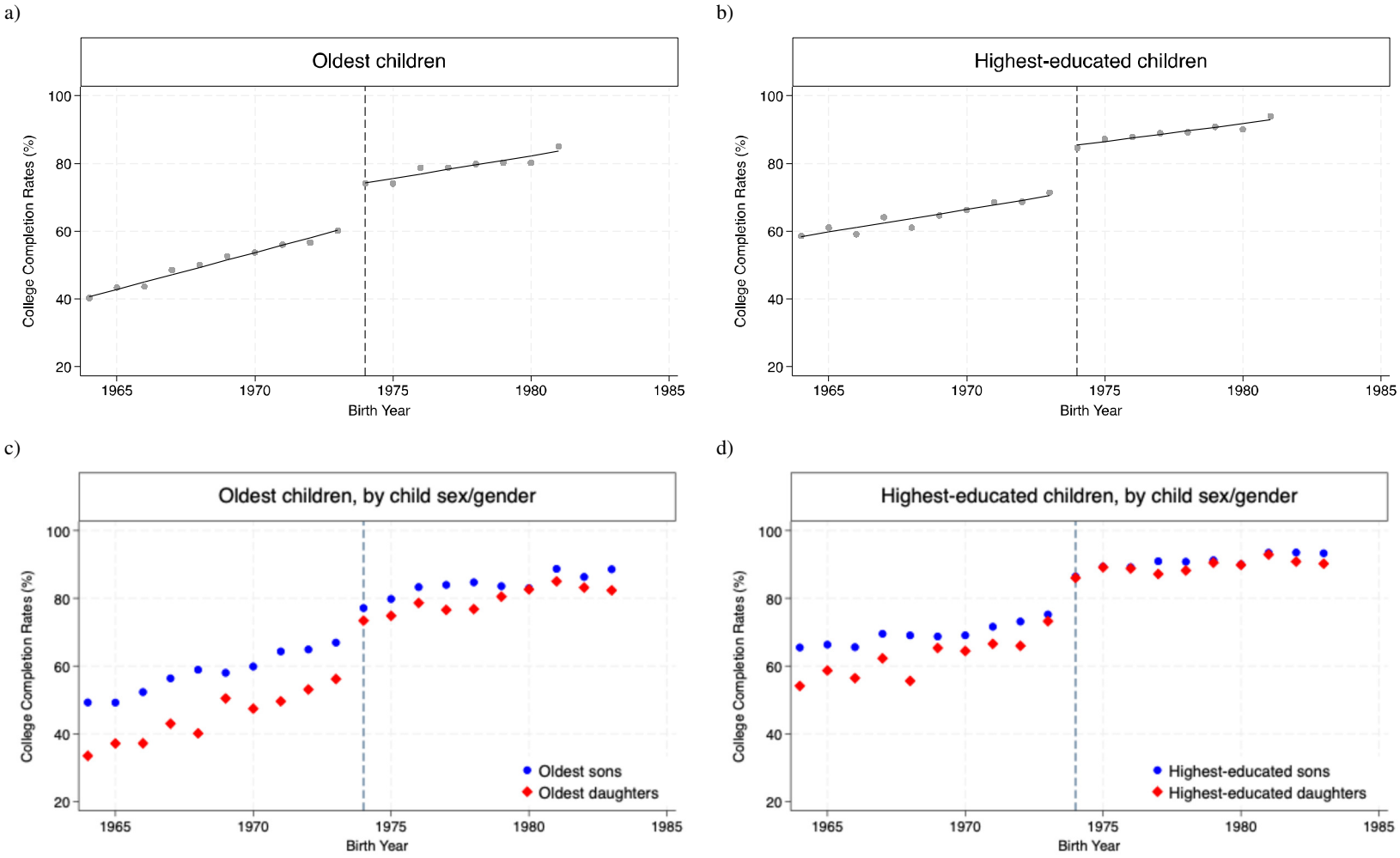


Figure 1. College completion rates by index child birth cohort: Overall and by child gender

Notes: The figures show college completion rates of the oldest and the highest-educated children on either side of the 1974 cutoff, the first cohort affected by the 1993 education reform. See **Table 3** for the first-stage estimates of the two-stage residual inclusion (2SRI) IV estimator, which predicts the probability of college completion for the index child based on their exposure to the 1993 reform.

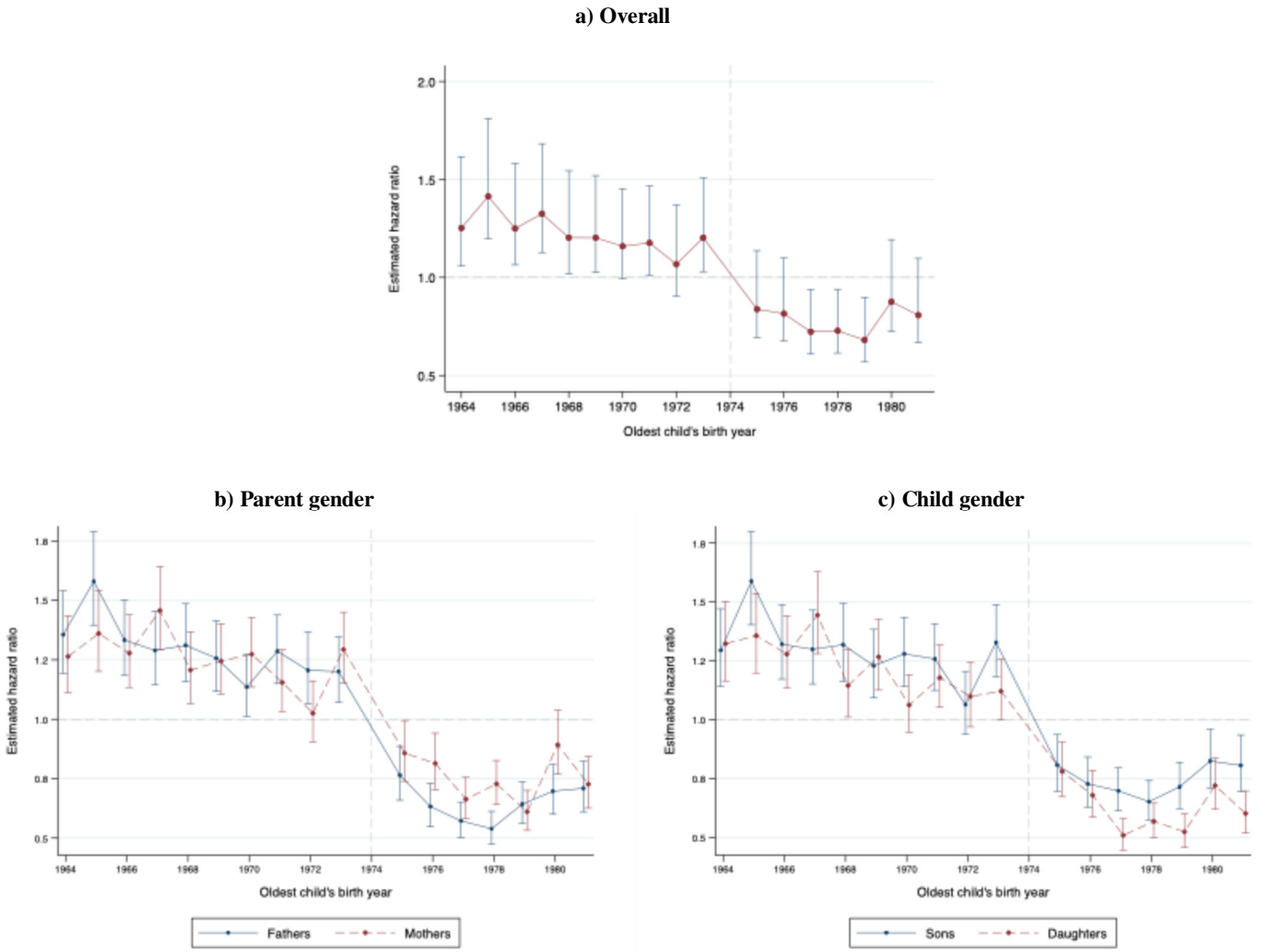


Figure 2. Respondent mortality by oldest child's birth year: Estimates from Cox proportional hazards model

Notes: 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. (2) Points plot estimated hazard ratios from Cox proportional hazards models of respondent mortality that include baseline covariates and a full set of single-year oldest-child birth-year indicators (reference year = 1974); vertical bars denote 95% confidence intervals. (3) The vertical dashed line marks the reform cutoff at oldest-child birth year 1974, and the horizontal dashed line indicates a hazard ratio of 1.0.

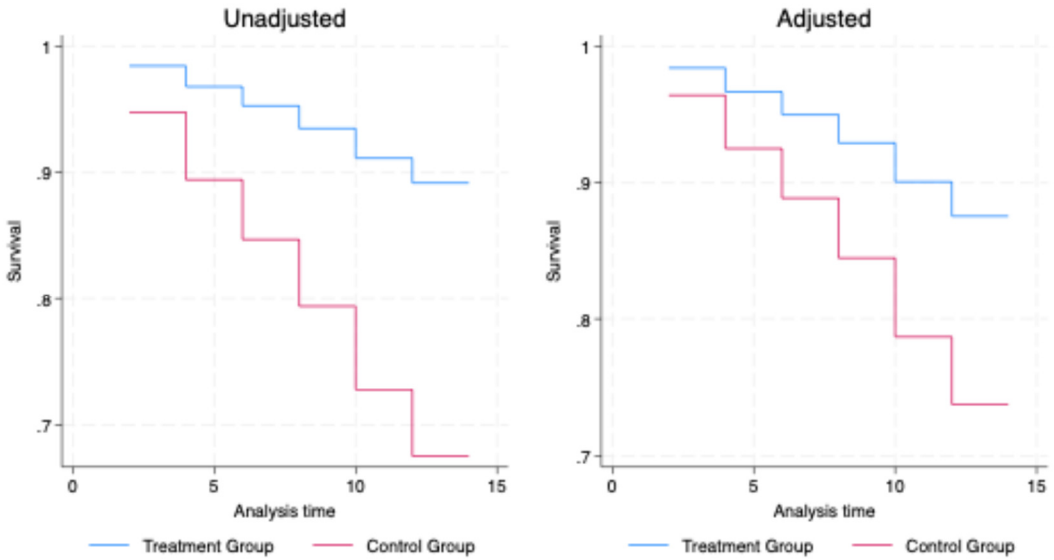


Figure 3. Kaplan-Meier survival curves by treatment status (exposure to the reform)

Notes: 1) The sample includes respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. Analysis time 0 begins with the KLoSA baseline wave in 2006 and continues through 2020. (2) The left panel plots unadjusted Kaplan-Meier survival functions by treatment status; the y-axis denotes the estimated survival probability and the x-axis denotes analysis time (years). (3) The right panel plots covariate-adjusted survival curves by treatment status, adjusting for baseline covariates.

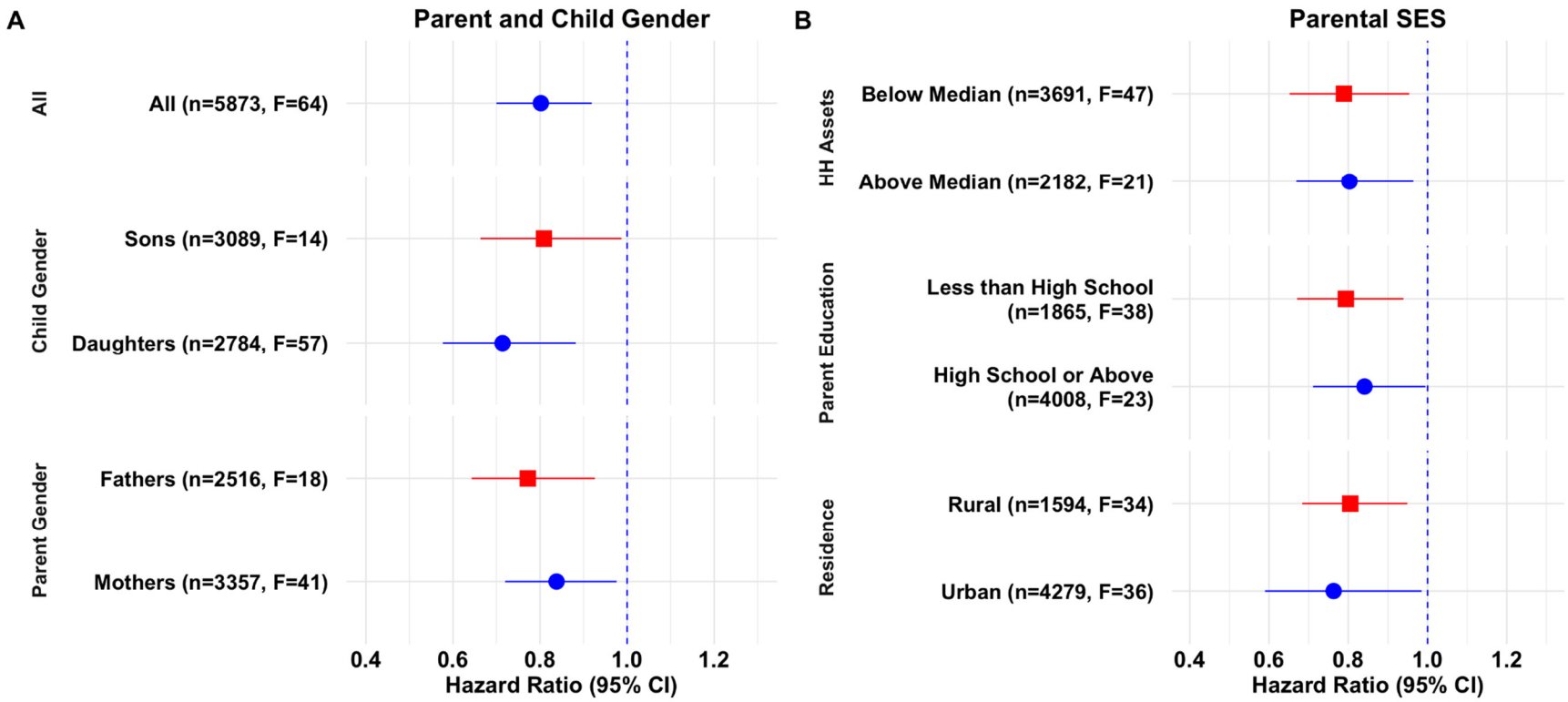


Figure 4. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards model with an instrumental variable: Impact of oldest child's college completion on parents' mortality, stratified by parent and child gender, and parental socioeconomic status

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors clustered at the household level. Confidence intervals are based on a bootstrap with 1,000 replications. 4) Refer to **eTable 5** and **eTable 6** for specific details of the estimates. 5) Kleibergen-Paap Wald F-tests are used for F-statistics. *** p<0.001, ** p<0.01, * p<0.05.

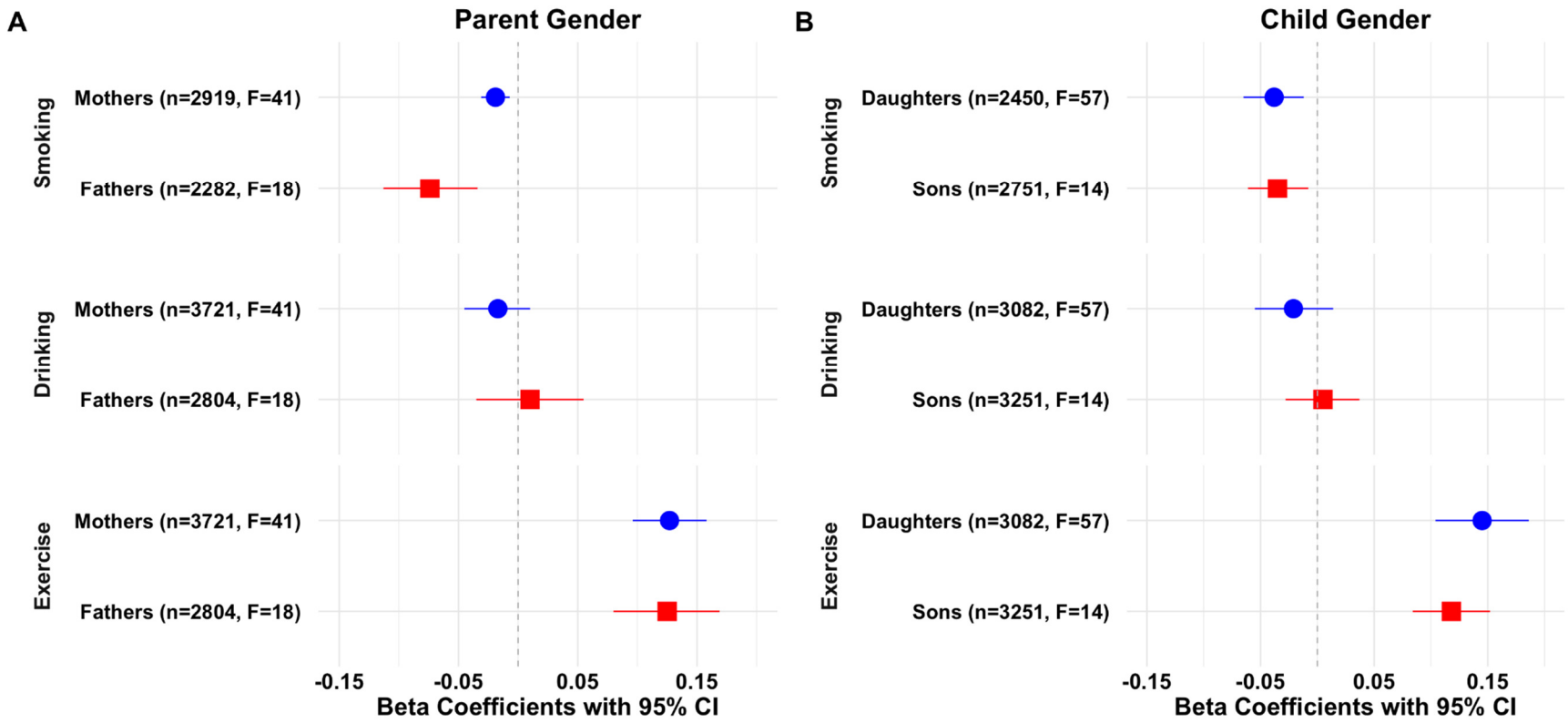


Figure 5. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression for predicting parents' health behaviors based on oldest child's education, by parent gender (panel A) and child gender (panel B)

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors clustered at the household level. Confidence intervals are based on a bootstrap with 1,000 replications. 4) Refer to **eTable 8** and **eTable 9** for specific details of the estimates. 5) Kleibergen-Paap Wald F-tests are used for F-statistics. *** p<0.001, ** p<0.01, * p<0.05.

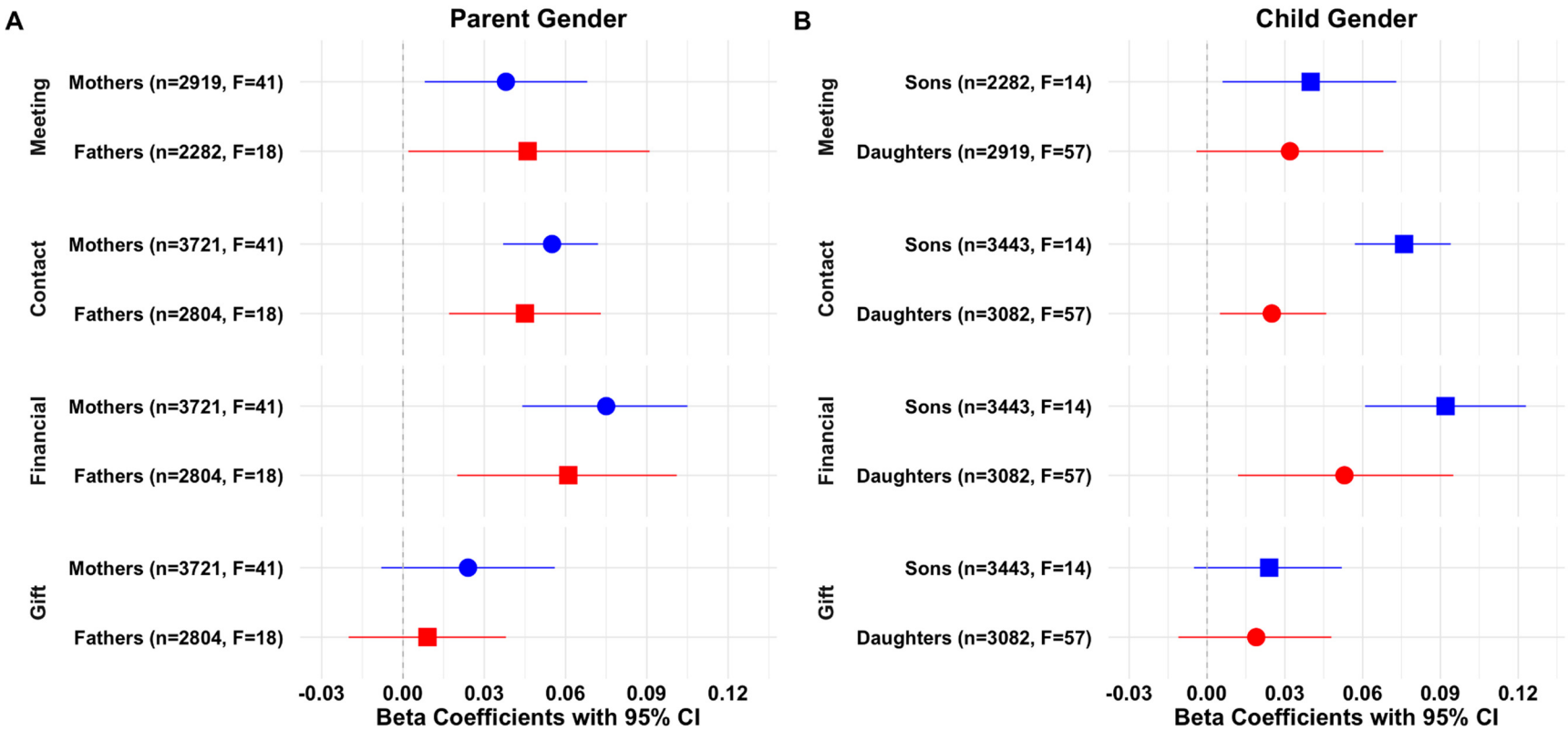


Figure 6. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression for predicting intergenerational relationships and support based on oldest child's education, by parent gender (panel A) and child gender (panel B)

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors clustered at the household level. Confidence intervals are based on a bootstrap with 1,000 replications. 4) Refer to **eTable 10** and **eTable 11** for specific details of the estimates. 5) Kleibergen-Paap Wald F-tests are used for F-statistics. *** p<0.001, ** p<0.01, * p<0.05

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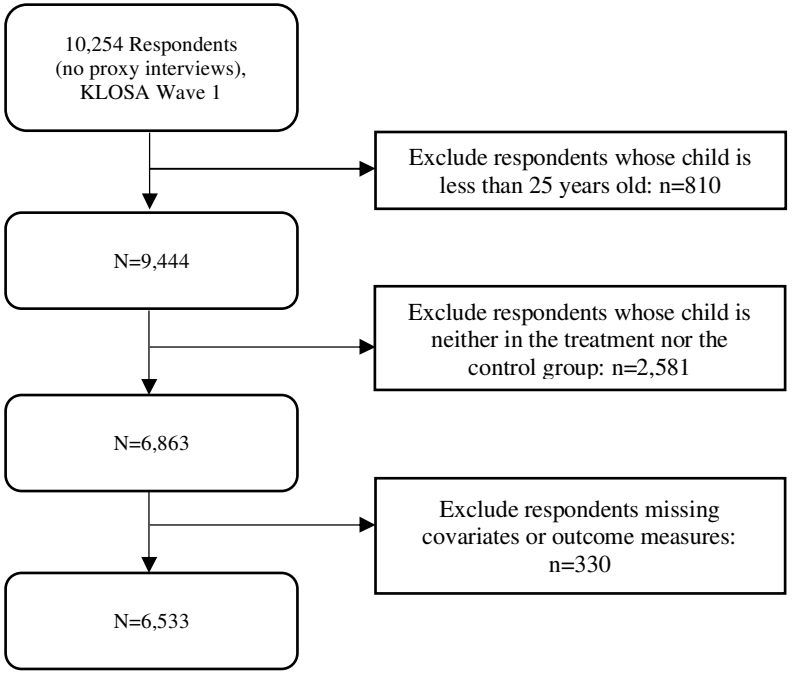
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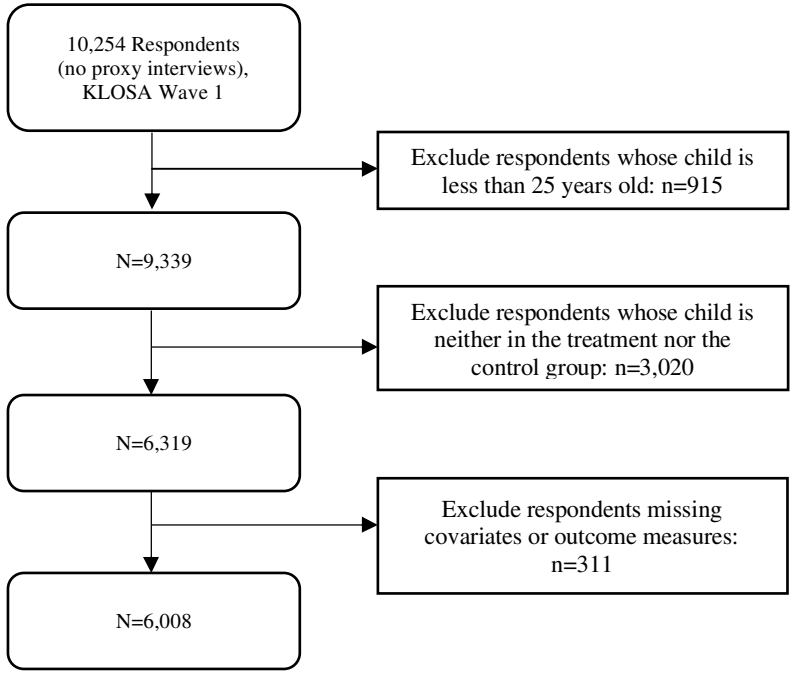
Supplementary Information

eFigure 1. Flowchart depicting analytic sample selection

a) Respondents with the oldest child



b) Respondents with the highest-educated child



eTable 1. Schoenfeld residual test for proportional hazards assumption

Overall	rho	chi2	Prob>chi2
Child college completion	0.02	0.75	0.39
Respondent education	0.01	0.22	0.64
Respondent age	0.07	10.41	0.13
Female	-0.03	2.05	0.15
Married	0.02	0.56	0.46
Rural residence	0.02	1.07	0.30
Household income	-0.04	2.36	0.12
Number of household residents	0.01	0.11	0.74
Regional dummy 1	0.03	1.22	0.27
Regional dummy 2	0.01	0.09	0.77
Regional dummy 3	0.01	0.11	0.74
Regional dummy 4	0.00	0.03	0.87
Regional dummy 5	0.04	1.83	0.18
Regional dummy 6	0.05	3.25	0.07
Regional dummy 7	-0.01	0.10	0.75
Regional dummy 8	-0.04	2.76	0.10
Regional dummy 9	-0.02	0.65	0.42
Regional dummy 10	0.04	1.75	0.19
Regional dummy 11	0.04	2.15	0.14
Regional dummy 12	0.03	1.04	0.31
Regional dummy 13	0.02	0.39	0.53
Regional dummy 14	0.02	0.64	0.42
Respondent mother alive	0.01	0.32	0.57
Respondent father alive	-0.03	1.38	0.24
Global test		**42.37**	**0.12**

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Schoenfeld residual tests assess the proportional hazards assumption for the Cox regression model. rho indicates the correlation between each covariate and the scaled Schoenfeld residual. Chi-squared statistics and associated p-values indicate whether covariates violate this assumption. Global test includes all covariates listed. *** p<0.001, ** p<0.01, * p<0.05.

eTable 2. Association between exposure to the 1993 higher education reform and other educational outcomes: estimates from logistic regression

	High School Completion	Graduate Education
Exposure to 1993 education reform	0.025	0.010
	(-0.014 0.064)	(-0.009 0.028)
Observations	2,787	6,533

***Notes*:** 1) Models include respondents' adult children (aged 25 or above) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models are estimated using logistic regression; coefficients are reported as average marginal effects (AMEs). 3) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental education, sample cohorts, and regional fixed effects. 4) Robust standard errors clustered at the household level. 5) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 3. Descriptive statistics by censoring status

	Died	Alive	Unknown death status	Total	p-values
N	1,549 (23.7%)	3,973 (60.8%)	1,011 (15.5%)	6,533 (100.0%)	
Age (in years)	67.7 (8.2)	60.4 (7.4)	60.6 (8.4)	62.2 (8.3)	<0.001
Gender					
Male	861 (55.6%)	1,555 (39.1%)	395 (39.1%)	2,811 (43.0%)	<0.001
Female	688 (44.4%)	2,418 (60.9%)	616 (60.9%)	3,722 (57.0%)	
Education					
1.No education (illiterate)	161 (10.4%)	181 (4.6%)	46 (4.5%)	388 (5.9%)	<0.001
2.No education (reading)	253 (16.3%)	366 (9.2%)	75 (7.4%)	694 (10.6%)	
3.Elementary school	526 (34.0%)	1,280 (32.2%)	292 (28.9%)	2,098 (32.1%)	
4.Middle school	249 (16.1%)	800 (20.1%)	210 (20.8%)	1,259 (19.3%)	
5.High school	240 (15.5%)	1,017 (25.6%)	274 (27.1%)	1,531 (23.4%)	
6.Two-year college	18 (1.2%)	32 (0.8%)	12 (1.2%)	62 (0.9%)	
7.College grad	97 (6.3%)	265 (6.7%)	93 (9.2%)	455 (7.0%)	
8.Post college (Master)	4 (0.3%)	26 (0.7%)	8 (0.8%)	38 (0.6%)	
9.Post college (PhD)	1 (0.1%)	6 (0.2%)	1 (0.1%)	8 (0.1%)	
Marital status					
Separated/divorced/widowed/single	418 (27.0%)	566 (14.2%)	209 (20.7%)	1,193 (18.3%)	<0.001
Married/partnered	1,131 (73.0%)	3,407 (85.8%)	802 (79.3%)	5,340 (81.7%)	
Urban locality					
Urban	1,093 (70.6%)	2,882 (72.5%)	893 (88.3%)	4,868 (74.5%)	<0.001
Rural	456 (29.4%)	1,091 (27.5%)	118 (11.7%)	1,665 (25.5%)	
Household income					
(unit: 10 million KRW)	0.1 (0.2)	0.1 (0.2)	0.1 (0.3)	0.1 (0.2)	<0.001
Number of people in household	2.7 (1.4)	2.9 (1.3)	3.1 (1.4)	2.9 (1.3)	<0.001
Number of living children	3.8 (1.6)	3.1 (1.3)	3.0 (1.3)	3.2 (1.4)	<0.001
Respondent's mother alive	0.1 (0.3)	0.3 (0.4)	0.3 (0.5)	0.2 (0.4)	<0.001
Respondent's father alive	0.0 (0.2)	0.1 (0.3)	0.1 (0.3)	0.1 (0.2)	<0.001

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006–2020 v2. 2) Between-group comparisons indicate differences for all variables ($p < 0.001$). Continuous variables tested with t-tests; categorical variables with χ^2 tests.

eTable 4. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models based on oldest child's education, with and without inverse probability weighting adjustment

First-stage Results	**Unweighted**	**Weighted**
	Beta [95% CI]	Beta [95% CI]
Exposure to 1993 education reform	0.136***	0.126***
	(0.105 0.167)	(0.095 0.157)
Observations	6,525	6,525
F	72	62
Second-stage Results		
	HR [95% CI]	HR [95% CI]
College completion	0.776***	0.802**
	(0.686 0.878)	(0.700 0.919)
Observations	5,873	5,873

Notes: 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) in the Korean Longitudinal Study of Aging (KLoSA), 2006–2020 v2. 2) Models control for individual- and family-level characteristics, including the respondent's age in years, gender, marital status, education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, 95% confidence intervals are based on a bootstrap with 1,000 replications. 4) The weighted specification applies inverse probability weighting (IPW) to adjust for potential bias from nonrandom censoring; the unweighted specification does not apply these weights. 5) Kleibergen–Paap Wald F-statistics are reported for the first-stage models. HR = hazard ratio; CI = confidence interval; IPW = inverse probability weighting. *** p<0.001, ** p<0.01, * p<0.05.

eTable 5. Hazard ratios and 95% confidence intervals from observational and two-stage residual inclusion (2SRI) Cox proportional hazards models, overall and by parent and oldest child gender

	All		Fathers		Mothers	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
A: Observational Cox Model: College Completion	0.785***	(0.687 0.897)	0.766***	(0.644 0.910)	0.813***	(0.664 0.997)
	(0.053)		(0.068)		(0.084)	
B: 2SRI Cox Model: College Completion	0.802**	(0.700 0.919)	0.772**	(0.643 0.926)	0.838**	(0.720 0.976)
	(0.069)		(0.093)		(0.065)	
Observations	5,873		2,516		3,357	
F	62		18		41	
	All		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
A: Observational Cox Model: College Completion	0.785***	(0.687 0.897)	0.790***	(0.661 0.945)	0.723***	(0.594 0.881)
	(0.053)		(0.072)		(0.073)	
B: 2SRI Cox Model: College Completion	0.802**	(0.700 0.919)	0.809**	(0.663 0.987)	0.714**	(0.577 0.882)
	(0.069)		(0.101)		(0.108)	
Observations	5,873		3,089		2,784	
F	62		14		57	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 6. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models, by parental socioeconomic status (urban locality, household assets, and education)

	Rural		Urban	
	HR	**95% CI**	**HR**	**95% CI**
College completion	0.805**	(0.684 0.949)	0.763**	(0.590 0.985)
	(0.084)		(0.131)	
Observations	1,594		4,279	
F	34		36	
	Below median		**Above median**	
	HR	**95% CI**	**HR**	**95% CI**
College completion	0.789**	(0.652 0.954)	0.803**	(0.669 0.964)
	(0.097)		(0.075)	
Observations	3,691		2,182	
F	47		21	
	Less than high school education		**High school or above**	
	HR	**95% CI**	**HR**	**95% CI**
College completion	0.841**	(0.711 0.995)	0.794**	(0.671 0.939)
	(0.072)		(0.089)	
Observations	1,865		4,008	
F	38		23	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. In stratified analyses, the stratifying variable (urbanicity, household assets, or education) is excluded from the covariate set. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 7. Interaction tests for heterogeneity by parental characteristics in reduced-form and two-stage residual inclusion (2SRI) Cox proportional hazards models

Interaction (first category vs reference)	Reduced-form HR (95% CI)	2SRI HR (95% CI)
Parent gender (fathers vs mothers)	0.927 (0.798 1.077)	0.921 (0.802 1.058)
Child gender (sons vs daughters)	1.103* (1.006 1.210)	1.133 (0.988 1.299)
Residence (urban vs rural)	0.850* (0.732 0.987)	0.948 (0.851 1.056)
Household assets (above vs below median)	1.005 (0.863 1.170)	1.018 (0.878 1.181)
Education (high school or above vs less than high school)	0.882** (0.802 0.970)	0.944 (0.840 1.060)
Observations	5,880	5,873

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Reduced-form interaction tests are from Cox proportional hazards models of reform exposure and respondent mortality. 2SRI interaction tests are from two-stage residual inclusion (2SRI) Cox proportional hazards models. 3) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 4) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 8. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression predicting parents' health behaviors based on oldest child's education, overall and by parent gender

Smoking	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	-0.041***	(-0.057 -0.024)	-0.074***	(-0.113 -0.034)	-0.019***	(-0.031 -0.007)
	(0.008)		(0.020)		(0.006)	
Observations	6,525		2,804		3,721	
Drinking	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	-0.006	(-0.026 0.015)	0.010	(-0.035 0.055)	-0.017	(-0.045 0.010)
	(0.011)		(0.023)		(0.014)	
Observations	6,525		2,804		3,721	
Exercise	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.124***	(0.099 0.149)	0.125***	(0.080 0.169)	0.127***	(0.096 0.158)
	(0.013)		(0.023)		(0.016)	
Observations	6,525		2,804		3,721	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 9. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression predicting parents' health behaviors based on oldest child's education, overall and by child gender

Smoking	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	-0.041***	(-0.057 -0.024)	-0.035**	(-0.061 -0.008)	-0.038***	(-0.065 -0.012)
	(0.008)		(0.013)		(0.014)	
Observations	6,525		3,251		3,082	
Drinking	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	-0.006	(-0.026 0.015)	0.005	(-0.028 0.037)	-0.021	(-0.055 0.014)
	(0.011)		(0.017)		(0.018)	
Observations	6,525		3,251		3,082	
Exercise	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.124***	(0.099 0.149)	0.118***	(0.084 0.152)	0.145***	(0.104 0.186)
	(0.013)		(0.017)		(0.021)	
Observations	6,525		3,251		3,082	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 10. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression predicting intergenerational relationships and support based on oldest child's education, overall and by parent gender

Meeting in person	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.042***	(0.015 0.069)	0.046**	(0.002 0.091)	0.038**	(0.008 0.068)
	(0.014)		(0.023)		(0.015)	
Observations	6,525		2,804		3,721	
Contact	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.051***	(0.035 0.068)	0.045***	(0.017 0.073)	0.055***	(0.037 0.072)
	(0.008)		(0.014)		(0.009)	
Observations	6,525		2,804		3,721	
Financial support	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.069***	(0.042 0.096)	0.061***	(0.020 0.101)	0.075***	(0.044 0.105)
	(0.014)		(0.021)		(0.016)	
Observations	6,525		2,804		3,721	
Gift	**All**		**Fathers**		**Mothers**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.017*	(-0.001 0.036)	0.009	(-0.020 0.038)	0.024	(-0.008 0.056)
	(0.010)		(0.015)		(0.016)	
Observations	6,525		2,804		3,721	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 11. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression predicting intergenerational relationships and support based on oldest child's education, overall and by child gender

Meeting in person	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.042***	(0.015 0.069)	0.040**	(0.006 0.073)	0.032*	(-0.004 0.068)
	(0.014)		(0.017)		(0.018)	
Observations	6,525		3,443		3,082	
Contact	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.051***	(0.035 0.068)	0.076***	(0.057 0.094)	0.025**	(0.005 0.046)
	(0.008)		(0.010)		(0.010)	
Observations	6,525		3,443		3,082	
Financial support	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.069***	(0.042 0.096)	0.092***	(0.061 0.123)	0.053**	(0.012 0.095)
	(0.014)		(0.016)		(0.021)	
Observations	6,525		3,443		3,082	
Gift	**All**		**Sons**		**Daughters**	
	Beta	**95% CI**	**Beta**	**95% CI**	**Beta**	**95% CI**
College completion	0.017*	(-0.001 0.036)	0.024	(-0.005 0.052)	0.019	(-0.011 0.048)
	(0.010)		(0.015)		(0.015)	
Observations	6,525		3,443		3,082	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 12. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models: Comparison before and after adjusting for intermediate outcomes, by parent gender

	Model 1: Before Adjusting for Intermediate Outcomes					
	All		Fathers		Mothers	
	HR	95% CI	HR	95% CI	HR	95% CI
College completion	0.802**	(0.700 0.919)	0.772**	(0.643 0.926)	0.838**	(0.720 0.976)
	(0.069)		(0.093)		(0.065)	
Observations	5,873		2,516		3,357	
	Model 2: After Adjusting for Intermediate Outcomes					
	All		Fathers		Mothers	
	HR	95% CI	HR	95% CI	HR	95% CI
College completion	0.824**	(0.717 0.947)	0.812**	(0.671 0.982)	0.833*	(0.679 1.022)
	(0.071)		(0.097)		(0.104)	
Observations	5,873		2,516		3,357	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, frequency of meeting or contact with children (Model 2 only), whether respondents received financial and non-financial support from children (Model 2 only), health behaviors including currently smoking and/or drinking (Model 2 only), birth cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 13. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models: Comparison before and after adjusting for intermediate outcomes, by oldest child gender

	Model 1: Before Adjusting for Intermediate Outcomes					
	All		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.802**	(0.700 0.919)	0.809**	(0.663 0.987)	0.714**	(0.577 0.882)
	(0.069)		(0.088)		(0.077)	
Observations	5,873		3,089		2,784	
	Model 2: After Adjusting for Intermediate Outcomes					
	All		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.824**	(0.717 0.947)	0.813**	(0.672 0.985)	0.770**	(0.625 0.947)
	(0.071)		(0.098)		(0.106)	
Observations	5,873		3,089		2,784	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, frequency of meeting or contact with children (Model 2 only), whether respondents received inancial and non-financial support from children (Model 2 only), health behaviors including currently smoking and/or drinking (Model 2 only), birth cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 14. Testing alternative specifications of offspring educational attainment: Oldest child vs. highest-educated child

	Model 1: Oldest Child		Model 2: Highest-Educated Child	
	HR	**95% CI**	**HR**	**95% CI**
College completion	0.802**	(0.700 0.919)	0.747***	(0.645 0.864)
	(0.069)		(0.075)	
Observations	5,873		5,381	
F	62		83	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** $p<0.001$, ** $p<0.01$, * $p<0.05$.

eTable 15. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models, overall and by parent and highest-educated child gender

	All		Fathers		Mothers	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.747***	(0.645 0.864)	0.754**	(0.619 0.919)	0.785**	(0.623 0.988)
	(0.075)		(0.093)		(0.097)	
Observations	5,381		2,281		3,100	
F	83		34		47	
	All		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.747***	(0.645 0.864)	0.728**	(0.598 0.886)	0.702**	(0.555 0.889)
	(0.075)		(0.101)		(0.108)	
Observations	5,381		3,231		2,150	
F	83		36		52	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 16. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models based on a cumulative measure of offspring education

	All Children	
	HR	**95% CI**
College completion	0.603***	(0.505 0.720)
Observations	5,701	
F	45	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) We instrumented the proportion of children who completed college with the proportion of children exposed to the 1993 higher education reform. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Robust standard errors clustered at the household level. 6) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 17. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models based on oldest child's education, stratified by parental birth cohort

	Oldest Child					
	All (1914-1961)		Japanese colonization (1914-1939)		Korean War/ Industrialization (1940-1954)	
	HR	95% CI	HR	95% CI	HR	95% CI
College completion	0.802**	(0.700 0.919)	0.802**	(0.664 0.967)	0.799***	(0.700 0.911)
	(0.069)		(0.096)		(0.067)	
Observations	5,550		1,817		3,129	
F	56		14		28	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Birth cohorts are distinguished as follows: Japanese Colonization Cohort (1914-1939), Korean War/Industrialization Cohort (1940-1954), and Baby Boomer Cohort (1955-1961). Baby Boomer cohort was not included due to limited sample size (n=652). 4) Robust standard errors clustered at the household level. 5) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 6) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 18. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models based on oldest child's education, with and without birth-cohort fixed effects, by parent gender

	Excluding Birth-Cohort Fixed Effects					
	All		**Fathers**		**Mothers**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.802**	(0.700 0.919)	0.772**	(0.643 0.926)	0.838**	(0.720 0.976)
	(0.069)		(0.093)		(0.065)	
Observations	5,873		2,516		3,357	
	Including Birth-Cohort Fixed Effects					
	All		**Fathers**		**Mothers**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.801***	(0.705 0.912)	0.779**	(0.646 0.939)	0.825**	(0.705 0.965)
	(0.066)		(0.095)		(0.066)	
Observations	5,873		2,516		3,357	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 19. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models based on oldest child's education: Sensitivity to birth-cohort definitions (3-year, 5-year, 10-year, and 15-year cohorts)

	3-Year		5-Year		10-Year		15-Year	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
College completion	0.741**	(0.607 0.905)	0.665**	(0.555 0.797)	0.802**	(0.700 0.919)	0.799**	(0.657 0.972)
	(0.102)		(0.092)		(0.069)		(0.080)	
Observations	3,098		4,262		5,873		6,423	
F	13		18		62		44	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 5/10/15-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Kleibergen-Paap Wald F-tests are used for F-statistics HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 20. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models by parent gender, under alternative household exposure classifications

Main specification	All		Fathers		Mothers	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
Oldest child exposed vs oldest child unexposed	0.802** (0.069)	(0.700 0.919)	0.772** (0.093)	(0.643 0.926)	0.838** (0.065)	(0.720 0.976)
Observations	5,873		2,516		3,357	
Robustness test 1	**All**		**Fathers**		**Mothers**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
Oldest child exposed (fully exposed) vs no children exposed	0.735*** (0.092)	(0.614 0.881)	0.724** (0.108)	(0.586 0.895)	0.717** (0.122)	(0.564 0.911)
Observations	3,643		1,618		2,025	
Robustness test 2	**All**		**Fathers**		**Mothers**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
At least one younger sibling exposed (partially exposed) vs no children exposed	0.731*** (0.078)	(0.627 0.852)	0.709** (0.110)	(0.572 0.878)	0.760** (0.113)	(0.609 0.948)
Observations	3,624		1,450		2,174	

Notes: 1) Models were estimated using respondents in KLoSA 2006–2020 v2 whose adult children (aged 25 or older) allowed classification of household reform exposure based on children's birth cohorts. 2) In the main specification, respondents were classified as "oldest-child exposed" (oldest child born 1974–1983) versus "oldest-child unexposed" (oldest child born 1964–1973), where the latter group could include households with reform-exposed younger siblings. Robustness test 1, restricted to respondents with multiple children, compared fully exposed respondents (all children born 1974–1983) with fully unexposed respondents (no children born 1974–1983). Robustness test 2, also restricted to respondents with multiple children, compared partially exposed respondents (oldest child not born 1964–1973 but at least one younger sibling born 1974–1983) with fully unexposed respondents (no children born 1974–1983). 3) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 4) Robust standard errors clustered at the household level. 5) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 21. Hazard ratios and 95% confidence intervals from two-stage residual inclusion (2SRI) Cox proportional hazards models by child gender, under alternative household exposure classifications

Main specification	All		Sons		Daughters	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
Oldest child exposed vs oldest child unexposed	0.802** (0.069)	(0.700 0.919)	0.809** (0.101)	(0.663 0.987)	0.714** (0.108)	(0.577 0.882)
Observations	5,873		3,089		2,784	
Robustness test 1	**All**		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
Oldest child exposed (fully exposed) vs no children exposed	0.735*** (0.092)	(0.614 0.881)	0.707** (0.111)	(0.568 0.880)	0.655*** (0.176)	(0.464 0.925)
Observations	3,643		2,144		1,499	
Robustness test 2	**All**		**Sons**		**Daughters**	
	HR	**95% CI**	**HR**	**95% CI**	**HR**	**95% CI**
At least one younger sibling exposed (partially exposed) vs no children exposed	0.731*** (0.078)	(0.627 0.852)	0.738** (0.101)	(0.606 0.899)	0.680*** (0.110)	(0.548 0.844)
Observations	3,624		2,008		1,616	

Notes: 1) Models were estimated using respondents in KLoSA 2006–2020 v2 whose adult children (aged 25 or older) allowed classification of household reform exposure based on children's birth cohorts. 2) In the main specification, respondents were classified as "oldest-child exposed" (oldest child born 1974–1983) versus "oldest-child unexposed" (oldest child born 1964–1973), where the latter group could include households with reform-exposed younger siblings. Robustness test 1, restricted to respondents with multiple children, compared fully exposed respondents (all children born 1974–1983) with fully unexposed respondents (no children born 1974–1983). Robustness test 2, also restricted to respondents with multiple children, compared partially exposed respondents (oldest child not born 1964–1973 but at least one younger sibling born 1974–1983) with fully unexposed respondents (no children born 1974–1983). 3) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 4) Robust standard errors clustered at the household level. 5) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eTable 22. Beta coefficients and 95% confidence intervals from two-stage least squares (2SLS) regression predicting other health outcomes based on oldest child's education

	Arthritis		Cancer		Diabetes	
	Beta	95% CI	Beta	95% CI	Beta	95% CI
College completion	-0.009	(-0.031 0.013)	-0.005	(-0.016 0.007)	0.002	(-0.019 0.022)
	(0.011)		(0.006)		(0.011)	
Observations	6,525		6,525		6,525	
	Heart Diseases		**High Blood Pressure**		**Liver Diseases**	
	Beta	95% CI	Beta	95% CI	Beta	95% CI
College completion	-0.005	(-0.017 0.008)	0.001	(-0.022 0.025)	0.001	(-0.008 0.010)
	(0.006)		(0.012)		(0.005)	
Observations	6,525		6,525		6,525	
	Lung Diseases		**Psych Problems**		**Stroke**	
	Beta	95% CI	Beta	95% CI	Beta	95% CI
College completion	0.000	(-0.009 0.010)	0.007*	(-0.001 0.015)	0.004	(-0.005 0.014)
	(0.005)		(0.004)		(0.005)	
Observations	6,525		6,525		6,525	

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. *** p<0.001, ** p<0.01, * p<0.05.

eTable 23. Placebo test: Two-stage estimation with an instrumental variable in the Health and Retirement Study (HRS), estimating the impact of oldest child's college completion on respondent mortality (hazard ratio)

First-stage Results	
	Beta [95% CI]
"Exposure" to 1993 education reform	-0.031
	(-0.382 0.319)
Observations	3,012
F	0.03
Second-stage Results	
	HR [95% CI]
College completion	1.021
	(0.775 1.343)
Observations	2,920

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the 1974-1983 birth cohort (treatment group) or in the 1964-1973 birth cohort (control group) from the Health and Retirement Study (HRS), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. HR = hazard ratio. *** $p<0.001$, ** $p<0.01$, * $p<0.05$.

eTable 24. Comparison of two-stage residual inclusion (2SRI) with second-stage Cox model (primary model) and conventional two-stage least squares (2SLS) model

	2SRI (Logit first stage and Cox second stage)	**Conventional 2SLS (Linear first and second stages)**
	HR [95% CI]	Beta [95% CI]
College completion	0.802**	-0.055***
	(0.700 0.919)	(-0.077 -0.033)
Observations	5,873	6,525

***Notes*:** 1) Models include respondents whose adult children (aged 25 or above) were reported to be in the birth cohort affected by the reform (treatment group) or in the birth cohort from the preceding 10-year period (control group) from the Korean Longitudinal Study of Aging (KLOSA), 2006-2020 v2. 2) Models control for individual and family-level characteristics, including the respondent's age in years, gender, marital status, levels of education, urbanicity, household income, household assets, number of living children, children's gender composition, whether the respondent's father or mother is alive, parental age at first child birth, parental education, sample cohorts, and regional fixed effects. 3) Robust standard errors are clustered at the household level. For 2SRI/2SLS estimates, confidence intervals are based on a bootstrap with 1,000 replications. 4) Inverse probability weighting (IPW) is used to correct for potential bias arising from nonrandom censoring. 5) Coefficients are reported with 95% confidence intervals in brackets. Statistical significance is denoted as HR = hazard ratio. *** p<0.001, ** p<0.01, * p<0.05.

eAppendix 1. Institutional background for the 1993 higher education reform

The 1993 higher education reform in South Korea marked a significant departure from the tightly regulated system that had previously kept college enrollment rates stable at around 35% since the late 1980s (Choi 1996). Under the previous regime, the government meticulously managed the capacity of college enrollment, allotting specific quotas across institutions. However, with the election of a new government in 1993, a wave of neoliberal reforms swept through the higher education sector (Lee and Sohn, 1994). The administration of President Kim Young-Sam introduced the 'Plan for the Promotion of College Enrollment Liberalization' and the seminal '5·31 Education Reform Plan' in subsequent years (Lee 2000). These reforms were geared toward increasing 'autonomy and competition' and drastically changed the educational landscape. They ushered in a phase of deregulation that allowed for a surge in both the number of higher education institutions and student enrollment rates. New universities were encouraged to enter the market, provided they met minimum conditions, and this led to an unprecedented rise in the college enrollment rate by more than 25 percentage points from 1993 to 1997 (Choi and Lee 2017).

The government's initiative shifted the focus from equity in educational opportunity and demand fulfillment to one that emphasized the excellence and diversity of higher education, advocating for universities' autonomy and accountability. This period also saw the adoption of the 'University Establishment Standard Principle', leading to the rapid increase of specialized universities and a significant expansion in the number of colleges and universities (Choi and Lee 2017). The 'College Enrollment Liberalization policy continued under the subsequent Kim Dae-Jung government, which further liberalized enrollment by abolishing the 'College Student Enrollment Decree' and establishing the 'Higher Education Act and its enforcement decree.' Despite some setbacks and unimplemented plans, the overarching aim was to prevent unrestrained expansion of enrollment while maintaining quality and efficiency through various reforms and structural adjustments. This reform era laid the groundwork for modernizing South Korean higher education, transforming it into a more open, diverse, and internationally competitive system that aligns more closely with global standards and practices. While these reforms received mixed reviews, they were undeniably pivotal in shaping the current state of higher education in South Korea.

eAppendix 2. Assessment of missing data and sensitivity analysis using multiple imputation

To assess the robustness of our findings in light of missing data on key variables (approximately 5% of observations), we conducted two additional analyses. First, we tested whether the missing data were missing completely at random (MCAR) using Little's MCAR test. The results indicated that the assumption of MCAR does not hold in our sample ($\chi^2 = 438,\ p < 0.001$). These results suggest that the missingness is not completely random, implying potential bias if only complete cases are analyzed. Given the rejection of MCAR, next, we conducted a sensitivity analysis using multiple imputation by chained equations (MICE) to account for missingness under the assumption that data are missing at random (MAR) (Azur et al., 2011). The imputation model included all variables used in the main analysis. We generated 5 imputed datasets and re-estimated the key models using the imputed data. The results from the imputed data were highly consistent with those from the complete-case analysis, suggesting that the observed relationships are not sensitive to the exclusion of cases with missing data. The results from the imputed analysis are presented in the tables below, and support the robustness of our conclusions.

Table 1. Tabulate missing values before dropping respondents with missing covariates

				Obs<.	
Variable	Obs=.	Obs<.	Unique values	Min	Max
Respondent education	1	6,862	9	1	9
Respondent household assets (log)	32	6,831	>500	-7.3	17.8
Respondent smoking now	2	6,861	2	0	1
Respondent mother alive	94	6,769	2	0	1
Respondent father alive	105	6,758	2	0	1
Respondent mother education	125	6,738	7	1	7
Respondent father education	156	6,707	9	1	9

Table 2. Little's MCAR test

Number of obs	**=**	**6,863**
Chi-square distance	=	438
Degrees of freedom	=	275
Prob > chi-square	=	0

Table 3. Robustness of Effects of Daughter's Education on Respondent Mortality (2nd Stage Cox Model): Complete-Case vs. Multiple Imputation Estimates

	Before Imputation (Main Model)		**After Imputation**	
	HR	**95% CI**	**HR**	**95% CI**
College completion	0.802**	(0.700 0.919)	0.802**	(0.702 0.915)
	(0.071)		(0.054)	
N	5,873		6,151	

eAppendix 3. Measurement of adult child educational attainment

The categorization of offspring education in the literature varies, with each method chosen for its relevance to the research question and the hypothesized pathways through which children's education impacts parental outcomes. Therefore, the way we measure adult children's education – whether as the oldest, the highest-educated, or by average schooling – may also account for the differences among studies. For readers interested in further exploration, we present the previous literature on offspring education and the different methods of measuring offspring education below. This list provides an overview of significant studies and their approaches to understanding the impact of children's education on parental outcomes. It is important to note that this list is not exhaustive and serves as a starting point for deeper investigation into the topic. Furthermore, most studies incorporate multiple measures of offspring education in their analyses to account for different potential influences in their sensitivity analysis. For more details on individual research studies and additional references, please refer to the Reference list at the end of this document.

1. Highest Educated Child: Zimmer et al. (2002) and Zimmer et al. (2007) use the highest educational level of children living closest to the older adult, allowing for the investigation of the direct influence of the most proximal and potentially supportive child. Similarly, Yang et al. (2016) consider the educational attainment of the most highly educated co-resident child, which could reflect the maximum potential socioeconomic benefits accruing to the parent from their children. Sabater et al. (2020) also utilize the highest level of educational attainment among children, emphasizing the likely scenario where the most educated child contributes disproportionately to parental care or resources. Ma (2019) and Ma et al. (2022) use the highest-educated child as an index, assuming that the highest-educated children are the most important in shaping the health of older adults.

2. Oldest Child: Torssander (2013) analyzes the education level of the oldest child, which could be significant in cultures where the oldest child bears more responsibility for parental care. This approach is also used by Sabater et al. (2020) who look at the oldest child with the highest level of education, thus combining age and educational attainment to potentially identify the child most capable of supporting the parent. Torres et al. (2022) use the years of schooling attained by the oldest child as the primary exposure but also consider the highest-educated child as an alternative index. Similarly, Gutierrez et al. (2024) focus on the oldest child, examining how their educational attainment may set a precedent or influence the educational pathways of younger siblings, and tests the highest-educated child as an alternative.

3. Proportional Measure: Friedman & Mare (2014) and Lee C. (2018) employ proportional measures, where the education levels are categorized and analyzed as proportions of the total number of children. This method allows researchers to capture a more nuanced picture of how

diverse educational achievements among children might collectively impact parental outcomes through a range of supportive behaviors or resources.

4. Cumulative Measures: Lee et al. (2017) use the average educational attainment of all living children, which provides a comprehensive view of the family's overall educational environment and its potential to affect parental well-being. This average could be reflective of the general socioeconomic status of the family, influencing the resources available for parental care. The approach by Yahirun et al. (2017) and Yahirun, Sheehan, & Mossakowski (2020) takes a cumulative measure of how many children have reached a specific educational threshold, exploring how widespread access to higher education within a family might impact parental health.

eAppendix 4. Measurement of covariates and intermediate outcomes

Covariates. We controlled for a range of demographic, socioeconomic, and family-level characteristics of the respondents. Specifically, we accounted for respondents' characteristics, including age (age in years at interview) and its quadratic term (age^2), gender (1. man/2. woman), marital status (1. married or living with a partner/2. separated, divorced, widowed, or never married), educational attainment (1. no education (illiterate)/2. no education (reading)/3. elementary school/4. middle school/5. high school/6. two-year college/7. college graduate/8. post-college (master's)/9. post-college (PhD)), and sample cohorts (1. Original KLoSA cohort/2. Refresher cohort); family-level characteristics, including the number of living children, children's gender composition (measured as the percentage of sons), whether the respondent's father or mother is alive, parental age at first child birth, household income (originally in 10,000 Korean Won, standardized), household assets (originally in 10,000 Korean Won, standardized), urban or rural residence (0. urban/1. rural), and regions (17 provinces and special cities); respondents' health behaviors including currently smoking, currently drinking, and frequent exercise (+1/week); and early life characteristics such as parental education (1. less than lower secondary/2. upper secondary and vocational school/3. tertiary).

Mechanisms (Intermediate Outcomes). First, we evaluated intergenerational support from the adult child to parents, including the frequency of contact and financial support. We used the frequency of interactions between an adult child and their parents and the exchange of financial and non-financial support to measure intergenerational support and relationships. The frequency of contact examines whether parents and, where applicable, their spouses engage with any of their children regularly, including various forms of communication such as in-person meetings, phone calls, or electronic messages. Financial support was assessed to determine if parents received any financial aid or non-financial gifts from their children or grandchildren within the past year. These dimensions of intergenerational support are critical indicators of the quality of life for older adults (Silverstein et al., 1997).

To quantify these relationships, respondents were asked to categorize the frequency of their contact with each child using a detailed scale ranging from daily interactions to never (1. almost every day or 4+ times a week/2. once a week/ 3. 2-3 times a week/ 4. once a month/5. twice a month/6. 1-2 times a year/7. 3-4 times a year/8. 5-6 times a year/9. rarely/10. never). We defined a binary variable, assigning a value of 1 to those parents who had contact with their children more than once a month, and a value of 0 to those with less frequent contact. Similarly, in-person meetings were categorized with a binary variable. For financial support, we assigned a value of 1 to respondents who had received regular or occasional financial help in the last year, and 0 if they did not. Non-financial gifts, such as clothing, leisure items, health products, household goods, and food, were also tracked with a binary variable to determine their presence or absence in the lives of the respondents. We adjust our models for these mediators of the association in our primary analysis.

Second, we examined health behaviors, including current smoking, usual alcohol consumption, and regular exercise. Current smoking was defined as whether or not the respondent smokes, while usual drinking assessed whether the respondent typically consumes alcohol. Regular exercise was defined as engaging in physical activity more than once a week.

Lastly, we considered the presence of chronic conditions as intermediate outcomes. These conditions included high blood pressure or hypertension, diabetes or high blood sugar, cancer or malignant tumors (excluding minor skin cancer), chronic lung disease, liver disease (excluding fatty liver), heart-related disease, cerebrovascular disease, psychiatric problems, and arthritis or rheumatism. For each condition, we created a binary variable indicating its presence or absence.

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