

# Parental Death, Adult Work and Family Support: a Source-Faithful Replication Manuscript

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## Abstract

Losing a parent during adulthood can disrupt work through grief and health, remove support for raising children, and change responsibilities toward a surviving parent. Extensive public services need not replace these family relationships. This results-unfilled manuscript prepares a replication of Jensen and Zhang’s (2026) Danish register study of sudden first parental deaths. Its main population comprises adults aged 25–50 at a first parental death during 1985–2014. Matched difference-in-differences comparisons distinguish employment, pension contributions among workers and earnings; complementary analyses examine health services, treatment and earnings onsets, childcare, surviving-parent health and household responses. The design reduces concerns about anticipation but requires comparable counterfactual trajectories and faithful reconstruction of the original samples. Replication evidence remains *[pending]*. Joint health and earnings responses cannot identify a mediated share, family-support proxies do not measure care hours, and parental wealth does not measure inheritance received. Unresolved disagreements between paper and code affect matching, several model summaries and subgroup definitions; dependent targets retain explicit no-estimate dispositions. The manuscript combines the scientific argument and supplementary methods; all empirical evidence remains pending.

**Keywords:** Parental death; bereavement; labor supply; mental health; informal care; Danish registers; replication.

**PRE-DATA TEMPLATE:** empirical results are pending; any illustrative mock-data figures or tables are not research findings.

## Introduction

**ILLUSTRATIVE MOCK DATA — NOT RESULTS.** Every displayed numerical figure and table is a hypothetical layout preview. These values are independent of the source paper’s estimates and do not select an expected effect. The empirical evidence and interpretation slots remain pending.

The death of a parent changes a family even when the child has long since become an adult. Economic independence does not end emotional attachment, practical help with children, or responsibility for an aging parent. A bereaved adult may therefore face several changes at once: reduced capacity to work, a loss of help that previously made work possible, and new demands on time from the surviving family. The labor-market consequence is not predetermined. The end of prior caregiving or a transfer of wealth could also change work decisions. Understanding adult parental loss requires distinguishing these possibilities rather than treating an earnings change as a direct measure of grief.

Jensen and Zhang (2026) place this question between three bodies of research. Studies of severe health shocks to spouses or children establish that a family member's health can matter for an adult's employment and earnings. The parental-death setting concerns another relationship and a life event experienced widely during working life. Work on parental loss during childhood instead emphasizes schooling, skill formation and the resources available while growing up. Those mechanisms cannot simply be carried over to a person whose education and entry into work largely precede the loss. Research on bereavement and on informal childcare and eldercare suggests that health and family support remain important in adulthood, but leaves open how they accompany changes in work after a parent's death. These are the original article's literature connections, including Fadlon and Nielsen (2021), van den Berg, Lundborg and Vikström (2017), Aaskoven, Kjær and Gyrð-Hansen (2022), Garcia-Moran and Kuehn (2017), and Ciccarelli and Van Soest (2018).

Denmark makes the role of the family particularly informative. Public healthcare, subsidized childcare and municipal eldercare support individuals outside the household. Yet enrollment in formal childcare does not necessarily cover a parent's working schedule or a child's illness, and publicly provided eldercare does not encompass every form of support exchanged within a family. Formal and informal provision can coexist. A parental death may expose the part of everyday life that still depends on a particular family member, even where public services are extensive. Conversely, those services may limit the adjustments required after a loss. The Danish setting therefore helps define the question; it does not predetermine a large or small effect.

The original study focuses on sudden first parental deaths. Suddenness reduces the prolonged anticipation and care demands associated with an extended terminal illness. Studying the first loss also separates the event from having already lost both parents, while making the circumstances of a surviving parent central to interpretation. Neither restriction randomizes parental death. Parents who die suddenly and families who experience the loss at different ages can still differ in ways relevant to labor outcomes. The matched comparison seeks adults at similar points in their economic and family lives whose parents remain alive over the comparison window. Its credibility rests on how their outcomes would have evolved without the treated adult's loss.

The empirical argument begins with the labor response itself. Employment, contributions conditional on work, earnings in currency units and earnings relative to a baseline describe distinct aspects of adjustment. Parent-by-child gender contrasts then locate that response within different family relationships. Health-service and prescription outcomes ask whether bereavement is accompanied by changes in treatment.

The timing of earnings declines and treatment starts addresses the further question of whether the same annual observations register both events. Child-age heterogeneity, formal-care enrollment and surviving-parent circumstances turn to the organization of family support. Spouse earnings, parental wealth, family formation and job choices test the reach and limits of those accounts. Each block can qualify the others without becoming an estimate of its causal contribution to total earnings.

This is an unexecuted replication manuscript of that fixed study, prepared from the journal article, its supplement and the frozen source audit. Published findings are comparison targets, not observations in this manuscript. The main text develops the original questions and the interpretation of pending evidence; the Supplementary Information (SI) contains the detailed reconstruction methods, full tables and all additional figures in the same document. Some original choices cannot yet be reconstructed consistently from the public materials. Those limitations remain visible where they alter the comparison or its interpretation, and their full dispositions remain in the unchanged source ledger. The present editorial amendment develops exposition without changing that scientific specification.

## **Institutional setting and source positioning**

The institutional account here follows the study-period setting described in Jensen and Zhang (2026, Section I), including its cited healthcare and family-policy sources. It is not an assertion that every historical rule remains current. The relevant point is how access to formal services affects the meaning of the observed health and care measures, and why families may continue to supply support alongside those services.

In the healthcare system described by the article, general practitioners are the first contact for most medical concerns and provide referrals to specialists and hospitals. Public funding makes GP care, specialist medical treatment and hospital care available without a patient charge for those services. Psychology differs: eligible treatment can receive public support but involves a co-payment, including for bereavement-related referrals. Prescription medicines also involve co-payments that depend on annual expenditure. The registers record contact with this system, which is valuable for observing treatment responses across a population. They do not directly record every symptom, untreated condition or privately borne burden.

These arrangements matter for comparing health outcomes. A psychologist visit can indicate that someone seeks and obtains help after bereavement, while a prescription records another route into treatment. A change in either measure can arise from a change in health need, a change in help-seeking, or access through the referral system. The article also describes a one-year referral requirement for publicly supported bereavement-related psychology. That timing makes the early treatment response informative about service use, while limiting any reading of its annual profile as a pure trajectory of distress. A short-lived rise in recorded consultations would not establish that grief itself was short-lived.

For childcare, the original institutional sources describe an entitlement from 26 weeks to school age, substantial municipal subsidies, and subsidized after-school provision. These arrangements make regular care available to many working families.

Their existence, however, does not establish that parents can substitute formal care for every task a grandparent performs. The article emphasizes gaps between care opening hours and parents' work schedules, as well as occasional care when a child is ill. The grandparenting literature it cites describes both regular and occasional help. Enrollment is consequently an incomplete measure of a family's capacity to maintain work schedules, even when formal care is widely used.

The loss of a grandparent can also affect stress and the reliability of the family's arrangements. The source treats that emotional and practical support as part of the broader informal-childcare account. A comparison by the age of the youngest child is useful because care needs vary across childhood, but a larger earnings response among parents of young children would not quantify lost hours of grandparent care. An enrollment response adds evidence about formal substitution. Reading those two comparisons together is more informative than assuming that universal provision has already removed the childcare mechanism.

Municipal eldercare similarly supplies home assistance and, where needed, residential care. The source's institutional account distinguishes care services from residents' rent and food payments in care homes. It also describes informal assistance alongside formal care, drawing on Gørtz, Christensen and Gupta (2023). Before a first parental death, the other parent may provide much of the needed informal care. After the death, that caregiver may be absent and the surviving parent may need help. Public provision can moderate the consequences without making the direction or allocation of adult children's responsibilities obvious. This is why the study examines the surviving parent's health rather than treating all parental deaths as the same care shock.

Inheritance provides a further reason to distinguish first from second parental deaths. The historical legal account cited by the article describes circumstances in which a surviving spouse can retain an undivided estate, postponing a distribution to children. Exceptions mean that the first death cannot be equated with no possible transfer. The study accordingly retains wealth-related comparisons while treating the first-death restriction as reducing the relevance of immediate bequests, not eliminating them by definition. Its wealth measure is parental net assets before the loss. It cannot establish whether a child actually received an inheritance, when it arrived or how it was used.

Taken together, these institutions motivate several competing accounts of a labor response. A health-related explanation concerns the bereaved adult's capacity and treatment. Childcare and eldercare explanations concern responsibilities and support within the family. A wealth or preference explanation concerns resources and choices. Their observable implications overlap, so the study's sequence of comparisons is needed to narrow interpretation. The population and counterfactual construction determine which of those comparisons can carry causal weight.

## Data and cohort construction

The source uses linked Danish population registers to connect adult children to parents and to observe demographic, labor, health and family outcomes. The main labor observation period is 1980–2019. The treated population comprises adults aged 25–50

in the year of a sudden first parental death between 1985 and 2014, with two known parents and the source-required parental birth and population information before the death. The first death is the earlier recorded death of the linked mother and father. Event time is calendar year minus the death year, ordinarily running from  $-5$  to  $+5$ . These annual coordinates distinguish the event year from the first complete calendar year after the event.

The final source cause classification includes heart and cerebrovascular disease, acute respiratory disease and external causes, represented by cause49-list codes 24, 25, 26, 29, 46, 47 and 49. Broader intermediate filters in the programs are followed by explicit final restrictions; they are not additional treatment definitions. Requiring a sudden cause is intended to reduce prolonged anticipation and prior care. It does not demonstrate that a parent had been healthy immediately before death or that all affected adults were unprepared for the loss. Nor does the first-death definition universally exclude a second death during follow-up: the source treats a longer gap between deaths as a separate, incompletely specified robustness comparison.

The basic observational unit is an adult-child year. In matched analyses, each treated adult and control are observed within their assigned match, and a reused control can appear in more than one match. The source panel requires the prescribed year set rather than merely eleven rows. Family linkage, duplicate records, simultaneous deaths and the interpretation of missing register entries affect whether that panel represents the intended individuals and years. Their unresolved cases retain the frozen dispositions detailed in SI S1. No missing year is converted into a covered year with a zero outcome simply to complete the panel.

The authorized planning boundary contains all 531 documented FSV tables; the fixed study names 18 of them. Their actual extracts, historical years, person-key correspondence and auxiliary mappings remain unverified. Missing inflation, education, service-specialty or geography crosswalks can prevent a source-defined measure even when a register is documented. This manuscript uses documentation and published source material, with no confidential rows or newly computed replication summaries. SI S1 gives the measurement details.

Employment is defined by positive ATP pension contributions. Contributions conditional on positive payments proxy the intensity of work among those working; the unconditional contribution outcome includes zeros. Neither is a direct record of hours. Earnings include employment and self-employment income, with the source's inflation adjustment to 2020 prices, winsorization and display-specific indexing. A baseline-100 earnings index uses the relevant sex or subgroup mean at event year  $-1$ . The additional rank measure compares earnings within the source annual population. The currency, index and rank outcomes therefore answer different scale questions rather than offering interchangeable transformations to choose after seeing results.

Outcome coverage varies. Private-service data begin in 1990, prescription data in 1995, and the relevant nonpsychiatric hospital displays use information from 1994 through 2018. Childcare enrollment is observed earlier in the source collection but is analyzed from 2005 because of coverage limitations, retaining its full original window. Co-occurrence uses its own lag construction and complete health-era sample. These restrictions matter when reading health and labor evidence together: two response

profiles may describe different eligible populations. The manuscript does not impose a new common sample to reconcile them.

The population description will locate first parental loss in the adult life course: *[pending]* *[pending]*. SI Figures B1–B4 describe the relevant age and parent-gender distributions, while Table 1 and Tables B1–B2 retain the complete baseline comparisons. The age-at-loss distribution is conditional on the source death-period population; it is not an age-specific mortality risk. Exact shares or histogram features that cannot be recovered from the documented definitions remain unavailable. Comparing sudden and nonsudden losses informs the population scope, but similarity on observed characteristics cannot establish either random treatment or the transportability of a sudden-death effect.

## Identification and estimation

### Life-course paths and the counterfactual

A before-and-after decline in earnings is not sufficient to identify the consequence of parental death. Adults age over the event window, their children grow older, and the return to age or education can change across calendar periods. Those processes may differ for women and men even without bereavement. For example, an adult’s labor supply can evolve as childcare needs change. The source’s descriptive event studies therefore establish the timing of observed paths while motivating a more explicit comparison group.

The descriptive specification estimates separate event profiles by sex with age-by-year and education-by-year categorical controls and robust standard errors. It uses no untreated comparison group. A discontinuity near the loss can be informative, but a smooth pre-event path does not remove unobserved differences between people losing a parent at different ages. The sudden and nonsudden profiles in SI S3 and S8 also distinguish an abrupt event from a potentially longer process of parental illness. Their displayed relative magnitudes require a source-defined fitted counterfactual mean. The code’s prediction and pooling population differs from the sex-specific description, so the percentage-scaled paths remain blocked under C04; an unscaled series cannot replace them as the original display.

### Matching and the difference-in-differences comparison

The matched design compares each treated adult with an observationally similar control whose parents do not die during the relevant five-year follow-up. Controls receive the treated adult’s event clock. This comparison asks whether the difference in outcomes between the two groups changes relative to the year before the treated adult’s loss. Matching on economic and family circumstances makes that counterfactual more plausible than a comparison across all bereaved and nonbereaved adults. It cannot remove differences in unobserved parental health, family attachment or other determinants of trajectories merely by making baseline means similar.

The journal describes exact matching on sex, earnings categories and whether the youngest child is below six, followed by nearest-neighbor Mahalanobis matching on

categorical versions of age, education, sector, cohabitation, region, children, parental ages and youngest-child age. It also describes three years of earnings and employment history. These histories are scientifically relevant because an adult already on a changing employment path may otherwise be compared with someone on a stable path. The family variables address another source of life-course differences. Table B3 in SI S3 distinguishes characteristics targeted in matching from additional health characteristics that describe the resulting sample.

The journal and public program do not fully agree on the matching groups and employment history. This changes who would serve as a control, so the intended matched sample remains unresolved. Consequential ties and duplicate records also require source correspondence. These limitations constrain every dependent matched comparison; SI S2 records the competing descriptions, and the preregistration retains the exact no-estimate rules.

The source dynamic model includes treatment-by-event-time indicators, common event-time effects, calendar-year effects, age effects and individual-by-match fixed effects, with event year  $-1$  omitted. Its coefficients describe the treated-control difference at each event time relative to that reference. The individual-by-match effect absorbs persistent level differences within the represented individual and match; the other terms allow common calendar and event-time patterns. Standard errors cluster at the individual-by-match level. The exact equations and uncertainty conventions appear in SI S2, including the original distinctions between event-study intervals and pooled-model intervals.

The central identifying assumption is that, absent the loss, treated adults' outcomes would have evolved like those of their matched controls. Suddenness helps with anticipation but does not prove that assumption. Pre-event coefficients are useful evidence about prior differences and the timing of adjustment, rather than a statistical certificate that post-event counterfactual trends must agree. Excluding already-treated controls avoids those comparisons within this design. It does not eliminate selection into a sudden death or assure that controls face no illness-related changes while their own parents remain alive. The registered future-treated comparison addresses that latter concern in a different way, discussed below and in SI S8.

## Summary estimands, inference and unresolved choices

The source also reports a pooled treatment-by-post coefficient and a model separating mother-first and father-first interactions, each estimated separately by adult-child sex. A dynamic profile distinguishes onset and persistence; a pooled effect summarizes the source post period. The two are not interchangeable. The code defines post as event years 0 through 5, whereas the narrative refers to effects after death over five years. Those six annual cells cannot silently be described as five post-event observations. A03 retains the ambiguity for pooled summaries and immediate-year ratios without blocking an otherwise defined dynamic coordinate solely because of that wording.

Age, calendar time and individual effects are collinear, and the source normalization is not fully specified for every restricted sample. The treatment contrast and its variance must be invariant to any engineering representation before it can be used.

Some displayed model references also disagree with their programs. SI S2 and the relevant outcome subsections retain those unresolved specifications.

The source 95% intervals, standard errors, baselines and reported gender contrasts remain the inferential objects. Comparisons use the source joint-model covariance where required; two separate estimates are not treated as independent to reconstruct a difference. No new balance cutoff, joint pretrend test, multiplicity procedure, equivalence band or replication-success threshold has been registered. A wide interval is evidence of uncertainty in an estimable contrast. A missing definition or failed execution supplies no estimate at all. Keeping that distinction is necessary before interpreting labor differences or the explanatory evidence that follows.

## Labor-market responses

### Employment, work intensity and earnings

The first empirical question is where a labor-market response appears. A change in the probability of positive ATP contributions concerns the extensive margin under the source definition. A change in contributions among those with positive payments concerns the conditional intensity proxy. Those who remain in work after a loss need not be the same composition as those who would have worked without it, so the conditional measure cannot be read as an hours response for an invariant group of workers. Unconditional ATP in Figure B6 supplies the source's complementary measure including nonworkers, rather than allowing the conditional margin to stand for total labor supply.

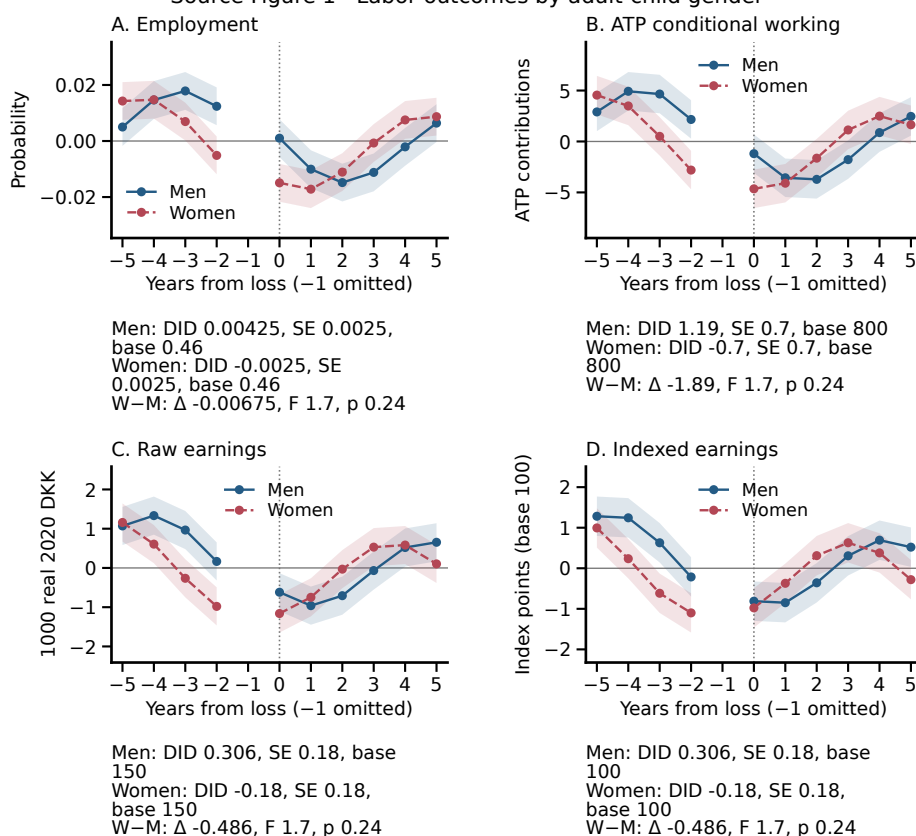
Earnings add another dimension. Their currency scale describes the absolute change in annual labor income, including self-employment. Their indexed scale describes that change relative to a baseline that differs by sex. Equal currency losses can therefore imply unequal relative losses if baseline earnings differ. A larger indexed response for women would not by itself show a more severe psychological effect or a larger hours reduction. The employment and ATP panels are needed to characterize the adjustment, while the rank analysis in Figure B8 asks how position within the earnings distribution changes.

Figure 1 brings these margins together: *[pending]*. The pre-event coordinates describe the observed treated-control relationship before the loss; the event and later coordinates distinguish an immediate change from one that accumulates or persists. If an earnings response accompanies little employment movement, the evidence points toward adjustment within the earnings distribution of workers and other income margins rather than only exit. If employment and earnings both change, the evidence includes an extensive-margin component, while leaving its contribution to total earnings unidentified. Mixed paths remain substantively meaningful because these measures have different denominators and capture different aspects of work.

The full labor interpretation, including magnitudes, persistence and uncertainty, is *[pending]*. The planned five-year horizon establishes the scope of later observations; it is not a lifetime horizon. A continuing response at its end would not establish how long the effect lasts beyond it, and an early change followed by recovery would not imply that the intervening loss was inconsequential. Source pooled summaries can

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure 1 · Labor outcomes by adult-child gender



Pooled post period remains unresolved (A03). Contrasts: W-M = women-men; F-Mo = father-mother.

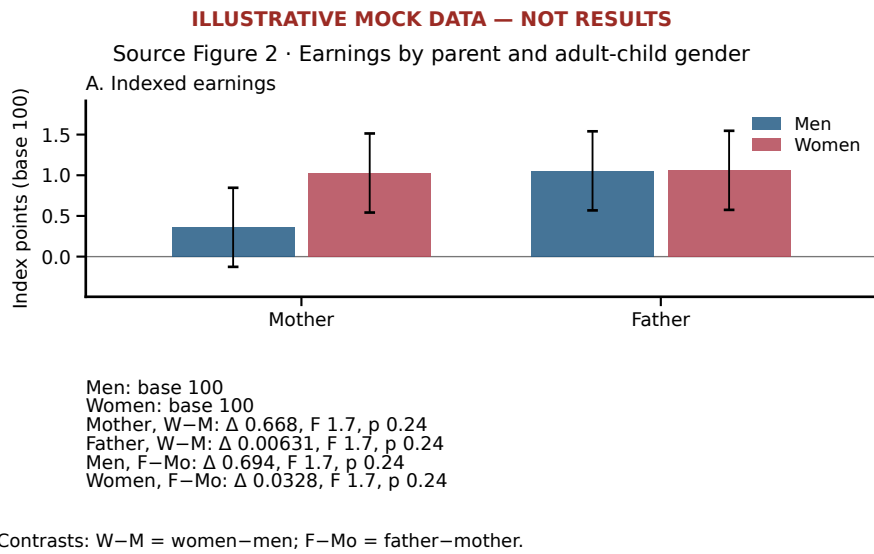
**Fig. 1** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 1: Labor outcomes by adult-child gender. A: Employment (Probability). B: ATP conditional working (ATP contribution units). C: Raw earnings (1000 real 2020 DKK). D: Indexed earnings (Index points; baseline 100). Actual results and availability: *[pending]*. The source matched population is adults aged 25–50 at sudden first loss in 1985–2014 and their assigned controls. Event time is years from loss, with -1 omitted. Bars/bands retain source 95% intervals and individual-by-match clustering; pooled annotations retain the unresolved post period (A03).

complement the profiles only where their post-period convention is determined. SI S4 retains the additional ATP, rank and parent-specific dynamic evidence needed to read the main panels together.

## Which parent is lost, and which adult child responds?

Mother-first and father-first deaths may alter different relationships and forms of support. The study separates these losses within women and men because pooling them could conceal differences associated with the deceased parent's role and with the adult child's circumstances. The parent-gender distribution also differs across sudden and nonsudden deaths in the source population description. A parent-specific comparison thus contributes to interpretation without assuming that mothers and fathers who die first constitute otherwise identical populations.

Figure 2 contains the mother/father-by-adult-gender earnings contrasts: *[pending]*. Its indexed coefficients compare responses relative to the appropriate baseline, while the source joint comparisons retain their uncertainty. The supplemental dynamic panels ask whether an aggregate difference reflects a different onset, persistence or broader profile. These dimensions matter for subsequent explanations: the loss of practical help and a change in treatment could have different timing, yet neither is uniquely identified by parent or child gender.



**Fig. 2** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 2: Earnings by parent and adult-child gender. A: Indexed earnings (Index points; baseline 100). Actual results and availability: *[pending]*. Mother-first and father-first contrasts are shown separately for men and women in the main matched population. Indexing uses the sex-specific pre-event mean. Source 95% intervals use individual-by-match clustering. The pooled post-period ambiguity A03 remains.

A difference between maternal and paternal losses can be compatible with differences in attachment, informal support or the circumstances of the surviving parent. A difference between women and men can also reflect their initial employment and family positions. None of these contrasts measures an intrinsic response to a parent's

gender. They describe which relationships carry the observed labor response in this population. The next question is whether that response is accompanied by changes in health-related treatment, and what those measures can reveal beyond earnings alone.

## Health and the timing of earnings changes

### Treatment responses and the meaning of medical contact

Health is a plausible link between bereavement and work, but it requires its own evidence. The original article connects this question to research on health and employment, including Stephens and Toohey (2022), and to the broader bereavement literature. A reduced ability to concentrate or sustain work may coexist with continued employment. Alternatively, changes in family responsibilities may affect earnings without producing recorded treatment. The health analyses therefore ask whether medical contacts and prescriptions respond to the loss, not whether an earnings decline can simply be renamed a health effect.

Figure 3 combines psychologist visits, any mental-health prescription, any opioid prescription and nonpsychiatric hospital visits: *[pending]*. Each remains in its own units and eligible observation period, with mother/father contrasts by adult sex. The mental-prescription classification includes the source ATC groups N05, N06A, N06B and N06C; opioid prescriptions use N02A. Figure B9 retains alcohol-treatment prescription counts and GP visits, while Figure B10 retains the dynamic health profiles. The private-psychiatrist claim has a separate pending disposition, *[pending]*, because the paper’s private-service target and the code’s combined measure are not interchangeable.

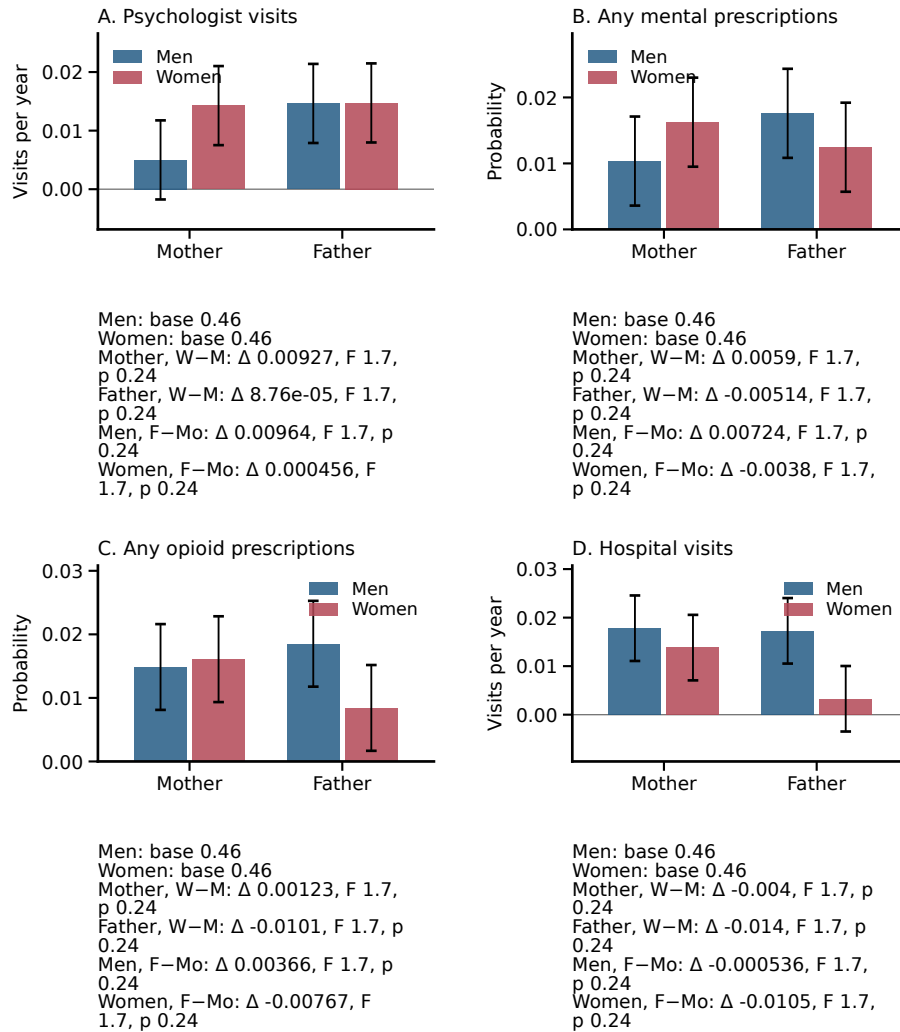
These measures allow several readings that must remain distinct. More psychologist contact can reflect a response to distress and successful access to assistance, rather than a direct scale of illness severity. Mental prescriptions reflect treatment or dispensing, not proof of ingestion or a particular diagnosis for each recipient. Opioid use has clinical as well as possible misuse-related interpretations; the original paper’s concern about substance use does not make every prescription an observed case of substance abuse. Alcohol-treatment records likewise indicate treatment rather than the prevalence of all alcohol problems. Baseline differences in service use can make absolute and proportional treatment changes differ across sexes.

General medical contact poses a further rival explanation. A parent’s fatal illness may prompt the adult child to seek screening or monitoring even without a deterioration in their own underlying health. The source separates screening from nonscreening hospital visits using its Z00/Z01 definition, implemented through the recorded diagnosis codes described in SI S5. A response concentrated in screening would be compatible with changed vigilance. A response in nonscreening encounters would be harder to explain by those screening categories alone, but would still not measure latent disease independently of help-seeking and clinical classification. The remainder category is not a verified inventory of new illnesses.

The health interpretation remains *[pending]*. Reading services, prescriptions, hospital categories and their timing together can strengthen or limit a health-related account. It cannot establish that all health needs are observed, that the same patients

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure 3 · Health-service and prescription effects



Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 3** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 3: Health-service and prescription effects. A: Psychologist visits (Visits per year). B: Any mental prescriptions (Probability). C: Any opioid prescriptions (Probability). D: Hospital visits (Visits per year). Actual results and availability: *[pending]*. Mother/father contrasts are separate within adult sex. Psychologist services begin in 1990, prescriptions in 1995, and hospital encounters cover 1994–2018; samples remain outcome-specific. Source 95% intervals and individual-by-match clustering apply. Treatment use is not a direct diagnosis; A03 remains for pooled effects.

account for each series, or that these health changes caused an observed earnings response. Differences in data coverage add another reason not to infer an individual pathway by comparing two aggregate figures. That gap motivates the joint-timing analysis.

## Persistent declines, new onsets and co-occurrence

An aggregate earnings profile can remain depressed because the same people continue on a declining trajectory or because different people begin declining in successive years. The source addresses that distinction with an indicator for a year-to-year earnings fall of at least 5% and an indicator for the onset of such a fall, defined as a decline not present in the previous year. The first is an annual state of declining earnings, not a measure of remaining below the pre-death earnings level. The second marks entry into that state. Reading the two prevents persistence in the aggregate curve from automatically becoming a claim that new declines arise throughout follow-up.

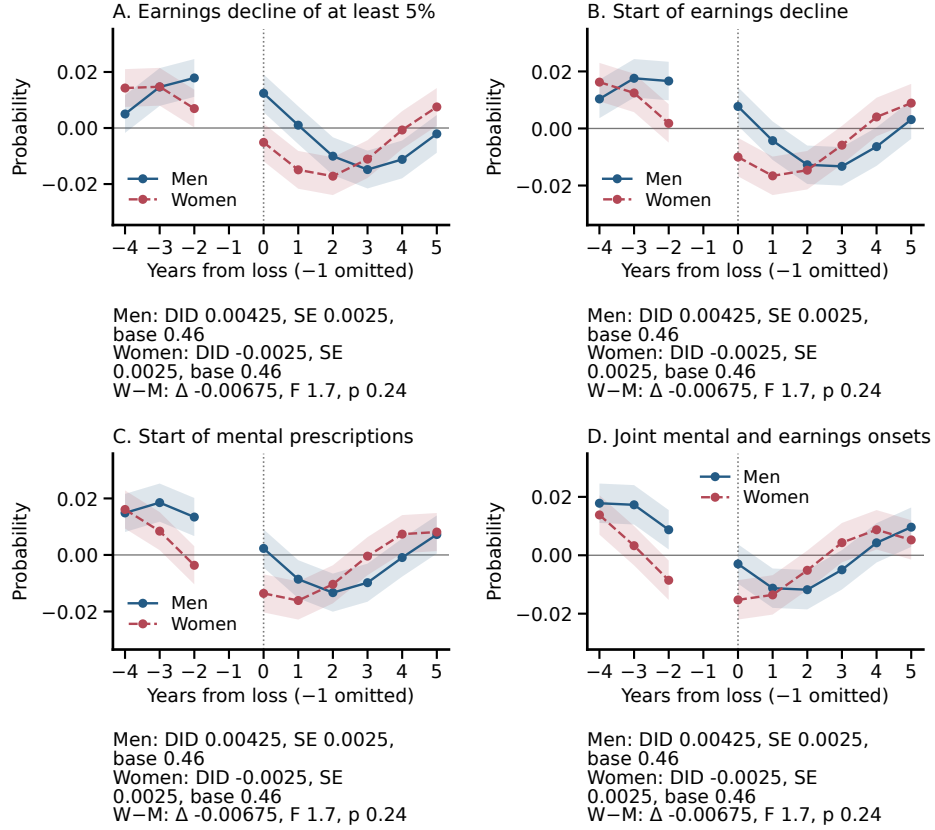
The health counterpart is entry into recorded treatment following a year without that treatment. It is not necessarily a first lifetime diagnosis. The joint indicator then asks whether earnings-decline onset and mental-prescription onset occur in the same year for the same observed adult. Figure 4 retains all four source panels: *[pending]*. The outcome window begins at event year  $-4$  because the lag construction uses the preceding year; SI S5 explains the additional earnings history and complete health-era restrictions. This timing design concerns annual co-occurrence rather than an ordering of health and earnings events within the year.

A joint response would establish information that separate labor and health responses do not contain: their overlap within observed individual-years. Under the source difference-in-differences assumptions, a faithful joint-outcome estimate can address the effect of parental loss on that co-occurrence. It still cannot identify the effect of health on earnings. Both may respond to the same bereavement or family disruption, and the annual data do not isolate a sequence in which health changes independently of the other consequences of the loss. Adding post-loss health as a control in an earnings regression would condition on something potentially caused by the treatment. It would not turn these analyses into a causal decomposition.

The source's four immediate-year conditional-share comparisons remain *[pending]*. They divide the joint-onset effect by the earnings-onset or mental-onset effect within sex, rather than identifying a share of lost earnings caused by health. Their event-year mapping remains ambiguous, and an undefined or unavailable denominator cannot yield a share. The figure captions' pooled-equation references also conflict with the dynamic program, retaining C05. The full timing interpretation is *[pending]*, with Figure B12 providing psychologist and opioid onsets and the original hospital-count interaction. Those extensions can show how specific the overlap is to a particular treatment measure; they do not remove the limits of mediation identification.

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure 4 · Earnings decline and mental-treatment onset



Caption Eq4 versus dynamic code Eq2 remains C05. Pooled post period remains unresolved (A03). Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 4** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 4: Earnings decline and mental-treatment onset. A: Earnings decline of at least 5% (Probability). B: Start of earnings decline (Probability). C: Start of mental prescriptions (Probability). D: Joint mental and earnings onsets (Probability). Caption Eq4 versus dynamic code Eq2 remains C05. Actual results and availability: *[pending]*. The matched health-era construction retains 1995–2018 and displays event years -4 to +5, with -1 omitted after lag construction. Both sexes and source 95% intervals with individual-by-match clustering remain. Joint onset is not a mediated share; the immediate-year ratio mapping remains A03.

## Family support after the first parental death

### Young children and the loss of informal help

A parental death may remove support that allowed the adult child to work, even if their own health does not change. The childcare question concerns the deceased

parent’s role as a grandparent. Families with a preschool-age child are likely to face different care demands from families without one, and the institutional setting makes occasional help outside formal care especially relevant. The source therefore compares earnings responses by young-child status and refines the comparison by the youngest child’s age. This is a comparison of circumstances in which lost help may matter, not a comparison of observed quantities of grandparental care.

Figure 5 contains the earnings contrasts by young-child status, parent gender and adult sex: *[pending]*. The classification is fixed at the treated adult’s event year and carried to the matched pair; it differs from the pre-event child-status information used in matching. Earnings are indexed within the source sex-by-child group, so variation in relative effects must be read alongside the groups’ baseline scales. Figure B13 refines youngest-child ages to 0–5 and 6–14. A larger response among adults with younger children would be compatible with greater dependence on practical and emotional help, but could also accompany differences in life-course labor trajectories or distress.

The population of the comparison remains consequentially unresolved. The journal describes adults with and without young children, whereas the program removes childless adults before forming groups. The latter would make the comparison with parents of older children, changing what the result says about adults without care demands. C06 retains that disagreement. Neither silently excluding childless adults nor adding a new childless estimate would reproduce the frozen design. The refined child-age analysis remains within its registered scope and associated source dispositions.

Formal-care enrollment asks a complementary question: whether the family’s use of an observed alternative changes after the grandparent’s death. Figure B14 retains enrollment of the linked youngest child in young-child and older-child care, including after-school provision. If enrollment rises alongside an earnings response, the pattern is compatible with substitution that does not necessarily replace all lost support. If enrollment changes little, the family may already be enrolled, need help outside covered hours, or adjust work and other arrangements instead. Thus little enrollment movement would not establish that grandparental assistance had been irrelevant. Conversely, an enrollment increase alone cannot show that a grandparent previously supplied measured hours of care.

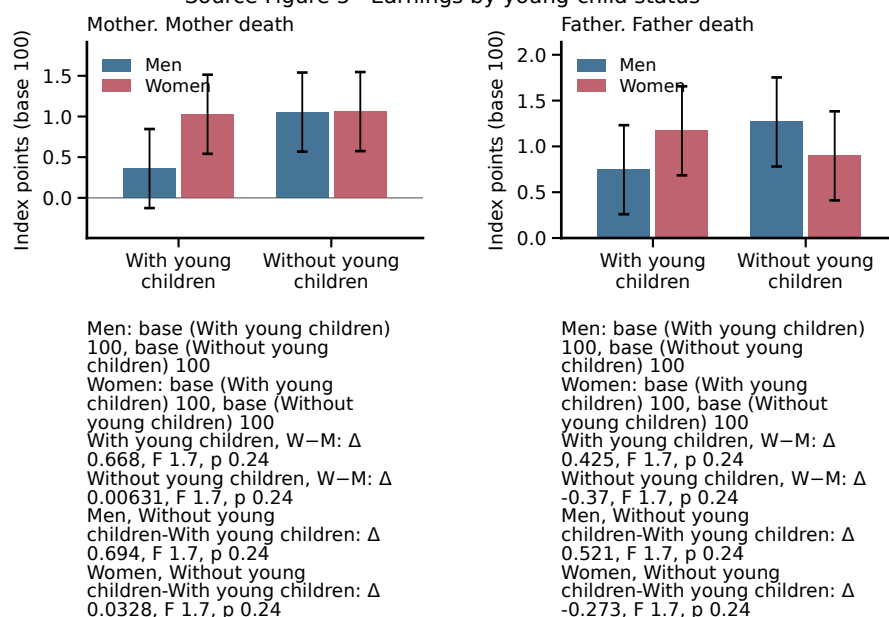
The childcare interpretation is *[pending]*. The enrollment analysis’s later observation period and narrow eligible populations mean it does not mechanically explain the full historical earnings comparison. Its age-five wording, youngest-child identity and treatment of age-ineligible follow-up remain A06. SI S6 keeps these measurement details next to Figures B13–B14. The family-support account also raises the question of help flowing in the other direction: what happens when the surviving parent loses their partner?

## Care released, care added and the surviving parent

A first parental death can reduce some obligations while increasing others. The deceased parent may have required help before dying; ending that care could release time for work. But the surviving parent may lose their own caregiver and emotional support, creating new demands on adult children. Restricting attention to sudden first deaths makes a long period of terminal care less central than in an unrestricted

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure 5 · Earnings by young-child status

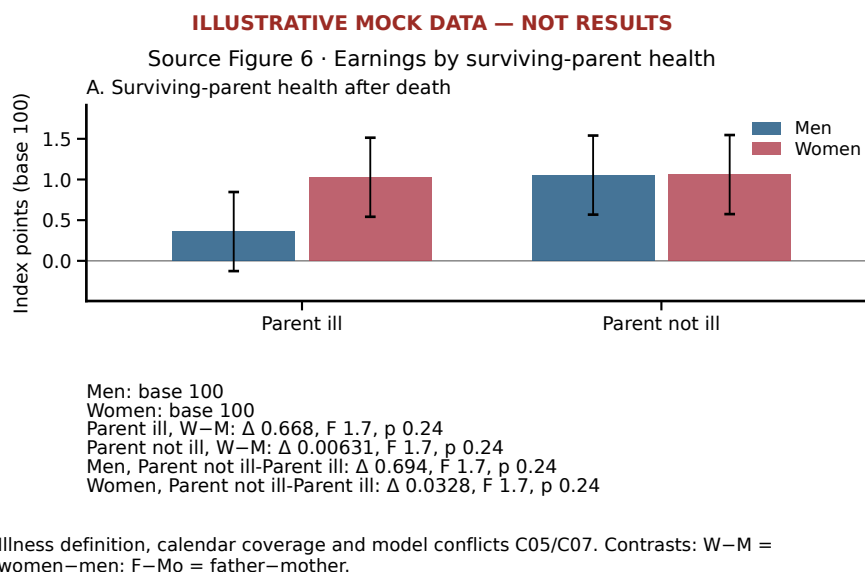


Eligibility conflict C06 remains unresolved. Contrasts: W–M = women–men; F–Mo = father–mother.

**Fig. 5** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 5: Earnings by young-child status. Mother: Mother death (Index points; baseline 100). Father: Father death (Index points; baseline 100). Eligibility conflict C06 remains unresolved. Actual results and availability: *[pending]*. The treated adult's child group is fixed at event year 0 and carried to the match; each parent panel retains men/women with/without young children. Earnings use source sex-by-child-group baseline means. Source 95% intervals use individual-by-match clustering; pooled timing remains A03.

death sample. It does not measure prior care or establish that it was absent. The source's emphasis on spouses as caregivers also makes the surviving parent's circumstances relevant rather than allowing the first-death restriction to rule out eldercare altogether.

The original comparison separates earnings responses by the surviving parent's health, restricting attention to adults without young children to reduce overlap with the most immediate childcare demand. Figure 6 contains that pending evidence: *[pending]*. If a response differs with the surviving parent's health, it is compatible with differences in responsibilities toward that parent. It may also reflect distress about the surviving parent's condition or a reduction in help that parent could still offer. Removing young-child households narrows one competing account; it does not isolate care hours or a pure eldercare effect.



**Fig. 6** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure 6: Earnings by surviving-parent health. A: Surviving-parent health after death (Index points; baseline 100). Illness definition, calendar coverage and model conflicts C05/C07. Actual results and availability: *[pending]*. The source restricts to adults without young children and their matched controls, with ill/not-ill surviving-parent groups by adult sex. Earnings use sex-specific pre-event means. Source 95% intervals use individual-by-match clustering. Post-event health is selected, and A03 remains for pooled timing.

The main health classification uses information after the first death. It is therefore a post-event characteristic, potentially related to bereavement and subsequent family behavior. Differences between those illness groups are selected comparisons, not the result of randomly assigning care needs. Figure B15 retains the source pre-loss health comparison to ask whether the pattern depends on circumstances already present before the loss. The two classifications serve different interpretive roles. A prior-health association would reduce reliance on the post-event grouping but would not remove differences between families or identify the amount of care provided.

The source text and program also differ in what constitutes prolonged surviving-parent hospital treatment and in the years over which it is observed. These differences can change illness-group membership and therefore the meaning of the earnings comparison. SI S6 preserves the exact accumulation, threshold, calendar and model discrepancies. The illness-share target is *[pending]*; the source's published share is not assumed for this population.

Residence and mental-health evidence further qualify the care account. Figure B16 asks whether adults live in the same NUTS3 region as the surviving parent, including its unresolved pre/post illness split. Proximity could facilitate care, but being in the same region does not measure visits or help; lack of a residential response would not exclude changes in care among families already nearby. The source also reports

a mental-prescription comparison by surviving-parent illness, retained as *[pending]* because its full specification is missing. The integrated surviving-parent interpretation is *[pending]*. These pieces can constrain the plausibility of competing explanations while leaving their relative contributions unresolved.

## Household responses and alternative explanations

### Spouse earnings and the household reach of a loss

Support from a parent can benefit both members of the adult child’s household. A grandparent’s death may therefore affect the earnings of a daughter-in-law or son-in-law as well as those of the biological child. The spouse analysis asks a distinct question about that spillover. An effect on a partner with young children would extend the consequences beyond the directly bereaved adult, while remaining compatible with changes in childcare, household work allocation or shared emotional burden. It cannot be assumed to measure only one of these responses.

The source constructs a separate matched population of married or cohabiting individuals who lose a first parent-in-law, with comparison individuals who do not lose a parent-in-law within the window. It also seeks comparability in their own parental circumstances. Figure B18 in SI S7 contains the spouse earnings evidence, *[pending]*, and its interpretation is *[pending]*. The event-year partner, spouse history, young-child restriction and source spouse-specific baseline all matter. This estimate cannot be obtained by relabeling the main adult-child match as a spouse comparison.

The spouse comparison also has unresolved population and model correspondence, explained in SI S7. It concerns eligible partners with young children, rather than all partners or total household income. Adding a biological child’s and a spouse’s coefficients would not establish household losses because the estimates come from separately constructed populations.

### Parental wealth, family formation and work choices

A wealth-based explanation starts from a different implication of parental death: a transfer may change the resources available to an adult child and hence their work choices. The first-death setting reduces the likelihood that all parental assets are immediately distributed, but does not remove that possibility. Table B4 in SI S9 relates the earnings response to pre-death parental net wealth: *[pending]*. A wealth interaction is informative about how the response varies with the measured assets, while remaining an indirect test of an inheritance account because actual bequests are unobserved.

The wealth interaction remains unresolved because both the scale and the ownership of the baseline assets can differ between the source table and program. SI S9 explains those choices. The own-asset, deceased-parent-only and surviving-parent-only variants remain *[pending]*, *[pending]* and *[pending]*, with incomplete source specifications. An imprecise interaction would not establish absence of inheritance effects. The inheritance interpretation is *[pending]*.

Bereavement could also change the organization of an adult’s own family. The source uses cohabitation and the number of children to examine that alternative. Figure B17 provides those outcomes, *[pending]*, with interpretation *[pending]*. A child-count stock is not a birth rate: it summarizes the source linked children born by the observation year. A cohabitation indicator likewise does not describe all changes in relationship quality or responsibilities. Even if their responses run against a simple explanation in which a larger family reduces labor supply, these outcomes do not identify what earnings would have been had family behavior remained fixed.

Finally, the source considers whether bereavement changes preferences for job type or time away from work, drawing on its cited literature about values and adjustment after loss. Occupational switching, firm changes and public-sector employment are observable choices relevant to that account. Table B5 in SI S9 retains them, *[pending]*, with interpretation *[pending]*. These outcomes are conditional on work and do not measure all preferences or changes within a job. An unchanged employer or occupation can coexist with different hours or responsibilities, while employment selection limits a comparison among workers. The occupation-history ordering remains A10; it cannot be repaired by substituting an adjacent-year switch. Supported firm and sector components retain their separate dispositions.

The household and alternative-outcome evidence therefore tests the breadth of a possible explanation without supplying a residual mechanism by elimination. Limited evidence for wealth or job switching does not establish that health must account for the remaining response. The robustness comparisons next ask whether the labor contrast itself is sensitive to its population, timing and counterfactual construction.

## Robustness and scope of the comparison

Several source comparisons address specific objections to the main design. Matching with parental inpatient history would ask whether observed health differences between parents account for the labor contrast beyond parental age. Matching on adult or parental health in the later data era would examine a related concern where those measures exist. Restricting to a gap of more than five years between parental deaths would ask whether a second loss contributes to the main follow-up response. Linear age/year controls address dependence on the fixed-effect specification. Their retained evidence slots are *[pending]*, *[pending]*, *[pending]* and *[pending]*. The source does not fully specify the exact rematching histories, combinations, reused samples or complete output sets, so these are unresolved targets rather than a license to design a new robustness battery.

The future-treated comparison addresses the possibility that controls’ parents remain alive but are ill. If that illness reduces controls’ labor supply, a treated-control contrast may understate a negative response to sudden parental death. The source supplement motivates a comparison with adults who experience the same sudden first loss six years later, using five-year birth-cohort groups. In that construction, future bereavement makes the comparison more similar in the type of eventual loss, while

shifting the timing outside the focal follow-up. It still requires comparable counterfactual trajectories; knowing a later death date does not establish random timing or eliminate all prior illness.

Figures B21–B22 preserve the alternative employment, earnings and parent-gender comparisons in SI S8. Their evidence is *[pending]* *[pending]*. The source example uses years  $-3$  through  $+5$ , the code uses  $-5$  through  $+5$ , the captions and code differ in clustering, and the alternative earnings program uses different winsorization tails despite language about the same specifications. C10 retains all three discrepancies. No version is chosen because its curve resembles the published one or its confidence intervals are narrower. Agreement between estimable versions would inform sensitivity to the comparison population; disagreement would require a qualified account of what differs, and overlapping intervals alone would not establish equivalence.

Geographic, cause, age and calendar comparisons address the scope of the labor response. Nearby parents may provide more practical help, but proximity may also reflect attachment and sorting between families. Figures B19–B20 retain the binary and four-category geographic comparisons, *[pending]* *[pending]*, without turning region into a distance in kilometers or an instrument for care. The five-versus-eleven-region discrepancy, contrast-model mismatch and reported test-statistic problem remain C09. Table B6 retains cause, parent-age, adult-age and period comparisons: *[pending]*. Similarities across sudden causes would concern the included causes; they would not establish the effect of nonsudden loss. Age contrasts can reflect care needs and the life-course timing of bereavement, while period contrasts can reflect more than a single institutional change. The printed age thresholds and code-recomputed medians remain A07.

The sudden and nonsudden descriptive profiles provide a final perspective on scope. The source supplement uses them to discuss how prior illness may begin affecting an adult before a nonsudden death. That can change what an event-centered response measures, even if the post-death paths appear similar. Figure B23’s evidence is *[pending]*, and the descriptive comparison is *[pending]*, subject to C04’s scaling boundary. It cannot identify equal causal effects across death types. These limits carry into the broader synthesis: the family and health mechanisms may be relevant elsewhere, but neither a descriptive resemblance nor extensive Danish public services establishes a universal effect size.

## Discussion

The central question is how adult parental loss changes work in a setting where public services coexist with substantial family relationships. The labor evidence describes the adjustment to be explained; health, timing and support evidence then restrict the set of plausible accounts. A persistent earnings change accompanied by employment movement would differ from a response confined to conditional contributions or the income scale. Parent-by-child gender contrasts would further locate the response without identifying its mechanism. The synthesis must therefore retain the outcome vector, its uncertainty and the scale on which comparisons are made, rather than reducing the study to a single earnings coefficient.

Health-related evidence and family-support evidence are potentially complementary. Recorded treatment may respond to grief or new family burdens, and losing informal support may make it harder to combine care with work. Joint onset can establish that earnings and treatment changes overlap in individual-years, while child-age and enrollment patterns speak to the circumstances of care. Surviving-parent health and residence ask whether support is being reorganized in another direction. A coherent pattern would make an account more plausible; discordant patterns would limit its reach. None of these outcomes identifies the mediated share of earnings attributable to distress, childcare or eldercare.

The household analyses also prevent an exclusively individual interpretation. A response among spouses would concern spillovers through a shared household, while family formation and job choices would describe other adjustments that can accompany bereavement. Parental wealth may moderate resources without revealing actual transfers. These comparisons cannot be used to declare a mechanism absent merely because an interaction or secondary coefficient is imprecise. The source supplies no threshold that turns a small or statistically uncertain estimate into proof of no economically meaningful effect. Nor can unexplained variation after inspecting these outcomes be assigned automatically to a favored account.

The integrated evidence is *[pending]*. Its eventual interpretation is confined to the source-defined estimates that can be produced with their required uncertainty. A partial set may answer a narrower question while leaving other explanations open. A boundary caused by missing input or source correspondence neither confirms nor contradicts a published result. This distinction is especially consequential when an unresolved matching choice affects a whole family of comparisons, or when the illness, child-status and wealth definitions change the meaning of a subgroup. The main identifying assumptions remain necessary even after every technical ambiguity is clarified.

The scope is sudden first parental death during prime working ages in the source Danish period. It does not directly cover loss in childhood, the death of a second parent, all nonsudden deaths, retirement-age responses or outcomes beyond the registered horizon. The source's international generalization argument is retained as contextual material, *[pending]*. Weaker formal support elsewhere could make family disruption more consequential, but differences in work, family organization and services also prevent a Danish estimate from becoming an established lower bound for other countries. No cross-country analysis or policy experiment belongs to this replication.

The original article discusses grief support, psychological screening, leave around bereavement and care outside regular hours as possible responses. These are implications suggested by the substantive problem, not interventions evaluated by the design. Even evidence of persistent labor or treatment effects would not identify their effectiveness, optimal form or cost. The contribution of a source-faithful replication would be to clarify the empirical pattern and the limits of its interpretation before such policy claims are made. In the present manuscript all replication evidence remains pending.

## **Ethics, access and reproducibility**

This manuscript uses the final AER article, its journal-linked supplement, supplied public author programs and the frozen replication documentation. Its September 10, 2026 amendment changes exposition and computational planning while preserving all registered scientific choices. No confidential observations or replication outcomes have been accessed. Published estimates remain attributed comparison targets. The actual source access and knowledge history are recorded in the provenance handoff.

The prior sealed World and September 8 editorial revision remain unchanged. This version contains reference implementations and labeled hypothetical exhibit previews. Synthetic inputs test implementation and presentation; they supply no empirical evidence. Actual test and rendered-inspection coverage are documented in the accompanying provenance handoff. All empirical slots remain pending.

## Supplementary Information

This SI accompanies the main argument in the same manuscript. Source supplementary Figures B1–B23 retain machine identifiers figure7–figure29; a machine number is six larger than its source B-series number. Tables retain the source labels Table 1 and B1–B6. Every authoritative empirical cell is unfilled. The visibly labeled table and figure previews demonstrate layout only; their numbers are not observations. An em dash in an empirical shell is not zero, and a pending panel is not a null result. Source-defined uncertainty accompanies estimable evidence; missing-source and input dispositions remain explicit. The detailed source ledger and result map remain the authorities for target correspondence.

### S1. Data construction, measurement and analytic populations

#### Family linkage and the event clock

The population construction uses historical FAIN records for 1980–1984, taking the referenced year minus one, and BEF snapshots thereafter, including the source year-end selection. The relevant person and family fields are PNR, MOR\_ID, FAR\_ID, FOED\_DAG, KOEN, ALDER, E\_FAELE\_ID, CIVST, KOM and REG. Source birthday, gender and parental information are carried through the historical panel according to the registered construction. These rules describe a particular vintage of linked records. A different pseudonym namespace or an unverified historical field cannot be treated as equivalent merely because its label looks similar.

The linked death sources are the original DODSAARS and DODSAASG cumulative extracts, using the source cause49 and death-date fields. The earlier of the mother’s and father’s dates defines first loss. The code assigns the mother on tied dates, while the simultaneous-death scientific treatment is unresolved under A11. The final cause filter and event-age limits are those stated in the main text. Broader intermediate pools are followed by the explicit final commands; the frozen audit distinguishes these staged exclusions from genuine paper-code conflicts.

A valid main panel contains one person-year at every event time from  $-5$  through  $+5$ . Both parental links, usable birth information and source pre-death presence are required. The candidate-control construction assigns a pseudo-event year, requires the first parental death after event year  $+5$  or no observed death, and uses its own source age and parent-presence rules. The complete matching audit retains the distinction between candidate ages and final treated eligibility. An unknown later death is not evidence of survival beyond the observed period. A second death within treated follow-up remains permitted in the main source comparison; its separate restriction is not imposed globally.

Population displays need not use exactly the final matched labor sample. The introductory cumulative distribution is constructed among first deaths in the source period, before its display suppression. Some baseline and age displays use later family or panel restrictions. These differences change denominators, so a common count cannot be assumed across exhibits. The source histogram suppression of cells below five is retained, without adding a new disclosure threshold or interpreting suppressed cells as zeros.

## Labor measures and baseline scales

Labor earnings use the historical IND employment-plus-self-employment fields, ERHVERVSINDK\_GL for 1980–1986 and ERHVERVSINDK\_13 thereafter, under the source mappings. The original inflation factors are required for 2020 prices. Source p1/p99 earnings winsorization is applied at each analysis’s specified stage, after its stated restrictions; it is not one universal cleaning pass shared by every table. Raw graphs use thousands of real 2020 DKK. The pre-existing table-shell labels below retain their exact wording; where a raw-DKK label differs from the source display’s thousands, the binding’s unit must be consulted before a numeric value is attached.

For the indexed outcome, the source transformation is  $100Y_{it}/\bar{Y}_{-1,g}$ , where  $g$  denotes the source sex or sex-by-subgroup baseline population and the mean includes its prescribed treated and control records. Group-specific indexing in childcare and geography is distinct from sex-specific indexing elsewhere. A changing denominator across groups changes the meaning of a relative comparison. The annual rank outcome instead uses mean tied ranks within the source population aged 20–55 and the transformation  $100(r - 1)/(N - 1)$ . It is neither a within-sex rank nor the earnings index expressed differently.

ATP employment is positive contributions. The source treats negative or missing ATP as zero within the applicable covered construction, rescales the early 1980/1981 values by 1167/432, and caps the intensive measure at each year’s 99th percentile. The conditional measure retains positive observations; the unconditional one includes zeros. Genuine missing coverage cannot be recoded as a non-event. Table 1’s conditional contribution row and Table B3’s unconditional contribution row therefore have different denominators even though the original article describes both as an intensive margin.

Main-job and sector information comes from the source IDAN/IDAS selections and historical public-ownership rules, with AKM supporting occupation and employment-type construction. These are vintage-specific fields and priorities, not interchangeable job records. The source income and employment histories influence matching as well as outcomes. An unavailable mapping can consequently affect the comparison sample before it affects a displayed dependent variable.

## Family variables, geography and service coverage

The source constructs children from parental links and birth dates, cohabitation from the relevant partner identifier, and parental marriage from the registered civil-status codes. Descriptive child-count caps, spouse-count caps and the family-response count remain separate consuming rules. In matching, a youngest-child age above 20 is coded 21 and a childless adult is represented by the sentinel 22. These are categories for construction, not actual child ages. An ambiguous identity among equally young children matters for enrollment and is not settled by choosing one child arbitrarily.

Education uses the source UDDA qualification and effective-date information, carried forward and mapped with the author crosswalks into the registered levels. Geography combines municipality, five-region information and the original eleven-region crosswalk, with detailed geography beginning in 1985. The historical map

and missing specialty, education and inflation inputs remain U02. No updated regional boundaries, inferred qualification mapping or replacement service taxonomy is introduced.

Medical contacts, prescriptions and childcare each require an observed register-year and their own aggregation. Source service and prescription absence can be zero only within verified coverage. Different start years and the hospital endpoint create different analytic windows: the source outcome restrictions are retained instead of making all analyses complete cases on every register. The exact current extracts and common person-key correspondence remain U01, including the LMDB PNR12 versus source PNR issue. Source construction order, missingness rules and unresolved collisions are documented in the frozen data contract; the exposition here does not resolve them.

## S2. Matching, models and statistical interpretation

### Matching correspondence

The journal exact-match description and the public matching program are both retained as source evidence. The code divides positive income into sex/year-specific terciles conditional on positive ATP and puts other observations into category zero at years  $-3$ ,  $-2$  and  $-1$ . It combines the latter two years' categories with young, older and childless status into 34 groups, sometimes sharing a group across child-status categories. Within sex, event year and group, it uses categorical Mahalanobis distance over the earliest earnings category, five-year adult and parental age bins, child count, cohabitation, region, public employment, education and youngest-age category. The full three matrices and all source category assignments remain in the unchanged matching specification.

The program's constant placeholder outcome requests neighbor indices; it is not an analyzed outcome. Sorting, seed 123456 and selection of the first generated match index document source mechanics. They do not pin all consequential ties without software semantics. Reused controls are relabeled under source matching logic; a purely isomorphic label change is different from selecting a different person. A01 retains that distinction. C01 retains the larger discrepancy between coarsened groups and the journal's exact earnings/young-child groups and three-year employment history. Neither discrepancy can be resolved by comparing resulting balance or choosing the more attractive estimates.

### Source descriptive and matched equations

Let  $i$  denote an adult,  $y$  a calendar year and  $t$  the assigned event time. The source descriptive equation, estimated separately by sex without a control group, is

$$Y_{ity} = \sum_{\substack{k=-5 \\ k \neq -1}}^5 \delta_k \mathbf{1}\{t = k\} + \text{AgeYear}_{iy} + \text{EduYear}_{iy} + \varepsilon_{ity}.$$

Age-by-year and education-by-year terms allow those observed relationships to vary across calendar time. The omitted event year is  $-1$ . The source relative display then scales by a fitted counterfactual mean. Because the prediction and collapse

population is inconsistent with the sex-specific description, the coefficients' existence would not settle the plotted percentage target under C04.

For the matched model, let  $m$  denote a match,  $D_m$  its event year,  $k = y - D_m$ , and  $T_{im}$  identify the treated member. Write the shared fixed-effect component as

$$F_{imy} = \alpha_{im} + \lambda_k + \tau_y + \eta_{\text{age}(i,y)}.$$

Here  $\alpha_{im}$  is the individual-by-match effect,  $\lambda_k$  the event-time effect,  $\tau_y$  the calendar-year effect and  $\eta$  the age effect. The source dynamic equation is

$$Y_{imy} = F_{imy} + \sum_{\substack{h=-5 \\ h \neq -1}}^5 \beta_h T_{im} \mathbf{1}\{k = h\} + \varepsilon_{imy}.$$

This restates original Equation 2, with a change in notation only. The coefficient  $\beta_h$  is the treatment-control difference at event time  $h$  relative to the omitted interaction at  $-1$ . The individual and match representation matters when controls are reused. Including common event-time effects does not itself establish the required untreated trajectories; the causal interpretation remains conditional on the main text's assumptions.

The source pooled specification and parent-gender specification can be written, respectively, as

$$Y_{imy} = F_{imy} + \beta T_{im} \text{Post}_k + \varepsilon_{imy},$$

$$Y_{imy} = F_{imy} + \beta_m M_{im} \text{Post}_k + \beta_f P_{im} \text{Post}_k + \varepsilon_{imy},$$

where  $M_{im}$  and  $P_{im}$  identify treated mother-first and father-first deaths. These are expository versions of original Equations 3 and 4, each estimated separately by adult sex. The symbol Post leaves the frozen A03 decision unresolved: source code uses 0–5 inclusive, while the prose refers to five years after death. These equations do not authorize choosing either convention. Age/calendar/individual normalization remains A02, and captions that refer to a different equation from their consuming program remain C05.

## Uncertainty and comparisons

The main source models are unweighted regressions with individual-by-match clustering. Spouse models use spouse-by-match clustering; the future-treated cluster is separately disputed. Descriptive Equation 1 uses robust standard errors. Dynamic source intervals use coefficient plus or minus 1.96 standard errors, whereas the pooled plots use the original coefficient-plot and residual-degree-of-freedom semantics. Source-equivalent endpoints are required rather than imposing one interval formula on every output. Tables that supply standard errors retain those fields without a newly invented interval or test.

Source cross-sex or cross-parent comparisons use the appropriate joint-model covariance. Their differences, F statistics and p-values cannot be reconstructed by assuming independent estimates from separate regressions. Missing software versions

or consequential normalization can therefore block uncertainty or contrasts even if an isolated point coefficient is available. An omitted reference year is marked as omitted, not treated as an estimated zero with perfect precision.

No source-defined power analysis, equivalence criterion or multiplicity adjustment is available to add. The original figure and table uncertainty describes the registered comparisons, and visual pre-event inspection does not become a new numerical acceptance test. The purpose of the full displays is to retain signed, mixed and uncertain findings in their original units. Local no-estimate boundaries concern what can be reconstructed; they are distinct from wide intervals or disagreement with a published point estimate.

### S3. Population composition and the descriptive comparison

Table 1 asks whether adults experiencing sudden and nonsudden first loss differ on the source pre-event characteristics. That comparison helps describe the population to which the main treatment restriction applies. It does not validate the matched design, because its two columns are different death types rather than treated adults and their matched controls. The original baseline, sex and parent-gender breakdowns are retained so that the source population can be described without assuming that a single aggregate mean represents all family relationships.

*Table 1. Baseline characteristics by death type*

Binding: *[pending]*. The baseline is event year  $-1$ . The rows preserve the original order, means, source pooled-variance two-sample comparison and final counts. Proportions use the source eligible individuals, ages are in years, children are counts and conditional ATP is in contribution units. The under-six label versus code's age-at-most-six rule remains C02. Earnings retain their original shell label and unresolved display-unit qualification in S1.

#### ILLUSTRATIVE MOCK DATA — NOT RESULTS

| Characteristic, in source order     | Sudden mean    | Non-sudden mean | Source p-value |
|-------------------------------------|----------------|-----------------|----------------|
| Adult age                           | 42.4           | 42.8            | 0.21           |
| Male                                | 0.265          | 0.280           | 0.24           |
| College or above                    | 0.310          | 0.320           | 0.27           |
| High school or vocational education | 0.445          | 0.440           | 0.30           |
| Below high school                   | 0.245          | 0.240           | 0.33           |
| Cohabitation                        | 0.345          | 0.360           | 0.36           |
| Number of children, capped at five  | 1.00           | 1.20            | 0.39           |
| Age of youngest child               | 8.4            | 8.8             | 0.42           |
| Child under six                     | unresolved C02 | unresolved C02  | unresolved C02 |
| Mother age                          | 68.4           | 68.8            | 0.21           |
| Father age                          | 71.4           | 71.8            | 0.24           |

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

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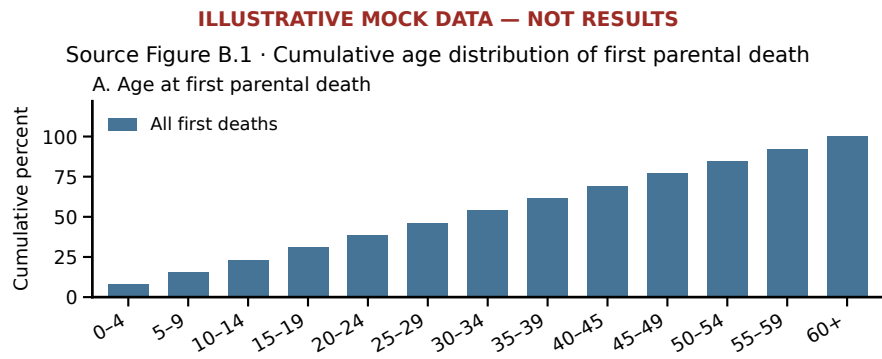
| Characteristic, in source order          | Sudden mean | Non-sudden mean | Source p-value |
|------------------------------------------|-------------|-----------------|----------------|
| Mother married                           | 0.345       | 0.360           | 0.27           |
| Father married                           | 0.245       | 0.260           | 0.30           |
| First-deceased parent age                | 69.4        | 69.8            | 0.33           |
| Employment                               | 0.285       | 0.300           | 0.36           |
| Intensive margin, conditional on working | 716.0       | 727.0           | 0.39           |
| Annual earnings, real 2020 DKK           | 183.0       | 196.0           | 0.42           |
| Observations                             | 1700        | 1800            | 3,500 (total)  |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

The cumulative age distribution and gender composition place this table in context: *[pending]* *[pending]*. Figure B1’s denominator is first deaths during 1985–2014, rather than the number of all people alive at each age. Its exact introductory age-share target cannot be read from grouped bars, and its printed 40–45 label conflicts with the code’s 40–44 bin. Figure B2 describes who the first deceased parent is within the source sample. These are population descriptions, not treatment effects.

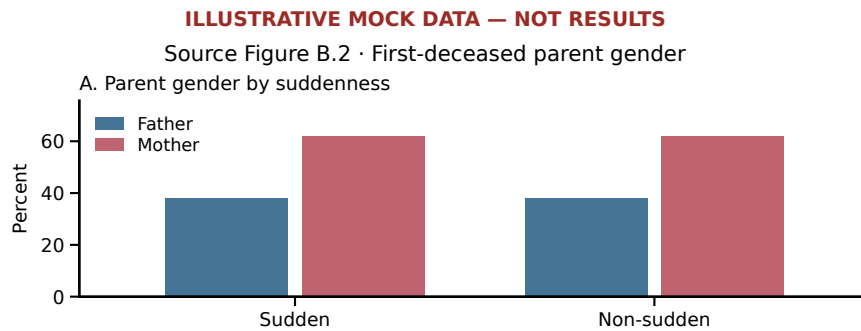
Parental and adult ages are separately informative because a death can occur late in the parent’s life while the adult child still has many working years ahead. Figures B3 and B4 retain the full distributions: *[pending]* *[pending]*. The parent’s age at death is not interchangeable with the age recorded the year before death; C03 retains that discrepancy for the sudden-death histogram. Source-defined bin origins, edges and suppression correspondence remain A04. The population interpretation is *[pending]*.

The uncontrolled sudden-death paths are *[pending]*. Their purpose is to show event timing while exposing the life-course problem addressed by matching. Age and education interactions do not create an untreated comparison group, and the unresolved counterfactual denominator prevents substitution of unscaled coefficients for the intended relative display.

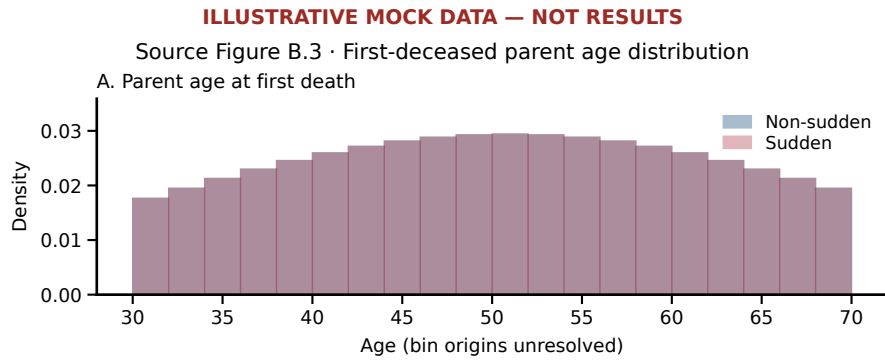


Source denominator is deaths1985–2014, not an age-specific population risk. Source label40–45 versus coded40–44 remains an affected-bin boundary under A04.

**Fig. 7** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.1: Cumulative age distribution of first parental death. A: Age at first parental death (Cumulative percent). Source denominator is deaths 1985–2014, not an age-specific population risk; A04. Actual results and availability: *[pending]*. The source 40–45 label versus coded40–44 category is retained as an affected-bin boundary (A04). All causes are included in the source death-period denominator; the numerator and denominator precede the source suppression of counts below five. This descriptive cumulative distribution has no treatment-effect interval.

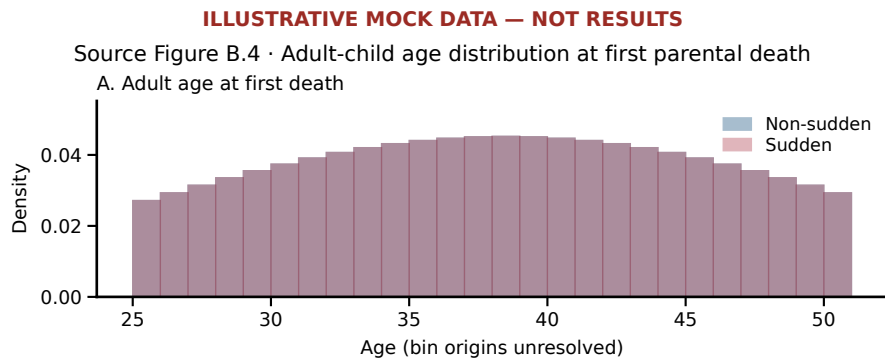


**Fig. 8** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.2: First-deceased parent gender. A: Parent gender by suddenness (Percent). Actual results and availability: *[pending]*. The baseline population comprises first losses in 1985–2014 among adults aged 25–50 with two known parents, separated into sudden/nonsudden deaths. Father/mother shares are descriptive and have no treatment-effect interval.



Source age variable mismatch C03; exact bins A04. Hypothetical bin geometry only; scientific bin origins remain unresolved.

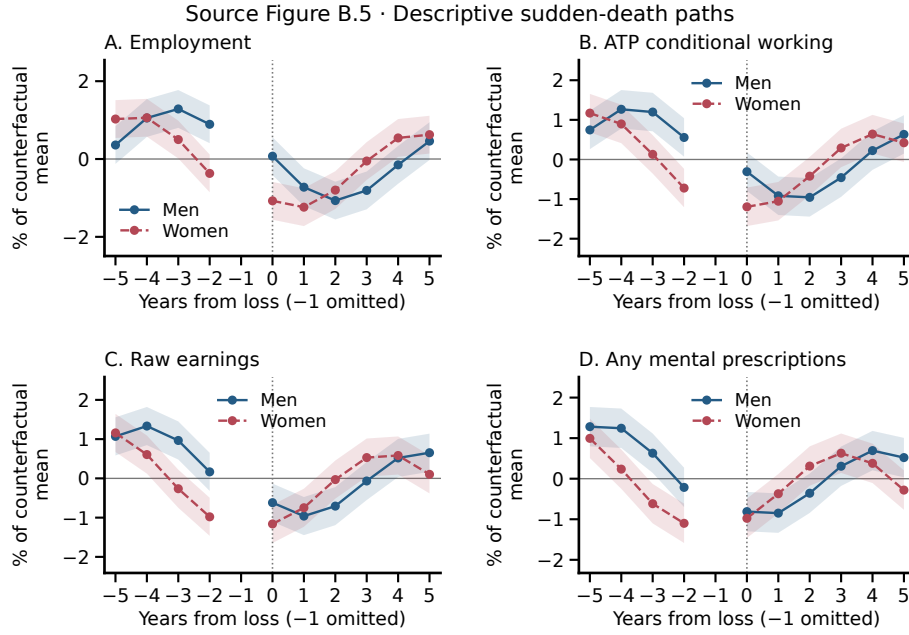
**Fig. 9** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.3: First-deceased parent age distribution. A: Parent age at first death (Density). Source age variable mismatch C03; exact bins A04. Actual results and availability: *[pending]*. The source describes first losses in 1985–2014 among adults aged 25–50 with two known parents. Source suppression below five remains. This density comparison is descriptive, with no treatment-effect interval.



Exact source histogram edges required; A04. Hypothetical bin geometry only; scientific bin origins remain unresolved.

**Fig. 10** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.4: Adult-child age distribution at first parental death. A: Adult age at first death (Density). Exact source histogram edges required; A04. Actual results and availability: *[pending]*. The source describes first losses in 1985–2014 among adults aged 25–50 with two known parents. Source suppression below five remains. These densities are descriptive and have no treatment-effect interval.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS



Prediction denominator C04.

**Fig. 11** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.5: Descriptive sudden-death paths. A: Employment (Percent of counterfactual mean). B: ATP conditional working (Percent of counterfactual mean). C: Raw earnings (Percent of counterfactual mean). D: Any mental prescriptions (Percent of counterfactual mean). Prediction denominator C04. Actual results and availability: *[pending]*. Women and men have separate uncontrolled Equation 1 paths over event years  $-5$  to  $+5$ , relative to omitted  $-1$ , with source robust 95% intervals. The sudden-loss labor population follows the source 1985–2014 event restrictions; prescriptions require their later coverage.

*Table B1. Baseline characteristics by deceased-parent gender and suddenness*

Binding: *[pending]*. These are the same source characteristics at event year  $-1$  under four source columns. They retain means and sample counts, without adding t-tests. The child-age boundary and earnings-unit qualifications above apply to the corresponding rows.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

| B1 characteristic | Mother,<br>sudden | Mother,<br>non-sudden | Father,<br>sudden | Father,<br>non-sudden |
|-------------------|-------------------|-----------------------|-------------------|-----------------------|
| Adult age         | 42.4              | 42.8                  | 43.2              | 43.6                  |
| Male              | 0.265             | 0.280                 | 0.295             | 0.310                 |
| College or above  | 0.310             | 0.320                 | 0.330             | 0.340                 |

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

Continued from preceding page

| B1 characteristic                        | Mother,<br>sudden | Mother,<br>non-sudden | Father,<br>sudden | Father,<br>non-sudden |
|------------------------------------------|-------------------|-----------------------|-------------------|-----------------------|
| High school or vocational education      | 0.445             | 0.440                 | 0.435             | 0.430                 |
| Below high school                        | 0.245             | 0.240                 | 0.235             | 0.230                 |
| Cohabitation                             | 0.345             | 0.360                 | 0.375             | 0.390                 |
| Number of children, capped at five       | 1.00              | 1.20                  | 1.40              | 1.60                  |
| Age of youngest child                    | 8.4               | 8.8                   | 9.2               | 9.6                   |
| Child under six                          | unresolved<br>C02 | unresolved<br>C02     | unresolved<br>C02 | unresolved<br>C02     |
| Mother age                               | 68.4              | 68.8                  | 69.2              | 69.6                  |
| Father age                               | 71.4              | 71.8                  | 72.2              | 72.6                  |
| Mother married                           | 0.345             | 0.360                 | 0.375             | 0.390                 |
| Father married                           | 0.245             | 0.260                 | 0.275             | 0.290                 |
| First-deceased parent age                | 69.4              | 69.8                  | 70.2              | 70.6                  |
| Employment                               | 0.285             | 0.300                 | 0.315             | 0.330                 |
| Intensive margin, conditional on working | 716.0             | 727.0                 | 738.0             | 749.0                 |
| Annual earnings, real 2020 DKK           | 183.0             | 196.0                 | 209.0             | 222.0                 |
| Observations                             | 1700              | 1800                  | 1900              | 2000                  |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

*Table B2. Baseline characteristics by parent and adult-child gender for sudden deaths*

Binding: *[pending]*. This breakdown separates mother–daughter, father–daughter, mother–son and father–son observations. It is needed for interpreting parent-gender and sex-specific baselines, particularly when relative earnings responses use different denominators. Source means and counts remain unfilled.

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

| B2 characteristic, sudden deaths    | Mother–<br>daughter | Father–<br>daughter | Mother–son | Father–son |
|-------------------------------------|---------------------|---------------------|------------|------------|
| Adult age                           | 42.4                | 42.8                | 43.2       | 43.6       |
| Male                                | 0.000               | 0.000               | 1.000      | 1.000      |
| College or above                    | 0.310               | 0.320               | 0.330      | 0.340      |
| High school or vocational education | 0.445               | 0.440               | 0.435      | 0.430      |

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Continued from preceding page

| B2 characteristic, sudden deaths         | Mother–daughter   | Father–daughter   | Mother–son        | Father–son        |
|------------------------------------------|-------------------|-------------------|-------------------|-------------------|
| Below high school                        | 0.245             | 0.240             | 0.235             | 0.230             |
| Cohabitation                             | 0.345             | 0.360             | 0.375             | 0.390             |
| Number of children, capped at five       | 1.00              | 1.20              | 1.40              | 1.60              |
| Age of youngest child                    | 8.4               | 8.8               | 9.2               | 9.6               |
| Child under six                          | unresolved<br>C02 | unresolved<br>C02 | unresolved<br>C02 | unresolved<br>C02 |
| Mother age                               | 68.4              | 68.8              | 69.2              | 69.6              |
| Father age                               | 71.4              | 71.8              | 72.2              | 72.6              |
| Mother married                           | 0.345             | 0.360             | 0.375             | 0.390             |
| Father married                           | 0.245             | 0.260             | 0.275             | 0.290             |
| First-deceased parent age                | 69.4              | 69.8              | 70.2              | 70.6              |
| Employment                               | 0.285             | 0.300             | 0.315             | 0.330             |
| Intensive margin, conditional on working | 716.0             | 727.0             | 738.0             | 749.0             |
| Annual earnings, real 2020 DKK           | 183.0             | 196.0             | 209.0             | 222.0             |
| Observations                             | 1700              | 1800              | 1900              | 2000              |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

*Table B3. Matching-target and other baseline characteristics*

Binding: *[pending]*. Unlike the death-type comparisons above, this table describes treated adults and matched controls at event year  $-1$ . Panel B3a retains the 15 targeted characteristics actually listed in source Table B.3; no additional row is inferred from the historical inventory’s count of 16. Panel B3b retains the nine other health characteristics and source counts. Counts are people in the source baseline comparison, not the person-year counts of a pooled regression.

### Table B3a. Matching-target characteristics

Similarity on variables used to form a match describes that comparison population. It cannot demonstrate similarity in unobserved circumstances or prove post-event parallel trends. The unconditional ATP row differs from the conditional contribution row in Table 1; education and earnings also depend on their required source mappings.

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

| Source characteristic | Treated mean | Control mean |
|-----------------------|--------------|--------------|
| Adult age             | 42.4         | 42.8         |

**ILLUSTRATIVE MOCK DATA — NOT RESULTS**

Continued from preceding page

| Source characteristic               | Treated mean   | Control mean   |
|-------------------------------------|----------------|----------------|
| Male                                | 0.265          | 0.280          |
| College or above                    | 0.310          | 0.320          |
| High school/vocational education    | 0.445          | 0.440          |
| Below high school                   | 0.245          | 0.240          |
| Cohabitation                        | 0.345          | 0.360          |
| Number of children                  | 1.00           | 1.20           |
| Youngest-child age                  | 8.4            | 8.8            |
| Child under six                     | unresolved C02 | unresolved C02 |
| Mother age                          | 68.4           | 68.8           |
| Father age                          | 71.4           | 71.8           |
| Employment                          | 0.345          | 0.360          |
| Unconditional ATP contribution      | 695.0          | 706.0          |
| Annual earnings, source raw measure | 183.0          | 196.0          |
| Public-sector employment            | 0.285          | 0.300          |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

**Table B3b. Other baseline characteristics and source counts**

The additional health rows describe characteristics not used in the main journal match. Their own coverage windows and the private-versus-combined psychiatry ambiguity are retained. They are not inputs to an added balance criterion, and their differences are not automatically adjusted away with new post hoc controls. The common Table B3 binding supplies both panels.

**ILLUSTRATIVE MOCK DATA — NOT RESULTS**

| Source characteristic           | Treated mean   | Control mean   |
|---------------------------------|----------------|----------------|
| Mother: any hospital contact    | 0.245          | 0.260          |
| Father: any hospital contact    | 0.265          | 0.280          |
| Adult: GP count                 | 2.50           | 2.80           |
| Adult: any hospital contact     | 0.305          | 0.320          |
| Adult: any psychologist contact | 0.325          | 0.340          |
| Adult: any psychiatrist contact | unresolved A05 | unresolved A05 |

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Continued from preceding page

| Source characteristic                     | Treated mean | Control mean |
|-------------------------------------------|--------------|--------------|
| Adult: any mental-health prescription     | 0.245        | 0.260        |
| Adult: any alcohol-treatment prescription | 0.265        | 0.280        |
| Adult: any opioid prescription            | 0.285        | 0.300        |
| Source counts                             | 1700         | 1800         |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

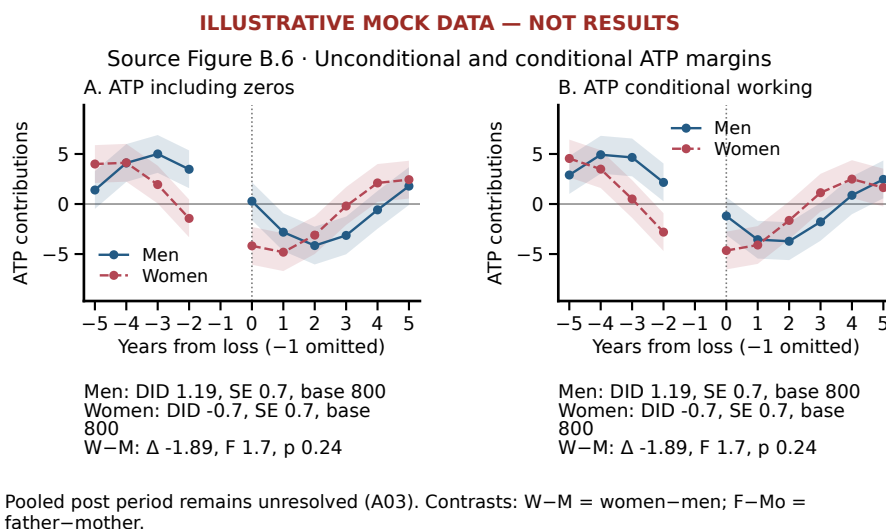
### S4. Additional labor margins and parent-specific dynamics

The main labor panels distinguish working, conditional contribution intensity and earnings. Figure B6 adds the contribution measure including zero payments, *[pending]*, allowing the extensive and conditional margins to be considered together in their original units. It remains a proxy based on ATP payments and does not become measured hours. Because its zero and positive-payment populations differ, the conditional panel does not identify the response among adults who would work under either treatment state.

Figure B7 preserves the separate mother- and father-death dynamics for employment, conditional ATP, raw earnings and indexed earnings: *[pending]*. These eight panels can distinguish an aggregate parent-gender contrast from differences in the path across event time. The dynamic code and pooled-equation caption disagreement remains C05; the display is not repaired by selecting whichever citation matches its visual form.

Figure B8 reports the source annual percentile-rank outcome, *[pending]*. A rank change describes movement relative to the annual earnings distribution. It is informative about position but does not express a fixed DKK amount. Its denominator is the source population aged 20–55, not solely the matched sample or one sex. The income-vintage and rank-construction restrictions in S1 remain necessary before comparing its pattern with the level and index panels.

The original footnote robustness targets remain attached to this labor question. Parental inpatient rematching addresses observed parental health risk; later-period own/parent health matching addresses measures absent from the earlier period; second-death spacing addresses another loss during follow-up; linear controls address functional-form dependence. Their specific unresolved histories, combinations and output scope are retained under U03/A02, as described in the main robustness section. The supplement supplies no additional authorized models to fill those gaps.



**Fig. 12** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.6: Unconditional and conditional ATP margins. A: ATP including zeros (ATP contribution units). B: ATP conditional working (ATP contribution units). Actual results and availability: *[pending]*. The main matched sudden-loss population and men/women event paths retain -5 to +5 relative to omitted -1. Source 95% intervals use individual-by-match clustering. Including or excluding zero payments changes the outcome denominator.

## S5. Health measurement and joint-outcome construction

### Services, prescriptions and screening

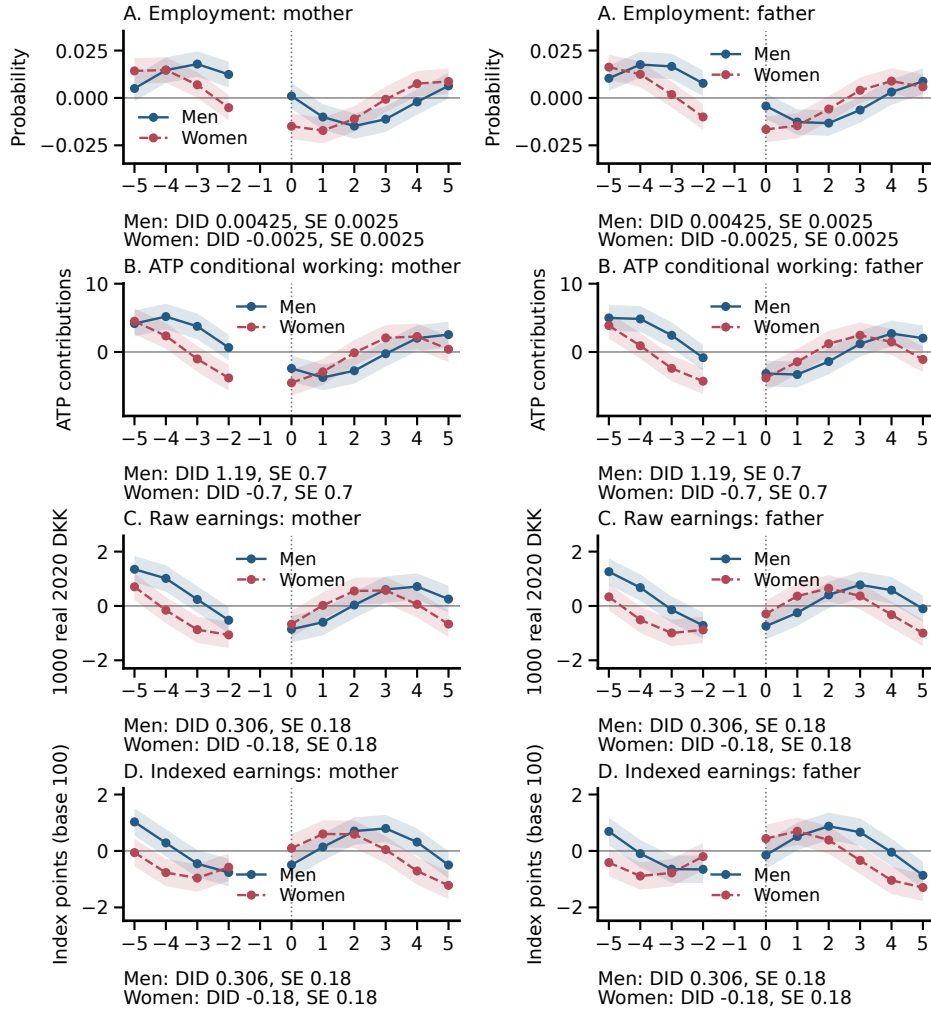
Private services combine source SYSI and SSSY vintages, aggregate service counts by person and year and use the registered GP codes or psychology/psychiatry mapping. Prefixes alone do not replace the missing specialty crosswalk. The source caps and handling of negative counts apply only at their specified aggregation stage. Prescription records are counted by person-year and source ATC prefix: mental-health groups N05/N06A/N06B/N06C, opioids N02A and alcohol-dependence treatment N07BB. Any-use indicators are derived only for outcomes requesting them; Figure B9's alcohol measure is a count of records.

Figure B9's alcohol-treatment and GP evidence is *[pending]*. These outcomes broaden the treatment picture beyond psychologist visits and the two main prescription indicators. They also illustrate why service counts and probabilities of any use cannot be read as the same margin of adjustment.

Figure B10's parent-specific dynamics are *[pending]*. It retains psychologist visits, any mental prescriptions, any opioid prescriptions and hospital visits with their own eligible periods. The same apparent event time can be estimated from different calendar cohorts across outcomes; the figure's repeated parent and sex labels do not

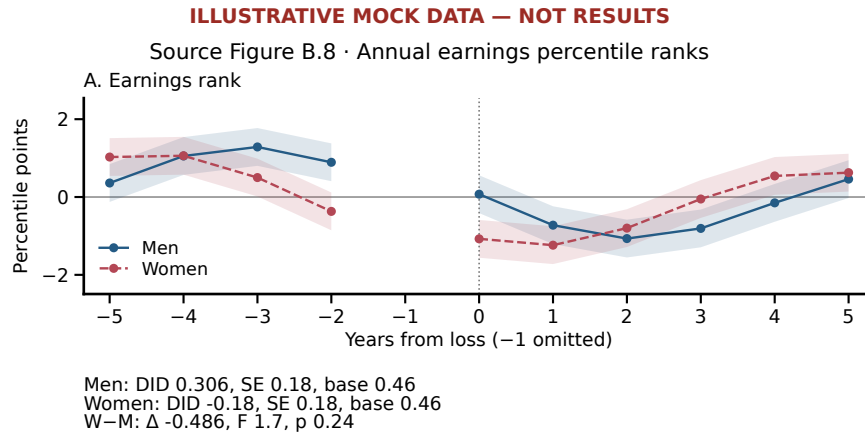
### ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure B.7 · Dynamic labor effects by parent gender



Caption Eq4 versus source Eq2 dynamic coefficients; C05. Event time: years from loss; -1 omitted. Pooled post period remains unresolved (A03).

**Fig. 13** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Source Figure B.7: Dynamic labor effects, with separate mother- and father-death panels for each outcome. A: Employment (probability). B: ATP conditional on working (contribution units). C: Raw earnings (1,000 real 2020 DKK). D: Indexed earnings (baseline 100). All eight original panels remain. The caption's Equation 4 versus code's dynamic Equation 2 conflict remains C05. Results and availability: *[pending]*. The main matched population retains event years -5 to +5 relative to omitted -1, both sexes and source 95% intervals with individual-by-match clustering. Parent-specific dynamics remain distinct from pooled summaries with unresolved post timing.



Pooled post period remains unresolved (A03). Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 14** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.8: Annual earnings percentile ranks. A: Earnings rank (Percentile points). Actual results and availability: *[pending]*. The rank denominator is the source annual population aged 20–55. Main matched men/women paths retain event years –5 to +5 relative to omitted –1, with source 95% intervals and individual-by-match clustering.

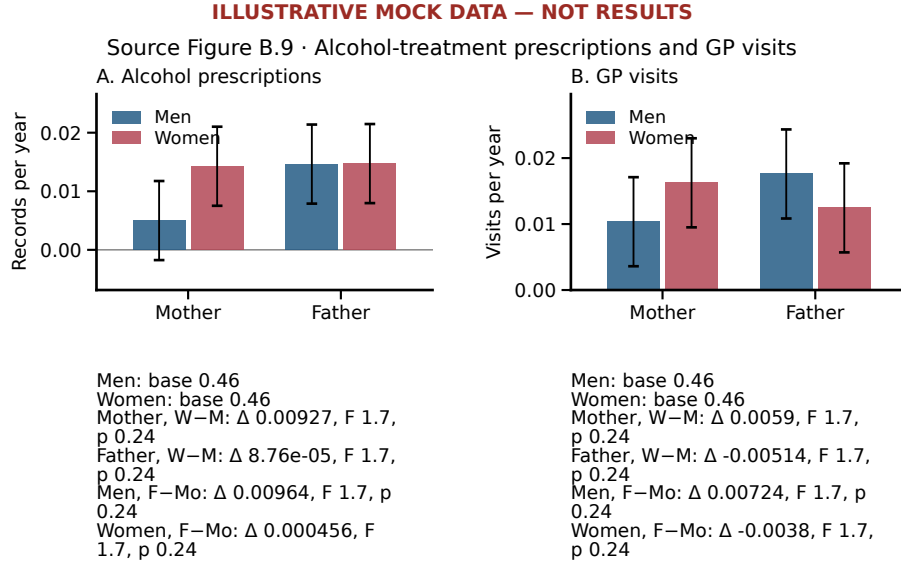
remove that population difference. Treatment onset and persistence must also be read in light of referral and access arrangements described in the main setting.

Hospital admissions are assigned to admission year after the source date checks and person/admission deduplication that retains the latest discharge. The original construction includes inpatient and outpatient nonpsychiatric encounters. Screening uses diagnoses containing DZ00 or DZ01 from 1994, corresponding to the supplement’s Z00/Z01 categories and subcategories. The consuming program defines nonscreening as total visits minus screening visits. It is that final definition, rather than an earlier intermediate variable, that determines Figure B11: *[pending]*.

The split addresses whether visits are confined to the recorded screening categories. It does not separate all preventive behavior from illness, because medical contact outside those categories can still reflect vigilance or help-seeking. Counts measure use of the system, while categories reflect recorded diagnoses and classification. A non-screening response would therefore qualify a pure measured-screening account without identifying a biological health effect or the severity of a mental condition.

## Declines, starts and joint timing

The source co-occurrence construction obtains earnings at event time –6 for lag calculations, applies the registered earnings transformation and restricts to the complete 1995–2018 health-era panel. Complete eleven-year windows imply event years 2000–2013 in this source construction; dropping event year –5 then yields the displayed



Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 15** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.9: Alcohol-treatment prescriptions and GP visits. A: Alcohol prescriptions (Records per year). B: GP visits (Visits per year). Actual results and availability: *[pending]*. Mother/father contrasts are separate within adult sex. Alcohol counts use N07BB records from 1995; GP services begin in 1990. Source 95% intervals and individual-by-match clustering apply, with outcome-specific windows and unresolved pooled timing A03.

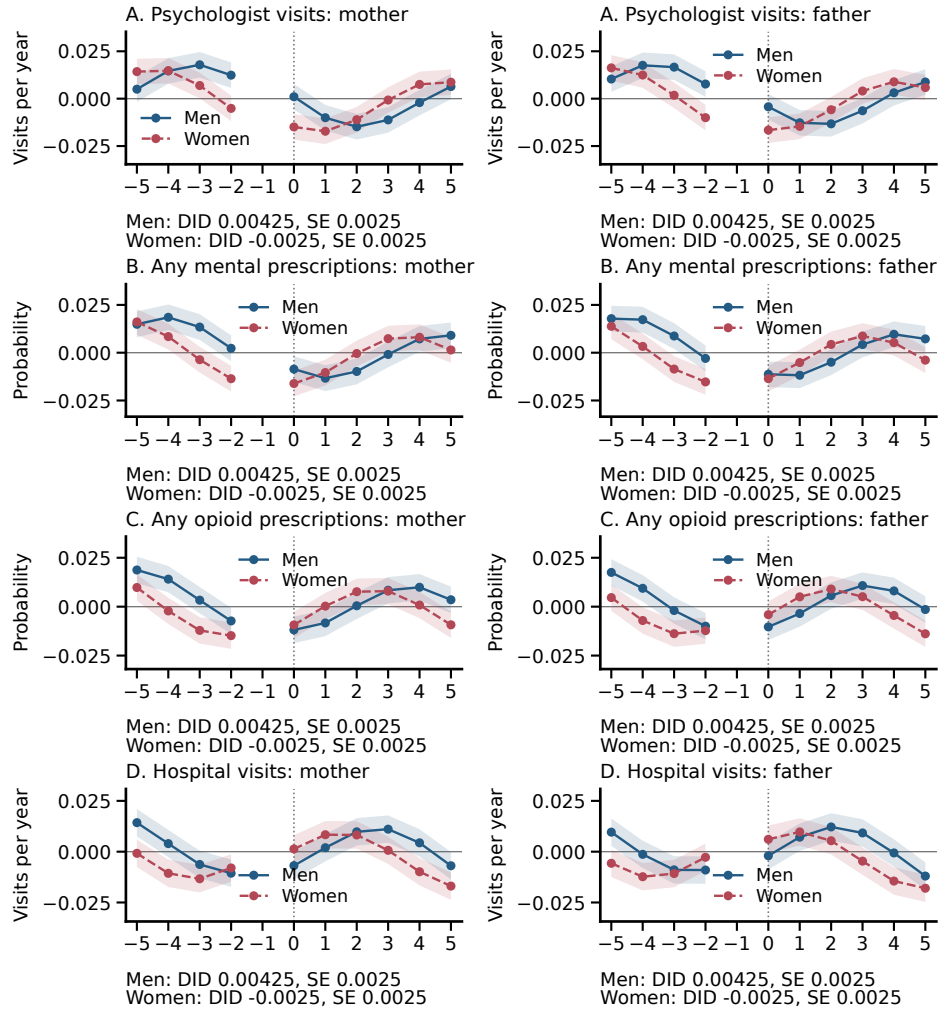
−4 through +5 coordinates. These restrictions are not imposed on the full historical labor analysis.

Write the source proportional change as  $(Y_t - Y_{t-1})/Y_{t-1}$ . A decline occurs at or below −0.05, and onset requires that decline now but not in the preceding year. The source’s handling of an undefined or missing ratio as zero is retained only within the fully covered construction; it does not authorize filling an unavailable year with a non-event or replacing the ratio by logs. Mental-prescription, psychologist and opioid starts likewise require current any-use and no use in the preceding year. Their joint indicators use the same individual-year components, rather than multiplying separately estimated coefficients.

Figure B12 supplies the additional co-occurrence evidence, *[pending]*. Its first four panels concern psychologist and opioid onsets and their joint occurrence with earnings-decline onset. The hospital panels are a visit count and that count multiplied by the earnings-onset indicator. They are not a binary hospital-onset analysis. Other onset, doubling or transformation outputs found only in author code are outside the retained source targets.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

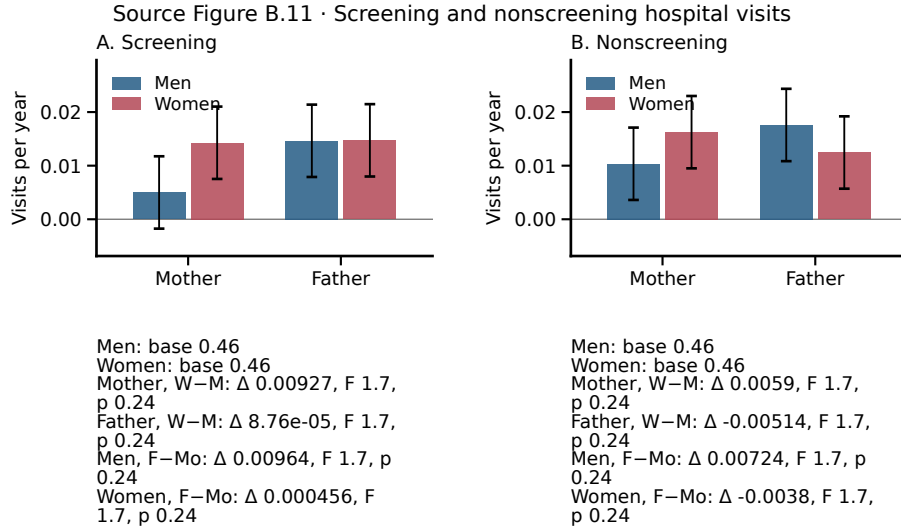
Source Figure B.10 · Dynamic health effects by parent gender



Event time: years from loss; -1 omitted. Pooled post period remains unresolved (A03).

**Fig. 16** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Source Figure B.10: Dynamic health effects, with separate mother- and father-death panels for each outcome. A: Psychologist visits (visits/year). B: Any mental prescriptions (probability). C: Any opioid prescriptions (probability). D: Hospital visits (visits/year). All eight original panels and their outcome-specific samples remain. Results and availability: *[pending]*. Event years run -5 to +5 relative to omitted -1, with source 95% intervals and individual-by-match clustering. Psychologist data begin in 1990, prescriptions in 1995, and nonpsychiatric hospital data cover 1994–2018.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS



Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 17** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.11: Screening and nonscreening hospital visits. A: Screening (Visits per year). B: Nonscreening (Visits per year). Actual results and availability: *[pending]*. Screening follows Z00/Z01 categories (recorded DZ00/DZ01); the final nonscreening count is total minus screening. Mother/father contrasts by adult sex use source hospital coverage 1994–2018, 95% intervals and individual-by-match clustering. A03 applies to pooled timing.

Immediate-year ratios require the exact joint and marginal effect and the intended event coordinate. A03 leaves year 0 versus year 1 unresolved; no source ratio interval or new denominator adjustment is supplied. C05 separately retains the dynamic-code versus pooled-caption disagreement. Joint timing can support a bereavement effect on the observed joint outcome under the source assumptions, while the health-to-earnings relation remains unidentified as mediation. Those interpretation limits hold even if all pending inputs become available.

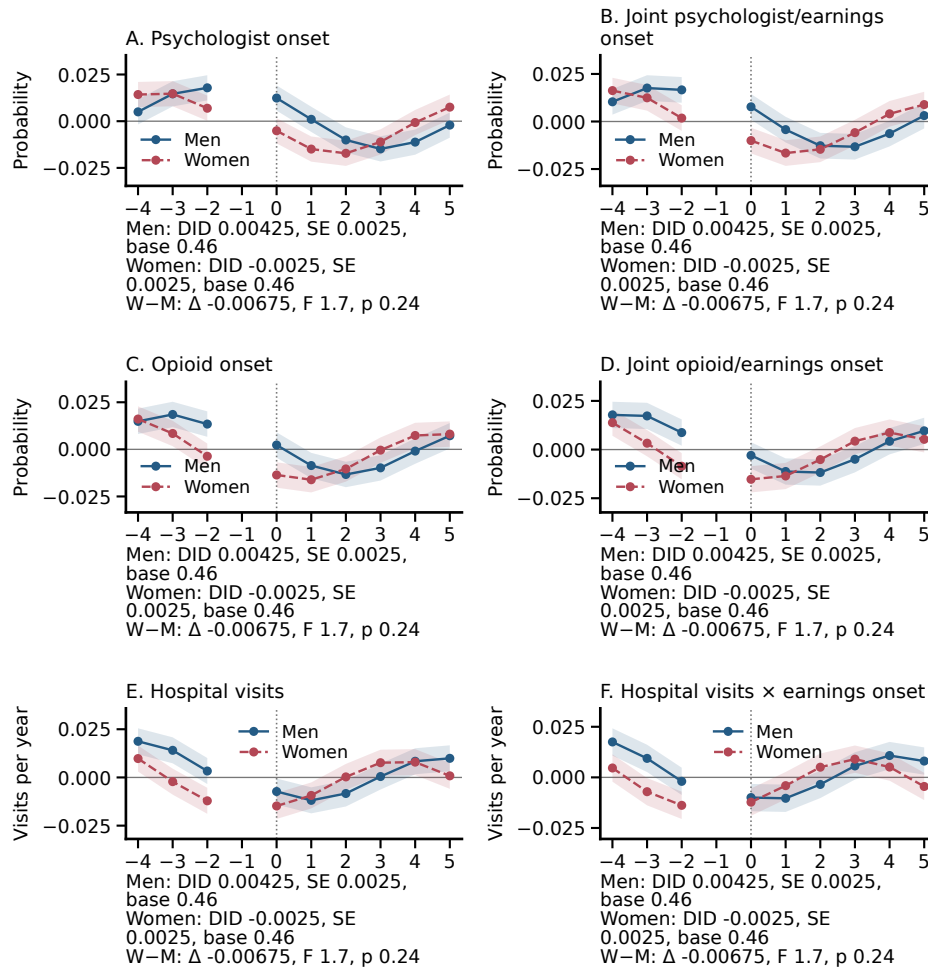
## S6. Childcare and surviving-parent comparisons

### Child-age heterogeneity and formal enrollment

The earnings comparison fixes the treated adult's youngest-child group at event year 0 and propagates it to the pair. Figure B13 narrows the original young-child comparison to adults with a youngest child aged 0–5 or 6–14: *[pending]*. Its winsorization stage and group-specific reindexing follow the original program, subject to the inherited C06 eligibility disposition. The panel therefore concerns differences in the relative earnings response by family stage, not a measured dose of informal childcare.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

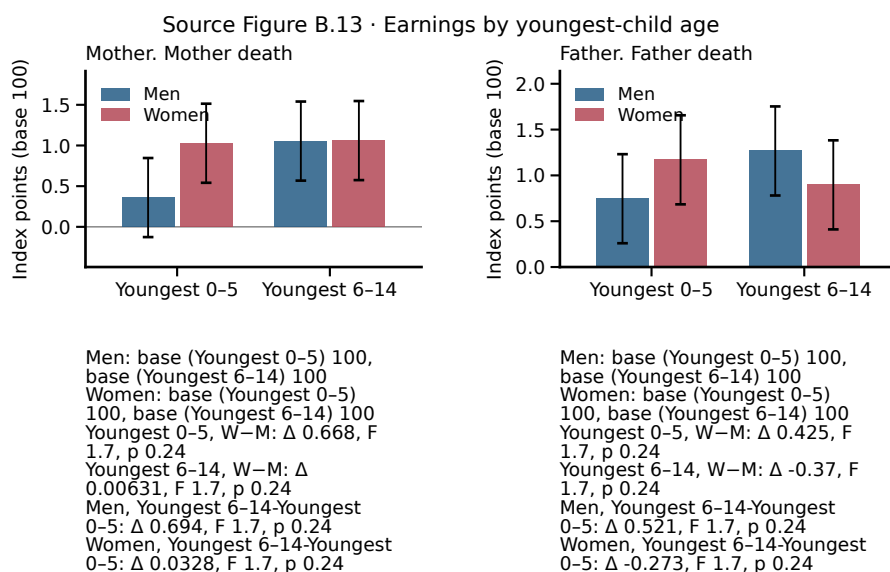
Source Figure B.12 · Additional health and earnings co-occurrence



PanelF is hospital count times earnings-decline onset; caption/model conflict C05 remains.  
 Event time: years from loss; -1 omitted. Pooled post period remains unresolved (A03).  
 Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 18** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.12: Additional health and earnings co-occurrence. A: Psychologist onset (Probability). B: Joint psychologist/earnings onset (Probability). C: Opioid onset (Probability). D: Joint opioid/earnings onset (Probability). E: Hospital visits (Visits per year). F: Hospital visits  $\times$  earnings onset (Visits per year). PanelF is hospital count times earnings-decline onset; caption/model conflict C05 remains. Actual results and availability: *[pending]*. The source complete 1995–2018 health-era construction displays event years -4 to +5, with -1 omitted. Both sexes retain source 95% intervals and individual-by-match clustering. Joint annual timing does not establish causal health-to-earnings mediation.

# ILLUSTRATIVE MOCK DATA — NOT RESULTS



Contrasts: W-M = women-men; F-Mo = father-mother.

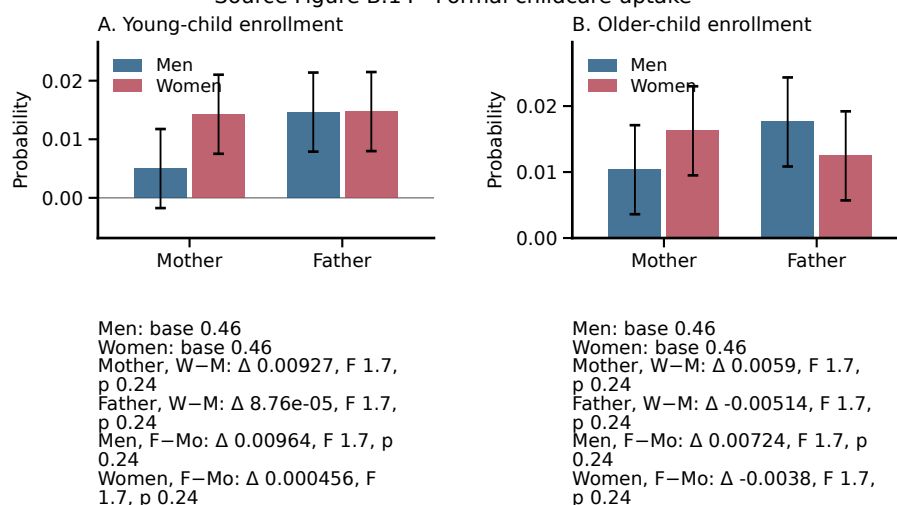
**Fig. 19** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.13: Earnings by youngest-child age. Mother: Mother death (Index points; baseline 100). Father: Father death (Index points; baseline 100). Actual results and availability: *[pending]*. Each parent panel retains men/women with youngest child aged 0-5 or 6-14 at the event, with source group-specific baseline indexing. Source 95% intervals use individual-by-match clustering; C06 eligibility and A03 timing remain.

Formal enrollment links the source youngest-child identity to child-level DAGI, BOERNFB and BOERNSB records. The earlier DAGI young-care codes are 151/152/154/200/201 and older-care codes 153/224/225/226/227; later registers carry the corresponding young and older enrollment records. The formal-care comparison retains calendar years 2005-2019 and complete eleven-year windows, implying source event years 2010-2014. Its uptake probabilities are *[pending]*. A narrower, later population cannot simply supply a numerical explanation for earnings effects over the whole main period.

The source program uses youngest age at most five and an older group aged 6-10, while display wording says under/above five. It also blanks age-ineligible follow-up observations before coding missing uptake as zero. A06 retains that eligibility and measurement problem, including youngest-child ties. An unavailable register-year cannot be treated as an observed non-enrollment. These distinctions matter because enrollment records do not tell how many hours of help the household received from either formal care or grandparents.

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure B.14 · Formal childcare uptake



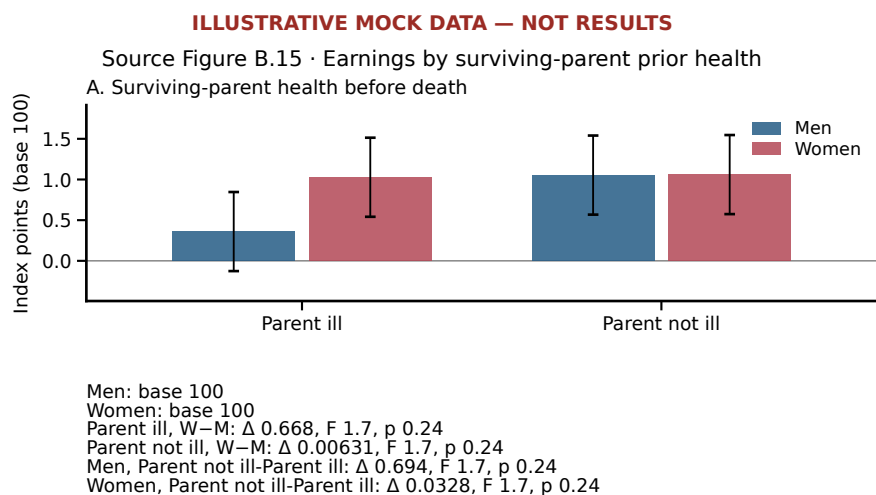
Source labels under5/above5, code<=5 and6-10; A06. Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 20** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.14: Formal childcare uptake. A: Young-child enrollment (Probability). B: Older-child enrollment (Probability). Source labels under5/above5, code<=5 and6-10; A06. Actual results and availability: *[pending]*. Enrollment of the linked youngest child uses complete 2005–2019 windows and source event years 2010–2014. Mother/father contrasts by adult sex retain source 95% intervals and individual-by-match clustering. This is uptake, not care hours; A03 timing remains.

## Hospital days, prior health and residence

For surviving-parent health, the source expands admission-to-discharge intervals inclusively, unions person-days to avoid double counting overlapping encounters, and aggregates within calendar years. Outpatient treatment is included. The result is not an inpatient bed-day measure or a direct care-hours record. The text's cumulative period and the code's annual threshold can classify different parents even before their differing endpoints and strictness are considered. C07 keeps those differences unresolved.

Figure B15 retains the pre-event surviving-parent-health earnings comparison, *[pending]*, with the source restriction to adults without young children, including childless adults in this module. The code uses pre-event years -5 through -1 and the annual threshold, whereas the scientific source definition remains subject to the same illness correspondence issue. Earnings indexing uses the source sex baseline, not a newly calculated sex-by-illness denominator. C05 also retains the illness-interaction versus parent-type-equation disagreement.



C05/C07. Contrasts: W–M = women–men; F–Mo = father–mother.

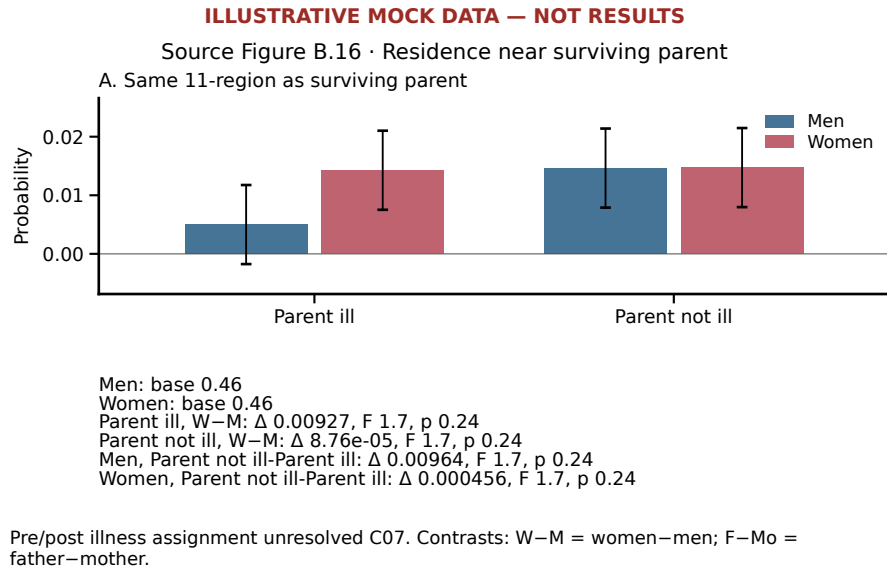
**Fig. 21** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.15: Earnings by surviving-parent prior health. A: Surviving-parent health before death (Index points; baseline 100). C05/C07. Actual results and availability: *[pending]*. The source compares illness groups using pre-event health among adults without young children and matched controls. Earnings use sex-specific pre-event means; 95% intervals use individual-by-match clustering. Hospital definition/coverage and model correspondence remain unresolved, as does pooled timing A03.

Residence provides a different observation from earnings: whether the adult and surviving parent live in the same eleven-region NUTS3 area during each year. Figure B16 retains that comparison, *[pending]*, and its original treatment of missing surviving-parent region. The source produces both pre- and post-illness splits without settling which belongs to the displayed result. The manuscript preserves that ambiguity and does not select a split based on the resulting relationship.

Prior health, post-event health and residence speak to different aspects of the care account. Predetermined illness can characterize circumstances before the loss; subsequent illness can capture new needs but introduces selection; residence can facilitate contact without measuring help. The source’s mental-prescription comparison by surviving-parent illness lacks the full sample, aggregation and model specification, so it remains a prose-only no-estimate target. This missing evidence limits the attempt to distinguish additional care from distress about an ill surviving parent.

## S7. Family formation and household spillovers

Cohabitation is based on the source partner identifier, and the family-response child count uses linked children born by the relevant year without applying the descriptive cap of five. Figure B17 retains the cohabitation probability and child-count outcomes



**Fig. 22** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.16: Residence near surviving parent. A: Same 11-region as surviving parent (Probability). Pre/post illness assignment unresolved C07. Actual results and availability: *[pending]*. The source residence indicator compares adults and their surviving parent in eleven NUTS3 regions, by adult sex and illness group. Geography begins in 1985 and hospital information ends in 2018. Source 95% intervals use individual-by-match clustering; A03/C07 remain.

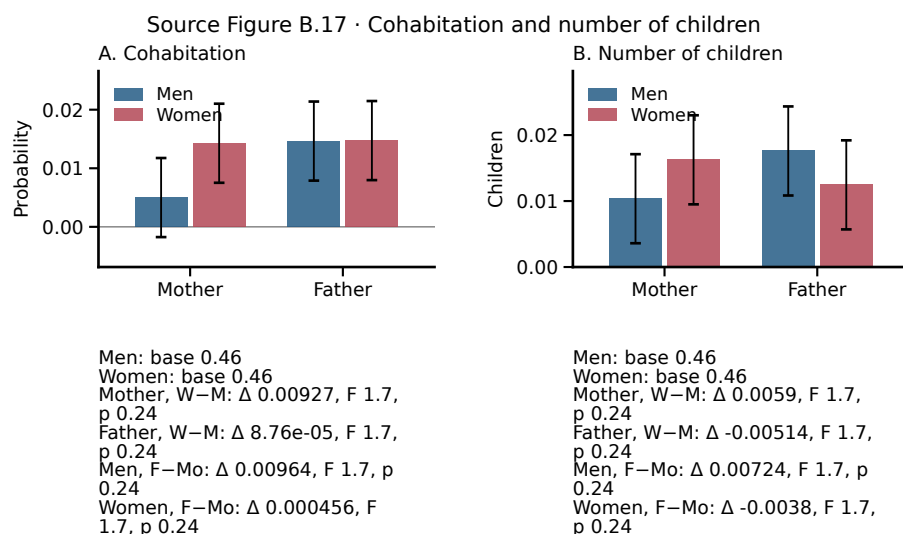
under the original parent-type model. These are different margins of family organization. Neither captures the quality of relationships, all births as a flow, or the unobserved division of household work.

The source supplement considers whether these responses could account for labor adjustments through family formation or changed values. Even an observed reduction in the number of children relative to controls would not, by itself, quantify an increase in labor supply that would otherwise have occurred. It can challenge a simple direction-of-change account, while leaving other family responses and joint determination unresolved. The main household section carries the interpretation and pending bindings for this evidence.

The spouse analysis identifies the event-time partner before expanding spouse years. Its separately matched population requires the source biological-adult and spouse histories and an eleven-row spouse window; source baseline age, child-count and cohabitation rules apply to this population. The program uses distinct age bins and earnings coarsening, with year-specific group matrices retained in the matching audit. These are not the main adult-child matched edges reused under another label.

Figure B18's parent-in-law comparison concerns spouses with young children, using source-consistent spouse identifiers from 1985 onward. The match includes variables for the spouse's own parental circumstances, but the program's presence indicators

## ILLUSTRATIVE MOCK DATA — NOT RESULTS



Contrasts: W-M = women-men; F-Mo = father-mother.

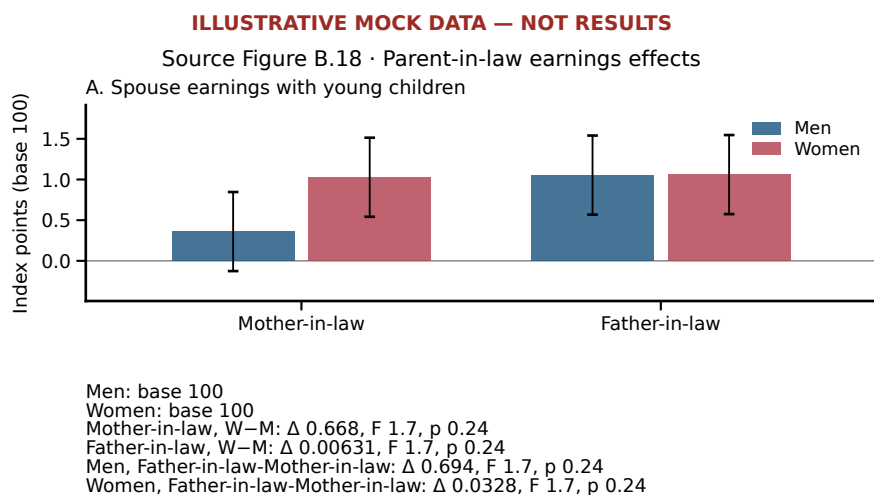
**Fig. 23** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.17: Cohabitation and number of children. A: Cohabitation (Probability). B: Number of children (Children). Actual results and availability: *[pending]*. The source main sudden-loss matched population supplies mother/father contrasts by adult sex, with source 95% intervals and individual-by-match clustering. Number of children is the linked stock, not a fertility rate; A03 remains for pooled timing.

do not establish the survival concept in the prose. Tenure ordering, duplicate event partners and the joint gender-comparison model remain A09. A resulting spouse coefficient would concern this eligible matched household population. It does not identify a general spouse effect outside that population, a sum of household losses or a childcare-mediated share.

## S8. Proximity, future-treated controls and nonsudden loss

The binary proximity comparison asks whether the earnings response differs when adult children and the deceased parent lived in the same region before death. This is a pre-event family characteristic rather than a randomized assignment of access to support. The code uses eleven regions where the caption refers to five, and its group-specific models differ from the pooled gender-test specification. One displayed p-value is assigned an F statistic in the source program. Those C09 discrepancies remain unresolved for Figure B19.

Figure B20 retains four ordered source categories: same municipality; same NUTS3 region but different municipality; same NUTS2 region but different NUTS3 region; and farther apart under that classification. The historic crosswalk is required. The



Spouse matching and contrast ambiguities A09. Contrasts: W–M = women–men; F–Mo = father–mother.

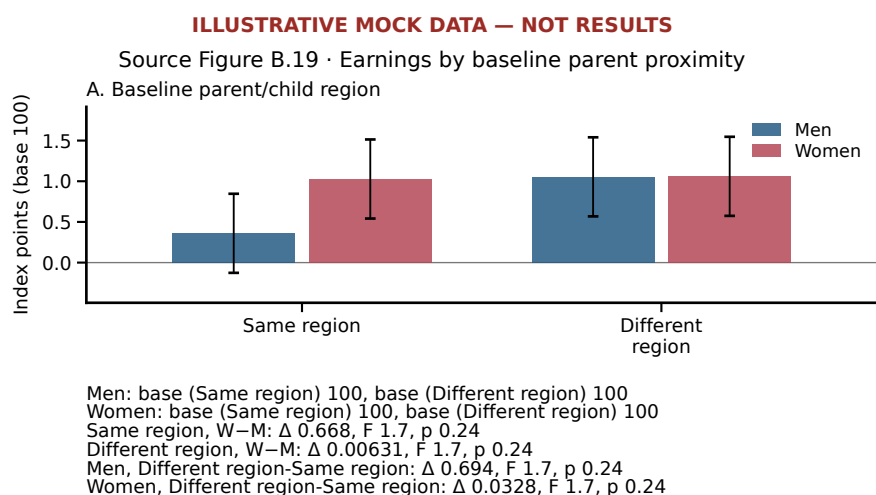
**Fig. 24** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.18: Parent-in-law earnings effects. A: Spouse earnings with young children (Index points; baseline 100). Spouse matching and contrast ambiguities A09. Actual results and availability: *[pending]*. The population is separately matched married/cohabiting partners with a child below six at the event, with spouse-ID information from 1985. Source 95% intervals use spouse-by-match clustering; A03 timing and A09 sample/contrast correspondence remain.

categories are not continuous distances, and the source does not license kilometer-based interpolation or a new proximity measure. Gradients can reflect practical help and attachment as well as other family differences, which is why the main text treats them as qualifying evidence.

The alternative control design selects adults in five-year birth-cohort bins whose first parent dies suddenly six years later. The source comparison can address whether eventual sudden loss and parental illness in the ordinary controls matter for the counterfactual. It cannot be treated as a replacement estimator chosen by fit to the source results. Figure B21 retains employment and indexed-earnings paths for both sexes; Figure B22 retains the mother/father earnings contrasts.

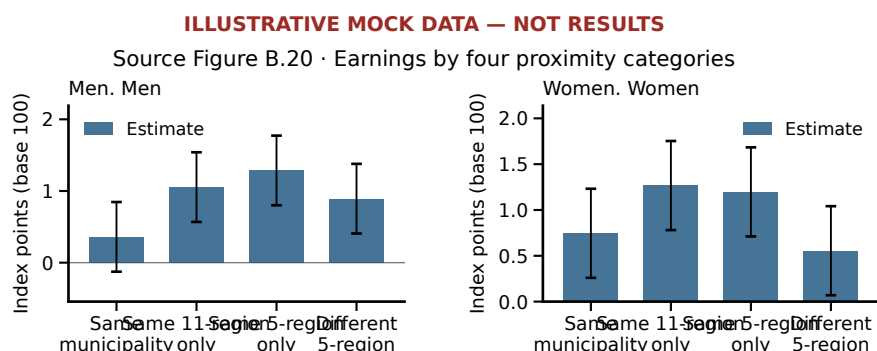
The C10 window, clustering and winsorization conflicts each affect a different aspect of the comparison: which years identify the response, how repeated observations contribute to uncertainty, and which earnings distribution is analyzed. Employment does not consume the earnings-tail choice but still has the other two disputes. A08 separately records the source fertility-block execution problem. It is not repaired here and does not authorize a missing scientific construction. The fully specified six-year timing gap is retained without inventing the unresolved remainder.

Figure B23 returns to the descriptive design for nonsudden deaths. Comparing it with B5 asks whether an event-centered picture differs when illness and anticipation



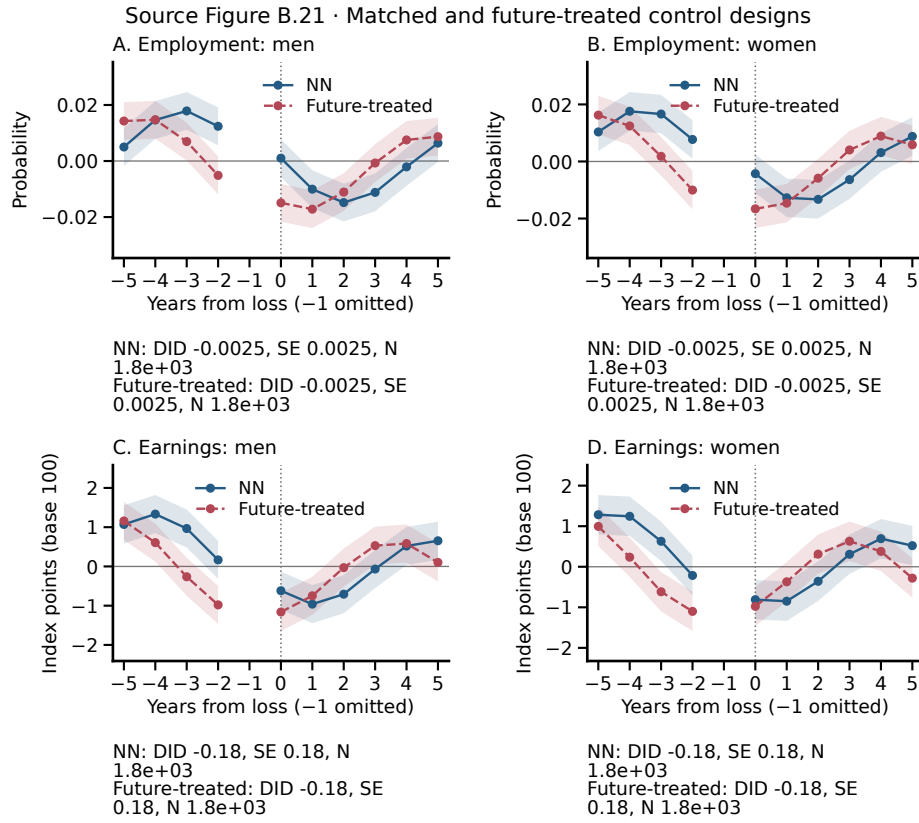
Source caption5 regions versus code11; C09. Contrasts: W-M = women-men; F-Mo = father-mother.

**Fig. 25** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.19: Earnings by baseline parent proximity. A: Baseline parent/child region (Index points; baseline 100). Source caption 5 regions versus code 11; C09. Actual results and availability: *[pending]*. Men/women comparisons use source baseline regional proximity to the deceased parent, group-specific earnings indexing and source 95% intervals. C09 also retains the contrast-model and F-versus-p assignment conflicts; A03 pooled timing remains.



**Fig. 26** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.20: Earnings by four proximity categories. Men: Men (Index points; baseline 100). Women: Women (Index points; baseline 100). Actual results and availability: *[pending]*. The four categories are same municipality, same NUTS3 only, same NUTS2 only and farther under the source map, with detailed geography from 1985. Source 95% intervals use individual-by-match clustering; A03 remains. Categories are not kilometer distances.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

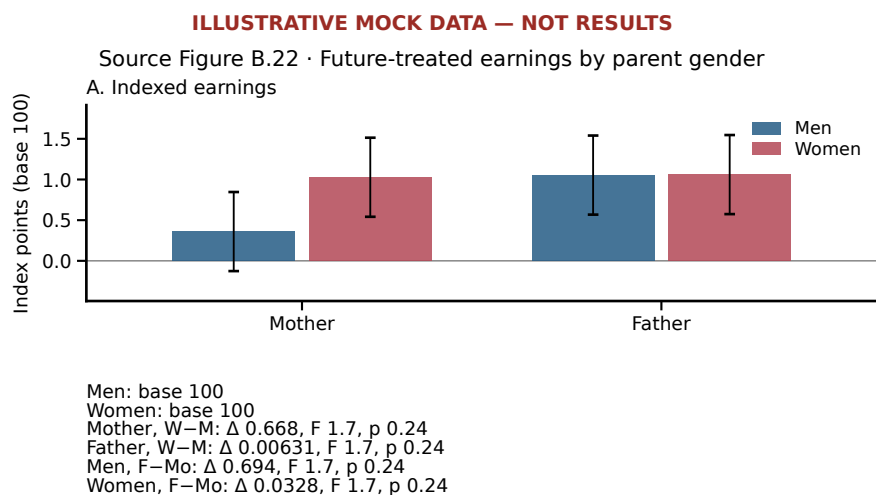


C10 blocks future-treated window, cluster and earnings tail choices. Pooled post period remains unresolved (A03).

**Fig. 27** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.21: Matched and future-treated control designs. A: Employment: men (Probability). B: Employment: women (Probability). C: Earnings: men (Index points; baseline 100). D: Earnings: women (Index points; baseline 100). C10 blocks future-treated window, cluster and earnings tail choices. Actual results and availability: *[pending]*. The source future-treated groups use five-year birth-cohort bins and a six-year gap to sudden first loss. Both designs retain source 95% intervals; disputed window and clustering are not assigned defaults. A03 summary timing and A08 execution correspondence remain.

can precede death. Any pre-event movement is itself part of that description, not a new statistical test or proof of an exact anticipatory effect. C04's prediction-denominator conflict remains for the relative display.

The source supplement's international discussion is conceptual. Formal care and health services can change the resources available after a loss, but the study does not estimate how an identical bereavement would affect an identical household under another country's institutions. The original lower-bound argument is therefore



C10. Contrasts: W–M = women–men; F–Mo = father–mother.

**Fig. 28** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.22: Future-treated earnings by parent gender. A: Indexed earnings (Index points; baseline 100). C10. Actual results and availability: *[pending]*. The source future-treated groups use five-year birth-cohort bins and a six-year gap to sudden first loss. Parent-by-sex contrasts retain source 95% intervals without selecting the disputed cluster, window or tail rule. A03/A08 remain.

retained with its scope limitation in the Discussion, without an added international experiment.

## S9. Wealth, work choices and labor heterogeneity

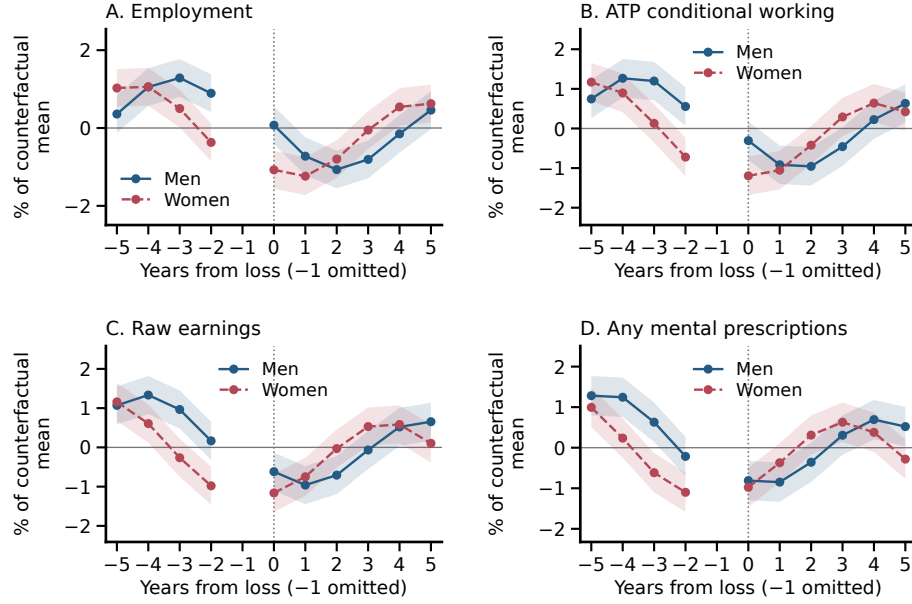
### Parental assets and the interpretation of an interaction

Table B4's scientific target is variation in the earnings response with pre-death parental net wealth. The source IND wealth fields are FORM for 1984–1997 and FORMREST\_NY05 thereafter, with the source within-person-year maximum and inflation adjustment. The net-wealth measure excludes pension wealth under the recorded documentation. It is not an observed inheritance, and the first death need not trigger a distribution of the whole estate.

The four source columns are transposed into rows while retaining coefficients, standard errors, observations and control means. Interaction denotes treatment  $\times$  post  $\times$  parental wealth; post effect denotes treatment  $\times$  post. The control-mean cell retains both source sex-specific control means, not merely the sex named in its row. The source caption's 1,000 DKK versus program's 10,000 DKK scaling and the potentially control-side pair maximum remain C08. No number receives a settled wealth unit until those choices are clarified.

### ILLUSTRATIVE MOCK DATA — NOT RESULTS

Source Figure B.23 · Descriptive non-sudden-death paths



Prediction denominator C04.

**Fig. 29** ILLUSTRATIVE MOCK DATA — NOT RESULTS. Replication of source Figure B.23: Descriptive non-sudden-death paths. A: Employment (Percent of counterfactual mean). B: ATP conditional working (Percent of counterfactual mean). C: Raw earnings (Percent of counterfactual mean). D: Any mental prescriptions (Percent of counterfactual mean). Prediction denominator C04. Actual results and availability: *[pending]*. Women and men have separate uncontrolled Equation 1 paths over event years  $-5$  to  $+5$ , relative to omitted  $-1$ , with source robust 95% intervals. The nonsudden-loss labor population follows the source 1985–2014 event restrictions; prescriptions require later coverage.

The own-wealth and separate deceased/surviving-parent variants have no complete source model to inherit automatically from this table. U04 retains their timing, scale, level-versus-change and specification gaps. The surviving-parent variant was discussed as relevant to transfers that might accompany care, but no actual transfer or care measure is observed here. An imprecise interaction cannot distinguish absence of a wealth mechanism from limited information or imperfect measurement.

*Table B4. Earnings response and parental baseline wealth*

Binding: *[pending]*.

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

| Source group | Interaction<br>(SE) | Post effect<br>(SE) | N    | Control<br>means              |
|--------------|---------------------|---------------------|------|-------------------------------|
| Mother–men   | -0.460 (0.120)      | -0.550 (0.230)      | 1200 | Men: 98.7;<br>Women:<br>101.6 |
| Mother–women | 0.540 (0.120)       | 0.630 (0.230)       | 1320 | Men: 98.7;<br>Women:<br>101.6 |
| Father–men   | -0.620 (0.120)      | -0.710 (0.230)      | 1440 | Men: 98.7;<br>Women:<br>101.6 |
| Father–women | 0.700 (0.120)       | 0.790 (0.230)       | 1560 | Men: 98.7;<br>Women:<br>101.6 |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

## Occupation, firms and sector

The occupation outcome uses the source four-digit codes and comparisons with up to eight earlier nonmissing records. Its within-person-match ordering is not chronologically specified, and there is no source-supplied harmonization for the relevant code changes. A10 blocks an order-dependent switch rather than replacing it with a newly defined last-year transition. The source firm measure requires adjacent valid employer identifiers; a gap in recorded employment is not automatically a firm-to-firm transition. Public-sector employment follows the source ownership rules and employment conditioning.

*Table B5. Job choice and labor-market transitions*

Binding: *[pending]*. All six source sex-by-outcome columns remain in the three rows below. Each sex-specific cell contains the coefficient with its standard error, observation count and original pre-period mean; the definition/status field retains any unresolved construction. The grouped statistical fields are a presentation of the existing result vector, not additional estimates. Their work-conditioned interpretation and the occupation chronology limitation remain visible.

# ILLUSTRATIVE MOCK DATA — NOT RESULTS

| Job-choice outcome     | Men: coefficient<br>(SE); N;<br>pre-mean | Women:<br>coefficient (SE);<br>N; pre-mean | Definition /<br>status         |
|------------------------|------------------------------------------|--------------------------------------------|--------------------------------|
| Occupational switching | -0.005 (0.012);<br>N=1100;<br>mean=0.420 | 0.006 (0.012);<br>N=1100;<br>mean=0.420    | Chronology<br>unresolved (A10) |

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Continued from preceding page

| Job-choice outcome       | Men: coefficient (SE); N; pre-mean | Women: coefficient (SE); N; pre-mean | Definition / status        |
|--------------------------|------------------------------------|--------------------------------------|----------------------------|
| Firm change              | 0.005 (0.012); N=1200; mean=0.420  | -0.006 (0.012); N=1200; mean=0.420   | Source definition retained |
| Public-sector employment | -0.006 (0.012); N=1300; mean=0.420 | 0.007 (0.012); N=1300; mean=0.420    | Source definition retained |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

These outcomes can describe transitions between observed jobs and sectors. They cannot measure all changes in work preferences, hours, tasks or flexibility, and their eligible working population can change after bereavement. Separate firm and sector components remain retainable subject to their own source and shared matching boundaries even if occupation switching is unavailable. The module is not declared wholly null because one component is unresolved.

### Cause, age and calendar period

Table B6 retains four separate sex-specific model families. Causes group the final sudden codes into heart, stroke, respiratory and accident categories. Parent and adult ages address the life-course timing of the loss; periods address differences across the source decades. An age contrast may also reflect family support or anticipation, and a calendar difference is not a causal effect of a specific policy.

*Table B6. Heterogeneity in the indexed-earnings response*

Binding: *[pending]*. Each category retains its coefficient and standard error, with the source model's observation count. The printed cutpoints of 70 and 38 versus code-recomputed sample medians remain A07; the lower/upper labels below do not resolve them. The cause and calendar categories are source-explicit. No new cross-category test or alternative cutoff is added.

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

| Separate source model and category | Men: coefficient (SE); N | Women: coefficient (SE); N |
|------------------------------------|--------------------------|----------------------------|
| Cause: heart, codes 24/25          | -0.460 (0.240); N=1400   | 0.550 (0.240); N=1500      |
| Cause: stroke, code 26             | 0.540 (0.240); N=1400    | -0.630 (0.240); N=1500     |
| Cause: respiratory, code 29        | -0.620 (0.240); N=1400   | 0.710 (0.240); N=1500      |
| Cause: accident, codes 46/47/49    | 0.700 (0.240); N=1400    | -0.790 (0.240); N=1500     |
| Parent age: lower source group     | -0.780 (0.240); N=1400   | 0.870 (0.240); N=1500      |

## ILLUSTRATIVE MOCK DATA — NOT RESULTS

Continued from preceding page

| Separate source model and category | Men: coefficient (SE); N | Women: coefficient (SE); N |
|------------------------------------|--------------------------|----------------------------|
| Parent age: upper source group     | 0.860 (0.240); N=1400    | -0.950 (0.240); N=1500     |
| Adult age: lower source group      | -0.940 (0.240); N=1400   | 1.030 (0.240); N=1500      |
| Adult age: upper source group      | 1.020 (0.240); N=1400    | -1.110 (0.240); N=1500     |
| Period: 1985–1994                  | -1.100 (0.240); N=1400   | 1.190 (0.240); N=1500      |
| Period: 1995–2004                  | 1.180 (0.240); N=1400    | -1.270 (0.240); N=1500     |
| Period: 2005–2014                  | -1.260 (0.240); N=1400   | 1.350 (0.240); N=1500      |

*All numbers and uncertainty are hypothetical layout examples. Empirical cells and source decisions remain pending.*

## S10. Source boundaries and reproducible evidence handoff

The unchanged uncertainty ledger distinguishes unavailable input (U01/U02), unresolved scientific specifications (U03/U04), paper-code conflicts (C01–C10) and ambiguities or execution correspondence (A01–A11). Their consequences attach to the specific target, row, panel, summary or shared comparison. The manuscript keeps conclusion-changing cases in the main text and the detailed methods above. In particular, unresolved matching can affect many otherwise distinct questions, while a disputed baseline row need not erase every other descriptive row.

The full ledger remains at [source deviations](#), with exact locators and no-estimate dispositions. C02 concerns the under-six descriptive rows; C03 and A04 concern age distributions and exact shares; A05 concerns private versus combined psychiatry; A11 includes consequential duplicates, simultaneous deaths and missing-versus-zero interpretation. These issues remain alongside the principal matching, normalization, post-period, scaling, child-eligibility, illness, wealth, proximity and future-treated conflicts explained in the main argument. Their presence in SI is not a resolution or a competing protocol.

The reference implementations and per-figure input interfaces are indexed in the execution and paper directories, with the current scalable implementation plan in the engineering handoff. Their kernels and engineering tests do not establish that the unresolved scientific choices are settled or that confidential inputs are available. The result map retains every exact RESULT name and source output binding. A renderer consumes reviewed plot-ready tables and does not estimate replacement results. Its status fields distinguish pending, unavailable/boundary and operational failure; only actual evidence can fill an empirical cell.

All 11 scientific units remain members of the same Molecule with their original activation and interpretation. A complete or partial coverage label does not supply a direction, magnitude or confirmation judgment. Source clarification would require a traceable authorized step before a dependent estimate, not an implicit choice during typesetting. This amendment changes exposition and display organization only. Final

host compilation, baseline freezing, validation, seals and the current archive remain separate from the scientific evidence status.

## References

The journal article and its supplement are the scientific source. The contextual works below are references inherited from that article and cited for the roles it gives them; this editorial amendment is not an independent systematic literature review. Historical institutional details are attributed to the article’s source-period account.

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