

Psychotherapy access and young adults' registered relationship trajectories in Denmark

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Abstract

Individual psychotherapy may change how young adults manage dependence, shared living and long-term commitments. Evidence about service use or education does not establish whether access changes these social transitions. We preregister an examination of Denmark's July 2021 extension of free psychologist care to ages 18–24, connecting publicly reimbursed consultations with population-register residence, partner and parent links. Residents aged 24 at roll-out are compared with residents aged 25, relative to the mean corresponding age gap in 2017–2019. Population and outcome-specific risk sets are fixed one year before rollout. Existing users remain included, and outcomes encompass transitions before and after rollout through 12 and 24 months of follow-up. The study separates address-verified residence changes, recognized or legal unions, and newborn-linked recorded parenthood. It also examines administrative reclassification around age 25 and describes whether specified transitions concern the same baseline people, without claiming mediation. Twenty uptake and relationship comparisons share simultaneous inference, observation bounds and prespecified differential-bias scenarios. The empirical disposition is *[pending]*. Service-use evidence is *[pending]*; residential, partnership and recorded-parenthood evidence is *[pending]*. Causal interpretation requires counterfactual age-gap stability that cannot be established by the policy threshold or reassuring pretrends. Any identified policy effect combines access, copayment relief and system responses; it is not a therapy-received effect or a measure of relationship quality.

Keywords: Psychotherapy; young adulthood; living arrangements; partnership; recorded parenthood; Denmark; policy evaluation

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

Introduction

Leaving a parental home, forming or ending a shared household, and becoming a parent involve decisions about security, mutual commitments and practical capacity.

These transitions can be difficult when anxiety or depression constrains everyday functioning. Individual psychotherapy could therefore matter beyond symptoms or labor-market participation. Its effects need not point in a common direction. Greater capacity might enable independent residence, make continued parental support more workable, or help someone leave an unsatisfactory partnership. A change in marital or residential status has no universal welfare sign.

The empirical question is whether an expansion of access to individual psychotherapy changes subsequent registered living arrangements and family transitions. Administrative records provide unusually consistent person and household histories, but they do not observe friendship networks, loneliness, relationship satisfaction or subjective recovery. We use “relationship trajectories” to describe residence, recognized partners and legal parent links. We do not use the term as a proxy for social wellbeing.

Existing Danish evidence establishes the relevance of studying access while also narrowing the contribution we can claim. Kruse, Olsen and Skovsgaard (2022) evaluate copayment removal in relation to psychologist use and suicide attempts. Their article and supplement do not answer the present household-transition question. Ejrnæs, Gørtz and Waltenburg (2026) study psychotherapy and education; the institutional and appendix material inspected for this preregistration concerns a different outcome domain and a different identification argument. Jacobsen and colleagues’ VIVE evaluation (2024) reports serious difficulties identifying longer-term socioeconomic effects from the earlier reform. Its discussion of placebo patterns, education policy and pandemic disturbances is a warning against interpreting an age comparison as automatically causal. Serena’s work on scaling up psychotherapy access is another close Danish policy benchmark for consequences beyond clinical outcomes. The current abstract and bibliographic record were retrieved, but the latest journal PDF and appendices were inaccessible; overlap with uninspected material remains a novelty limitation. These studies motivate the present access audit; they do not validate our comparison.

Individual-treatment research also gives reasons to avoid a claim that interpersonal consequences are entirely unexplored. Renner, Cuijpers and Huibers (2014) synthesize controlled psychotherapy studies measuring social functioning, and Mufson and colleagues (2004) compare adolescent interpersonal psychotherapy with usual care using social-adjustment measures. The inspected abstracts report improvements, strengthening the intervention basis for the question. These clinical scales assess functioning and adjustment over treatment periods; moving address, joining or leaving a registered union, and acquiring a newborn parent link are different events. Neither the controlled-study synthesis nor the adolescent trial supplies an expected effect size for a population access reform. Their clinical results also do not identify a symptom-mediated path to household transitions. Gamble and colleagues (2011) examine interpersonal outcomes in a trial comparing interpersonal psychotherapy with individual usual care among depressed women with childhood sexual-abuse histories. Whisman (2001) examines marital adjustment across individual depression treatments. Hunsley and Lee’s review (1995) explicitly concerns marital consequences of individual treatment. These populations, treatments and self-reported outcomes differ from registered transitions

following a broad access reform. Conversely, associations between mental health and leaving home or cohabitation, such as Halidu and colleagues (2024), do not estimate an intervention effect. The contribution sought here is a joint, carefully delimited account of public-access policy and observable young-adult household transitions, not a “first study” claim.

The social-policy question has two separate levels. At the individual level, therapy may change the capacity to sustain or reconsider commitments. At the policy level, reducing a price barrier changes who can seek care, the resources of existing users and the allocation of provider time. Even a well-measured shift in household transitions would identify the second level only under a credible counterfactual. This distinction makes the reform useful for policy evaluation without treating population eligibility as randomized clinical treatment.

The outcomes are joined by a common substantive question about the organization of young-adult life, not by an assumed sequence everyone follows. Leaving home can precede, coincide with or follow partnership; a parent can remain in the parental household; returning home need not dissolve a union. We therefore preserve separate baseline populations and allow mutually compatible transitions. Their complementarity comes from distinguishing initiation, persistence and reversal of arrangements that a prevalence measure would combine.

The evidence proceeds through three questions. First, what changes in access and in administrative observation at the relevant ages? We separate new-referral opportunity from billed standard individual use, and actual address changes from family recognition. Second, do residential, partnership and recorded-parenthood margins differ under the same fixed age-by-era comparison? Their distinct baseline populations and one common multiplicity family keep that comparison explicit. Third, do specified transitions concern the same people, and what can quarterly recognition establish about their order? The joint descriptions retain everyone in their original baseline intersections, including those with neither event. They add information that separate marginal contrasts cannot provide, without identifying a mediation chain.

Policy setting and conceptual framework

What changed, and for whom

The publicly subsidized psychologist scheme requires clinical eligibility and referral. Free care for young people with mild or moderate anxiety or depression began for ages 18–20 on 1 July 2018. Executive Order 989 made the extension through age 21 effective on 1 October 2019. We use the legal effective date rather than the September issue date. The Ministry announced the permanent scheme and the subsequent extension through age 24 on 6 December 2020. Executive Order 1213 then made the broader entitlement effective on 1 July 2021. The order permits up to 12 consultations and, under the stipulated conditions, up to 12 more following rereferral. It permits more than one treatment format.

The historical July 2021 agreement between the regions and psychologist association is pivotal. Its transition provision covers existing patients aged 22–24, and free

courses may continue after the patient turns 25. A December 2021 agreement, effective January 2022, preserves continuation and reorganizes billing codes. Thus a 25th birthday is not an abrupt withdrawal of ongoing treatment. Fee week is also not referral date. Neither an age discontinuity in entitlement nor a change in a free-care code demonstrates a discontinuity in cumulative individual therapy.

We focus on residents aged 24 versus 25 on 1 July 2021. The age-24 birth interval is 2 July 1996 through 1 July 1997. These individuals were too old for the earlier 2018 and 2019 waivers. Within this age band, the remaining window for a new eligible referral ranges from very short to almost one year, while a course begun during that window can continue afterward. Adjacent historical cohorts indexed in 2017, 2018 and 2019 supply reference age gaps. The index individuals in these historical comparisons also remain too old for the waiver throughout their specified follow-up. This cohort bookkeeping avoids prior entitlement of index people; it does not eliminate exposure of their partners or provider responses to treatment of younger patients.

Table 1. Policy and observation clocks (determined design facts).

Item	Date/window	Meaning
Initial waiver	1 July 2018; ages 18–20	Additional 40% subsidy within clinical referral scheme.
First expansion	1 October 2019; through age 21	Effective date differs from order issue date.
Known announcement	6 December 2020	Known anticipation opportunity; not an exhaustive earliest-date finding.
Main expansion	1 July 2021; through age 24	New-referral opportunity; existing courses may continue after 25.
Baseline for index c	30 June ($c-1$)	Risk sets fixed before verified announcement for 2021.
Main ages at R_c	24 versus 25	Historical gaps in 2017/2018/2019 receive equal weights.
h_{12} / h_{24}	30 June ($c+1$) / 30 June ($c+2$)	Events accumulate from B, two/three years since baseline.

Sources: official orders and agreements in the policy text and source ledger. These are determined design facts, not hypothetical or empirical observations. This is a pre-data manuscript template.

The distinction between announcement and implementation also changes the population of interest. Restricting the analysis to people who remain at home

or unpartnered until rollout could discard transitions made in anticipation of the announced reform. The earlier baseline retains those people. Its cost is that the registered cumulative outcome includes months when new free referrals were not yet available; that dilution is part of the fixed policy comparison and will not be repaired by moving the baseline after seeing the trajectory. Figure 1 separates potential entitlement duration from billed use for the same reason: neither clock is itself treatment duration.

Why household outcomes could change in either direction

Clinical improvement could reduce avoidance, improve planning or change the ability to tolerate and address interpersonal conflict. These are theoretical pathways; the records do not measure them. Residential independence also requires housing resources. The parental household can provide insurance, support or constraints, so either departure or return could follow improved capacity. We distinguish leaving from sustained residence apart and from return after a period of living apart. A transitory move is different from a durable change, but duration still does not establish benefit.

Partnership entry and continuation depend on another person's behavior and on relationship-specific investments. Treatment could support a stable existing relationship, make separation possible, or change the timing of forming a shared household. Parenthood involves a different, often longer planning horizon. An observed newborn link could reflect plans formed before the reform, a change in timing, or changes in legal recognition. No information here establishes intentions or conception. A two-year horizon can reveal some registered transitions while remaining too short to characterize completed fertility or enduring interpersonal recovery.

Evidence from selected reproductive-treatment settings cannot supply an expected population effect on parenthood. Poulter and colleagues (2026) evaluate a self-guided intervention for infertility-related distress, with a treatment format and population distinct from this therapist-access reform. The inspected intervention and outcome descriptions reinforce the need to distinguish symptom relief, relationship measures and reproductive events. This study registers the newborn-link question without borrowing an effect size or conception mechanism from that setting.

The financial and system pathways are equally important. Copayment relief can directly change resources available for rent or family formation, including for existing patients who do not increase consultations. The reform may affect waiting times, provider allocation, other therapy formats and access among the comparison ages. A positive uptake contrast therefore supports an access margin; it cannot isolate the therapeutic mechanism. We neither instrument therapy with eligibility nor divide household contrasts by an uptake first stage.

Data and empirical strategy

Population, clocks and observable outcomes

BEF population snapshots identify residential addresses, recognized or legal partners, and parent links; SSSY records publicly reimbursed psychologist services. These documented sources are within the user’s 47-table boundary, but delivered histories and linkage quality remain unvalidated. Optional certified DODSAASG dates help distinguish death from unexplained exits. The study represents baseline registered residents, including people who never receive therapy. It does not require labor-market data or the partial AMRUN extract. Detailed fields, coverage and linkage accounting are in SI S1.

For index year c , residents are selected on 30 June $c - 1$ and grouped by age 24 or 25 on 1 July c . The 2021 baseline therefore precedes the verified December 2020 announcement. Events are followed from baseline through June of $c + 1$ and $c + 2$, the endpoints labeled 12 and 24 months after rollout: two and three years after baseline. People remain in their original denominators after moving, partnering, treatment, exit or death. An earlier relevant announcement before this fixed baseline would undermine the registered timing; its absence has not been exhaustively established.

Table 2 gives each outcome’s pre-exposure population and confirming evidence. Residential departure and sustained residence apart concern original parental co-residents; return concerns people already living apart over the required baseline history. Entry and continuation of partnerships use different fixed baseline populations, while legal status remains distinct from inferred cohabitation. Recorded parenthood requires the full prebaseline history with no observed child link. These populations overlap but cannot be combined into one flow or treated as selected transition hazards. SI S1–S2 explains the exact histories and SI S4 the parenthood ascertainment.

A family classification changes at age 25 even when a child and parent remain at the same address. We therefore compare actual contemporaneous addresses of frozen parent identities. The independent recognition diagnostic in Figure S1 examines administrative changes with unchanged people and addresses. Partnership events similarly follow the original persons: recognition loss alone is not separation, and end-point residence with the same partner permits an intervening separation and reunion. Legal spouse and recognized partner can be different people; only appropriately matched death evidence changes the corresponding competing-event interpretation. The detailed identity and widowhood rules are in SI S2 and S5.

Newborn-linked parenthood records a timely legal parent link among people without prior links in the fixed history. It neither establishes lifetime biological childlessness nor dates conception, intentions or treatment response. Missing birth or parent histories cannot be replaced by household child counts. All family measures retain limits from unrecognized relationships, incomplete same-sex inferred coverage, children abroad and events occurring between quarterly snapshots. SI S4 explains the separate birth, recognition and death clocks.

Incomplete follow-up is bounded within known baseline populations; unresolved baseline membership limits the population those bounds describe. Death, unknown

exits and absent required periods have different meanings, and independently supported endpoints remain reportable. Those distinctions determine the scope of the main answer, with full accounting in SI S1–S5. Billed standard individual consultations also identify a code-use margin rather than a particular modality, duration, referral date or total private therapy; dated service construction is explained in SI S1.

Table 2. What each relationship measure represents (determined definitions).

Outcome	Fixed baseline population	Required confirmation	Non-equivalent concept
Leave / sustained-away	Parental co-residents	Young-person address move; for persistence four final quarters apart	Financial independence; family-ID departure
Return	Known apart at four baseline snapshots	Young person moves to parent’s prior/current address	Parent-only move; selected return hazard
Union entry	Known unpartnered at two snapshots	Reciprocal identity and new shared residence	All romance; recognition alone
Separation / same partner	Stable original pair	Known original-person address split / endpoint shared residence	Relationship quality; no intervening separation
Legal entry / dissolution	Relevant baseline legal status	Dated status and original spouse identity	New cohabitation; widowhood
Recorded parenthood	Full observed-history nonparent risk set	Timely newborn parent-link recognition and confirmation	Lifetime biological first birth; pregnancy intention

These are determined definitions from the native protocols, not hypothetical or empirical observations. All use contemporary addresses and frozen identities. Missing membership and observation are explicit; legal and inferred-union coverage differ.

Comparison and identifying assumptions

For outcome j and horizon h , let $p_{c,a,j,h}$ be the event probability in index year c , age a and that outcome’s baseline risk set. The registered comparison is

$$\theta_{j,h} = (p_{2021,24,j,h} - p_{2021,25,j,h}) - \frac{1}{3} \sum_{c \in \{2017,2018,2019\}} (p_{c,24,j,h} - p_{c,25,j,h}).$$

Every person has equal weight within a cell, and each historical year has equal weight. There is no outcome-selected adjustment model, pooling of earlier eligible ages, regression discontinuity or treatment-received instrumental variable. The estimand is initially an age-by-era comparison of registered transition probabilities. Calling it a policy effect additionally requires that the untreated age gap in 2021 would equal its historical reference, within the specified sensitivity allowance, and that differential spillovers and concurrent shocks do not invalidate that reference.

This assumption is demanding. Pandemic restrictions and recovery can affect adjacent ages differently. Education-linked housing allowances and cohort patterns may create age gradients that change across eras. The documented 2014 SU rules require a cohort-policy audit; family-reunification rules involve age 24, with historical comparability unresolved. The December 2020 announcement also concerns nationwide headspace expansion. Broad time effects cancel only if their consequences are equal across comparison ages. Provider crowding out and partners' eligibility could produce differential spillovers. Birth-month clustering addresses one form of dependence; it does not make these confounders disappear.

We therefore report fixed historical age gaps, pre-rollout trajectory comparisons, ages 25 versus 26 as a negative-control comparison, and omission of each historical reference year. These diagnostics are challenges to the comparison, not a model-selection test. They cannot establish the missing counterfactual. We also widen simultaneous observation sets by hypothetical bias allowances of 0, 1 and 2 percentage points. No allowance has been verified as a maximum plausible policy-confounding bias. A directional finding conditional on the 2-point scenario remains conditional; with unrestricted differential bias, the causal direction is unidentified. This elementary bias calculation is not an implementation of HonestDiD.

Observation and uncertainty

Quarterly snapshots interval-censor events. A whole missing required delivery blocks the affected construct. The requirements are component-specific: missing return lookback does not suppress baseline co-residents' departure outcomes, and missing recognized-pair history does not suppress a source-valid legal-status comparison. Partial support remains visible in the headline scope and all twenty comparisons retain their place in the inferential family. Individual disappearance or an unresolved address is not coded as no event, emigration or death. Each person contributes a lower and upper event indicator: a confirmed cumulative event remains one, a known non-event is zero, and unresolved observation spans zero to one. The linear contrast combines cell bounds according to the sign of its coefficients. Thus missingness among negatively weighted comparison cells can lower, rather than raise, the contrast's lower bound.

Ten endpoints at two horizons form a fixed family of 20 comparisons: standard individual use, three residential margins, five partnership margins and recorded parenthood. Simultaneous intervals for the contrast bounds use a Bonferroni allocation across 40 one-sided bound endpoints. We also report ordinary 95% intervals and 20-comparison adjusted p values for the confirmed-record contrasts, clearly distinguished from inference about incompletely observed outcomes. Unplayed or unavailable comparisons do not reduce the family size. No equivalence margin is registered, and imprecision is never “no effect.”

Inference clusters by pre-exposure family components and birth-year-month, subtracting their intersection component. Family components connect baseline parents, recognized partners and co-residents across index cohorts. Small numbers of clusters, a dominant family component or invalid variance leave point bounds available while precision is unavailable. Correlated outcomes of new postbaseline partners or co-parents are a residual limitation of this fixed clustering scheme; a numerical bias allowance does not repair covariance misspecification. Confidence statements remain conditional on the stated cluster approximation and independence assumptions, as detailed in SI.

The policy comparison is reported only within its documented exposure and measurement scope. A favorable uptake finding is not a prerequisite for reporting the fixed relationship comparisons. Missing or unexecuted evidence remains distinguishable from an observed absence of events; the exact execution and interpretation contingencies are registered in the accompanying protocol. These rules preserve the question and all outcomes without choosing hypotheses from their results.

Results

The empirical results are pending. Any exhibit labeled ILLUSTRATIVE MOCK DATA — NOT RESULTS is a presentation preview of the registered display, not evidence that an analysis is available or a result state has occurred. Every empirical narrative slot and every underlying table-cell binding remains pending in the editable manuscript. Mock numbers are neither predictions nor scientific evidence, and production analysis and plotting have no mock fallback.

The access margin

The policy and service-code audit is *[pending]*. Figure 1 places the verified new-referral opportunity beside billed standard individual use. The fixed baseline population is *[pending]*, with outcome-specific risk sets in Tables S1a–S1b, and the uptake comparison is *[pending]*. Reading entitlement and use together establishes which observable access margin the evidence can support. It does not identify clinical change or total therapy received.

The measured use margin provides the context for interpreting household outcomes, while financial and provider channels remain open. Copayment relief for existing users could change living decisions with little measured change in standard consultation use. Conversely, a clear use increase without a resolved household contrast would not establish that social consequences are absent. Format-specific descriptive shares are *[pending]*. The fixed precision and observation accounting

ILLUSTRATIVE MOCK DATA — NOT RESULTS

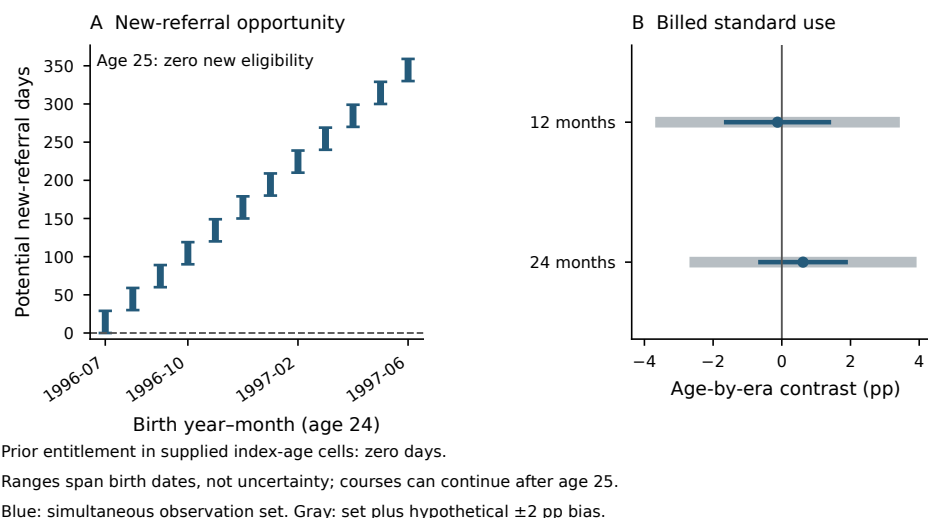


Fig. 1 ILLUSTRATIVE MOCK DATA — NOT RESULTS. Potential access and use recorded under standard individual first/follow-up consultation codes. Panel A shows the documented range of new-referral eligibility days by birth month among the age-24 cohort in 2021; age 25 has no new eligibility. It is not days of therapy. Panel B shows the age-24-versus-25 contrast relative to the mean historical age gap at 12 and 24 months, over the fixed baseline population, with simultaneous observation bounds for the 20-comparison family. Gray intervals add a hypothetical 2 pp differential-bias allowance to the blue simultaneous observation sets. Existing users are included and courses can continue after age 25. These billing codes do not establish office setting, encounter duration or psychotherapeutic modality. A supported point remains visible when precision is unavailable; no interval is fabricated. Empirical results remain pending.

accompany the access comparison in SI. The fee-week sensitivity is *[pending]* (Table S7): weeks spanning a boundary can move across analytical windows without representing a different clinical referral. Its comparison checks the registered billing clock and does not replace the primary uptake definition.

Relationship trajectories

Establishing the access margin leaves open whether a change in publicly billed care reaches observable living arrangements. Residential persistence addresses a first distinction: more recorded departures need not mean more sustained time apart from parents. Return examines a second distinction in a different population: support from parents may remain relevant after independence has already been established. These outcomes test changes in arrangements, while housing prices, resources, preferences and unmeasured support remain possible explanations for the pattern.

The residential disposition is *[pending]*, with endpoint evidence *[pending]*. Departure and sustained residence apart jointly distinguish a recorded move from persistence at separate addresses. Return provides a separate margin among those already living apart before exposure. A positive contrast on one margin and a negative contrast on another describes different movements through living arrangements, not an incoherent welfare index. Parent-only and joint moves, death and unresolved observation remain visible in the construction accounting.

The main return comparison concerns people already living apart at baseline. It cannot say how often a new departure is followed by return among the original co-residents. That separate descriptive accounting is *[pending]* in Table S9. Its denominator includes all original co-residents, not only people who leave. Departure, sustained residence apart and departure-followed-by-return can overlap; their fractions are not a household-stage partition or a new policy-effect decomposition.

Residence alone cannot show whether a young adult is living with a partner or merely at a different address. Partnership outcomes therefore carry the argument from location to the identity and continuity of the person with whom a household is shared. Their interpretation needs particular care at an age where the register can change family recognition without anyone moving. The independent historical recognition experiment addresses that rival explanation directly; it does not supply a substitute treatment comparison.

The partnership disposition is *[pending]*, with evidence *[pending]*. Entry into a newly shared recognized union, separation from the original partner, endpoint residence with that same person, legal entry and legal dissolution answer related but distinct questions. A result confined to legal status could reflect formalization rather than a new shared household. A result confined to recognized unions could be sensitive to recognition coverage. Loss of recognition at an unchanged address is never sufficient evidence of separation. The baseline partner-age and waiver-history accounting is *[pending]*, with unique dyads in Table S6. Differences in partner exposure remain part of the regime comparison and cannot be interpreted as an independently identified partner-treatment effect. Construction flags are *[pending]* (Table S8). The independent historical diagnostic is *[pending]*, with Figure S1 and the recognition accounting showing the observed distinction when available.

A partnership change leaves a distinct commitment unresolved: becoming a recorded parent. Parenthood is included as an outcome of the same access regime, not as evidence that a residential or partnership response caused a birth. A directional household pattern alongside unresolved parent links would distinguish the evidence available for those margins without establishing or refuting a longer reproductive response.

The recorded-parenthood disposition is *[pending]*, with evidence *[pending]* and ascertainment in Tables S4a–S4b. A directional newborn-link contrast would concern timing of recorded parenthood in this observed-history population. It would not identify fertility intentions, conceptions after therapy, biological first births or completed family size. An unresolved contrast would be especially limited by the rarity and timing of qualifying events if runtime evidence establishes those features; their frequency and precision are not known at registration.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

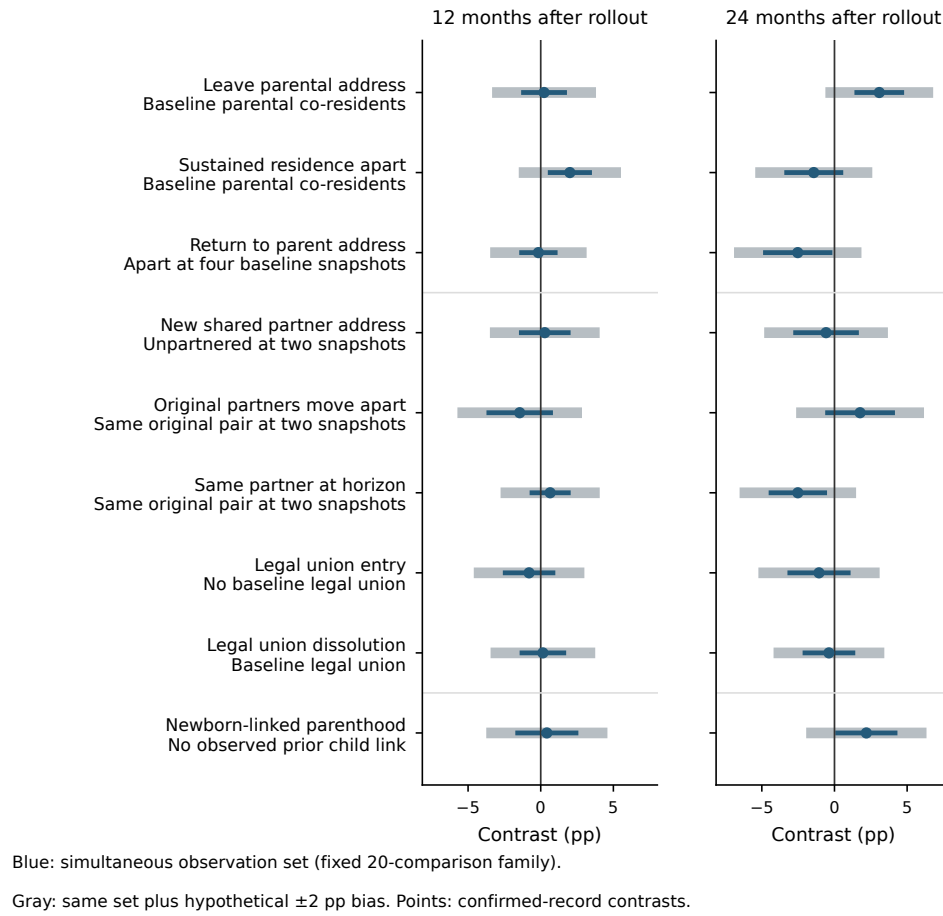


Fig. 2 ILLUSTRATIVE MOCK DATA — NOT RESULTS. Registered relationship trajectories at 12 and 24 months after rollout. Each row uses its own fixed pre-exposure risk set: parental co-residence, prior residence apart, no recognized partner, baseline partner, legal status, or no observed prior child link. Points are confirmed-record contrasts in percentage points. Blue lines are simultaneous missing-observation sets; gray lines additionally allow hypothetical differential bias of 2 pp. The family includes 20 comparisons, including uptake. No row has a universal welfare sign, and newborn-linked parenthood is not true first biological parenthood. Empirical results remain pending.

Timing and counterfactual interpretation

The two main horizons summarize magnitudes and uncertainty, but they cannot show when a registered pattern emerges or changes. Figure 3 uses the already specified quarterly comparisons to place uptake, residential transitions, partnership outcomes

and recorded parenthood on the same policy clock. Every panel keeps its own baseline population. The paths show confirmed-record contrasts; ordinary pointwise intervals describe their precision without replacing the main simultaneous observation and bias sets.

The earlier and later parts of these paths answer different questions. Pre-rollout differences can challenge an immediate-rollout account, while observations after the December 2020 announcement can also contain anticipation. Sustained residence apart requires four final quarterly observations, and parenthood uses timely recognition of a newborn link; apparent delays can therefore reflect measurement as well as behavior. Continued co-residence is a state at each horizon, not a cumulative absence of separation. A trajectory does not identify the first causal effect, a recovery time or a sequence of clinical mechanisms.

The sequence of outcomes cannot by itself distinguish a common policy response from a common age-specific disturbance. A similar directional pattern across residence, partnership and parenthood may be compatible with improved capacity, but it can also arise from financial relief or a pandemic-related change in housing and family timing. This is why the counterfactual checks apply to each outcome, and why the evidence is not strengthened merely by narrating the domains as successive stages.

The fixed counterfactual diagnostics are *[pending]*, and the sensitivity to observation and differential bias is *[pending]*. Tables S2a–S2c show all 20 prespecified comparisons. Tables S3a–S3b and Figure 3 retain the reference-year, adjacent-age and lead comparisons even when they are unfavorable. A failure to reject a lead or negative-control contrast does not certify parallel counterfactual age gaps. A resolved diagnostic difference weakens the simple stable-gap account, without itself identifying the direction of bias in any household endpoint.

Whose transitions coincide?

Separate marginal contrasts do not identify the overlap of individual treatment effects. A distinct, observable question is which registered events occur in the same baseline people. We therefore register three descriptive comparisons in populations fixed before exposure: baseline parental co-residents without a recognized partner; baseline paired people already living apart from parents; and people without a recognized partner or prior child link under the full observed-history definition. Their joint-transition disposition is *[pending]*. These comparisons are defined regardless of uptake or the direction of the policy contrasts. They use the same event constructors but do not require those contrasts to be identified.

Figure 4 describes which events are jointly observed in the 2021 cohorts by 24 months, retaining people with neither event, only one event, both events and uncertain observation. The supported pattern description is *[pending]*. Home departure with union entry, return with original-pair separation, and union entry with newborn-linked recorded parenthood answer different same-person questions. Each has its own known baseline denominator; no shared-population flow or relative welfare ranking follows from comparing their fractions.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

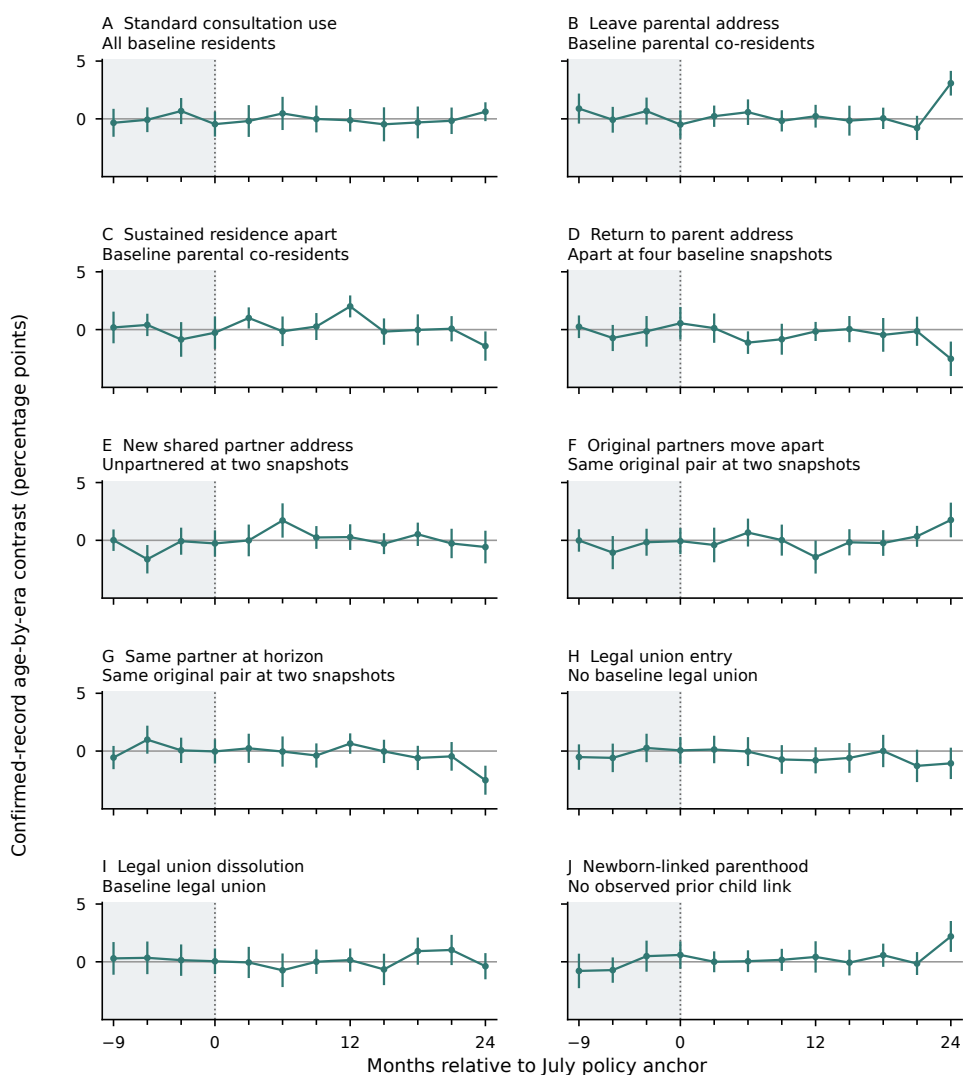


Fig. 3 ILLUSTRATIVE MOCK DATA — NOT RESULTS. Quarterly confirmed-record age-by-era contrasts for uptake and all nine relationship endpoints, in their fixed baseline populations. Points connect adjacent observed quarters; vertical bars are ordinary pointwise conditional 95% intervals, not simultaneous observation or bias bounds. All panels use percentage points and the same scale. Shading marks pre-rollout quarters, which may include anticipation after December 2020; month 0 ends immediately before rollout. Missing quarters break the path. Timing reflects the registered observation rules and does not identify causal onset or recovery. Main inference remains in Figure 2 and Tables S2a–S2c. Empirical results remain pending.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

A Leaving home and union entry

A: leave parental address; B: new shared partner address

Baseline co-resident and recognized unpartnered

	Neither	A only	B only	A before B	Same observed quarter	B before A	Both; order unresolved	Event membership unresolved
Age 24 n=1,000; b?=13	38.7	15.5	10.4	8.2	14.0	6.2	2.7	4.3
Age 25 n=1,100; b?=13	37.9	16.5	11.0	8.0	12.1	7.4	2.5	4.7

B Return and original-pair separation

A: return to parent; B: separate from original partner

Baseline away from parents and stably paired

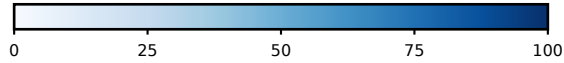
Age 24 n=1,000; b?=13	45.9	16.6	10.2	7.9	10.8	0.0	3.2	5.4
Age 25 n=1,100; b?=13	37.2	17.2	11.3	7.7	11.3	6.5	3.9	4.9

C Union entry and recorded parenthood

A: union entry; B: first qualifying parent-link recognition

Baseline unpartnered and recorded nonparent

Age 24 n=1,000; b?=13	40.2	15.3	10.6	7.7	12.0	6.7	3.8	3.7
Age 25 n=1,100; b?=13	37.5	15.5	12.2	8.6	11.9	6.7	3.4	4.2



Share of known baseline intersection (%)

2021 index; 24 months after rollout. b?: unresolved baseline membership.

Hatching: unresolved order/event status. Crosshatching + NA: unavailable, not zero.

Fig. 4 ILLUSTRATIVE MOCK DATA — NOT RESULTS. Joint observation patterns in three pre-exposure intersection populations, 2021 at 24 months. Each facet contains ages 24 and 25; all eight pattern columns remain present. Color and printed percentages denote shares of known baseline members, with n and unresolved baseline membership shown separately. Same observed quarter does not establish simultaneity. Parenthood timing is first qualifying link recognition, not conception or birth timing. Unknown and unavailable states have distinct marks. Finite-cohort description only; no causal mediation or statistical significance is inferred. Empirical results remain pending.

Table 3 gives the corresponding conservative co-occurrence bounds. The ordering assessment is *[pending]*. A confirmed sequence requires known first-observation intervals with no earlier unknown opportunity; a shared quarter or uncertain preceding history limits order. Even an established observed sequence does not show that

one transition caused another. Tables S10–S11 retain all historical cohorts and both horizons, including unavailable cells.

Table 3. Joint-event bounds in fixed baseline intersections. A. Denominators and observation scope

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Event unknown n	Scope / reason
Home / union	2021	24	24	1,000	13	43	[1]
Home / union	2021	25	24	1,100	13	52	[1]
Return / separation	2021	24	24	1,000	13	54	[1]
Return / separation	2021	25	24	1,100	13	54	[1]
Union / parenthood	2021	24	24	1,000	13	37	[1]
Union / parenthood	2021	25	24	1,100	13	46	[1]

Table 3. Joint-event bounds in fixed baseline intersections. B. Marginal and joint observation bounds

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	A bounds (%)	B bounds (%)	Both bounds (%)
Home / union	2021	24	24	46.6 – 50.9	41.5 – 45.8	31.1 – 35.4
Home / union	2021	25	24	46.4 – 51.1	40.9 – 45.6	29.9 – 34.6
Return / separation	2021	24	24	38.5 – 43.9	32.1 – 37.5	21.9 – 27.3
Return / separation	2021	25	24	46.6 – 51.5	40.7 – 45.6	29.5 – 34.4
Union / parenthood	2021	24	24	45.5 – 49.2	40.8 – 44.5	30.2 – 33.9
Union / parenthood	2021	25	24	46.1 – 50.3	42.8 – 47.0	30.6 – 34.8

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

A/B: home_union=leave/union_entry; return_separation=return/original-pair separation; union_parenthood=union_entry/newborn-linked recorded parenthood. Bounds describe observation among known baseline members, not sampling uncertainty or a policy effect. Unknown baseline membership is outside these fractions.

Discussion

The study connects public psychotherapy access to how young adults arrange residence, partnerships and parenthood. Its integrated answer is *[pending]*. These domains matter because initiation, persistence and reversal can carry different consequences, even when a single prevalence measure changes little. An informative answer therefore names the supported transitions, their original populations and the uncertainty that remains, without assigning a common welfare sign.

Entitlement, billed use and household transitions provide different pieces of that answer. Public billing establishes an observable access margin, while the policy also changes existing users' costs and provider allocation. Consequently a relationship contrast cannot isolate the effect of receiving individual therapy. The quarterly profiles reveal the timing hidden by endpoint summaries, but neither aligned curves nor an apparently delayed response identifies clinical onset, recovery or a causal chain. Recognition and construction rules must remain part of their interpretation.

The joint descriptions add information about observed lives that marginal estimates cannot supply. Departure and union entry may be observed in the same people; return and original-pair separation concern a separately fixed population; union entry and newborn linkage connect household and family formation. Their reported overlap and ordering remain descriptions, including unresolved observation. Even perfectly observed joint events would not reveal whose individual treatment effects were jointly nonzero or what share of a policy effect was mediated.

The main inferential limit is counterfactual age-gap stability. A national rollout provides no independent replication of arbitrary age-specific policy shocks. Pandemic conditions, education or housing changes, differential partner access and provider responses can affect neighboring cohorts differently. The registered historical, lead and adjacent-age comparisons make these objections inspectable but cannot verify the hypothetical bias allowances. Direction under a stated allowance is a conditional statement; unrestricted differential bias leaves policy attribution unresolved. New partnerships or co-parents can also create residual dependence beyond the registered baseline clusters, which bias widening does not repair.

The observation limit is equally consequential. Shared addresses do not measure emotional or financial independence; administrative couples do not cover all romantic relationships; newborn links do not establish lifetime first biological births or intentions. Within-quarter episodes and incomplete baseline membership restrict what can be described even with complete delivered files. The population is the specified baseline residents and observable risk sets, rather than all clinically eligible young adults. Each outcome domain remains part of the question when its evidence is imprecise or unavailable.

These distinctions support a substantive evaluation of access policy while leaving the therapeutic and welfare meanings appropriately open. Residential independence, partnership and family formation remain central to the answer alongside its measured scope. The study's contribution depends on what those comparisons resolve, not on a predetermined direction or a successful statistical test.

Data and code availability

No confidential rows or computed empirical summaries were accessed during authoring. The schema boundary, source locators, protocols, existing reference code and final implementation interfaces accompany this scientific handoff. Empirical access requires the data custodian’s approved environment and disclosure review; no row-level data are distributed. Tests construct small synthetic counterexamples in memory. Exhibit preview sources generate aggregate mock inputs only and are separate from production analysis. Ethical authorization, custodian approvals and any disclosure suppression actually applied remain *[pending]*. This is a disclosed pre-data refinement of an unexecuted scientific baseline. The amendment distinguishes the earlier six-Atom scientific additions from this editorial allocation and timing-display refinement. No registry deposit, runtime availability or empirical result is asserted.

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Supplementary Information

S1 Population accounting and observation semantics

The unit of analysis is a baseline person within an index cohort, followed on a fixed calendar. Birth dates determine completed age on 1 July; the same individual may qualify in more than one reference year. Baseline residence is established on 30 June of the preceding year. The analysis does not select on subsequent service use, partnership, parenthood, migration, response or survival. These rules maintain the interpretation of a total trajectory contrast within a pre-exposure stratum. Quarter-end event observations cannot recover a move followed by a return between snapshots, and no interpolation creates such an event.

The residence history freezes the identities of the baseline-linked parents but follows their contemporaneous addresses. This avoids redefining a parent after exposure while allowing parental mobility. A young adult who remains in place as a parent moves away is not coded as a qualifying departure. A young adult already apart qualifies for return only when their own address changes to a parent’s address that is stable across the relevant observations. These restrictions favor an interpretable physical transition over a broader change in family classification. They may miss short or ambiguously ordered moves, which is reported as a measurement limit rather than assigned a behavioral explanation.

Tables S1a and S1b separate the shared cohort from the risk sets that determine the meaning of each outcome. A person can belong to more than one risk set, and the same person can appear in more than one historical index year. Counts must not be added across columns or years as though they were independent samples. Ages 25 and 26 used for the negative-control analysis follow the same construction. Baseline sex, municipality, citizenship and recorded immigration categories accompany the full confidential cohort accounting; their actual category support is unknown before delivery. They describe the comparison population and do not select a preferred adjustment specification.

Table S1a. Baseline populations

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Cohort	At parent address	Prior apart
2017	24	5,000	1,000	3,000
2017	25	5,000	1,000	3,000
2018	24	5,000	1,000	3,000
2018	25	5,000	1,000	3,000
2019	24	5,000	1,000	3,000
2019	25	5,000	1,000	3,000
2021	24	5,000	1,000	3,000
2021	25	5,000	1,000	3,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Empirical counts remain pending; the displayed counts are hypothetical. Columns overlap; no sum across risk sets or index years is defined.

Table S1b. Relationship-specific baseline risk sets

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	No pair	Paired	Legal entry risk	Legal union	No prior link
2017	24	2,000	2,500	3,500	1,500	3,400
2017	25	2,000	2,500	3,500	1,500	3,400
2018	24	2,000	2,500	3,500	1,500	3,400
2018	25	2,000	2,500	3,500	1,500	3,400
2019	24	2,000	2,500	3,500	1,500	3,400
2019	25	2,000	2,500	3,500	1,500	3,400
2021	24	2,000	2,500	3,500	1,500	3,400
2021	25	2,000	2,500	3,500	1,500	3,400

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Empirical counts remain pending; the displayed counts are hypothetical. Columns overlap; no sum across risk sets or index years is defined.

BEF contributes PNR, FOED_DAG and REPERNCETID for identity and time, ADRESSE_ID for actual residence, MOR_ID and FAR_ID for parents, E_FAELE_ID and AEGTE_ID for recognized partners and spouses, FAMILIE_ID, FAMILIE_TYPE and PLADS for recognition audits, and CIVST and CIV_VFRA for civil status. Optional contextual fields are KOEN, KOM, STATSB and SEN-ESTE_INDVANDRING. The last immigration field is baseline context, not a contemporaneous emigration date. Dates and encrypted identifiers must be normalized consistently by a documented mechanical adapter. Delivered quarter-end dates are certified directly; dates are never inferred from filenames.

The first linkage assertion is one unambiguous row per person and reference date. Exact repeated rows may be collapsed; conflicting rows halt the affected construction. Birth dates must agree across observations. Parent and partner joins are person-to-person within the same reference date and must be many-to-one; they never cross-join unrelated dates. The baseline parent identities remain fixed. A missing linked parent is unresolved unless the optional certified death source establishes the relevant event. Child-to-parent inversion retains each distinct child-parent-snapshot link from both parent fields, removing only identical duplicates. All resident-child rows are required, not only children selected by the index cohort's household.

Documented deaths are competing events, with no voluntary move or separation attributed to death. For cumulative events already confirmed, a later death or disappearance does not erase the event. For endpoint residence with a partner or sustained residence apart, death before the endpoint precludes the defined living state. The bounded outcome covers unresolved individual observation; it does not impute an event time. Required entire periods or a missing parent/child linkage field are evidential data boundaries, while a failed read or unfinished certification is an operational

no-result. These distinctions are retained in the narrative disposition and cannot be converted into zero incidence.

SSSY contributes PNR, SPEC2, SPECIALE, HONUGE, YDLANT, YDERNR and AFRPER. Specialty 63 is used only with the SSSY dictionary, not a different specialty system. A dated dictionary must distinguish standard individual first/follow-up consultations from double sessions, remote/advice, two-person and group formats, and from administrative or erroneous-referral records. The October 2021 schedule supports standard consultation codes 0110/0111, 0210/0211 and 0410/0411 in their documented regimes; it is not evidence that every earlier vintage used the same code meanings. The January 2022 restructuring requires separate validity intervals. Primary use counts any positive, net-final standard consultation claim under any documented copayment regime during the event window, regardless of indication or prior use. It is not the count of free-coded claims and not a referral or initiation measure.

Fee weeks are mapped through a certified official calendar to their Monday. AFRPER remains a settlement period, and neither field is treated as a clinical appointment or referral date. The net-final adapter must resolve revisions and cancellations before person-period use is derived. Negative unresolved adjustments, conflicting specialty prefixes, unknown primary semantics or incomplete vintage coverage block the uptake construct. A certified complete period with no standard consultation use can support zero; absence of an extract cannot. Other formats are descriptive shares of the same baseline population and horizons, with no independent causal or confirmatory treatment comparison.

Table S7a. Boundary-week accounting

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Mo.	N	Primary users	Retained users	Rows removed
2017	24	12	5,000	320	310	26
2017	24	24	5,000	320	310	26
2017	25	12	5,000	320	310	26
2017	25	24	5,000	320	310	26
2017	26	12	5,000	320	310	26
2017	26	24	5,000	320	310	26
2018	24	12	5,000	320	310	26
2018	24	24	5,000	320	310	26
2018	25	12	5,000	320	310	26
2018	25	24	5,000	320	310	26
2018	26	12	5,000	320	310	26
2018	26	24	5,000	320	310	26
2019	24	12	5,000	320	310	26
2019	24	24	5,000	320	310	26
2019	25	12	5,000	320	310	26
2019	25	24	5,000	320	310	26

Continued on next page.

Table S7a continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Mo.	N	Primary users	Retained users	Rows removed
2019	26	12	5,000	320	310	26
2019	26	24	5,000	320	310	26
2021	24	12	5,000	320	310	26
2021	24	24	5,000	320	310	26
2021	25	12	5,000	320	310	26
2021	25	24	5,000	320	310	26
2021	26	12	5,000	320	310	26
2021	26	24	5,000	320	310	26

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Monday–Sunday weeks containing baseline, rollout or horizon are excluded only in this sensitivity. N is unchanged. Users count people with standard individual consultation claims; removed rows cover all positive net-final psychologist formats. Counts are descriptive, not new tests.

Table S7b. Fixed contrast after boundary-week exclusion

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Mo.	Contrast (pp)	Ordinary95% interval
12	0.6	−2.1, 3.3
24	0.6	−2.1, 3.3

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

The era weights and risk set are unchanged. This descriptive sensitivity never replaces primary uptake or changes its registered state; unavailable precision remains unavailable.

S2 Recognition and residence

Statistics Denmark’s E-family rules treat an at-home child as belonging to the parent’s family only below age 25, subject to other criteria. Crossing that boundary can change the family identifier without changing either person’s address. For childless inferred couples, opposite-sex, age-gap, kinship and household-composition restrictions further limit interpretation. Historical definitions and their validity periods must be checked rather than inferred from current documentation. The legal-partner and recognized-cohabitation measures consequently retain separate reporting. Lolk and colleagues’ Danish study of depression and family dissolution illustrates the adjacent register question, but its annual family-ID-based measurement and opposite-sex study population are not transferred to the present age-policy design.

The independent descriptive experiment compares the quarter immediately before and the first quarter on or after birthdays 24, 25 and 26 during 2017–2019. Its denominator is fixed from the pre-birthday observation. Among parent-co-resident people sharing a family identifier, it records family departure and the subset with unchanged young-person and parent addresses. Among recognized pairs, it records recognition loss at a still-shared address and address separation from the original partner. Birthdays on quarter ends use the prior quarter as the pre-birthday observation. A 29 February birthday uses completed age on 1 March in nonleap years. Changes in household membership and residents crossing age 18 are descriptive context flags for the same recognized-pair opportunities; they do not identify why recognition changed. These are measurement opportunities, not an age-cutoff causal experiment.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

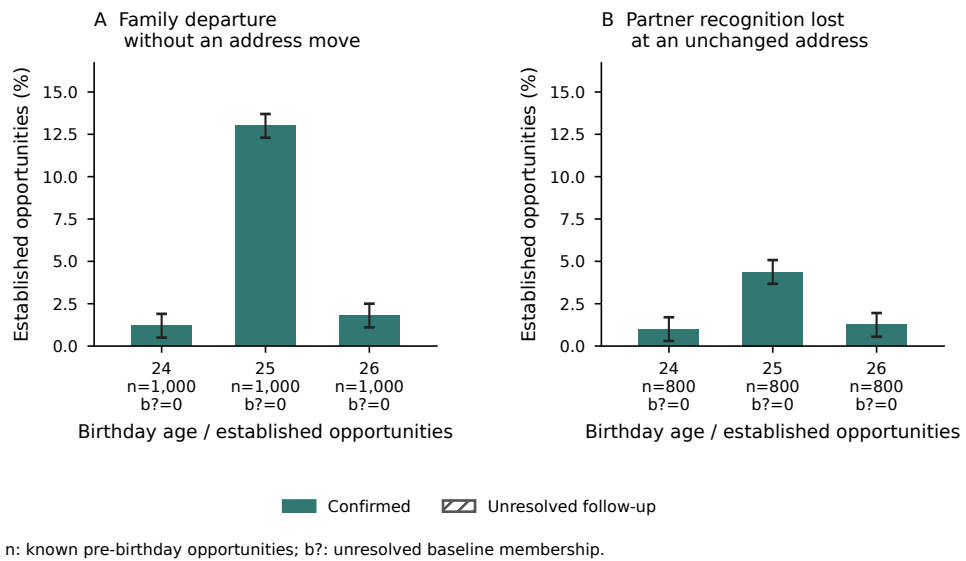


Fig. S1 ILLUSTRATIVE MOCK DATA — NOT RESULTS. Historical reclassification around birthdays 24,25 and 26 in 2017–2019. Panel A is family departure despite unchanged young-person and parent addresses. Panel B is lost partner recognition while both original people still share an address. Solid bars show confirmed fractions among established pre-birthday opportunities; hatched extensions, when present, show unresolved opportunities within that denominator. Descriptive 95% intervals clustered by person appear only with complete ascertainment and all required historical coverage. Missing baseline membership is outside those bounds; these are not policy effects or equivalence tests. Full numerators, denominators and unknown opportunities remain in the registered output. Empirical results remain pending.

The detailed diagnostic disposition is *[pending]*. Confirmed artefacts demonstrate failure of a family-ID-only definition. Absence of detected artefacts in observed opportunities does not establish that all romantic partnerships are measured or that birthday-adjacent events are causal. Unknown follow-up opportunities remain in established denominators, with separate counts and an upper fraction that includes their possible events. Missing baseline family IDs, addresses, reciprocal links or family-code meaning are unresolved membership, not negative events. Sampling intervals are withheld with partial ascertainment. The year-by-age component ledger is *[pending]* (Table S5); pooled rows cannot establish delivery of every historical birthday bracket. Plotted fractions do not bound events in people whose baseline membership or entire snapshots remain unobserved. Person clustering acknowledges repeated birthday opportunities. The diagnostic retains its own scientific meaning even if the access comparison encounters a boundary.

Table S5a. Historical family coverage

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Opps.	Baseline unknown	Coverage
2017	24	1,000	12	partial
2017	25	1,000	12	partial
2017	26	1,000	12	partial
2018	24	1,000	12	partial
2018	25	1,000	12	partial
2018	26	1,000	12	partial
2019	24	1,000	12	partial
2019	25	1,000	12	partial
2019	26	1,000	12	partial

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Opps. counts identifiable birthday opportunities. Entire undelivered populations cannot be counted; coverage requires every birthday bracket. No sampling interval applies to a delivery status.

Table S5b. Historical partner coverage

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Opps.	Baseline unknown	Coverage
2017	24	1,000	12	partial
2017	25	1,000	12	partial
2017	26	1,000	12	partial
2018	24	1,000	12	partial
2018	25	1,000	12	partial

Continued on next page.

Table S5b continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Opps.	Baseline unknown	Coverage
2018	26	1,000	12	partial
2019	24	1,000	12	partial
2019	25	1,000	12	partial
2019	26	1,000	12	partial

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Opps. counts identifiable birthday opportunities. Entire undelivered populations cannot be counted; coverage requires every birthday bracket. No sampling interval applies to a delivery status.

Table S5c. Recognized diagnostic opportunities

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Diagnostic	Age	N	Yes	Unknown	Bounds (%)
Family departure	24	1,000	40	10	4.0, 5.0
Family departure	25	1,000	40	10	4.0, 5.0
Family departure	26	1,000	40	10	4.0, 5.0
No address move	24	1,000	40	10	4.0, 5.0
No address move	25	1,000	40	10	4.0, 5.0
No address move	26	1,000	40	10	4.0, 5.0

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Fractions concern established denominators only; baseline uncertainty remains in Tables S5a–S5b. They are observation bounds, not sampling intervals. Complete-ascertainment sampling intervals accompany Figure S1 and the registered output.

Table S5d. Recognized diagnostic opportunities

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Diagnostic	Age	N	Yes	Unknown	Bounds (%)
Recognition lost	24	1,000	40	10	4.0, 5.0
Recognition lost	25	1,000	40	10	4.0, 5.0
Recognition lost	26	1,000	40	10	4.0, 5.0
Address separation	24	1,000	40	10	4.0, 5.0
Address separation	25	1,000	40	10	4.0, 5.0
Address separation	26	1,000	40	10	4.0, 5.0
Household changed	24	1,000	40	10	4.0, 5.0

Continued on next page.

Table S5d continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Diagnostic	Age	N	Yes	Unknown	Bounds (%)
Household changed	25	1,000	40	10	4.0, 5.0
Household changed	26	1,000	40	10	4.0, 5.0
Resident turns18	24	1,000	40	10	4.0, 5.0
Resident turns18	25	1,000	40	10	4.0, 5.0
Resident turns18	26	1,000	40	10	4.0, 5.0

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Fractions concern established denominators only; baseline uncertainty remains in Tables S5a–S5b. They are observation bounds, not sampling intervals. Complete-ascertainment sampling intervals accompany Figure S1 and the registered output.

Residential episodes require consecutive supplied snapshots, actual addresses and baseline-linked parent identities. Unknown intervals can contain departure and return, so a verified endpoint apart is not enough to date a move through a missing interval. Sustained residence apart requires four terminal quarterly observations apart after at least one qualifying departure; it is a quarterly persistence measure, not proof of uninterrupted daily residence. Parent-only moves and joint moves are separate construction flags. If a parent dies, the index person’s address is not automatically classified as independent. Missing registration is not assumed to be international migration, and no voluntary separation claim follows from a disappeared partner.

For union entry, recognition in the current snapshot must refer reciprocally to the same other person at the same address, and those two people must have had different known addresses in the preceding snapshot. A newly recognized couple already sharing an address is kept in the recognition-only accounting. Separation starts from the stable baseline pair and follows both original identities; changes in family ID or recognition alone are insufficient. Legal entry requires a transition to the verified marriage or registered-partnership code, a spouse identity and an effective status date inside the observation interval. Dissolution requires the corresponding dated civil transition with the baseline spouse identified immediately before it. Widowhood and certified death are absorbing for that union. An unexplained new spouse makes the baseline dissolution history unresolved; a later union’s divorce is not evidence that the missing original event has been recovered. A legal event whose valid date precedes a later death remains counted, including when both dates lie within the same observation interval. These rules do not recover short episodes that begin and end between quarterly snapshots, nor do they infer intimate relationship quality.

S3 Counterfactual comparison, multiplicity and precision

Figure 3 in main text displays all previously registered quarterly confirmed-record contrasts. The quarter ending immediately before rollout is month0, not a treated baseline normalization. The same baseline population and equal historical weights

apply at every quarter. Uptake and most transition outcomes are cumulative from B; sustained-away and same-partner are states defined at the displayed horizon and can reverse. A difference of cumulative probabilities can also rise or fall. Four final quarters apart are required for sustained-away, and timely newborn recognition uses its own observation window, so apparent lags do not by themselves identify delayed behavior. Ordinary pointwise intervals are neither simultaneous path bands nor observation/bias bounds. Missing quarters break the displayed profile; no interpolation or new between-quarter test is registered.

The historical age gaps and each leave-one-year-out estimate are reported regardless of direction. Historical years are not dropped for a significant placebo or for improving precision. The ages-25-versus-26 comparison asks whether a similar era change is present outside the newly eligible age band. Its interpretation is limited by age-specific shocks and exposure of partners; it is a negative-control challenge, not an estimate that is subtracted from the primary comparison. Pre-rollout leads use exactly the baseline risk sets, with cumulative outcomes at months -9 , -6 , -3 and 0 relative to rollout. Post-rollout curves continue at 3-month intervals through month 24. No lead-defined survivor cohort or preferred event-study window is selected. September 2020 precedes the verified announcement, but December 2020 and later pre-rollout observations may already contain anticipation. A resolved later lead is therefore not an untreated falsification by definition; all leads remain displayed and interpreted with their policy timing.

Let each person's observable indicator be bounded by $L_i \leq Y_i \leq U_i$, and let $w_{c,a}$ be the signed coefficients in the main-text contrast. The sharp linear observation bounds under these individual restrictions are

$$\underline{\theta} = \sum_{w_{c,a} > 0} w_{c,a} \bar{L}_{c,a} + \sum_{w_{c,a} < 0} w_{c,a} \bar{U}_{c,a}, \quad \bar{\theta} = \sum_{w_{c,a} > 0} w_{c,a} \bar{U}_{c,a} + \sum_{w_{c,a} < 0} w_{c,a} \bar{L}_{c,a}.$$

For inference, the influence contribution for each cell mean is its signed weight times the deviation from that cell mean divided by cell size. Cluster scores sum those contributions within baseline family components, birth-year-month and their intersection. Each component uses the finite-cluster multiplier $G/(G-1)$; the covariance is family plus birth-month minus intersection. The degrees of freedom are the smaller of the family and birth-month cluster counts minus one. Fewer than 30 clusters in either dimension, a family component exceeding 20% of distinct persons, or a nonpositive/nonfinite variance produces unavailable precision. These are registered computational safeguards, not a theorem that the remaining national policy comparison is well identified or its finite-sample approximation exact.

Ordinary intervals use the two-sided 95% critical value. Simultaneous bound endpoints use the one-sided critical value at $1 - 0.05/40$, retaining the fixed family of 20 comparisons. Bias scenarios widen the lower and upper simultaneous endpoints by B , for $B = 0, 0.01, 0.02$ in probability units. The bias-breakdown quantity is zero when the zero-bias set contains zero; otherwise it is the smallest distance of its endpoints from zero. It reports how large an additive allowance would remove a direction, not an empirically calibrated confounding bound. We also report a design-conditional

detectable contrast, $(q + 0.841621) \times SE$, as a precision description after delivery; it is not an ex-ante power calculation based on invented event rates. No minimum meaningful effect or equivalence region is declared.

Tables S2a–S2c fully enumerate the inferential family. “Estimate” means the contrast in confirmed-record event rates; “Sim. B0” allows unresolved observation and multiplicity; “Sim. B2” additionally permits hypothetical 2-percentage-point differential bias. All effects and limits are in percentage points. The displayed adjusted p value pertains to the confirmed-record contrast and does not replace the observation-bound conclusion. Unavailable fields remain explicitly unavailable with their reason; neither suppression nor nonactivation produces a zero. The unrounded plot-ready outputs are authoritative for numerical display after review.

Table S2a. Complete registered contrasts

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Outcome	Mo.	Est.	95% CI	Sim. B0	Sim. B2	Adj. p
Standard consultation use	12	-0.1	-1.1, 0.9	-1.7, 1.4	-3.7, 3.4	1.000
Standard consultation use	24	0.6	-0.2, 1.4	-0.7, 1.9	-2.7, 3.9	1.000
Leave	12	0.2	-0.8, 1.2	-1.3, 1.8	-3.3, 3.8	1.000
Leave	24	3.1	2.0, 4.2	1.4, 4.8	-0.6, 6.8	1.000
Sustained apart	12	2.0	1.1, 3.0	0.5, 3.5	-1.5, 5.5	1.000
Sustained apart	24	-1.4	-2.7, -0.2	-3.5, 0.6	-5.5, 2.6	1.000
Return	12	-0.2	-1.0, 0.7	-1.5, 1.2	-3.5, 3.2	1.000
Return	24	-2.5	-4.0, -1.0	-4.9, -0.1	-6.9, 1.9	1.000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Limits are lower, upper. All contrasts are in percentage points. Adj. p is the fixed 20-comparison Bonferroni p value for the confirmed-record contrast. It does not test the partially observed outcome without additional assumptions.

Table S2b. Complete registered contrasts

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Outcome	Mo.	Est.	95% CI	Sim. B0	Sim. B2	Adj. p
Union entry	12	0.3	-0.8, 1.4	-1.5, 2.1	-3.5, 4.1	1.000
Union entry	24	-0.6	-2.0, 0.8	-2.8, 1.7	-4.8, 3.7	1.000
Separation	12	-1.4	-2.9, -0.0	-3.7, 0.8	-5.7, 2.8	1.000
Separation	24	1.8	0.3, 3.3	-0.6, 4.2	-2.6, 6.2	1.000
Same partner	12	0.6	-0.2, 1.5	-0.8, 2.1	-2.8, 4.1	1.000
Same partner	24	-2.5	-3.8, -1.3	-4.5, -0.5	-6.5, 1.5	1.000
Legal entry	12	-0.8	-1.9, 0.3	-2.6, 1.0	-4.6, 3.0	1.000
Legal entry	24	-1.1	-2.4, 0.3	-3.2, 1.1	-5.2, 3.1	1.000

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Table S2b continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Outcome	Mo.	Est.	95% CI	Sim. B0	Sim. B2	Adj. p
Legal dissolution	12	0.1	-0.9, 1.2	-1.5, 1.8	-3.5, 3.8	1.000
Legal dissolution	24	-0.4	-1.5, 0.8	-2.2, 1.4	-4.2, 3.4	1.000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Limits are lower, upper. All contrasts are in percentage points. Adj. p is the fixed 20-comparison Bonferroni p value for the confirmed-record contrast. It does not test the partially observed outcome without additional assumptions.

Table S2c. Complete registered contrasts**ILLUSTRATIVE MOCK DATA — NOT RESULTS.** Empirical results pending.

Outcome	Mo.	Est.	95% CI	Sim. B0	Sim. B2	Adj. p
Parenthood	12	0.4	-0.9, 1.8	-1.7, 2.6	-3.7, 4.6	1.000
Parenthood	24	2.2	0.9, 3.5	0.1, 4.3	-1.9, 6.3	1.000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Limits are lower, upper. All contrasts are in percentage points. Adj. p is the fixed 20-comparison Bonferroni p value for the confirmed-record contrast. It does not test the partially observed outcome without additional assumptions.

Tables S3a–S3b expose the full fixed comparison structure. “Gap” means age 24 minus age 25 in a named year; “NC” is the ages-25-versus-26 era contrast. Omission estimates always retain 2021 and average the remaining two historical gaps equally. These columns are descriptive counterfactual challenges, outside the 20 primary comparisons, and are not a source of additional confirmatory directional claims. Their uncertainty and the quarterly curves are retained in the accompanying registered diagnostic outputs.

Table S3a. Historical age gaps**ILLUSTRATIVE MOCK DATA — NOT RESULTS.** Empirical results pending.

Outcome	Mo.	Gap 2017	Gap 2018	Gap 2019
Standard consultation use	12	-1.5 (-3.9, 0.9)	0.4 (-2.0, 2.8)	1.1 (-1.3, 3.5)
Standard consultation use	24	0.5 (-1.9, 2.9)	0.9 (-1.5, 3.3)	-1.2 (-3.6, 1.2)
Leave	12	0.5 (-1.9, 2.9)	0.1 (-2.3, 2.5)	-0.6 (-3.0, 1.8)
Leave	24	0.5 (-1.9, 2.9)	1.2 (-1.2, 3.6)	0.0 (-2.4, 2.4)
Sustained apart	12	-1.5 (-3.9, 0.9)	-1.2 (-3.6, 1.2)	1.4 (-1.0, 3.8)
Sustained apart	24	-1.5 (-3.9, 0.9)	-0.4 (-2.8, 2.0)	-0.4 (-2.8, 2.0)
Return	12	0.6 (-1.8, 3.0)	0.8 (-1.6, 3.2)	0.5 (-1.9, 2.9)
Return	24	-1.2 (-3.6, 1.2)	0.0 (-2.4, 2.4)	-0.7 (-3.1, 1.7)
Union entry	12	-0.4 (-2.8, 2.0)	0.7 (-1.7, 3.1)	0.7 (-1.7, 3.1)

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Table S3a continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Outcome	Mo.	Gap 2017	Gap 2018	Gap 2019
Union entry	24	0.6 (-1.8, 3.0)	1.2 (-1.2, 3.6)	0.1 (-2.3, 2.5)
Separation	12	1.5 (-0.9, 3.9)	-1.1 (-3.5, 1.3)	1.0 (-1.4, 3.4)
Separation	24	-0.4 (-2.8, 2.0)	0.5 (-1.9, 2.9)	1.4 (-1.0, 3.8)
Same partner	12	1.1 (-1.3, 3.5)	0.8 (-1.6, 3.2)	1.3 (-1.1, 3.7)
Same partner	24	-0.9 (-3.3, 1.5)	0.9 (-1.5, 3.3)	0.5 (-1.9, 2.9)
Legal entry	12	0.1 (-2.3, 2.5)	1.3 (-1.1, 3.7)	-1.5 (-3.9, 0.9)
Legal entry	24	1.3 (-1.1, 3.7)	1.1 (-1.3, 3.5)	-0.7 (-3.1, 1.7)
Legal dissolution	12	-0.9 (-3.3, 1.5)	-0.8 (-3.2, 1.6)	-0.7 (-3.1, 1.7)
Legal dissolution	24	-1.4 (-3.8, 1.0)	-0.8 (-3.2, 1.6)	-0.5 (-2.9, 1.9)
Parenthood	12	0.2 (-2.2, 2.6)	0.7 (-1.7, 3.1)	0.3 (-2.1, 2.7)
Parenthood	24	-1.4 (-3.8, 1.0)	-1.0 (-3.4, 1.4)	-1.0 (-3.4, 1.4)

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Descriptive diagnostics; not additional confirmatory tests or criteria for choosing the primary model. NC uses ages 25 and 26. Effects are in percentage points.

Table S3b. Fixed comparison challenges**ILLUSTRATIVE MOCK DATA — NOT RESULTS.** Empirical results pending.

Outcome	Mo.	NC	Omit 2017	Omit 2018	Omit 2019
Standard consultation use	12	-0.3 (-2.7, 2.1)	0.0 (-2.4, 2.4)	0.7 (-1.7, 3.1)	0.2 (-2.2, 2.6)
Standard consultation use	24	-0.9 (-3.3, 1.5)	-0.3 (-2.7, 2.1)	0.4 (-2.0, 2.8)	-1.4 (-3.8, 1.0)
Leave	12	-0.8 (-3.2, 1.6)	0.9 (-1.5, 3.3)	-0.4 (-2.8, 2.0)	-1.0 (-3.4, 1.4)
Leave	24	-0.5 (-2.9, 1.9)	1.4 (-1.0, 3.8)	-1.0 (-3.4, 1.4)	-0.1 (-2.5, 2.3)
Sustained apart	12	-0.8 (-3.2, 1.6)	1.5 (-0.9, 3.9)	-0.1 (-2.5, 2.3)	1.4 (-1.0, 3.8)
Sustained apart	24	0.2 (-2.2, 2.6)	-0.7 (-3.1, 1.7)	1.2 (-1.2, 3.6)	-0.3 (-2.7, 2.1)
Return	12	1.4 (-1.0, 3.8)	1.2 (-1.2, 3.6)	1.5 (-0.9, 3.9)	-1.3 (-3.7, 1.1)
Return	24	0.2 (-2.2, 2.6)	-0.6 (-3.0, 1.8)	0.9 (-1.5, 3.3)	0.7 (-1.7, 3.1)
Union entry	12	1.2 (-1.2, 3.6)	-1.0 (-3.4, 1.4)	-0.7 (-3.1, 1.7)	-0.5 (-2.9, 1.9)
Union entry	24	-0.5 (-2.9, 1.9)	0.9 (-1.5, 3.3)	0.5 (-1.9, 2.9)	-0.7 (-3.1, 1.7)

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Table S3b continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Outcome	Mo.	NC	Omit 2017	Omit 2018	Omit 2019
Separation	12	-1.4 (-3.8, 1.0)	1.3 (-1.1, 3.7)	0.8 (-1.6, 3.2)	1.3 (-1.1, 3.7)
Separation	24	-0.5 (-2.9, 1.9)	-0.9 (-3.3, 1.5)	1.4 (-1.0, 3.8)	-0.9 (-3.3, 1.5)
Same partner	12	0.1 (-2.3, 2.5)	-0.9 (-3.3, 1.5)	1.0 (-1.4, 3.4)	0.3 (-2.1, 2.7)
Same partner	24	0.2 (-2.2, 2.6)	0.4 (-2.0, 2.8)	-0.6 (-3.0, 1.8)	-1.0 (-3.4, 1.4)
Legal entry	12	0.3 (-2.1, 2.7)	-0.6 (-3.0, 1.8)	-0.9 (-3.3, 1.5)	1.4 (-1.0, 3.8)
Legal entry	24	0.5 (-1.9, 2.9)	0.7 (-1.7, 3.1)	1.2 (-1.2, 3.6)	1.3 (-1.1, 3.7)
Legal dissolution	12	-0.7 (-3.1, 1.7)	0.4 (-2.0, 2.8)	0.0 (-2.4, 2.4)	-1.4 (-3.8, 1.0)
Legal dissolution	24	0.4 (-2.0, 2.8)	-1.1 (-3.5, 1.3)	0.5 (-1.9, 2.9)	0.5 (-1.9, 2.9)
Parenthood	12	-0.3 (-2.7, 2.1)	-0.4 (-2.8, 2.0)	0.1 (-2.3, 2.5)	-0.9 (-3.3, 1.5)
Parenthood	24	-1.4 (-3.8, 1.0)	-0.1 (-2.5, 2.3)	-0.8 (-3.2, 1.6)	0.9 (-1.5, 3.3)

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Descriptive diagnostics; not additional confirmatory tests or criteria for choosing the primary model. NC uses ages 25 and 26. Effects are in percentage points.

Observed use of the cluster approximation and precision safeguards is *[pending]*. If usable point bounds coexist with unavailable precision, the manuscript reports the bounds and the precise reason, without a confidence claim. Family connections formed after baseline may induce covariance across the fixed clusters. National age-specific shocks can also create dependence not captured by the two dimensions. These concerns qualify uncertainty statements independently of the additive-bias sensitivity analysis.

S4 Recorded parenthood and ascertainment

The parenthood risk set requires five December snapshots preceding the baseline year plus the four quarters ending at baseline. An individual must be observed throughout these pre-exposure dates and must have no parent link in them. This fixes a common ascertainment window instead of exploiting different lengths of available history by cohort. It still does not establish lifetime childlessness. A known child may have died or left Denmark before the window, and children abroad may never appear. The denominator therefore describes residents without observed prior links in a specified history, not all true nonparents.

For each newly linked child, recorded birth date and first link date remain separate. The qualifying window consists of the first two quarter ends on or after the child's birth, including a quarter end exactly on the birth date. The child's birth must fall after baseline and through the selected endpoint. Two confirmation quarters must be supplied even when the child is born near the endpoint; this checks recognition rather than lengthening the birth window. Links first recognized later and children born before baseline are retained as delayed or older-child recognition, with no imputed biological timing. A child can generate two person-level parent transitions. The ascertainment counts describe residents carrying flags, not distinct births, and flags can overlap.

Tables S4a–S4b present the fixed 24-month ascertainment accounting; the same 12-month accounting remains in its registered output. “Candidates” is the baseline cohort prior to the history requirement. “History observed” is complete observation of the required prebaseline person history; “Prior link” marks a child link in that window; “Eligible” is the resulting observed-nonparent denominator. “Newborn,” “Late,” “Older” and “Unknown exit” count residents with the corresponding follow-up flag. A structural zero flag can be reported only after complete relevant processing; absent processing or a missing whole child snapshot is unavailable.

Table S4a. Parenthood baseline ascertainment

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Candidates	History observed	Prior link	Eligible
2017	24	5,000	4,900	1,500	3,400
2017	25	5,000	4,900	1,500	3,400
2018	24	5,000	4,900	1,500	3,400
2018	25	5,000	4,900	1,500	3,400
2019	24	5,000	4,900	1,500	3,400
2019	25	5,000	4,900	1,500	3,400
2021	24	5,000	4,900	1,500	3,400
2021	25	5,000	4,900	1,500	3,400

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Counts concern residents, not distinct births. Empirical values remain pending; the displayed values are hypothetical. Zero is permitted only when complete processing establishes an absent flag; missing processing remains unavailable.

Table S4b. Parenthood follow-up ascertainment

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Newborn	Late	Older	Unknown exit
2017	24	130	14	18	30
2017	25	130	14	18	30

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Table S4b continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Newborn	Late	Older	Unknown exit
2018	24	130	14	18	30
2018	25	130	14	18	30
2019	24	130	14	18	30
2019	25	130	14	18	30
2021	24	130	14	18	30
2021	25	130	14	18	30

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Counts concern residents, not distinct births. Empirical values remain pending; the displayed values are hypothetical. Zero is permitted only when complete processing establishes an absent flag; missing processing remains unavailable.

The actual ascertainment disposition is *[pending]*. Death/link timing conflicts are *[pending]* (Table S4c). A post-death birth link and a pre-death birth recognized only after death both fail the strict recorded-before-death definition. They leave the index person in the denominator; an earlier confirmed link stays counted and prior unknown intervals still receive observation bounds.

Table S4c. Parenthood death and recognition timing

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Death	Birth/link after death	Birth before; link after
2017	24	12	3	3
2017	25	12	3	3
2018	24	12	3	3
2018	25	12	3	3
2019	24	12	3	3
2019	25	12	3	3
2021	24	12	3	3
2021	25	12	3	3

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Counts are unique residents with each flag and can overlap. No confirmed recorded-parenthood event is assigned on/after documented death. Certified historical evidence and unavailable components must be reported at their actual scope. Real unsupported periods remain unavailable, not zero. Twelve-month flags accompany the narrative summary.

A transition based on this rule is meaningful for recorded family formation but cannot support a claim about conception after psychotherapy. Births with plans or conceptions predating the rollout remain included. Excluding them according to future

birth timing would change the risk set and is not allowed. No pregnancy, treatment-response or eventual-mover selection is used elsewhere in the study either.

S5 Partner exposure and construction accounting

Partners may face different access histories even when the index persons have no earlier waiver entitlement. We describe the partner fixed at baseline, using age on the index rollout date and potential new-referral days under the actual dated reforms. Age bands separate under18,18–20,21,22–23,24 and 25plus, with missing birthdate/partner rows explicitly unknown. Prior days run from July2018 to the day before the index rollout; new days run from rollout through the 12-month endpoint. Never, prior-only, new-only and prior-and-new refer to those two windows. They do not measure treatment receipt or continued-course entitlement. Table S6 reports marginal person counts over the joint registered strata, separately for stable recognized partners and legal spouses. Those contexts can overlap. Within each year and context, the dyad roster counts each unordered pair once; counts cannot be summed across age strata or years as independent couples.

The flag accounting in Table S8 separates recognized entry without a newly shared address, recognition loss at unchanged addresses, unknown recognition, address separation, reunion, new identity and legal dissolution/competing events. These are evaluations of the fixed populations, not subgroups selected to re-estimate effects. Counts of repeated person-quarter opportunities are distinct from unique dyads. A reunion requires a known-apart spell followed by known joint residence of the same original people. An unknown code does not provide either transition. Missing component evidence remains unavailable; a numerical zero requires completed evaluation.

Table S6a. Baseline partner exposure, 2017

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Partner stratum	R24	R25	R26	L24	L25	L26
Age <18	20	20	20	20	20	20
Age18–20	150	150	150	150	150	150
Age21	120	120	120	120	120	120
Age22–23	230	230	230	230	230	230
Age24	210	210	210	210	210	210
Age25+	250	250	250	250	250	250
Age unknown	20	20	20	20	20	20
Never	380	380	380	380	380	380
Prior only	200	200	200	200	200	200
New only	200	200	200	200	200	200
Prior and new	200	200	200	200	200	200
History unknown	20	20	20	20	20	20

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

R is recognized and L legal context; the suffix is index age at rollout. Age and waiver-history rows are separate margins of the same baseline population, not additive. Partner ages use baseline birthdates; histories use actual waiver rules. Recognized and legal contexts overlap. No partner-treatment effect or confidence interval is estimated.

Table S6b. Baseline partner exposure, 2018

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Partner stratum	R24	R25	R26	L24	L25	L26
Age <18	20	20	20	20	20	20
Age18–20	150	150	150	150	150	150
Age21	120	120	120	120	120	120
Age22–23	230	230	230	230	230	230
Age24	210	210	210	210	210	210
Age25+	250	250	250	250	250	250
Age unknown	20	20	20	20	20	20
Never	380	380	380	380	380	380
Prior only	200	200	200	200	200	200
New only	200	200	200	200	200	200
Prior and new	200	200	200	200	200	200
History unknown	20	20	20	20	20	20

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

R is recognized and L legal context; the suffix is index age at rollout. Age and waiver-history rows are separate margins of the same baseline population, not additive. Partner ages use baseline birthdates; histories use actual waiver rules. Recognized and legal contexts overlap. No partner-treatment effect or confidence interval is estimated.

Table S6c. Baseline partner exposure, 2019

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Partner stratum	R24	R25	R26	L24	L25	L26
Age <18	20	20	20	20	20	20
Age18–20	150	150	150	150	150	150
Age21	120	120	120	120	120	120
Age22–23	230	230	230	230	230	230
Age24	210	210	210	210	210	210
Age25+	250	250	250	250	250	250
Age unknown	20	20	20	20	20	20
Never	380	380	380	380	380	380
Prior only	200	200	200	200	200	200
New only	200	200	200	200	200	200
Prior and new	200	200	200	200	200	200
History unknown	20	20	20	20	20	20

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

R is recognized and L legal context; the suffix is index age at rollout. Age and waiver-history rows are separate margins of the same baseline population, not additive. Partner ages use baseline birthdates; histories use actual waiver rules. Recognized and legal contexts overlap. No partner-treatment effect or confidence interval is estimated.

Table S6d. Baseline partner exposure, 2021

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Partner stratum	R24	R25	R26	L24	L25	L26
Age <18	20	20	20	20	20	20
Age18–20	150	150	150	150	150	150
Age21	120	120	120	120	120	120
Age22–23	230	230	230	230	230	230
Age24	210	210	210	210	210	210
Age25+	250	250	250	250	250	250
Age unknown	20	20	20	20	20	20
Never	380	380	380	380	380	380
Prior only	200	200	200	200	200	200
New only	200	200	200	200	200	200
Prior and new	200	200	200	200	200	200
History unknown	20	20	20	20	20	20

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

R is recognized and L legal context; the suffix is index age at rollout. Age and waiver-history rows are separate margins of the same baseline population, not additive. Partner ages use baseline birthdates; histories use actual waiver rules. Recognized and legal contexts overlap. No partner-treatment effect or confidence interval is estimated.

Table S6e. Unique original baseline pairs

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Context	Index persons	Unique dyads	Unknown risk
2017	recognized	3,000	1,800	12
2017	legal	3,000	1,800	12
2018	recognized	3,000	1,800	12
2018	legal	3,000	1,800	12
2019	recognized	3,000	1,800	12
2019	legal	3,000	1,800	12
2021	recognized	3,000	1,800	12
2021	legal	3,000	1,800	12

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Each unordered pair counts once within its cohort/context even when both index people qualify. Do not sum contexts or cohorts. Unavailable contexts retain their reason and NaN values, never zero.

Table S8a. Partnership construction, 2017, month 12

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	59 / 8,000	56 / 8,000	14 / 8,000
Recognition-only loss	59 / 8,000	34 / 8,000	40 / 8,000
Unknown recognition	26 / 8,000	17 / 8,000	30 / 8,000
Apart address	32 / 8,000	22 / 8,000	36 / 8,000
Reunion	23 / 8,000	59 / 8,000	29 / 8,000
New identity	10 / 8,000	35 / 8,000	49 / 8,000
Pair competing	18 / 8,000	21 / 8,000	42 / 8,000
Baseline divorce	39 / 8,000	56 / 8,000	18 / 8,000
Legal competing	46 / 8,000	56 / 8,000	46 / 8,000
Legal identity unknown	44 / 8,000	58 / 8,000	23 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8b. Partnership construction, 2017, month 24

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	45 / 8,000	42 / 8,000	22 / 8,000
Recognition-only loss	53 / 8,000	54 / 8,000	45 / 8,000
Unknown recognition	14 / 8,000	58 / 8,000	59 / 8,000
Apart address	14 / 8,000	14 / 8,000	37 / 8,000
Reunion	50 / 8,000	31 / 8,000	13 / 8,000
New identity	23 / 8,000	26 / 8,000	59 / 8,000
Pair competing	50 / 8,000	31 / 8,000	13 / 8,000
Baseline divorce	32 / 8,000	47 / 8,000	26 / 8,000
Legal competing	51 / 8,000	39 / 8,000	31 / 8,000
Legal identity unknown	55 / 8,000	22 / 8,000	11 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8c. Partnership construction, 2018, month 12

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	31 / 8,000	42 / 8,000	35 / 8,000
Recognition-only loss	33 / 8,000	24 / 8,000	38 / 8,000
Unknown recognition	13 / 8,000	39 / 8,000	48 / 8,000
Apart address	15 / 8,000	31 / 8,000	20 / 8,000
Reunion	22 / 8,000	50 / 8,000	41 / 8,000
New identity	40 / 8,000	56 / 8,000	52 / 8,000
Pair competing	58 / 8,000	28 / 8,000	19 / 8,000
Baseline divorce	23 / 8,000	51 / 8,000	17 / 8,000
Legal competing	13 / 8,000	49 / 8,000	44 / 8,000
Legal identity unknown	30 / 8,000	24 / 8,000	18 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8d. Partnership construction, 2018, month 24

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	19 / 8,000	41 / 8,000	23 / 8,000
Recognition-only loss	42 / 8,000	42 / 8,000	58 / 8,000
Unknown recognition	40 / 8,000	36 / 8,000	27 / 8,000
Apart address	33 / 8,000	30 / 8,000	39 / 8,000
Reunion	22 / 8,000	10 / 8,000	16 / 8,000
New identity	50 / 8,000	56 / 8,000	26 / 8,000
Pair competing	27 / 8,000	35 / 8,000	57 / 8,000
Baseline divorce	12 / 8,000	56 / 8,000	13 / 8,000
Legal competing	28 / 8,000	36 / 8,000	25 / 8,000
Legal identity unknown	19 / 8,000	35 / 8,000	19 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8e. Partnership construction, 2019, month 12

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	43 / 8,000	40 / 8,000	16 / 8,000
Recognition-only loss	51 / 8,000	60 / 8,000	47 / 8,000
Unknown recognition	46 / 8,000	38 / 8,000	49 / 8,000
Apart address	10 / 8,000	35 / 8,000	39 / 8,000
Reunion	41 / 8,000	52 / 8,000	36 / 8,000
New identity	39 / 8,000	15 / 8,000	58 / 8,000
Pair competing	23 / 8,000	24 / 8,000	49 / 8,000
Baseline divorce	54 / 8,000	41 / 8,000	13 / 8,000
Legal competing	53 / 8,000	60 / 8,000	21 / 8,000
Legal identity unknown	10 / 8,000	36 / 8,000	13 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8f. Partnership construction, 2019, month 24

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	53 / 8,000	10 / 8,000	18 / 8,000
Recognition-only loss	52 / 8,000	18 / 8,000	56 / 8,000
Unknown recognition	44 / 8,000	33 / 8,000	30 / 8,000
Apart address	24 / 8,000	37 / 8,000	46 / 8,000
Reunion	30 / 8,000	40 / 8,000	20 / 8,000
New identity	19 / 8,000	11 / 8,000	44 / 8,000
Pair competing	18 / 8,000	26 / 8,000	13 / 8,000
Baseline divorce	47 / 8,000	44 / 8,000	22 / 8,000
Legal competing	40 / 8,000	41 / 8,000	23 / 8,000
Legal identity unknown	48 / 8,000	27 / 8,000	45 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8g. Partnership construction, 2021, month 12

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	56 / 8,000	55 / 8,000	25 / 8,000
Recognition-only loss	48 / 8,000	47 / 8,000	15 / 8,000
Unknown recognition	21 / 8,000	46 / 8,000	43 / 8,000
Apart address	42 / 8,000	15 / 8,000	23 / 8,000
Reunion	22 / 8,000	33 / 8,000	46 / 8,000
New identity	17 / 8,000	41 / 8,000	53 / 8,000
Pair competing	45 / 8,000	52 / 8,000	34 / 8,000
Baseline divorce	11 / 8,000	60 / 8,000	31 / 8,000
Legal competing	32 / 8,000	30 / 8,000	46 / 8,000
Legal identity unknown	15 / 8,000	45 / 8,000	12 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

Table S8h. Partnership construction, 2021, month 24

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Flag	Index age24	Index age25	Index age26
Recognition-only entry	40 / 8,000	24 / 8,000	51 / 8,000
Recognition-only loss	29 / 8,000	20 / 8,000	33 / 8,000
Unknown recognition	19 / 8,000	33 / 8,000	50 / 8,000
Apart address	57 / 8,000	29 / 8,000	32 / 8,000
Reunion	32 / 8,000	31 / 8,000	36 / 8,000
New identity	42 / 8,000	40 / 8,000	39 / 8,000
Pair competing	48 / 8,000	12 / 8,000	27 / 8,000
Baseline divorce	28 / 8,000	14 / 8,000	29 / 8,000
Legal competing	60 / 8,000	38 / 8,000	55 / 8,000
Legal identity unknown	40 / 8,000	55 / 8,000	18 / 8,000

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Cells are flagged person-quarter evaluations / opportunities in the corresponding baseline risk set. Repeated apart/competing flags can count multiple intervals; a confirmed baseline legal divorce is counted once. Recognition entry/loss requires known semantics; missing components are unavailable. These are not unique dyad counts.

S6 Reporting completeness and scientific scope

Every registered analysis is reported with its actual scope, including unavailable measurement, incomplete precision and work that did not execute. These dispositions differ from an observed absence of events. A missing component does not erase independently interpretable residence, legal-partnership or parenthood evidence. The complete activation predicates and result-dependent alternatives are in preregistration §§13–14 and the result map; they govern execution and interpretation rather than add empirical findings to this paper.

The analysis uses certified data definitions and auditable aggregate outputs. Reference implementation entry points, metadata requirements and synthetic test commands are documented in the accompanying execution materials. Synthetic checks examine code behavior; all displayed mock values are labeled presentation examples, and actual study evidence remains pending. Access and disclosure status is reported in the main Data and code availability statement. No public source or successful synthetic test establishes delivered data quality or causal identification.

S9 Residential reversibility in the original co-resident population

The residential path description reuses frozen baseline parents, the original co-resident denominator and all required quarter snapshots. Confirmed round trips require a qualifying leave and a later strict return to a parent’s address occupied in both snapshots around that return. A parent-only move, joint relocation or family reclassification cannot confirm either action. A finite-state scan retains feasible histories through unknown intervals; it does not impute a confirmed event from a gap. Fully observed quarter snapshots can exclude a registered sequence while still missing moves completed entirely within a quarter. This is a description of detectable registered paths, not every physical residential move.

Table S9. Residential departure, persistence and round trips. A. Original co-resident population and scope

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] leave: partial unresolved events; sustained: partial unresolved events; round trip: partial unresolved events.

Year	Age	Mo.	Co-resident n	Baseline unknown n	Scope / reason
2017	24	12	1,000	12	[1]
2017	24	24	1,000	12	[1]
2017	25	12	1,000	12	[1]

Continued on next page.

Table S9 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.*Scope: [1] leave: partial unresolved events; sustained: partial unresolved events; round trip: partial unresolved events.*

Year	Age	Mo.	Co-resident n	Baseline unknown n	Scope / reason
2017	25	24	1,000	12	[1]
2018	24	12	1,000	12	[1]
2018	24	24	1,000	12	[1]
2018	25	12	1,000	12	[1]
2018	25	24	1,000	12	[1]
2019	24	12	1,000	12	[1]
2019	24	24	1,000	12	[1]
2019	25	12	1,000	12	[1]
2019	25	24	1,000	12	[1]
2021	24	12	1,000	12	[1]
2021	24	24	1,000	12	[1]
2021	25	12	1,000	12	[1]
2021	25	24	1,000	12	[1]

Table S9. Residential departure, persistence and round trips. B. Departure, persistence and round-trip bounds**ILLUSTRATIVE MOCK DATA — NOT RESULTS.** Empirical results pending.

Year	Age	Mo.	Leave bounds (%)	Sustained- away bounds (%)	Leave-then- return bounds (%)
2017	24	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2017	24	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2017	25	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2017	25	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2018	24	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2018	24	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2018	25	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2018	25	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2019	24	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2019	24	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2019	25	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2019	25	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2021	24	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2021	24	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0
2021	25	12	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0

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Table S9 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Year	Age	Mo.	Leave bounds (%)	Sustained-away bounds (%)	Leave-then-return bounds (%)
2021	25	24	42.0 – 48.0	20.0 – 24.0	7.0 – 10.0

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

All three measures use the same original co-resident population. They are not exclusive: leave–return–leave can satisfy both sustained-away and round-trip. Return is a later observed strict young-person move, not a parental move. These are finite-cohort observation bounds, without confidence intervals or new hypothesis tests.

S10 Joint event recognition and timing

The three intersections use only pre-exposure evidence. Unknown baseline status remains separate, and missing parenthood histories do not erase the other modules. Every person with established membership contributes to exactly one of the eight observation patterns. For confirmed A and B, certified half-open first-detection intervals establish A-before-B only when A’s right endpoint is no later than B’s left endpoint, and conversely for B-before-A. Identical adjacent-quarter intervals are ties in observation; an earlier unknown opportunity or other overlap leaves order unresolved. A newborn’s birth date qualifies the parenthood endpoint but does not replace the first qualifying parent-link recognition clock. Confirmation snapshots may extend beyond the outcome horizon as required by Atom 5.

The eight categories distinguish different kinds of information. “Neither” requires both events to be excluded by the registered observation rules. “A only” and “B only” require one confirmed event and an excluded counterpart. The three ordered or same-quarter categories require both events to be confirmed and their first-recognition intervals to be certified. “Both, order unresolved” confirms occurrence but leaves sequencing open; “event membership unresolved” leaves the occurrence of at least one event open. Neither uncertainty category can be merged with a no-event cell.

The pattern classification reports observed joint events without estimating a selected hazard or conditioning any policy effect on a later transition. Module/year/age/horizon cells are separate descriptions; people may appear across modules or index years. All384 category values are retained below as48 rows with eight columns.

Table S10. All joint observation patterns. A. Denominators and observation scope
ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Scope / reason
Home / union	2017	24	12	1,000	13	[1]
Home / union	2017	24	24	1,000	13	[1]
Home / union	2017	25	12	1,100	13	[1]
Home / union	2017	25	24	1,100	13	[1]
Home / union	2018	24	12	1,000	13	[1]
Home / union	2018	24	24	1,000	13	[1]
Home / union	2018	25	12	1,100	13	[1]
Home / union	2018	25	24	1,100	13	[1]
Home / union	2019	24	12	1,000	13	[1]
Home / union	2019	24	24	1,000	13	[1]
Home / union	2019	25	12	1,100	13	[1]
Home / union	2019	25	24	1,100	13	[1]
Home / union	2021	24	12	1,000	13	[1]
Home / union	2021	24	24	1,000	13	[1]
Home / union	2021	25	12	1,100	13	[1]
Home / union	2021	25	24	1,100	13	[1]
Return / separation	2017	24	12	1,000	13	[1]
Return / separation	2017	24	24	1,000	13	[1]
Return / separation	2017	25	12	1,100	13	[1]
Return / separation	2017	25	24	1,100	13	[1]
Return / separation	2018	24	12	1,000	13	[1]
Return / separation	2018	24	24	1,000	13	[1]
Return / separation	2018	25	12	1,100	13	[1]
Return / separation	2018	25	24	1,100	13	[1]
Return / separation	2019	24	12	1,000	13	[1]
Return / separation	2019	24	24	1,000	13	[1]

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Scope / reason
Return / separation	2019	25	12	1,100	13	[1]
Return / separation	2019	25	24	1,100	13	[1]
Return / separation	2021	24	12	1,000	13	[1]
Return / separation	2021	24	24	1,000	13	[1]
Return / separation	2021	25	12	1,100	13	[1]
Return / separation	2021	25	24	1,100	13	[1]
Union / parenthood	2017	24	12	1,000	13	[1]
Union / parenthood	2017	24	24	1,000	13	[1]
Union / parenthood	2017	25	12	1,100	13	[1]
Union / parenthood	2017	25	24	1,100	13	[1]
Union / parenthood	2018	24	12	1,000	13	[1]
Union / parenthood	2018	24	24	1,000	13	[1]
Union / parenthood	2018	25	12	1,100	13	[1]
Union / parenthood	2018	25	24	1,100	13	[1]
Union / parenthood	2019	24	12	1,000	13	[1]
Union / parenthood	2019	24	24	1,000	13	[1]
Union / parenthood	2019	25	12	1,100	13	[1]
Union / parenthood	2019	25	24	1,100	13	[1]
Union / parenthood	2021	24	12	1,000	13	[1]

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Scope / reason
Union / parenthood	2021	24	24	1,000	13	[1]
Union / parenthood	2021	25	12	1,100	13	[1]
Union / parenthood	2021	25	24	1,100	13	[1]

Table S10. All joint observation patterns. B. Neither, single events and certified observation order

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Neither	A only	B only	A before B	Same obs. quarter	B before A
Home / union	2017	24	12	39.8	16.1	11.1	9.0	10.9	5.7
Home / union	2017	24	24	38.2	15.6	10.1	8.1	12.0	6.8
Home / union	2017	25	12	39.4	14.5	11.5	6.6	13.1	7.6
Home / union	2017	25	24	37.5	18.0	10.9	8.1	11.4	7.0
Home / union	2018	24	12	39.7	15.8	11.1	7.4	12.2	7.7
Home / union	2018	24	24	35.4	18.3	12.1	7.9	11.7	7.6
Home / union	2018	25	12	40.6	14.5	9.5	8.2	13.8	6.5
Home / union	2018	25	24	39.8	16.3	9.3	8.5	12.9	5.7
Home / union	2019	24	12	40.7	14.7	10.6	7.0	11.4	7.4
Home / union	2019	24	24	39.5	16.7	8.4	7.8	13.1	6.6
Home / union	2019	25	12	40.4	16.3	10.9	8.5	12.8	5.8
Home / union	2019	25	24	39.0	14.9	9.8	7.9	11.5	8.7

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Neither	A only	B only	A before B	Same obs. quarter	B before A
Home / union	2021	24	12	34.7	16.8	13.1	8.6	9.8	8.0
Home / union	2021	24	24	38.7	15.5	10.4	8.2	14.0	6.2
Home / union	2021	25	12	38.5	17.0	10.8	7.4	12.0	6.5
Home / union	2021	25	24	37.9	16.5	11.0	8.0	12.1	7.4
Return / separation	2017	24	12	42.1	15.3	12.1	8.4	10.5	5.8
Return / separation	2017	24	24	35.9	15.6	13.3	9.6	10.9	7.4
Return / separation	2017	25	12	40.4	16.5	9.2	8.0	11.7	6.9
Return / separation	2017	25	24	39.5	16.2	12.3	7.8	11.5	5.7
Return / separation	2018	24	12	39.0	17.3	10.0	8.2	10.6	7.0
Return / separation	2018	24	24	42.4	14.2	9.4	7.8	12.5	7.3
Return / separation	2018	25	12	39.8	14.9	10.9	8.5	12.0	6.0
Return / separation	2018	25	24	38.4	16.1	10.9	8.5	12.8	6.0
Return / separation	2019	24	12	41.8	15.3	10.3	8.5	11.3	5.7
Return / separation	2019	24	24	42.1	15.5	10.4	8.5	11.1	6.5
Return / separation	2019	25	12	38.5	15.0	9.5	9.2	12.9	6.8
Return / separation	2019	25	24	38.6	14.7	10.7	9.4	12.6	7.7
Return / separation	2021	24	12	40.2	14.8	11.0	8.0	11.5	6.9
Return / separation	2021	24	24	45.9	16.6	10.2	7.9	10.8	0.0
Return / separation	2021	25	12	37.7	16.5	11.2	8.1	12.0	7.5

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Neither	A only	B only	A before B	Same obs. quarter	B before A
Return / separation	2021	25	24	37.2	17.2	11.3	7.7	11.3	6.5
Union / parent- hood	2017	24	12	38.5	16.4	9.8	9.8	12.3	7.6
Union / parent- hood	2017	24	24	39.0	15.7	11.2	7.9	11.9	6.4
Union / parent- hood	2017	25	12	40.5	14.2	10.0	8.4	12.5	7.4
Union / parent- hood	2017	25	24	37.5	16.9	10.9	8.0	13.1	5.6
Union / parent- hood	2018	24	12	40.7	15.0	11.8	6.9	12.7	6.3
Union / parent- hood	2018	24	24	38.6	15.8	10.4	6.8	13.0	8.2
Union / parent- hood	2018	25	12	40.6	15.5	11.7	8.4	10.2	6.5
Union / parent- hood	2018	25	24	38.5	16.7	11.9	7.7	11.8	6.6
Union / parent- hood	2019	24	12	39.3	15.6	11.3	8.7	12.3	6.1
Union / parent- hood	2019	24	24	37.5	16.6	9.8	8.9	12.1	7.8
Union / parent- hood	2019	25	12	39.2	15.7	10.5	9.2	12.3	7.3
Union / parent- hood	2019	25	24	36.9	16.7	12.7	7.6	12.3	5.7

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Neither	A only	B only	A before B	Same obs. quarter	B before A
Union / parent- hood	2021	24	12	37.2	16.0	11.1	8.4	10.8	7.8
Union / parent- hood	2021	24	24	40.2	15.3	10.6	7.7	12.0	6.7
Union / parent- hood	2021	25	12	39.0	17.2	10.6	8.3	11.9	7.4
Union / parent- hood	2021	25	24	37.5	15.5	12.2	8.6	11.9	6.7

Table S10. All joint observation patterns. C. Unresolved order and event membership

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Both; order unresolved	Event membership unresolved
Home / union	2017	24	12	2.9	4.5
Home / union	2017	24	24	4.8	4.4
Home / union	2017	25	12	3.4	3.8
Home / union	2017	25	24	3.2	4.0
Home / union	2018	24	12	2.2	3.9
Home / union	2018	24	24	2.8	4.2
Home / union	2018	25	12	3.5	3.5
Home / union	2018	25	24	3.4	4.1
Home / union	2019	24	12	3.7	4.5
Home / union	2019	24	24	3.5	4.4
Home / union	2019	25	12	2.8	2.5
Home / union	2019	25	24	3.5	4.7
Home / union	2021	24	12	3.8	5.2
Home / union	2021	24	24	2.7	4.3
Home / union	2021	25	12	3.0	4.8
Home / union	2021	25	24	2.5	4.7
Return / separation	2017	24	12	3.3	2.5
Return / separation	2017	24	24	2.7	4.6

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Table S10 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	Both; order unresolved	Event membership unresolved
Return / separation	2017	25	12	3.5	3.9
Return / separation	2017	25	24	3.0	4.0
Return / separation	2018	24	12	3.2	4.7
Return / separation	2018	24	24	2.7	3.7
Return / separation	2018	25	12	3.2	4.7
Return / separation	2018	25	24	3.1	4.3
Return / separation	2019	24	12	3.1	4.0
Return / separation	2019	24	24	3.0	2.9
Return / separation	2019	25	12	3.6	4.5
Return / separation	2019	25	24	2.2	4.0
Return / separation	2021	24	12	4.5	3.1
Return / separation	2021	24	24	3.2	5.4
Return / separation	2021	25	12	3.5	3.5
Return / separation	2021	25	24	3.9	4.9
Union / parenthood	2017	24	12	2.5	3.1
Union / parenthood	2017	24	24	3.4	4.5
Union / parenthood	2017	25	12	3.4	3.7
Union / parenthood	2017	25	24	3.5	4.5
Union / parenthood	2018	24	12	2.3	4.3
Union / parenthood	2018	24	24	3.6	3.6
Union / parenthood	2018	25	12	3.1	4.0
Union / parenthood	2018	25	24	2.9	3.8
Union / parenthood	2019	24	12	1.8	4.9
Union / parenthood	2019	24	24	3.0	4.3
Union / parenthood	2019	25	12	3.4	2.5
Union / parenthood	2019	25	24	3.9	4.1
Union / parenthood	2021	24	12	3.1	5.6
Union / parenthood	2021	24	24	3.8	3.7
Union / parenthood	2021	25	12	2.1	3.5
Union / parenthood	2021	25	24	3.4	4.2

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.

Shares are percentages of known baseline intersection members. All eight patterns partition each available cell, subject only to display rounding. Same_observation_interval means same observed quarter, not same-day events. Both_order_unresolved has confirmed events; event_membership_unresolved has

uncertainty about occurrence. Unavailable cells are not zero. The full aggregate output additionally retains counts and baseline candidate accounting.

S11 Conservative joint bounds and interpretation

For each known baseline member, let $[L_A, U_A]$ and $[L_B, U_B]$ be the registered event-observation intervals. The means of L_AL_B and U_AU_B bound joint occurrence conservatively. The upper endpoint does not impose additional cross-event compatibility assumptions. Both-event order uncertainty is distinct from uncertain event membership. No sampling interval, equality test or treatment contrast is attached to these finite-cohort descriptions. Known $n=0$ has undefined fractions; absent/uncertified histories are unavailable, never zero. Unknown baseline membership is outside the within- n bounds and limits generalization.

No p-values, confidence intervals, treatment contrasts or cross-module totals are registered. Marginal and joint bounds use the same known members. The conservative joint upper bound is not tightened using unregistered assumptions about event compatibility.

Table S11. Marginal and joint observation bounds, all registered cells. A. Denominators and observation scope

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Event unknown n	Scope / reason
Home / union	2017	24	12	1,000	13	45	[1]
Home / union	2017	24	24	1,000	13	44	[1]
Home / union	2017	25	12	1,100	13	42	[1]
Home / union	2017	25	24	1,100	13	44	[1]
Home / union	2018	24	12	1,000	13	39	[1]
Home / union	2018	24	24	1,000	13	42	[1]
Home / union	2018	25	12	1,100	13	38	[1]
Home / union	2018	25	24	1,100	13	45	[1]
Home / union	2019	24	12	1,000	13	45	[1]
Home / union	2019	24	24	1,000	13	44	[1]

Continued on next page.

Table S11 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Event unknown n	Scope / reason
Home / union	2019	25	12	1,100	13	28	[1]
Home / union	2019	25	24	1,100	13	52	[1]
Home / union	2021	24	12	1,000	13	52	[1]
Home / union	2021	24	24	1,000	13	43	[1]
Home / union	2021	25	12	1,100	13	53	[1]
Home / union	2021	25	24	1,100	13	52	[1]
Return / separation	2017	24	12	1,000	13	25	[1]
Return / separation	2017	24	24	1,000	13	46	[1]
Return / separation	2017	25	12	1,100	13	43	[1]
Return / separation	2017	25	24	1,100	13	44	[1]
Return / separation	2018	24	12	1,000	13	47	[1]
Return / separation	2018	24	24	1,000	13	37	[1]
Return / separation	2018	25	12	1,100	13	52	[1]
Return / separation	2018	25	24	1,100	13	47	[1]
Return / separation	2019	24	12	1,000	13	40	[1]
Return / separation	2019	24	24	1,000	13	29	[1]
Return / separation	2019	25	12	1,100	13	49	[1]
Return / separation	2019	25	24	1,100	13	44	[1]
Return / separation	2021	24	12	1,000	13	31	[1]

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Table S11 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Scope: [1] partial; unresolved baseline / events.

Module	Year	Age	Mo.	Known n	Baseline unknown n	Event unknown n	Scope / reason
Return / separation	2021	24	24	1,000	13	54	[1]
Return / separation	2021	25	12	1,100	13	39	[1]
Return / separation	2021	25	24	1,100	13	54	[1]
Union / parenthood	2017	24	12	1,000	13	31	[1]
Union / parenthood	2017	24	24	1,000	13	45	[1]
Union / parenthood	2017	25	12	1,100	13	41	[1]
Union / parenthood	2017	25	24	1,100	13	49	[1]
Union / parenthood	2018	24	12	1,000	13	43	[1]
Union / parenthood	2018	24	24	1,000	13	36	[1]
Union / parenthood	2018	25	12	1,100	13	44	[1]
Union / parenthood	2018	25	24	1,100	13	42	[1]
Union / parenthood	2019	24	12	1,000	13	49	[1]
Union / parenthood	2019	24	24	1,000	13	43	[1]
Union / parenthood	2019	25	12	1,100	13	27	[1]
Union / parenthood	2019	25	24	1,100	13	45	[1]
Union / parenthood	2021	24	12	1,000	13	56	[1]
Union / parenthood	2021	24	24	1,000	13	37	[1]
Union / parenthood	2021	25	12	1,100	13	39	[1]
Union / parenthood	2021	25	24	1,100	13	46	[1]

Table S11. Marginal and joint observation bounds, all registered cells. B.
Marginal and joint observation bounds
ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	A bounds (%)	B bounds (%)	Both bounds (%)
Home / union	2017	24	12	44.6 – 49.1	39.6 – 44.1	28.5 – 33.0
Home / union	2017	24	24	47.3 – 51.7	41.8 – 46.2	31.7 – 36.1
Home / union	2017	25	12	45.3 – 49.1	42.3 – 46.1	30.7 – 34.5
Home / union	2017	25	24	47.6 – 51.6	40.5 – 44.5	29.6 – 33.6
Home / union	2018	24	12	45.3 – 49.2	40.6 – 44.5	29.5 – 33.4
Home / union	2018	24	24	48.3 – 52.5	42.1 – 46.3	30.0 – 34.2
Home / union	2018	25	12	46.5 – 49.9	41.4 – 44.8	31.9 – 35.4
Home / union	2018	25	24	46.8 – 50.9	39.8 – 43.9	30.5 – 34.6
Home / union	2019	24	12	44.2 – 48.7	40.1 – 44.6	29.5 – 34.0
Home / union	2019	24	24	47.7 – 52.1	39.4 – 43.8	31.0 – 35.4
Home / union	2019	25	12	46.2 – 48.7	40.8 – 43.4	29.9 – 32.5
Home / union	2019	25	24	46.5 – 51.2	41.4 – 46.1	31.5 – 36.3
Home / union	2021	24	12	47.0 – 52.2	43.3 – 48.5	30.2 – 35.4
Home / union	2021	24	24	46.6 – 50.9	41.5 – 45.8	31.1 – 35.4
Home / union	2021	25	12	45.9 – 50.7	39.7 – 44.5	28.9 – 33.7
Home / union	2021	25	24	46.4 – 51.1	40.9 – 45.6	29.9 – 34.6
Return / separation	2017	24	12	43.3 – 45.8	40.1 – 42.6	28.0 – 30.5
Return / separation	2017	24	24	46.2 – 50.8	43.9 – 48.5	30.6 – 35.2
Return / separation	2017	25	12	46.5 – 50.5	39.3 – 43.2	30.1 – 34.0
Return / separation	2017	25	24	44.2 – 48.2	40.3 – 44.3	28.0 – 32.0
Return / separation	2018	24	12	46.3 – 51.0	39.0 – 43.7	29.0 – 33.7
Return / separation	2018	24	24	44.5 – 48.2	39.7 – 43.4	30.3 – 34.0
Return / separation	2018	25	12	44.5 – 49.3	40.5 – 45.3	29.6 – 34.4
Return / separation	2018	25	24	46.5 – 50.7	41.3 – 45.5	30.4 – 34.6
Return / separation	2019	24	12	43.9 – 47.9	38.9 – 42.9	28.6 – 32.6
Return / separation	2019	24	24	44.6 – 47.5	39.5 – 42.4	29.1 – 32.0
Return / separation	2019	25	12	47.5 – 52.0	42.0 – 46.5	32.5 – 37.0

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Table S11 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	A bounds (%)	B bounds (%)	Both bounds (%)
Return / separation	2019	25	24	46.6 – 50.6	42.6 – 46.6	31.9 – 35.9
Return / separation	2021	24	12	45.7 – 48.8	41.9 – 45.0	30.9 – 34.0
Return / separation	2021	24	24	38.5 – 43.9	32.1 – 37.5	21.9 – 27.3
Return / separation	2021	25	12	47.5 – 51.1	42.2 – 45.7	31.0 – 34.5
Return / separation	2021	25	24	46.6 – 51.5	40.7 – 45.6	29.5 – 34.4
Union / parenthood	2017	24	12	48.6 – 51.7	42.0 – 45.1	32.2 – 35.3
Union / parenthood	2017	24	24	45.3 – 49.8	40.8 – 45.3	29.6 – 34.1
Union / parenthood	2017	25	12	45.7 – 49.5	41.5 – 45.3	31.5 – 35.3
Union / parenthood	2017	25	24	47.1 – 51.5	41.1 – 45.5	30.2 – 34.6
Union / parenthood	2018	24	12	43.2 – 47.5	40.0 – 44.3	28.2 – 32.5
Union / parenthood	2018	24	24	47.4 – 51.0	42.0 – 45.6	31.6 – 35.2
Union / parenthood	2018	25	12	43.6 – 47.6	39.8 – 43.8	28.1 – 32.1
Union / parenthood	2018	25	24	45.8 – 49.6	41.0 – 44.8	29.1 – 32.9
Union / parenthood	2019	24	12	44.5 – 49.4	40.2 – 45.1	28.9 – 33.8
Union / parenthood	2019	24	24	48.4 – 52.7	41.6 – 45.9	31.8 – 36.1
Union / parenthood	2019	25	12	47.8 – 50.3	42.6 – 45.1	32.1 – 34.5
Union / parenthood	2019	25	24	46.3 – 50.4	42.3 – 46.4	29.5 – 33.6
Union / parenthood	2021	24	12	46.1 – 51.7	41.2 – 46.8	30.1 – 35.7
Union / parenthood	2021	24	24	45.5 – 49.2	40.8 – 44.5	30.2 – 33.9
Union / parenthood	2021	25	12	46.8 – 50.4	40.3 – 43.8	29.6 – 33.2

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Table S11 continued.

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Empirical results pending.

Module	Year	Age	Mo.	A bounds (%)	B bounds (%)	Both bounds (%)
Union / parenthood	2021	25	24	46.1 – 50.3	42.8 – 47.0	30.6 – 34.8

ILLUSTRATIVE MOCK DATA — NOT RESULTS. These values demonstrate table layout only; all empirical cells remain pending.