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Childhood mentoring, adult friendships and well-being after childhood disadvantage

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Abstract

Childhood adversity is increasingly differentiated by type, yet supportive relationships are still treated as interchangeable buffers; whether particular resources correspond to particular disadvantages is unclear. Among 27,630 Japanese respondents we distinguished event/condition adversity from felt lack of parental love and compared their interactions with adult friendship ties and childhood non-parental mentoring, using split-sample, longitudinal and exploratory intergenerational designs. A crossed contrast showed differing adversity–resource associations (L = + 0.489, 95% CI [+ 0.120, + 0.858]; Holm-adjusted P = 0.017), concentrated descriptively in a stronger mentoring–satisfaction association among adults recalling a lack of parental love. Friendship ties were more strongly associated with lower loneliness under event/condition adversity, whereas the friendship side of the satisfaction contrast was small and imprecise. Within-person friendship changes tracked loneliness, with a similar, less precise moderation pattern. Relational resources are not uniformly associated with well-being after childhood adversity; both the form of disadvantage and the relational context must be distinguished.

Introduction

Childhood adversity is associated with enduring differences in mental health and well-being [1, 2], yet outcomes among people exposed to adversity vary widely. This heterogeneity has motivated a shift from treating adverse childhood experiences as a single cumulative exposure towards identifying dimensions of experience with distinct developmental consequences [3, 4]: prospective cohorts separate dimensions such as threat and deprivation [5], and person-centred analyses describe recurring adversity profiles [6]. In parallel, supportive relationships within families, friendships, schools and communities have emerged as correlates of resilience across development [7], and social connection has become a public-health priority [8]. The two literatures have developed largely separately: adversity is increasingly differentiated, whereas relational resources are still often treated as interchangeable sources of protection.

This matters because relationships need not offset every form of childhood disadvantage in the same way. Stress-buffering accounts propose that social support attenuates the consequences of stress [9], and optimal-matching models predict that support helps most when its function corresponds to the demands of a particular stressor [10]. Developmental timing adds a further dimension: relationships available in childhood may provide experiences that are hard to reproduce later, whereas relationships formed in adulthood may act through more proximal pathways [11]. Consistent with this view, adult social support is associated with better outcomes after childhood adversity [12, 13], and community-related positive childhood experiences, including trusted non-parental adults, are associated with better adult outcomes even among people with adverse childhood experiences [14, 15], and childhood social capital, including relationships with non-parental adults, has been associated with adult subjective well-being in a Japanese sample [16]. Evidence that any single relational resource broadly buffers childhood adversity is nonetheless inconsistent: natural mentoring shows modest overall associations with youth outcomes [17], reviews find clearer evidence for direct than for buffering associations of positive

childhood experiences [7, 18], and recent Add Health analyses do not support uniform buffering across forms of childhood abuse [19, 20]. These inconsistencies raise a more specific possibility: they may partly reflect a mismatch between the childhood disadvantage considered and the relational resource expected to offset it.

A cumulative adversity score summarizes exposure burden while collapsing distinctions among the experiences that contribute to it [4]. Such distinctions may matter especially when the resource is itself relational. Recalled lack of parental love concerns perceived parental care, whereas an event/condition index combines experiences with heterogeneous relational content. Prospective work linking maternal warmth to later social safety and mental health supports retaining information about caregiving experience [21]; natural-mentoring research highlights relationship quality [17], and life-course models emphasize the timing of support [11]. Together with recent studies to differentiate childhood adversity beyond cumulative burden [22], this raises a specific question: does the relative association of childhood mentoring versus adult friendship with well-being vary across childhood histories?

Here we addressed this question in 27,630 Japanese respondents by distinguishing event/condition adversity from felt lack of parental love ('felt unloved' in tables and figures) and comparing childhood non-parental mentoring, measured as the recalled availability of a mentor-like adult, with adult friendship ties. Using a discovery–confirmation split, we estimated all four adversity–resource interactions jointly and tested their relative ordering for life satisfaction with a single crossed contrast, rather than inferring differentiation from separate significance tests [23]. We separately examined friendship–loneliness moderation and corresponding patterns in four-wave within-person analyses. This design does not assume one-to-one matching; it tests whether broadly favourable relational associations coexist with differences linked to childhood history, connecting dimensional approaches to adversity with social environments in population mental health. The crossed contrast showed differing adversity–resource associations, concentrated descriptively in a stronger mentoring–satisfaction association among adults recalling a lack of parental love; friendship ties were more strongly associated with lower loneliness under event/condition adversity, and within-person friendship changes tracked loneliness with a similar, less precise moderation pattern.

Methods

Study overview and inferential strategy

We combined JACSIS 2025 and its longitudinal panels with two additional surveys: a 2024 commercial life-course survey of recalled parenting and a 2025 parent–child survey. JACSIS provided the co-primary cross-sectional tests and within-person analyses; the additional surveys provided construct context and an exploratory intergenerational extension. Hypotheses and outcome assignments were developed in a discovery sample and estimated in a confirmation sample; the specification history is described under Sample splitting [24]. The study was not prospectively preregistered. Confirmation-sample estimates are

primary; full-sample estimates are supplementary precision estimates. Tests were two-sided, with 95% confidence intervals.

JACSIS 2025 cross-sectional sample

JACSIS is a repeated internet survey with links to the JASTIS panel [25]. The winter 2025 survey was conducted through a commercial panel in December 2025. Of 44,571 panellists entering from the combined JACSIS/JASTIS frame, 33,456 consented to linkage and 30,346 completed the JACSIS module. All completers answered the childhood-history items. We excluded 2,716 respondents who met any of four criteria, the JACSIS study's standard exclusions: completion in under 600 s, failure of an instructed-response item, reporting all eleven listed chronic conditions as current, or reporting more than 15 co-residents. The analytic sample comprised 27,630 respondents aged 16–85 years (mean 48.0 years; 51.8% women; 117 respondents, 0.4%, were aged 16–17; restricting the confirmation sample to respondents aged 18 or older left the co-primary estimates very similar, Supplementary Table S5). All JACSIS items used in the analyses were mandatory, so respondents who completed a wave had no item-level missingness on model variables; in the extended covariate tier, recoding education responses of 'other' or 'unknown' as missing excluded 81 confirmation-sample respondents (n = 13,849). Linkage was one-to-one on respondent identifiers, and duplicate identifiers were checked before merging.

JACSIS cross-sectional outcome regressions used provider-supplied calibration weights raked to the 2022 Comprehensive Survey of Living Conditions (variable JACSIS_WEIGHT_2025_byKisocho2022; 2019-calibrated alternative JACSIS_WEIGHT_2025_byKishocho2019). Weights were treated as fixed; confidence intervals therefore condition on the supplied weights. An alternative set calibrated to 2019 margins was retained for sensitivity analysis. We also examined unweighted models and weights capped at the 99th or 95th percentile. Weight construction and diagnostics are reported in Supplementary Table S11.

Measures

Well-being. Loneliness was assessed with the three-item UCLA scale [26], using four response options and a summed score of 3–12, with higher scores indicating greater loneliness. This format differs from the validated Japanese three-option version [27]. Internal consistency across waves was α = 0.88–0.91. Item wording was unchanged, but response anchoring and numeric direction differed between the JACSIS and JASTIS series; coding was harmonized before analysis. Formal measurement invariance was not tested. Wave-specific formats are documented in Supplementary Table S12. Life satisfaction was a single item scored 0–10 after subtracting one from the original 1–11 response (Q74.1); the separate living-conditions satisfaction item, Q74.13, was not used. Analytic-sample means were 5.78 for loneliness and 5.98 for life satisfaction.

Relational resources. The Lubben Social Network Scale-6 provided three-item family and friend subscales, each scored 0–15 [28, 29]. Individual items assessed monthly-contact friends and confidants using ordered count categories. Confidant categories were 0, 1, 2, 3–4, 5–8 and ≥ 9. Professional-helper access indicated whether any professional was available for consultation in person or remotely. Intergenerational exchange counted age bands other than the respondent's own with contact at least monthly and was mean-centred. The source grid contained six age bands (0–5, 6–12, 13–19, 20–39, 40–64 and ≥ 65 years); respondents aged ≤ 19, 20–39, 40–64 and ≥ 65 years were assigned to the third to sixth bands, respectively, so the two child bands were never counted as own-band. Childhood mentoring was a single retrospective item asking whether an older person other than the respondent's parents cared about them (Q76.5), adapted from the positive-childhood-experience item on having non-parental adults who took a genuine interest in the respondent [30], scored 0–3 for increasing consistency of presence ('never' to 'always').

Childhood experiences. Event/condition adversity (E) was a study-specific index from the JACSIS questionnaire indicating at least two of nine experiences before age 18: parental death, divorce or separation, mental illness, addiction, witnessing domestic violence, physical abuse, neglect, verbal abuse and psychological control; the items were adapted from the adverse-childhood-experience framework [31]. Felt lack of parental love (D) was coded 1 for a 'no' response to whether the respondent had felt loved by their parents before age 18 (Q75.9; [illegible]). These are distinct retrospective indicators, not objective versus subjective measures; D is not a comprehensive deprivation measure. In the analytic sample, 17.6% met the E threshold, 43.5% were coded D = 1, and 41.1% reported a mentor-like adult usually or always present.

Exposure sensitivities used a ten-item index including D (two or more items; 23.3%); a six-item index excluding neglect, verbal abuse and psychological control; leave-one-item-out indices; thresholds of one or three adversities; and continuous standardized event counts. An alternative D indicator used the four-point unconditional-love-from-anyone item (Q76.1), analysed both dichotomously (deficit coded for 'sometimes' or 'never') and as an ordinal z-score of the four-point response, with higher values denoting less consistent love. Item-level sensitivities paired mentoring with each event/condition indicator (Supplementary Table S5).

Latent class analysis

An unweighted latent class analysis summarized twelve binary indicators: the nine event/condition items, D, and deficits in unconditional love and household consistency (Q76.1 and Q76.2, coded as deficits for 'sometimes' or 'never'). Models with two to six classes were fitted to complete discovery-sample responses using expectation–maximization in log space. Starting probabilities were sampled from Uniform(0.2, 0.8), with 120 random starts for two to four classes and 40 for five or six classes, seeds from 0, tolerance 10^{-7} and a maximum of 800 iterations.

We considered BIC, confirmation-half log-likelihood, interpretability, entropy and minimum class size. Four classes were retained as a descriptive summary, although fit criteria continued to improve with additional classes (Supplementary Table S18). The profiles were labelled low-adversity, deprivation-dominant, multi-adversity and household-adversity; the labels are relative descriptions of endorsement profiles (felt lack of parental love was endorsed by 30% of the low-adversity profile), not clinical categories. Posterior probabilities for all respondents were computed from discovery-sample parameters. Reported mixing proportions are distinguished from modal-assignment proportions. Outcome regressions used modal assignments and calibration weights; the class model itself was unweighted and was not required for either co-primary test.

Sensitivity analyses refitted the model in the confirmation sample and omitted D, unconditional love, or both. Classes were matched by item-profile correlations. To examine classification uncertainty, outcome models were repeated with the class posterior probabilities entered as continuous predictors (class shares and their products with friendship ties, replacing the modal class indicators) and with twenty pseudo-class draws combined by Rubin's rules. Bias-adjusted three-step estimation was not performed [32, 33]. Class–outcome analyses were secondary.

Panel construction and linkage

Loneliness panel. We linked JACSIS 2022–2024 to JASTIS spring 2025, conducted in March 2025, using harmonized respondent identifiers [34]. The 2022 and 2023 files use USER_ID and the 2024 file Monitor_ID within one numbering system; the spring-wave 'RR1-' identifier prefix was removed before linkage. Applying the duration, attention, household-size and chronic-condition exclusions left 25,424 valid spring-wave respondents; the chronic-condition screen contained fourteen conditions. The balanced panel comprised 13,579 respondents and 54,316 person-wave observations. Harmonized wave means for loneliness were 5.75, 5.71, 5.72 and 6.67; the spring-wave increase coincided with the change in response anchoring, so measurement and temporal contributions to this difference cannot be separated. The primary moderator was E measured in 2024 (two or more items; 15.5%, n = 2,103). Measurement-consistency analyses restricted estimation to the three identically anchored JACSIS waves and separately standardized loneliness and friendship ties within each of the four waves. Retention diagnostics are reported in Supplementary Table S13.

Satisfaction panel. JACSIS 2022–2024 and winter 2025 provided 8,909 respondents with four observations each. Life satisfaction was harmonized to 0–10, and D was measured in winter 2025. Models used time-varying monthly-contact friend scores with the corresponding family-contact measure; a sensitivity specification used subscale adjustment (Supplementary Table S9). This panel examined adult friendship contact, not change in childhood mentoring.

Fixed-effects estimation. Models used within-person demeaning and wave indicators, with standard errors clustered by respondent. Time-invariant childhood indicators entered through interactions with time-varying ties. The cluster-robust covariance matrix used the finite-sample multiplier $G/(G-1) \times (N-$

1)/(N − K), where G is the number of respondents, N the number of person-wave observations and K the number of coefficients in the demeaned regression, excluding absorbed person effects; confidence intervals and P values used the normal reference distribution. An alternative calculation counted absorbed effects in K; estimator settings for both calculations are listed in Supplementary Table S7.

Further sensitivities used attrition weights, adversity measured in 2025, an unbalanced panel with at least three waves, a five-wave extension and adversity-by-wave interactions to allow group-specific time trends (Supplementary Table S7). Descriptive one- and two-lag models included standardized variables, individual and wave effects, and lagged outcomes. These short-panel models are susceptible to dynamic-panel bias and were not interpreted causally [35, 36] (Supplementary Table S8).

Independent commercial panel: the 2024 life-course survey

The 2024 life-course survey included 10,715 respondents. Analyses used 3,422 respondents with complete provider-derived scores for six parenting dimensions and life satisfaction. The sixteen-item Q4s2 module was administered to a design subset; 817 analytic respondents had scores derived from the broader parenting item set without this module. Item routing and analytic-sample selection are described in Supplementary Table S13.

Trust, autonomy support, shared time, indifference, norm imposition and scolding, each scored 1–5, were entered jointly into an ordinary least-squares model of Satisfaction With Life Scale scores (5–35) [37], with HC3 standard errors. We estimated standardized coefficients, direct pairwise Wald contrasts and collinearity diagnostics (all variance-inflation factors ≤ 2.6; largest inter-factor correlation r = 0.75, trust–autonomy; Supplementary Table S5). Unstandardized estimates are reported in Supplementary Table S14.

Parent–child dyad survey

The March 2025 survey sampled 1,575 parent–child dyads, one parent and one child per respondent, with quotas by child age band (0–3, 4–6 and 7–10 years) and parent sex (779 mothers and 796 fathers). Child well-being was assessed only for children aged 4–10 years (n = 1,041): the twelve-item Kiddy-KINDL for 4–6-year-olds was completed by the parent after asking the child (parent-assisted report), and the twenty-four-item Kid-KINDL for 7–10-year-olds was completed by the child [38–40]. Scores were calculated within form, z-standardized and pooled; sensitivity analyses stratified by form therefore also separated the two reporters. Parental depression was assessed with the PHQ-9 [41, 42].

The mother model related child well-being to the interaction between the mother's relationship with her own parents and non-kin informal support. The own-parent relationship was a mean-centred composite of four items rated 1–4 (parents can be trusted; would take advantage of her, reversed; usually help each other; think only of themselves, reversed), higher values indicating a better relationship. Non-kin informal

support indicated that, for any of six childcare-support functions, the mother named a trusted acquaintance who was neither a relative nor a paid service; the three consultation and stress-relief functions defined emotional support and the three practical functions (care when ill, minding the home for two to three days, emergency childcare) instrumental support. Models first used any support, then emotional and instrumental support separately; their interaction coefficients were compared in a joint model. Covariates were maternal age, household income, employment, number of children, child developmental stage and marital happiness. Ordinary least-squares models with HC3 errors used the 512 mother–child and 529 father–child dyads with child well-being data; dyads with children aged 0–3 years were not analysed because well-being was not assessed, and eligible dyads had no item-level missingness. Each respondent contributed one dyad, the unit of analysis. We also tested the parent-sex × support × own-parent-relationship interaction. Screening covariates were linked for all respondents. Selection and form-specific analyses are documented in Supplementary Table S13. All dyadic analyses were exploratory (Supplementary Table S10; Supplementary Table S22).

Statistical Analyses

Sample splitting. Respondents were assigned by independent Bernoulli(0.5) draws using numpy.random.RandomState(20260805).rand(n) < 0.5 on the valid-respondent file's row order; True denoted discovery. Assignments were saved (13,700 discovery and 13,930 confirmation respondents). One specification per co-primary hypothesis, including its outcome, was fixed after discovery and evaluated once in the confirmation sample. Confirmation-sample estimates of related interactions under earlier specifications preceded that fixing (specification timeline in the deposited record), so the split-sample estimates are internal sensitivity analyses rather than an outcome-blind test; the study was not prospectively preregistered. The specification record, the frozen results manifests and the checksummed analysis package are deposited with the code (Code availability).

Cross-sectional estimation and adjustment. JACSIS outcome models used calibration-weighted least squares with HC3 errors. The primary covariates were concurrent family ties, age and sex. Family ties were included to condition on current family relationships when comparing the measured resources, not to identify a causal effect. Adjustment sensitivities used age and sex alone; the primary covariates plus education, household income and cohabiting partnership; and additional adjustment for current psychological distress (Kessler-6, Japanese version [43]; six items scored 0–4 and summed to 0–24, entered as a continuous covariate). The latter assessed sensitivity to current distress but cannot distinguish shared reporting processes from adjustment for variables downstream of childhood experiences or current relationships [44]. Covariate tiers are illustrated in Supplementary Fig. 1 and reported in Supplementary Table S6.

Co-primary tests. H1 used life satisfaction as the outcome, $Y = \beta_0 + \beta_F F + \beta_M M + \beta_E E + \beta_D D + \beta_{FE} F{\cdot}E + \beta_{FD} F{\cdot}D + \beta_{ME} M{\cdot}E + \beta_{MD} M{\cdot}D + \gamma X + \varepsilon$, where F and M denote friendship ties and mentoring, z-standardized within each estimation sample, E and D are the binary childhood indicators, and X contains the primary covariates. We tested the Wald contrast $L = (\beta_{FE} - \beta_{ME}) - (\beta_{FD} - \beta_{MD})$,

with predicted direction L > 0. The decomposition L = C_F + C_M, where C_F = β_FE − β_FD and C_M = β_MD − β_ME, was descriptive. H2 tested the friendship × E interaction for loneliness using the original 0–15 friendship scale, including both main effects and the primary covariates. H1 coefficients are reported in Supplementary Table S15. Raw-scale margins accompany standardized estimates (Supplementary Tables S2 and S3).

Secondary models and direct contrasts. Secondary models examined friendship × E for life satisfaction; mentoring × D for satisfaction, additionally adjusting for friendship ties; and friendship × latent-class interactions, with low-adversity as reference. Class models included class main effects and the primary covariates (Supplementary Table S19). The intergenerational-exchange model included exchange, multi-adversity status, their interaction and the primary covariates, with loneliness as outcome. Direct Wald contrasts compared the multi-adversity and deprivation-dominant friendship interactions, informal and professional ties on standardized scales, and emotional and instrumental support in the dyad models. Simple slopes and contrast intervals were derived from coefficient covariance matrices. Separate significance levels were not treated as evidence that coefficients differed [23]. No equivalence tests were performed, and no smallest effect size of interest was prespecified. Post hoc mentor comparisons using E and the ten-item adversity index were descriptive and outside the frozen plan (Supplementary Table S5).

Multiplicity and specification sensitivity. Holm's procedure controlled the two-test co-primary family [45]. Holm adjustment addresses multiplicity within the stated families; it does not account for the earlier exploratory analyses or specification choices involving the confirmation sample (Sample splitting). A broader six-test sensitivity family added friendship × E for satisfaction, mentoring × D for satisfaction, friendship × multi-adversity class for satisfaction and contact-friends × D for satisfaction (Table 2). Additional analyses were labelled secondary or exploratory. H1 sensitivities included alternative exposure definitions, full-sample standardization and loneliness as outcome. Two post hoc model-form checks in the confirmation sample relaxed the additive E/D specification. A saturated four-group model (neither indicator, D only, E only, both) estimated a separate intercept and separate standardized friendship and mentoring slopes for each group, with the primary covariates, calibration weights and HC3 errors; its contrast L□ subtracted the friendship-minus-mentoring slope difference of the D-only group from that of the E-only group, group-specific slopes replacing the interaction coefficients of L. A second model replaced the mentoring slope with indicators for mentor scores 1–3 (reference 0) within each group and tested the D-only versus E-only difference in the predicted change from score 0 to 3 (Supplementary Table S5). Re-estimation across 100 random splits assessed split-choice sensitivity within the same dataset, not independent replication (Supplementary Table S4).

Table 2 | Differential adversity–resource associations: co-primary and secondary confirmation-sample tests

Panel a: co-primary hypotheses (confirmation-sample tests; Holm-corrected as a pair)

Hypothesis	Discovery	Confirmation	Holm P	Full sample (precision)
H1 Crossed contrast L (satisfaction)	+0.523 [+0.162, +0.884], P = 0.0046	+0.489 [+0.120, +0.858], P = 0.0094	0.0172	+0.489 [+0.229, +0.749], P = 2×10^{-4}
H2 Friend ties × event/condition adversity (loneliness)	−0.098 [−0.154, −0.041], P = 6.7×10^{-4}	−0.072 [−0.126, −0.018], P = 0.0086	0.0172	−0.085 [−0.124, −0.046], P = 1.9×10^{-5}

Panel b: broader six-test multiplicity sensitivity across the co-primary and principal secondary confirmation tests

Interaction (outcome)	Confirmation estimate	Raw P	Holm-6 P
Crossed contrast L (satisfaction)	+0.489 [+0.120, +0.858]	0.0094	0.0259
Friend × event adversity (loneliness)	−0.072 [−0.126, −0.018]	0.0086	0.0259
Friend × event adversity (satisfaction)	+0.050 [+0.005, +0.094]	0.029	0.0293
Mentor × felt-unloved (satisfaction)	+0.302 [+0.179, +0.425]	1.6×10^{-6}	9.4×10^{-6}
Friend × multi-adversity class (ref. low-adversity) (satisfaction)	+0.100 [+0.039, +0.161]	1.3×10^{-3}	0.0052
Contact friends × felt-unloved (satisfaction)	+0.165 [+0.078, +0.253]	2.2×10^{-4}	0.0011

The exploratory two-generation dyad extension is reported in Supplementary Table S10 and Extended Data Table 8, reflecting its exploratory status.

Notes. JACSIS models: calibration-weighted WLS, HC3; covariates family ties, age, sex (the four-interaction model additionally includes all four main effects; the mentor × felt-unloved model additionally includes friend ties; the class-moderation model additionally includes the three class dummies). Decision tree for H1: discovery-half direction check, then a single confirmation-half Wald test; only L, not its components, is a co-primary test. Full-sample estimates are precision estimates, not replacement evidence. The dyad extension (Supplementary Table S10): OLS, HC3; exploratory; the emotional-minus-instrumental difference in a joint model was −0.246 [−0.819, +0.328], P = 0.40.

Confidant categories. Within the multi-adversity class, we estimated unweighted satisfaction means across confidant categories and tested adjacent-category differences and first-step primacy using Wald contrasts. The main summary omitted the sparse ≥ 9 category (n = 17). A sensitivity analysis retained these respondents through category merging and applied calibration weights with age–sex adjustment (Supplementary Fig. 2; Supplementary Table S21).

Software and reproducibility. Original analyses used Python 3.12.3, pandas 3.0.2, numpy 2.4.4 and statsmodels 0.14.6; linearmodels 7.0 was used for the alternative covariance calculation. A clean-environment rerun from raw provider files on 14–16 September 2026 used statsmodels 0.15.0 and reproduced the primary contrasts, panel samples and headline coefficients, class-moderation estimate and latent class solution to seven decimal places. The co-primary tests, Holm adjustments, latent class analysis and class-moderation model, intergenerational-exchange, professional-helper and confidant-category analyses, item-level mentor sensitivities, ten-item and alternative-D sensitivities, both panel constructions and the three-wave, within-wave-standardized, adversity-by-wave and unbalanced panel variants are implemented in one script (full_pipeline_20260916.py) with 31 automated identity checks against the frozen results manifest. The life-course-survey and dyad models, the lagged, attrition-weighted, 2025-measured and five-wave panel variants, and the Kessler-6 and covariate tiers are implemented in the separate scripts of the same code record; latent-class restart schedules are logged. No prospective sample-size calculation was performed. Analyses used available observations after the stated exclusions; data collection and analysis were not blinded. Randomization was limited to sample splitting.

Results

The JACSIS 2025 analytic sample comprised 27,630 respondents, randomly divided into discovery (n = 13,700) and designated confirmation (n = 13,930) samples (Table 1). Primary cross-sectional estimates are from the confirmation sample, with full-sample estimates reported for precision. Because related analyses in the confirmation sample preceded final specification, we treat the split as internal sensitivity rather than outcome-blind confirmation or independent replication (Methods). Holm correction was applied within two defined families, the two co-primary tests and a six-test sensitivity family (Supplementary Table S1); all other P values are nominal and unadjusted.

Table 1
Datasets, samples and inferential roles

Dataset	Design; analytic N	Deficit constructs	Resource constructs	Inferential role
JACSIS 2025	Cross-sectional; 27,630 (13,700 discovery / 13,930 confirmation)	Event/condition adversity (E); felt-unloved (D)	Friend ties; childhood mentor	Co-primary tests (confirmation sample; H1, H2), Holm-corrected
JACSIS/JASTIS panel	4-wave balanced; 13,579	E measured in 2024	Friend ties (time-varying)	Within-person estimate of the friendship × E interaction
JACSIS panel	4-wave balanced; 8,909	D (felt-unloved)	Friend contact (time-varying)	Related within-person evidence (friendship contact × D)
2024 life-course survey	Cross-sectional; 3,422	Parenting dimensions	NA	Construct context (Supplementary Table S16)
Parent–child dyad 2025	Cross-sectional dyads; 512 mothers (529 fathers)	Mother's current relationship with her own parents	Non-kin support	Exploratory extension (Supplementary Table S10; Supplementary Table S22)

Life-satisfaction associations differed across adversity–resource pairings

We first tested whether the relative associations of adult friendship ties (F) and childhood non-parental mentoring (M) with life satisfaction differed across event/condition adversity (E; at least two of nine items) and felt lack of parental love (D) (Fig. 1). With both resources standardized and all four resource-by-disadvantage interactions estimated jointly, the crossed contrast, L = [(F × E) − (M × E)] − [(F × D) − (M × D)], was positive in the confirmation sample (L = + 0.489, 95% CI [+ 0.120, + 0.858], P = 0.0094; Holm-adjusted P = 0.0172; Supplementary Table S1). Discovery- and full-sample estimates were + 0.523 [+ 0.162, + 0.884] (P = 0.0046) and + 0.489 [+ 0.229, + 0.749] (P = 2×10^{-4}), respectively (Fig. 2a and Table 2).

Descriptive decomposition located most of the contrast on the mentoring side. The mentoring differential, C = (M × D) − (M × E), was + 0.461 [+ 0.224, + 0.698], whereas the friendship differential, C = (F × E) − (F × D), was + 0.028 [− 0.189, + 0.246] (Fig. 2b). The four confirmation-sample components were F × E = + 0.178 [+ 0.008, + 0.348], F × D = + 0.150 [+ 0.028, + 0.272], M × E = − 0.222 [− 0.397, − 0.046] and M × D = + 0.239 [+ 0.111, + 0.367] (Supplementary Table S15). The negative M × E component denotes a

less positive mentoring–satisfaction slope under E, not a negative mentoring association; it was near zero in the discovery sample, so agreement in L across samples did not extend to every component. Co-primary inference concerns L; its decomposition is descriptive.

One standard deviation corresponded to 3.7 friend-subscale points and 0.97 mentor-score levels. Among respondents with both childhood indicators, model-predicted satisfaction was 4.72 versus 5.59 at the lower and upper friendship quartiles, and 4.71 versus 5.11 across a one-level difference in mentor score (Supplementary Tables S2 and S3).

The contrast remained positive with a six-item event index (L = + 0.559 [+ 0.142, + 0.975], P = 0.0086), continuous event counts (+ 0.296 [+ 0.062, + 0.531], P = 0.013) and full-sample standardization (+ 0.489, P = 0.0093). Across 100 alternative random splits, all validation-half estimates were positive (median + 0.476; interquartile range + 0.405 to + 0.570) and 84% had P < 0.05; these analyses assess sensitivity to the split choice within the same dataset, not independent replication (Supplementary Table S4).

With the mentor score modelled categorically, the predicted satisfaction difference between scores of 3 and 0 was + 2.13 under D alone and + 0.64 under E alone. A post hoc model allowing separate intercepts and resource slopes for all four E/D groups produced a contrast whose interval included zero (L□ = +0.415 [− 0.018, + 0.848], P = 0.061), so the crossed-contrast inference depends on the additive specification of the primary model. The corresponding crossed contrast for loneliness was negative, but its interval included zero (− 0.406 [− 0.828, + 0.016]; Supplementary Table S5). Adjustment for current psychological distress (Kessler-6) attenuated the life-satisfaction contrast to + 0.312 [− 0.045, + 0.669] (P = 0.087; Supplementary Table S6).

Friendship–loneliness associations were stronger under event/condition adversity

In the second co-primary model, friendship ties were more strongly associated with lower loneliness among respondents with high event/condition adversity (interaction b = − 0.072 [− 0.126, − 0.018], P = 0.0086; Holm-adjusted P = 0.0172), adjusting for family ties, age and sex in a calibration-weighted model with HC3 standard errors. The slope difference amounted to 0.072 loneliness points per friend-subscale point, or 0.43 points across the friendship interquartile range. Discovery- and full-sample estimates were − 0.098 ($P = 6.7 \times 10^{-4}$) and − 0.085 ($P = 1.9 \times 10^{-5}$), respectively (Table 2). Both co-primary results also remained below 0.05 when included in the broader six-test Holm sensitivity family (adjusted P = 0.0259 for each; Table 2 and Supplementary Table S1).

The nine-item E index excluded felt lack of parental love. Because the co-primary model included only the friendship × E term, it tested moderation by E rather than specificity to E relative to D. In a descriptive model including all four standardized resource-by-disadvantage interactions, friendship × E was − 0.309 and friendship × D was + 0.073, a difference of − 0.382 [− 0.635, − 0.130]; these coefficients are per standard deviation of friendship ties, not per subscale point as in the co-primary model (Supplementary

Table S5). Kessler-6 adjustment attenuated the co-primary interaction to − 0.013 [− 0.060, + 0.035] (P = 0.60; Supplementary Table S6).

A secondary model also showed a positive friendship × E interaction for life satisfaction (b = + 0.050 [+ 0.005, + 0.094], P = 0.029), with discovery- and full-sample estimates of + 0.056 (P = 0.016) and + 0.052 (P = 0.0017). With the ten-item adversity index, which includes felt lack of parental love, the interactions were − 0.081 [− 0.128, − 0.034] for loneliness ($P = 6.5 \times 10^{-4}$) and + 0.065 [+ 0.026, + 0.104] for satisfaction ($P = 1.0 \times 10^{-3}$; Supplementary Table S5).

Within-person friendship–loneliness associations varied by adversity history

The four-wave loneliness panel linked JACSIS 2022–2024 with JASTIS spring 2025 and included 13,579 respondents (Fig. 3a). In individual and wave fixed-effects models, within-person increases in friendship ties were associated with lower loneliness overall (b = − 0.040 [− 0.048, − 0.032]). The association was stronger under high event/condition adversity measured in 2024 (interaction b = − 0.030 [− 0.055, − 0.004], P = 0.024; P = 0.051 under an alternative variance estimator that counts the absorbed person effects in the degrees-of-freedom correction, Supplementary Table S7), with estimated within-person slopes of − 0.066 and − 0.036 in the high- and low-adversity groups, respectively. With the ten-item index, the interaction was − 0.023 [− 0.045, − 0.002] (P = 0.034; Supplementary Table S7).

The interaction remained negative when analysis was restricted to the three identically anchored JACSIS waves (b = − 0.067 [− 0.098, − 0.035], P < 0.001) and when loneliness and friendship ties were standardized within wave (− 0.048 [− 0.084, − 0.012], P = 0.008). Estimates were also negative with attrition weighting, with adversity measured in 2025 and in an unbalanced panel of respondents contributing at least three waves, although intervals included zero in the latter two specifications. A five-wave extension produced − 0.025 [− 0.048, − 0.002] (P = 0.031; n = 13,135; Supplementary Table S7).

In exploratory one-lag models, friendship ties were associated with next-wave loneliness (standardized β = −0.019 [− 0.032, − 0.006], P = 0.005), whereas the reverse estimate was imprecise (β = −0.009 [− 0.020, + 0.003], P = 0.13). Two-lag models likewise associated lagged friendship ties with later loneliness at both lags, without clear reverse associations. These analyses were not interpreted as establishing directional asymmetry (Supplementary Table S8).

Mentoring–satisfaction associations were stronger under felt lack of parental love

In a separately specified secondary model that omitted the other three resource-by-disadvantage interactions, the mentoring–satisfaction slope was + 0.647 [+ 0.552, + 0.741] per mentor-score level among respondents who recalled lacking parental love, compared with + 0.345 [+ 0.259, + 0.431] among those who recalled feeling loved. The interaction was + 0.302 [+ 0.179, + 0.425] in the confirmation sample ($P = 1.6 \times 10^{-6}$) and + 0.347 [+ 0.257, + 0.437] in the full sample ($P = 4.0 \times 10^{-14}$; Fig. 2c). These

unstandardized estimates are distinct from the standardized M × D component of L. With Kessler-6 adjustment, the secondary interaction was + 0.271 [+ 0.153, + 0.389] ($P = 6.6 \times 10^{-6}$; Supplementary Table S6).

Post hoc models outside the frozen plan provided mentoring interaction estimates of − 0.109 [− 0.284, + 0.065] for event/condition adversity (P = 0.22) and − 0.004 [− 0.156, + 0.148] for the ten-item cumulative index (P = 0.96; Supplementary Table S5). These alternative exposure specifications do not directly test whether aggregation cancels opposing interactions.

In the independent 2024 life-course survey (n = 3,422), conducted through a separate commercial panel, parental trust had the largest standardized point estimate for satisfaction among six jointly modelled parenting factors (β = +0.19 [+ 0.12, + 0.25]); its direct contrast with autonomy support was nevertheless inconclusive (Supplementary Table S16).

Adult friendship contact also covaried with satisfaction under felt lack of parental love

In the separate four-wave satisfaction panel (n = 8,909; JACSIS 2022–2024 and winter 2025), within-person increases in monthly-contact friend scores were more strongly associated with satisfaction among respondents who recalled lacking parental love (interaction b = + 0.039 [+ 0.004, + 0.075], P = 0.029, adjusting for family contact). The subscale-adjusted estimate was + 0.032 [− 0.003, + 0.068] (P = 0.072). Corresponding cross-sectional interactions were + 0.165 [+ 0.078, + 0.253] in the confirmation sample ($P = 2.2 \times 10^{-4}$) and + 0.166 [+ 0.103, + 0.229] in the full sample (Fig. 3b and Supplementary Table S9). This panel assessed adult friendship contact, not within-person variation in childhood mentoring.

Event/condition adversity and felt lack of parental love were often discordant

In the confirmation sample, weighted proportions were 8.0% for E alone, 35.1% for D alone, 10.3% for both and 46.6% for neither; 43.1% of respondents were therefore discordant on the two indicators under these definitions (Fig. 4a and Supplementary Table S17). Their unweighted association was modest (φ = 0.11; odds ratio = 1.77 [1.62, 1.94]).

A latent class analysis of twelve retrospective indicators (the nine event/condition adversities, felt lack of parental love and two positive-environment deficits) provided a complementary descriptive summary. The model was fitted without weights in the discovery sample and applied to all 27,630 respondents. Four classes were retained on the basis of the elbow criterion, interpretability, classification quality (relative entropy = 0.79) and minimum class size, although BIC and confirmation-half log-likelihood continued to improve with additional classes (Fig. 4b and Supplementary Table S18).

Model-estimated proportions were 55.3% for the low-adversity, 24.2% for the deprivation-dominant, 10.2% for the multi-adversity and 10.3% for the household-adversity profiles. The deprivation-dominant profile combined low verbal-abuse endorsement (4%) with high endorsement of felt lack of parental love

(68%) and lack of unconditional love (95%). The multi-adversity profile combined verbal abuse (85%), physical abuse (30%) and felt lack of parental love (85%). The household-adversity profile included verbal abuse (43%) and parental divorce (25%), with comparatively low endorsement of felt lack of parental love (12%; Fig. 4c).

Re-estimation in the confirmation sample produced closely similar profiles (r = 0.984–1.000) and proportions (54.9%, 25.2%, 9.9% and 10.0%). The deprivation-dominant profile persisted when at least one deprivation indicator was retained (Methods). Neither co-primary analysis depended on the class solution.

Friendship–satisfaction associations were steepest in the multi-adversity profile

Secondary class-based analyses showed a steeper friendship–satisfaction slope in the multi-adversity than in the low-adversity class (difference = + 0.100 [+ 0.039, + 0.161], P = 1.3×10^{-3}); differences from the low-adversity class were + 0.003 for both the deprivation-dominant and household-adversity classes (P = 0.89 and 0.91; Supplementary Table S19). The direct multi-adversity-minus-deprivation-dominant contrast was + 0.097 [+ 0.030, + 0.164] (P = 0.0047; Supplementary Table S20). The multi-adversity estimate was similar with posterior probabilities entered as continuous predictors (+ 0.120, P = 5.1×10^{-4}) and pseudo-class draws (+ 0.100, P = 3.2×10^{-3}).

Within the multi-adversity class, mean satisfaction ranged from 3.66 among respondents with no confidants to 6.38 among those with five to eight. The largest adjacent-category difference was between none and one (Δ = +1.07 [+ 0.66, + 1.47]), but the weighted, age–sex-adjusted first-step primacy contrast was imprecise (+ 0.495 [− 0.549, + 1.539], P = 0.35), providing no clear evidence of a first-confidant threshold (Supplementary Fig. 2 and Supplementary Table S21).

In additional supporting analyses, the professional-helper-access × multi-adversity interaction for loneliness was imprecise (b = + 0.41 [− 0.19, + 1.01], P = 0.18), whereas the standardized informal-minus-professional interaction contrast was − 0.394 [− 0.785, − 0.003] (P = 0.048). Intergenerational exchange, defined as at least monthly contact with age bands other than one's own, also interacted with multi-adversity status (b = − 0.215 [− 0.410, − 0.020], P = 0.030). These analyses were outside the co-primary family (Supplementary Table S20).

Exploratory intergenerational associations

The parent–child survey included 1,575 parent–child dyads (779 mothers and 796 fathers); well-being was assessed for the 1,041 children aged 4–10 years (child-reported at 7–10 years, parent-assisted at 4–6 years). In the exploratory analysis of the 512 mother–child dyads with child well-being data (the remaining 267 mothers had children aged 0–3 years, for whom well-being was not assessed; Supplementary Table S13), maternal non-kin support interacted with the mother's relationship with her own parents in relation to child well-being (b = − 0.297 [− 0.549, − 0.044], P = 0.022). The corresponding

paternal estimate, the emotional-versus-instrumental support contrast and the parent-sex contrast were imprecise (Supplementary Table S10 and Supplementary Table S22).

Discussion

In the primary specifications, the associations of relational resources with adult well-being differed according to the retrospective childhood indicators considered. The life-satisfaction contrast was concentrated descriptively in the mentoring differential: mentoring showed a stronger association among respondents who recalled lacking parental love, whereas the friendship differential was small and imprecise. Friendship ties were more strongly associated with lower loneliness under event/condition adversity, with a corresponding within-person pattern. Together, these findings suggest relative differentiation among the measured resources, rather than a symmetric or exclusive pairing of each resource with one disadvantage.

Optimal-matching accounts propose that support helps most when its function corresponds to the demands of the stressor [10], and life-course accounts propose that its relevance depends on when support is available [11, 46]. Related work on later-life loneliness has examined distinct childhood stressors with family and friend support treated separately [47]. For life satisfaction, our contribution is to compare adversity–resource interactions directly within one model, rather than infer differences from separate significance tests. Such differentiation can coexist with broadly favourable associations: adult friendship contact also covaried with satisfaction among respondents who recalled lacking parental love, cross-sectionally and in the primary panel specification, although the subscale-adjusted panel estimate was imprecise. The contrast therefore concerns relative associations, not an allocation of unique protective roles.

One possibility is that caring non-parental adults provide acceptance not experienced in parental relationships. Prospective associations linking observed maternal warmth, social-safety schemas and adolescent mental and physical health lend developmental plausibility to this hypothesis [21], but do not establish a mentoring mechanism. Our mentoring item measured consistency of presence, not acceptance, relationship quality or support functions. Moreover, mentoring and friendship differed simultaneously in source, developmental timing, measurement and recall. Standardization does not separate these dimensions.

Disaggregation should complement rather than replace cumulative adversity measures [48], which themselves omit informative dimensions such as childhood unpredictability [22]. Both childhood indicators were retrospective: event/condition adversity is not an objective benchmark, and felt lack of parental love is not a comprehensive deprivation measure. Replacing parental love with unconditional love from anyone reduced the contrast (L = + 0.355 [− 0.032, + 0.741]; Supplementary Table S5), limiting generalization beyond the original indicator. When separate intercepts and resource slopes were allowed across the four combinations of the childhood indicators, the contrast was inconclusive, so the inference depends on the additive specification of the primary model. The findings therefore concern particular

measured associations, not a validated typology of relational needs or established superiority in individual-level prediction.

Retrospective reports require particular caution. Prospective and retrospective maltreatment measures differ in their associations with psychopathology [49], and depressive symptoms predicted subsequent increases in recalled adversity within persons in a longitudinal study of Chinese adolescents [50], and within-person fluctuations in adversity reports covaried with parental support and strain in emerging adults [51]. Historical information and current-state influences may therefore coexist. In our analyses, Kessler-6 adjustment attenuated the crossed contrast (+ 0.489 to + 0.312) and reduced the friendship × E interaction for loneliness from − 0.072 to − 0.013 [− 0.060, + 0.035], whereas the mentoring × D component of the crossed contrast changed little (+ 0.239 to + 0.223 [+ 0.100, + 0.347]; Supplementary Table S6). This could reflect shared distress or reporting processes, but adjustment could also remove associations operating through distress [44]. Neither attenuation nor persistence distinguishes these explanations.

The designs address different uncertainties but do not identify causal effects. Related confirmation-sample analyses preceded final specification (Methods), so split-sample results provide internal sensitivity evidence rather than outcome-blind confirmation or external replication. Fixed effects address stable additive person-level differences, not time-varying common causes or reciprocal changes, and the panels did not test childhood mentoring or the full contrast. Attenuation of associations involving maternal warmth and support from adults during childhood in a twin-differences study further points to possible familial confounding [52]. Response-anchor differences, unestablished measurement invariance and sensitivity to variance estimation limit panel inference; population calibration cannot eliminate selection bias or establish cross-cultural generality.

Because the outcomes were loneliness and life satisfaction rather than psychiatric symptoms or treatment response, the findings bear on population mental health and prevention. Their immediate implication concerns measurement: surveillance and programme evaluations should distinguish the source, timing and function of relational support rather than collapse it into one score; they do not justify assigning support by recalled childhood history or substituting informal relationships for professional care. The intervention question remains open. A randomized community-based mentoring trial reported benefits for selected behavioural and mental-health outcomes [53], whereas an Add Health analysis found no clear evidence that natural mentor presence moderated the abuse-related associations it examined [20]. Programme efficacy and moderation by naturally occurring relationships are different questions; neither establishes how childhood history should guide support selection. Similarly, our exploratory mother–child association does not establish intergenerational protection or maternal specificity; it is compatible with evidence that intergenerational alignment of social capital tracks relational rather than socioeconomic factors [54].

An independent, prospectively registered study should test the crossed contrast with component predictions and outcomes specified in advance. Parallel measures of support functions across sources

and developmental periods, combined with repeated prospective and multi-informant assessments, would help separate historical experience from current-state contributions. Randomized or quasi-experimental studies could then test whether support addressing a specified relational need outperforms equally intensive generic support. The present findings provide model-sensitive evidence of heterogeneity in relational associations, while leaving open whether that heterogeneity can guide more effective support.

Declarations

Generative AI assisted code development under author supervision. The authors verified the analyses and take responsibility for the work.

Ethics

JACSIS was approved by the Osaka International Cancer Institute (20084; 19 June 2020) and Tohoku University Graduate School of Medicine (2024-1-231; 2024-1-517). JASTIS was approved by the Osaka International Cancer Institute (1412175183) and the National Institute of Public Health (NIPH-IBRA#12112). Both datasets were used with permission from the principal investigator, T.T. The 2024 life-course survey, its linked profile database and the 2025 dyad survey were approved by Tohoku University (23A-01). Procedures followed the Declaration of Helsinki. Participants provided online informed consent; parents consented and children assented in the dyad survey. Commercial-panel participants received reward points under provider arrangements.

Competing interests

T.T. received financial support for research (research funding) from Daiichi Sankyo Healthcare Co., Ltd., Johnson & Johnson K.K., Data Seed Inc., Workout-Plus LLC, EMMA Co., Ltd., Addness Inc., CotoIT Inc. and Laugh Inc. in the last 36 months, and serves as a research advisor to Data Seed Inc., for which he receives personal consulting fees. These entities had no role in the study design, data collection, analysis, interpretation of the data, or the decision to publish. C.H. and K.H. declare no competing interests.

Author contributions

C.H. conceived the study, developed the hypotheses and methodology, conducted the primary analyses, developed the analysis software and wrote the original draft. T.T. established and leads the JACSIS study, contributed to data acquisition. K.H. conducted the parent–child dyad survey analyses and contributed to interpretation. All authors critically reviewed the manuscript, approved the submitted version and accept accountability for their respective contributions.

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Data availability

Academic researchers may apply for access to JACSIS and JASTIS individual-level data through the JACSIS collaborative research framework (applications to the principal investigator, T.T.; applications are reviewed and access is granted under a data-use agreement restricting use to the approved analyses) [25, 34]. The 2024 life-course survey data, including profile-database variables used under license from an online-panel provider identified to the editors, are subject to the provider's terms and cannot be redistributed; access conditions are available from the corresponding author (C.H.). De-identified analysis files for the 2025 parent–child survey are included in the Zenodo record (https://doi.org/10.5281/zenodo.22860207) under the conditions of protocol 23A-01 and will be made public on publication. Source data are provided for Figs. 2–4 and Supplementary Fig. 2.

Code availability

The analysis code, automated identity tests, and the frozen results manifest are deposited at Zenodo (https://doi.org/10.5281/zenodo.22860207); the record will be made public on publication, and reviewer access is provided through the link supplied at submission.

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Figures

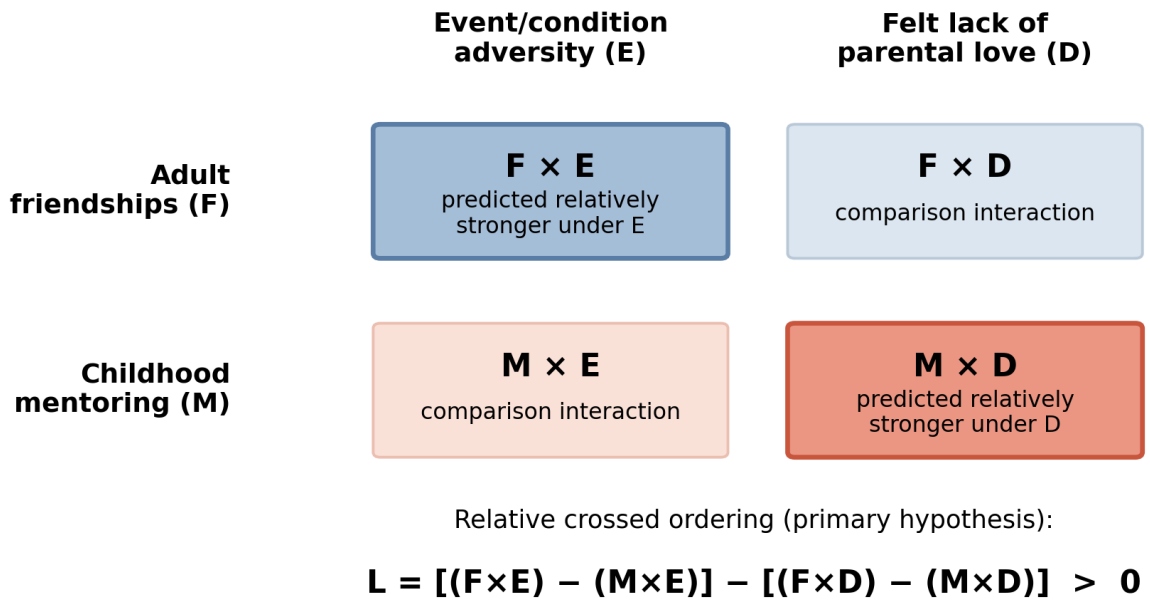


Figure 1

Framework for comparing adversity–resource associations. Adult friendship ties (F) and childhood non-parental mentoring (M) are crossed with event/condition adversity (E) and felt lack of parental love (D). Darker cells indicate the hypothesized relative ordering for life satisfaction, summarized by the primary contrast L = [(F × E) − (M × E)] − [(F × D) − (M × D)] > 0, where each term denotes the corresponding interaction coefficient. The hypothesis concerns the overall contrast, not the individual interactions. Shading represents hypothesized associations, not observed results or causal pathways.

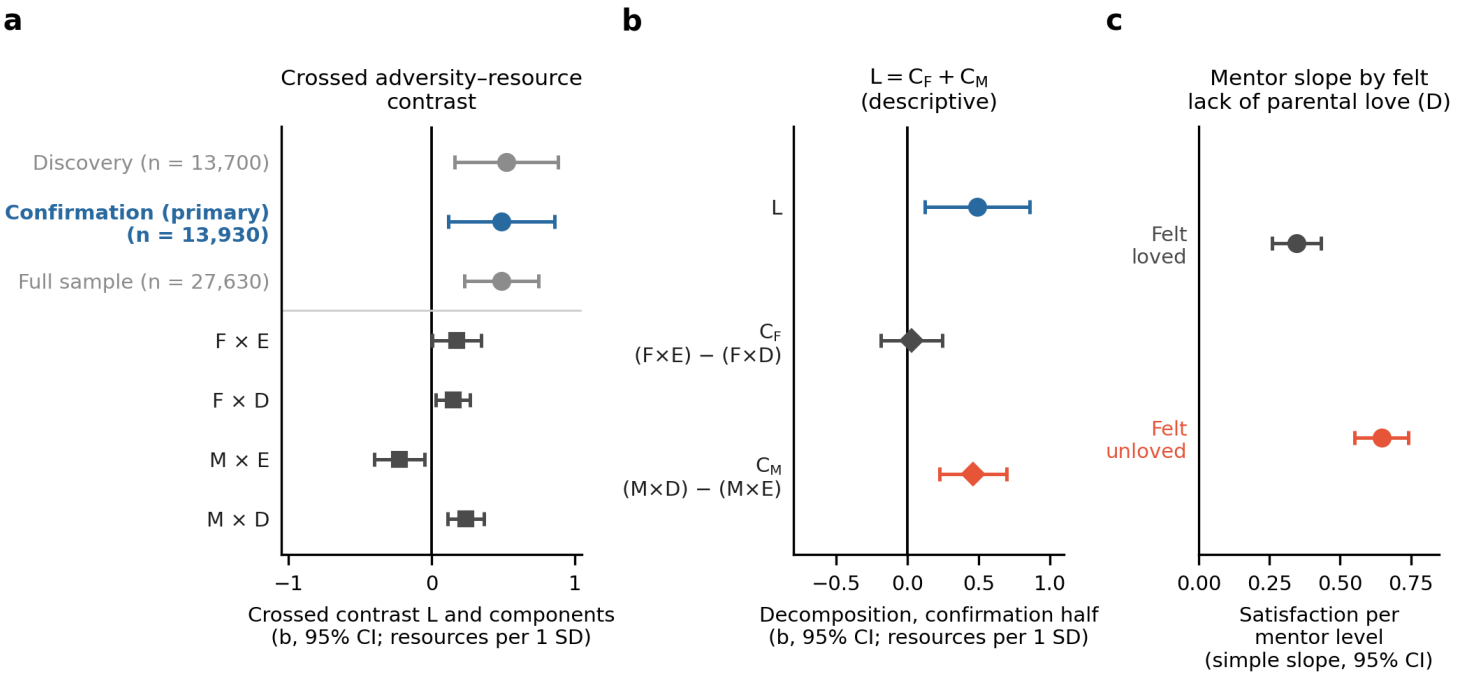


Figure 2

Crossed contrast and component associations for life satisfaction. a, Crossed contrast L (circles) in the discovery (n = 13,700), confirmation (n = 13,930; primary) and full (n = 27,630; precision) samples, and confirmation-sample interaction coefficients (squares). L = [(F × E) − (M × E)] − [(F × D) − (M × D)], where F and M denote adult friendship ties and childhood non-parental mentoring, standardized within sample; E denotes event/condition adversity (at least two of nine items) and D denotes felt lack of parental love. **b**, Descriptive decomposition of L (circle) into the friendship differential, C□ = (F × E) − (F × D), and the mentoring differential, C□ = (M × D) − (M × E) (diamonds), with L = C□ + C□. **c**, Mentoring–satisfaction slopes by recalled parental love, per mentor-score level. Panels a and b use the joint four-interaction model; c uses a separate mentoring × D model. Panels b and c use confirmation-sample data. Models used calibration-weighted least squares with HC3 robust standard errors, adjusting for family ties, age and sex; c additionally adjusted for friendship ties. Points and error bars show estimates and 95% confidence intervals. Tests were two-sided Wald tests; the confirmation-sample L had P = 0.0094 (P = 0.0172 after Holm adjustment across the two co-primary tests).

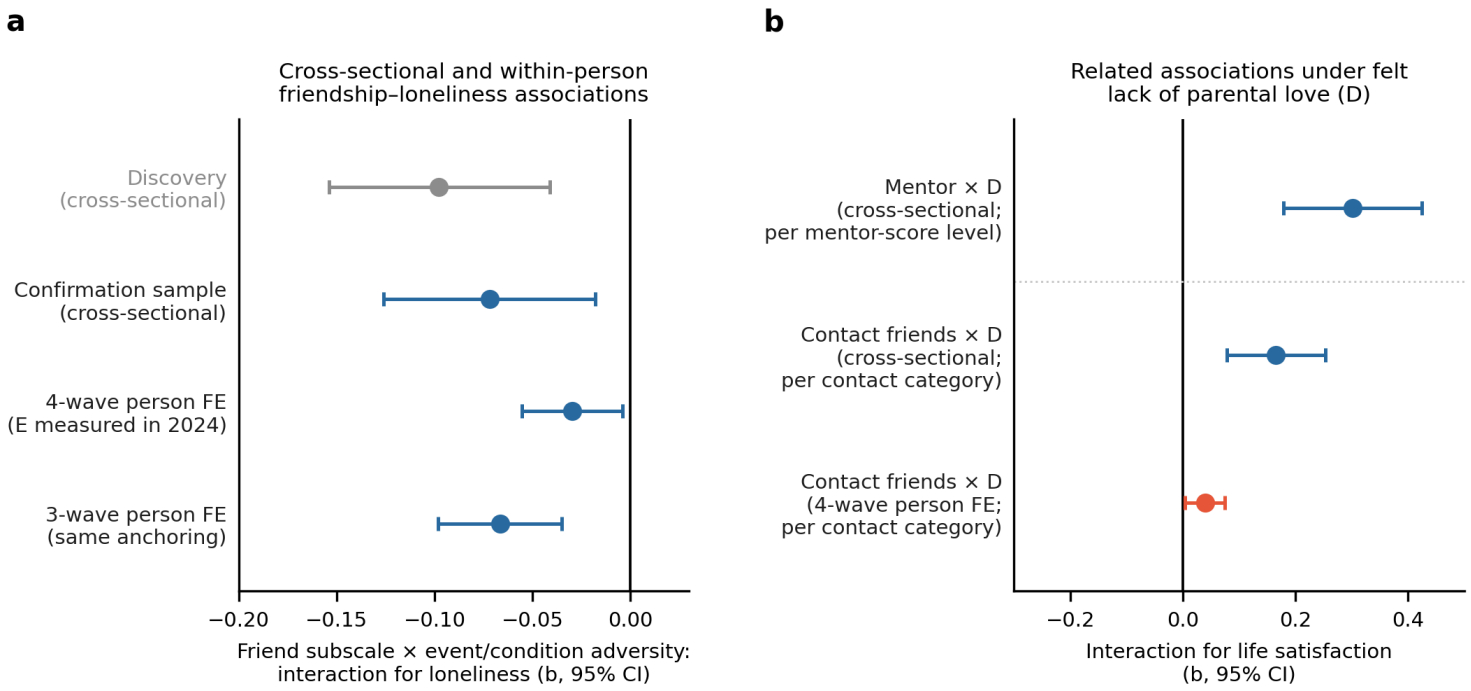


Figure 3

Cross-sectional and within-person adversity–resource interactions. a, Friendship × event/condition adversity interactions for loneliness in the discovery (n = 13,700) and confirmation (n = 13,930) samples and in the four-wave panel (13,579 respondents; 54,316 person-wave observations), alongside a three-JACSIS-wave analysis of the same cohort with identical response anchoring. Panel adversity used the nine-item index measured in 2024. **b**, Mentoring × felt lack of parental love (D) and monthly-contact friend score × D interactions for life satisfaction in the confirmation sample (n = 13,930), alongside the friendship-contact interaction in a separate four-wave panel (8,909 respondents; 35,636 person-wave observations). Points show interaction coefficients; error bars show 95% Wald confidence intervals. Cross-sectional models used calibration-weighted least squares with HC3 robust standard errors; panel models included individual and wave fixed effects with respondent-clustered standard errors. Coefficients are per friend-subscale point in a and per mentor-score level or friend-contact score increment in b; magnitudes are not directly comparable across measures. Panel b does not provide a longitudinal test of childhood mentoring.

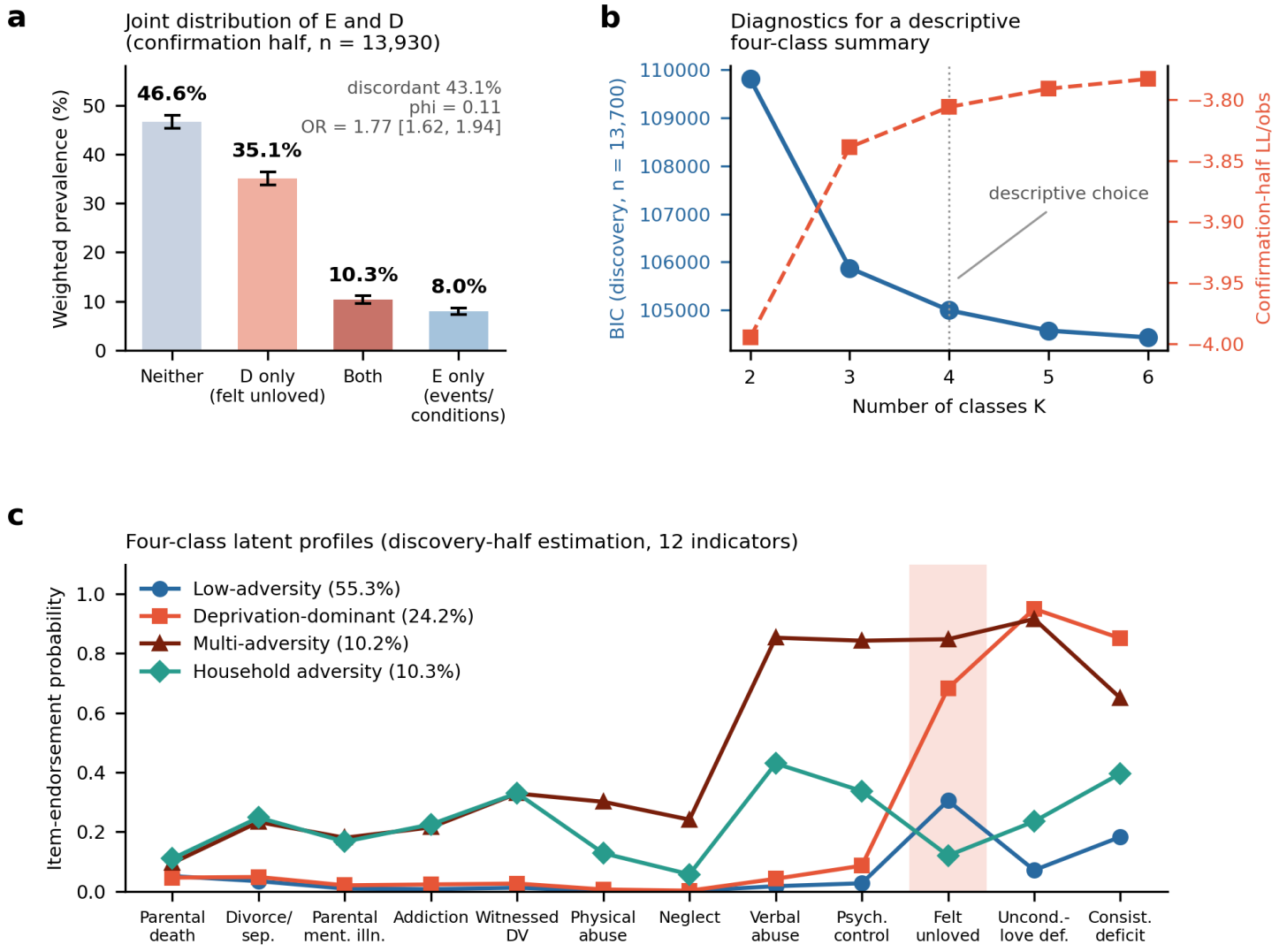


Figure 4

Joint distribution and latent profiles of childhood disadvantage. a, Calibration-weighted joint distribution of event/condition adversity (E; at least two of nine items) and felt lack of parental love (D) in the confirmation sample (n = 13,930). Bars show proportions; error bars show 95% confidence intervals from a normal approximation using the calibration-weighted effective sample size (Extended Data Table 3). Discordance denotes E alone or D alone; φ and the odds ratio (OR) use unweighted counts. **b**, Bayesian information criterion (BIC; discovery sample, n = 13,700) and log-likelihood per observation (confirmation sample, n = 13,930) for two to six classes. Four classes were retained as a descriptive summary despite continued improvement in fit. **c**, Item-endorsement probabilities from the unweighted discovery-sample model. Colours and marker shapes distinguish classes; percentages denote estimated mixing proportions. Shading highlights felt lack of parental love. Neither co-primary test depended on the class solution. DV, domestic violence.

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- ExtendedDatav721.pdf
- Supplementaryv737.pdf