Coparental Formation and Child Adjustment

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Abstract

People considering whom they might raise a child with have limited guidance grounded in evidence about children's later adjustment. The Upstream Coparental Formation Model asks whether observing how two adults work together before shared caregiving improves prediction of child adjustment beyond information about each adult and how they later provide care. Four propositions specify when early dyadic observations should add predictive value, when unresolved conflict should disrupt caregiving, when joint difficulties should limit compensatory care, and whether early repair should forecast child internalizing symptoms. The key test compares predictions in held-out families from models that differ only in whether they include observed early interaction. A genetic extension describes how partner resemblance can shape the distribution of inherited variation and complicate interpretation of family associations. The model distinguishes prediction among people who become coparents from the decisions facing people who are still considering shared caregiving. It specifies what evidence would be needed before observations of repair or coordination could inform guidance.

Keywords: coparenting, coparental formation, dyadic interaction, conflict repair, interparental conflict, child adjustment

Coparental Formation and Child Adjustment

The developmental significance of coparental formation

Two parents may enter parenthood equally committed to responding patiently to a distressed child. One pair routinely renegotiates responsibilities when plans fail. In the other, unresolved disagreements leave each adult uncertain whether the other will follow through. The difference may become visible only once sleep loss, work demands, or a child's illness makes coordination costly. A study beginning at preschool can measure differences in caregiving and child adjustment while missing when those differences first began to emerge.

Research gives families many ways to learn about parenting once a caregiving arrangement is in place. A different question arises earlier, when someone has a meaningful choice about whom they might raise a child with. What could they observe in an adult or in the way the two of them work together that would help them anticipate shared caregiving? Studies of partner resemblance describe who pairs with whom, studies of adult relationship quality track the relationship itself, and much of the evidence on caregiving begins once adults are already raising children. Few studies in the literatures integrated here connect what adults could observe while making a decision about shared caregiving to later care and child adjustment.

Relationship education supplies a starting point. PREP teaches communication and conflict management to couples who are already together (Markman et al., 1993). Family Foundations supports coparenting across the transition to parenthood (Feinberg & Kan, 2008). Programs do reach individuals, including adolescents and emerging adults, though long-term follow-up has been limited (Simpson et al., 2018). A recent analysis of the perinatal MotherWise trial linked relationship education to child outcomes through measured skills and attitudes in statistical path models (Lee et al., 2026). These studies show what education can address at

different stages. Evidence that could guide the earlier decision about whom to raise a child with requires observing both the decision context and what follows for caregiving and children.

The Upstream Coparental Formation Model (UCFM) puts the composition and early interaction of the caregiving pair into developmental explanation. Upstream refers to the period before the two adults begin sharing caregiving. Beginning observation after that transition can obscure the origins of differences that are already visible in how families care for children. The proposed tests distinguish what can be learned from established pairs from what must be studied while people are still deciding whether and with whom to share care. The UCFM predicts that observing both adults before shared caregiving can improve forecasts of child adjustment, even when researchers already know how each adult later parents. The model also asks a stronger question. Under comparable measurement conditions, can the early information predict as well as, or better than, observations of later caregiving? Figure 1 connects this prediction to two relational pathways. A separate genetic extension addresses the interpretation of family associations when coparents are also genetic parents.

Coparental formation is the part of family formation through which adults come to share responsibility for a child. Partner selection refers to how adults become paired, through their own decisions, family involvement, institutional arrangements, or the opportunities available to them. Assortment describes statistical resemblance between partners. Coparents are adults who share meaningful responsibility for a child's care, and coparenting is how they coordinate that responsibility. Pairing identifies the particular combination of adults in a study. Genetic parents and the gestational parent are identified separately where those roles matter. These roles can coincide or be distributed among several people. The model provides no basis for genetic or psychiatric screening of prospective coparents.

Relationship to prior models

Belsky's (1984) process model placed parental characteristics, child characteristics, and contextual support and stress among the determinants of parenting. Feinberg (2003) defined the coparenting subsystem and showed how it connects to its ecological context. Davies and Cummings (1994) explained how interparental conflict can affect children through emotional security. These frameworks supply much of the developmental mechanism used here. The Cowans made the transition to parenthood central, following couples from before birth and documenting changes in roles and relationships (Cowan & Cowan, 2000; Cowan et al., 1985). The UCFM adds a joint test of whether the composition and observed interaction of the early pair improve prediction over adult traits and later caregiving, and whether early indicators carry forward through separable postnatal pathways.

Evidence on early relationship prediction sets a second baseline for the model. Huston et al. (2001) used the PAIR project to link newlywed patterns to later marital outcomes. Markman et al. (2010) followed couples from premarital communication assessments to later marital adjustment and divorce. Those studies motivate the timing of the observations. The UCFM asks whether independently observed behavior at a different transition adds predictive information about the child's adjustment, after accounting for both adults' traits and global relationship ratings. Table 1 distinguishes the early prediction hypotheses from the spillover mechanism established in prior theory and clarifies where the composition of the pair matters.

This theoretical integration prioritizes peer-reviewed reviews and studies with temporally ordered, genetically informative, or experimental designs. Each estimate is interpreted in relation to the population, measure, and contrast that produced it.

Relational pathways from coparental formation to child adjustment

Exposure to interparental interaction

Children can respond to how their caregivers manage disagreements even when neither adult directs the exchange at them. Emotional security theory links repeated threats within the interparental relationship to children's vigilance, involvement in disputes, or efforts to avoid them (Davies & Cummings, 1994). A broad meta-analysis documented associations between several dimensions of the interparental relationship and children's adjustment and responses to conflict (van Eldik et al., 2020). Conflict frequency alone can obscure the difference between unresolved hostility and disagreement followed by repair. In a three-wave study that began in preschool, constructive conflict predicted gains in children's emotion-situation knowledge and social problem solving after adjustment for hostile conflict and earlier child characteristics (Davies et al., 2025). Whether improving repair changes child adjustment through those lessons remains an intervention question.

This pathway predicts that children's exposure to interparental behavior and their sense of safety can account for some association with later adjustment, even when each adult's caregiving is measured. It is most informative where exposure varies meaningfully, such as across episodes or caregiving transitions. Child reports of what they witnessed and independent ratings of caregiving help distinguish the pathways. A coefficient remaining after adjustment for a brief measure of warmth may still capture aspects of caregiving that were missed, so it provides limited evidence for a separate exposure pathway on its own.

Spillover into caregiving

The same disagreement can affect caregiving hours later when a parent becomes less responsive, when a planned handoff falls through, or when inconsistent expectations lead to

contradictory responses to a child. Research on divorced families links interparental conflict, parenting, and child adjustment in ways compatible with spillover (van Dijk et al., 2020, 2021). Meta-analytic mediation of observational associations leaves causal direction and timing unresolved. Chronic stress, financial insecurity, or a child's escalating symptoms can affect both coparenting and caregiving.

Spillover should be strongest when the next caregiving episode depends on cooperation. Return to the two families in the opening example. When a child becomes ill, both pairs may disagree about who should leave work. The proposed difference is whether the disagreement derails the handoff and disrupts the child's routine. The same disagreement may have less effect on a routine one adult can manage alone. Measuring the coordination demand before the disagreement makes that claim testable.

Four propositions

Preschool externalizing is the primary child outcome for P1 through P3 because responses to limits and routines are visible during the period when adults first establish shared caregiving. The immediate P2 outcome is caregiving disruption. School-entry internalizing is primary for P4 because the predicted route through emotional insecurity makes worry and withdrawal especially relevant when children face new social demands. These choices prioritize different mechanisms. Other ages, outcomes, and reporters should be declared secondary before analysis.

Proposition 1 Early dyadic information adds predictive value

Adult characteristics and observed interaction before shared caregiving will improve out-of-sample prediction of preschool externalizing beyond the characteristics of either adult alone and common baseline circumstances. The crucial increment is from observing the relationship

after measuring both adults. It should remain when both models receive identical postnatal caregiving measures. A stronger, secondary prediction is that the early adult and dyadic measures will outperform later caregiving measures when the two sets have comparable observation time and reliability. An increment confined to the second adult's characteristics favors a stable trait account. A caregiving advantage would count against the stronger ranking. Neither ranking estimates the effect of intervening.

Proposition 2 Unresolved conflict disrupts cooperative caregiving

Within a family, unresolved conflict will predict less consistent or responsive care at the next planned episode, especially when the routine requires coordination. Repeated disruption is expected to predict preschool externalizing. Acute strain could generate both conflict and disruption, and child distress could precede both. The proposed spillover is weakened by a precisely small forward association after accounting for measured earlier strain and child behavior, or by evidence that coordination demands do not modify that association.

Proposition 3 Joint difficulties can reduce compensatory care

Two coparents with persistent difficulty regulating negative affect before shared caregiving will have children with higher externalizing scores than an additive model predicts. This narrower measure complements P1's broad personality measures. It targets recurring difficulties in settling distress and regaining composure, which are relevant to whether one adult can sustain care when the other is struggling. The predicted excess should be smaller where reliable backup care is available. An additive pattern with adequate precision challenges amplification. A similar excess regardless of observed backup challenges the buffering explanation. Assortment can make joint exposure more frequent even if the child-outcome association is additive. Conversely, a strong interaction could have limited population relevance

where joint exposure is rare. The prevalence of the joint profile and its association with child outcomes answer separate questions.

Proposition 4 Early repair predicts internalizing at school entry

Repeated observations of repair before shared caregiving will improve prediction of child internalizing beyond conflict severity, relationship satisfaction, parenting intentions, and adult symptoms. Emotional security theory implies that recurrence after the transition and the child's exposure to unrepaired conflict should help explain this prospective association. A lack of incremental gain in independent ratings counts against the early marker. Weak postnatal continuity or exposure would separately undercut the proposed pathway.

Evidence needed to test the model

The first test asks what observing the pair adds. An established-pair cohort can compare predictions based on adult characteristics with predictions that also incorporate observed interaction before shared caregiving. A later comparison can give both models the same caregiving information and ask whether the earlier observations still add predictive value. These are comparisons of forecasts for the same families. They address the added value of measuring a dyadic process even when researchers have access to later caregiving data.

The two relational pathways require different observations. Testing spillover requires knowing what happened after a specific disagreement and whether a routine depended on both adults. Testing the emotional security pathway requires knowing whether early repair patterns recurred and what children later witnessed. The buffering proposition adds a separate question about whether backup care changes the association between joint difficulties and child adjustment. Within-family evidence also makes it possible to examine influence in the other

direction. Using five annual pairfam waves, Zemp et al. (2018) found that higher-than-usual child externalizing predicted later paternal withdrawal and coparenting conflict.

Benchmarks and a staged test

Van Eldik et al. (2020) reported correlations of .11 (95% CI [.07, .16]) for independently observed hostile conflict and externalizing and .18 (95% CI [.16, .20]) when hostility was reported by questionnaire. In a meta-analysis of 154 trials, Leijten et al. (2019) reported effects on disruptive behavior from $d = -0.21$ in universal prevention to $d = -0.69$ in treatment. These benchmarks concern conflict associations and intervention effects, respectively. Incremental prediction from observing a pair calls for its own benchmark. A one-percentage-point gain in held-out R^2 is an illustrative planning value. The calculation in the Appendix shows that validating a gain of this size can require thousands of families.

Existing pregnancy cohorts can first establish how much information the second adult adds. A coordinated multisite study could then add brief, reliable observations of interaction and independent child ratings to test the full prediction. Table 2 maps existing cohorts to the questions they can address. The Appendix specifies the analytical contrasts, sample-size assumptions, and interpretation of imprecise results.

Studying coparental formation while people are still deciding

Observing adults during pregnancy gives a measure before shared caregiving begins, often well after they have chosen each other. Couples who remain together and enroll may differ from couples who separate, decline participation, or never commit to sharing care. The range of conflict severity and repair may therefore be narrower in established pairs than among adults still deciding. A narrower range can diminish the gain from P4 in held-out prediction. Conditioning

on relationship continuation can also distort associations in either direction when continuation depends on both interaction and other factors linked to child outcomes.

Recruitment before adults have discussed parenthood would capture a broader range of possible arrangements. It would also require a longitudinal consortium able to recontact participants across relationship changes, long intervals before childbearing, and changes in who shares care. Some people would never enter shared caregiving during follow-up. Others would enter too late for the planned outcome assessment. Enrolling adults who are actively considering shared caregiving is a more tractable intermediate step, with a correspondingly narrower view of how arrangements form. At enrollment, measure early interaction in existing relationships, whether and when participants have discussed having children, expectations about sharing care, perceived options, and available support. Follow all participants across these transitions, including those who proceed with the initial partner, change plans, or never enter parenthood. Their decisions and transitions are outcomes in their own right. Child-adjustment analyses concern the subset that enters eligible shared caregiving and has reached the assessment window.

Compare predictor distributions across recruitment stages and report transitions and attrition. Validate within each stage for which outcomes are available. Weighting can address measured differences where the samples overlap. Estimates for arrangements absent from the data require new recruitment. In particular, a null increment in an established pregnancy cohort answers a question about that cohort and cannot settle the value of observations at the decision stage.

Existing cohorts can help specify an earlier study. Where relevant waves bracket a transition, pairfam can describe changes in relationships and reported conflict at annual intervals. The Future of Families and Child Wellbeing Study begins at birth and can track subsequent

changes in family relationships and caregiving arrangements. Those measures can inform what a consortium would need to follow and when. Direct study of prospective deliberation, coded repair, and event-level spillover would require additional observations (Future of Families and Child Wellbeing Study, n.d.; Huinink et al., 2011; Zemp et al., 2018).

Forming a family with a different genetic partner would generally change which child exists. Consequently, a comparison across genetic pairing arrangements concerns distributions of outcomes in a defined population. To ask how an intervention could help a particular child, study changes in coordination, support, or caregiving in existing families.

Genetic extension

Partner resemblance matters for interpreting family associations. Horwitz et al. (2023) documented positive resemblance across many traits, with stronger correlations for some educational and social characteristics than for many personality traits. Golovina et al. (2026) analyzed 1,271,242 cohabiting or married partnerships formed in Finland from 2000 through 2020. They found similarities in diagnoses recorded before cohabitation or marriage, including resemblance across disorders. Those similarities can reflect social sorting, opportunities for contact, access to diagnosis, or selection on correlated traits. Recorded diagnoses provide limited information about behavior in a caregiving relationship.

For harmonized genetic scores A and B, the variance of the midparent score is [Var(A) + Var(B) + 2 Cov(A, B)] / 4. Holding the parent-role margins fixed within strata, positive covariance increases this variance relative to independent pairing. Genetic assortment has already been documented (Robinson et al., 2017). The covariance or variance difference can describe the composition of a specific cohort. It supplies no independent test of the model's

relational predictions and does not measure the full variance of realized offspring scores, which also reflects segregation within each pair.

The extension helps distinguish relational pathways from two rival explanations for family associations: genetic transmission that covaries with the rearing environment and child characteristics that evoke changes in care. Passive gene–environment correlation occurs when characteristics that children inherit covary with environments their genetic relatives provide. Nontransmitted parental scores offer one way to study indirect parental associations. In a meta-analysis of educational outcomes, Wang et al. (2021) reported standardized indirect and direct genetic estimates of $\beta = .08$ (95% CI [.07, .09]) and $\beta = .17$ (95% CI [.13, .20]), respectively. Estimates vary across family designs (Demange et al., 2022; Kong et al., 2018). In a Norwegian extended-family study, indirect associations attenuated when wider family processes were taken into account (Nivard et al., 2024). These results inform sensitivity analyses for shared origins of child characteristics and family resources. They do not isolate a particular caregiving behavior or provide guidance about individual partners.

Children can also evoke changes in care. For example, Austerberry et al. (2024) reported some age-specific associations consistent with evocation in adoptive families and found no evidence for the proposed mediation of language or achievement through their parenting measures. Adoption and other genetically informative comparisons can help separate these processes, with attention to prenatal exposure and placement (Jami et al., 2021). These comparisons can test whether relational associations persist when genetic relatedness and the timing of child-evoked responses are better separated.

What coparental formation leaves open to change

Initial risk and later intervention

Coparental formation helps set the initial conditions under which children are likely to encounter conflict, repair, and reliable care. These are probabilistic starting conditions. Their consequences depend on what happens as responsibilities, strain, and available support change. Even if early pair measures predict child adjustment better than later caregiving measures in a given cohort, that comparison does not estimate the effect of improving care. An early measure can forecast exposure to a developmental process that remains responsive to intervention.

Parenting programs and coparenting support can change different parts of that process. Parenting programs can reduce child disruptive behavior (Leijten et al., 2019). Family Foundations provides an experimental example of support delivered after the pair has formed. An initial trial found improvements in coparenting support (Feinberg & Kan, 2008). In a later trial, program effects at 10 months postpartum varied with prenatal negative communication or depressive symptoms on selected outcomes, including observed coparenting withdrawal (Feinberg et al., 2016, Table 3). These findings show that support after pairing can improve family processes. The separate question posed by the UCFM is whether improvements are larger, or whether the association between early risk and later adjustment is weaker, for the specific joint difficulties described in P3. Those are distinct interaction tests, and the trial needs to measure the relevant early profile to assess either.

What evidence could inform guidance before shared caregiving

If early dyadic observations improve prediction in P1 and P4, the next step is to test whether observable repair and coordination account for that gain across settings. If those skills also prove teachable and persist as caregiving demands change, they could inform education for

people considering shared caregiving. Candidate topics include how two adults respond when plans fall through, how they come back to an unresolved disagreement, and how they agree on backup care. Research would need to establish whether teaching people to notice and discuss those behaviors improves decisions or preparation. A predictor of child adjustment alone cannot establish that guidance changes behavior, choices, or outcomes.

A trial of guidance could start with individuals and couples who are considering future shared caregiving. It could test whether a structured discussion improves understanding of each adult's expectations, available support, and options for sharing care. Longer follow-up would be needed to assess actual caregiving arrangements and child outcomes. If guidance affects whether or when participants enter parenthood, analyses of later child outcomes must account for that change in who enters the outcome population. Access to partners, family expectations, resources, and safety shape whether participants can act on any guidance. Guidance should avoid assigning individual responsibility for outcomes shaped by constrained choices and unequal access to support. The same skills may also be relevant to planning and support when a caregiving arrangement is already in place.

Limits

Cultural scope

Most evidence motivating the model comes from Europe and North America. In family-mediated or arranged unions, the adults, kin, and institutions may have different roles in choosing partners and organizing care. Evidence from India, for example, documents variation in the degree of individual and family involvement in marriage decisions (Allendorf & Pandian, 2016). Record who participated in arranging the union and who actually performs or backs up caregiving. Where institutions assign care or a kin network provides it, the relevant formation

process may be placement or the allocation of responsibilities. An assigned caregiver need not have a history of romantic interaction with another adult. If observed repair or compensatory care has a different meaning across settings, establish comparability before pooling estimates. In extended households the relevant unit for buffering may be a caregiving network.

Measurement validity

Observed discussions sample behavior under particular conditions. Adults may change how they disagree when they know they are being observed. Repair in a structured task may also differ from repair at home, particularly after shared caregiving begins. Repeated, participant-selected discussions broaden the sample of behavior. Where feasible, compare coded repair with brief diary reports from both adults after everyday disagreements. Agreement across settings is part of validating the measure. Independent ratings of child outcomes also reduce reliance on a single adult to report both family interaction and child adjustment.

Changing circumstances and safety

Economic shocks, housing instability, illness, and changes in support can alter a family's capacity to coordinate long after formation. Separation can reduce children's exposure to destructive interactions or create new coordination demands, depending on the circumstances. Where there is coercion or violence, safety and the feasibility of joint participation require separate assessment. Clinical diagnoses provide limited information about what an adult can do in a particular caregiving arrangement. The evidence reviewed here provides no basis for using genetic scores or diagnoses to screen prospective coparents or allocate caregiving rights.

Conclusion

The UCFM links who enters a caregiving arrangement to what children encounter once caregiving begins. Its main contribution is a prospective comparison: measure both adults and

their interaction before shared caregiving, then test what those measures add after observing later care. The composition of the pair shapes how often children encounter joint difficulties. Their developmental consequences can change as caregivers gain support, improve coordination, or respond differently to their child. Prediction can help locate when support might matter. Intervention studies can establish whether offering that support improves care and child adjustment.

A first study should preregister the two-adult prediction comparison in a pregnancy cohort with independent child ratings. A new prospective sample should then enroll adults earlier in their decisions about shared caregiving, add repeated coded interactions, and follow participants as their plans and relationships change. That sample can test whether the relationship measures add information and whether the predicted spillover is observable. If the second adult's measures improve prediction but observing the pair provides a precisely negligible increment, the added value lies in measuring both adults. If early interaction adds information and its postnatal continuity is observable, the next question is whether changing coordination or repair improves care. Existing theories of coparenting and emotional security supply the mechanisms for that intervention test. This sequence would show whether guidance before shared caregiving can rest on behaviors people can observe, discuss, and learn.

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Table 1

Predictions and Tests of the Upstream Coparental Formation Model

Proposition	Prediction	Rival account	Key design	Evidence against
P1 Early dyadic prediction Requires early measures of the formed pair	Early dyadic measures add predictive value for preschool externalizing beyond both adults' traits and later caregiving.	Stable adult traits explain the available gain.	Prospective dyadic cohort with nested prediction models and independent ratings.	The second adult adds information, but observed interaction adds a negligible increment.
P2 Conflict spillover Established coparenting mechanism	Unresolved conflict is followed by disrupted care, especially when a routine requires coordination.	Acute shared strain or child distress precedes both.	Within-family comparisons of time-stamped conflict and subsequent caregiving episodes.	The forward association or its moderation by coordination demand is negligible.
P3 Joint vulnerability Pairing sets joint exposure frequency	Joint affect regulation difficulties predict above-additive externalizing, with a smaller excess where backup care is reliable.	Additive risk or a joint effect unrelated to backup care.	Prospective dyadic measures, continuous interaction, and independent measures of backup.	The joint effect is additive, or the excess fails to vary with reliable backup.
P4 Early repair Extends emotional security theory upstream	Observed repair before shared care adds prediction of school-entry internalizing. Later exposure may account for part of the link.	General relationship distress explains both early repair and later symptoms.	Coded discussions before shared care, postnatal follow-up, and multiple child informants.	Repair adds negligible prediction, or poor repair shows little continuity into child exposure.

Note. P1, P3, and P4 can first be tested after the adults have paired. Whether the results apply earlier is a separate question about generalizability. The text specifies how to interpret precisely negligible effects.

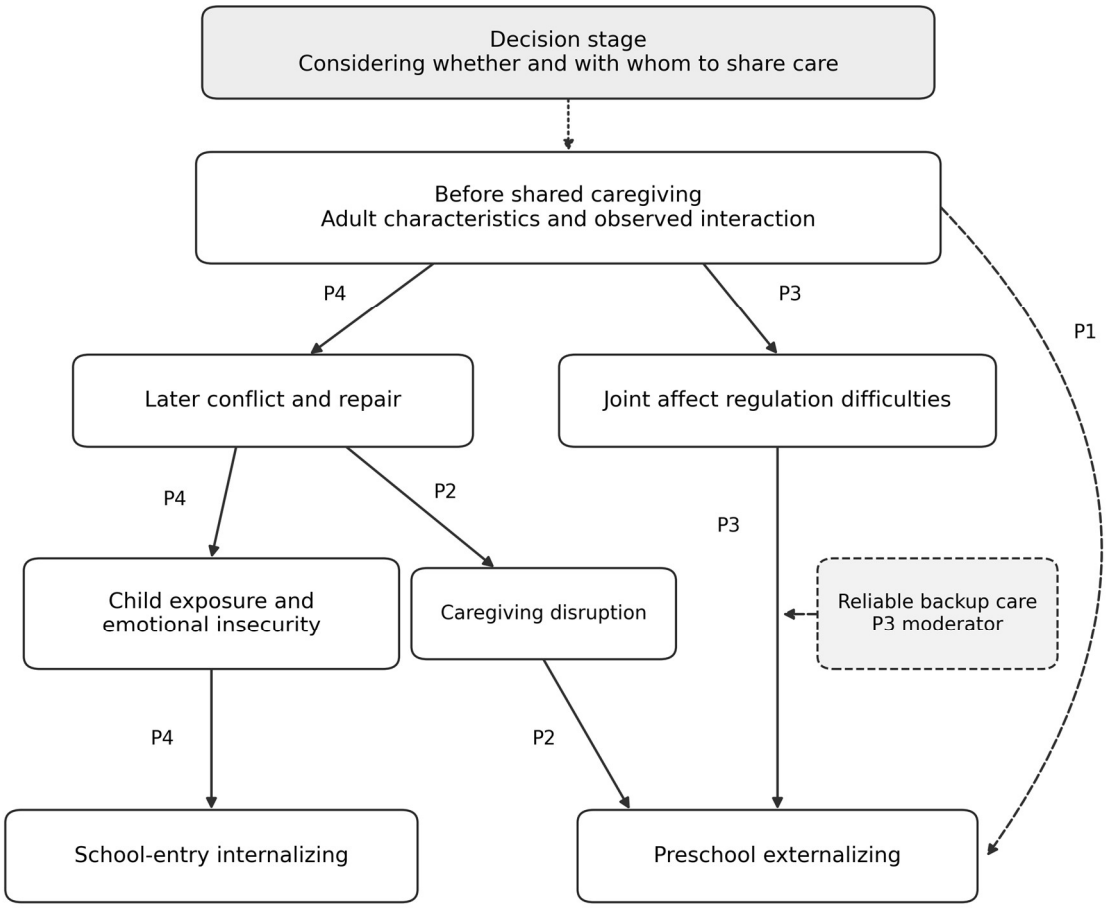
Table 2

Existing Cohorts and Their Uses

Cohort	What it offers	Model application and limit
German Family Panel (pairfam)	Repeated interviews with family members about partnerships, family transitions, and conflict (Huinink et al., 2011; Zemp et al., 2018).	P2 and P4 context. Annual reports can track broad patterns. Episode timing and coded repair require additional measures.
Future of Families and Child Wellbeing Study	U.S. interviews with mothers and fathers beginning at the child's birth, with follow-up on relationships, parenting, and caregiving arrangements (Future of Families and Child Wellbeing Study, n.d.).	P1 and P2 context. Useful for birth-to-childhood analyses with later caregiving measures. Baseline is at birth.
Generation R	Parental symptoms reported by mothers and fathers around 20 weeks of pregnancy, with child behavior ratings from both reporters at later waves (Xerxa et al., 2021).	Partial P1 and P3 tests. Tests whether two-adult symptom data add information. Symptoms only approximate affect regulation, and the published measures do not supply coded dyadic interaction or independent outcome ratings.
Norwegian Mother, Father and Child Cohort Study (MoBa)	Genotyped families and national school achievement data used in intergenerational analyses (Nivard et al., 2024).	Genetic extension. Can describe parental score covariance and estimate associations with school achievement.
Early Growth and Development Study	U.S. adoption cohort following birth parents, adoptive parents, and children (Leve et al., 2019).	Genetic extension and child evocation. Separates some genetic and rearing relationships. Placement and prenatal exposure still matter.

Figure 1

Proposed Pathways from Coparental Formation to Child Outcomes



Note. The decision-stage band marks an earlier opportunity to collect data. The dotted connector indicates timing for arrangements that proceed. The dashed P1 arrow marks incremental prediction. The dashed arrow from backup care points to the P3 association, which is expected to be weaker where reliable backup is available. P2 predicts greater spillover when routines require coordination. P4 also tests whether early repair adds prediction beyond other early measures.

Appendix

Methodological Blueprint and Analytical Strategy

Prospective prediction before and after shared caregiving

Recruit both adults before their first shared caregiving transition and retain them after separation or a change in caregiver. Record when the relationship began, whether the adults anticipated sharing care, and when that arrangement began. A pregnancy or preplacement cohort can test how measures taken before shared caregiving predict later adjustment among enrolled adults who go on to share care.

For P1, start with baseline economic conditions, adult symptoms, prior children, and site. Measure each adult's conscientiousness, emotional stability, prior caregiving experience, and affect regulation. Fit nested models adding one adult, the second adult, and then observed interaction. Reverse the order of the adult blocks as a sensitivity check. Use independent preschool externalizing ratings where feasible. Compare the models with and without observed early interaction on the same held-out families or sites. Repeat the comparison after adding the same caregiving measures to both models. Compare the early and postnatal blocks using comparable assessment intensity and reliability.

For P4, observe repeated discussions of ordinary topics selected by participants. Code acknowledgment, de-escalation, and return to cooperative problem solving. Rate initial conflict severity separately. Follow up with observations and reports of what the child witnessed and their emotional security. Estimate the contribution of early repair to school-entry internalizing with multiple informants. Treat persistence and exposure as secondary pathway analyses because they occur after the baseline predictor. Strong contemporaneous predictors can be poor forecasters of later change, as research on adult relationship quality illustrates (Joel et al., 2020).

Within family spillover and compensatory care

For P2, collect time-stamped reports from both adults, planned caregiving handoffs, and independently coded observations where feasible. Specify the lag between unresolved conflict and the next planned episode. Record coordination demands, child behavior, sleep disruption, and stress before that window. Compare forward and reverse sequences within families. This reduces confounding by stable family characteristics. Time-varying strain affected by earlier conflict still requires explicit assumptions. To test whether a change in coordination can affect care, randomize a defined coordination program in an eligible population and confirm that it changed the targeted process. A factorial trial assigning coordination support and a parenting program independently could test each program's added benefit and their interaction. Randomization estimates the effect of offering the assigned program. A claim about a particular mediator needs additional assumptions or a separate experimental contrast.

For P3, keep the two adults' affect regulation measures continuous and allow nonlinear main effects. Prespecify meaningful high and low values within the region where all four combinations have support. If H and L denote these values for each adult in fixed order, the excess on the additive outcome scale is (HH − HL) − (LH − LL), equivalently HH − HL − LH + LL. Each term is a model-estimated mean averaged over the same covariate distribution. Record reliable backup care from the other adult or a named third caregiver. Report overlap and role differences. A profile-by-backup association supports the proposed buffering pattern, while causal attribution requires a design that addresses why backup is available.

To test whether support changes the P3 pattern, measure both adults' affect regulation before randomizing families and prespecify the early risk profile and outcome scale. An intervention-by-risk interaction tests whether program benefit varies with that profile. To test

whether support attenuates the above-additive excess, estimate the contrast (HH − HL − LH + LL) in each assignment group and compare the two contrasts. Use the same high and low values and covariate distribution in both groups. Stratifying randomization by baseline risk can improve balance.

To quantify how often the joint profile occurs, report its observed prevalence and compare it with the prevalence expected under independent pairing within prespecified strata, retaining each adult role's marginal distribution. This descriptive comparison shows how the observed grouping of adults changes the frequency of joint exposure. Estimate the symptom interaction separately. A high prevalence of joint difficulties can coexist with an additive outcome association, while rare joint exposure limits population reach even when the interaction is large.

Validation sample size and uncertainty

A .01 increase in held-out R^2 is an illustrative planning value for P1 and P4. Its practical value depends on the burden and reliability of observing interactions. For P1, a paired comparison of squared prediction errors would need about 2,800 independently assessed validation families for 80% power at two-sided $\alpha = .05$ under a simple scenario. Assume a standardized continuous outcome, two fixed prediction functions, R^2 values of .10 and .11, and a baseline prediction error that is the sum of two independent, mean-zero normal components: the added prediction and the error remaining in the expanded model. The loss difference has mean .01 and variance $2(.01)^2 + 4(.01)(.89) = .0358$. The normal approximation gives $N \approx (1.96 + 0.84)^2(.0358)/(.01)^2$. With 20% unavailable validation outcomes, that scenario implies enrolling roughly 3,500 validation families. Model development requires a separate sample under this design.

The calculation concerns detecting an improvement over zero. To plan a study that also distinguishes a practically negligible gain from an inconclusive result, simulate the full analysis, including model fitting and tuning, measurement reliability, site clustering, recruitment-stage differences, and loss to follow-up. Resample at the family or site level as the design requires. For P2 and P3, use pilot data to set practical effect thresholds and simulate realistic lag structure and the frequencies of joint and discordant exposure. Prespecify the outcome windows, scales, missing-data analyses, and uncertainty intervals. An interval that spans both negligible and meaningful effects leaves the test inconclusive. An interval fully inside a prespecified negligible range supports a practically null interpretation.

Harmonize measures across sites, assess reliability, and hold out entire sites when testing transport. Cross-validation with overlapping training sets requires an uncertainty calculation that accounts for the overlap.

With baseline R^2 fixed at .10, a gain of .02 requires about 1,400 independent validation families. Under these assumptions the variance of the loss difference grows with the gain, so sample size falls roughly in inverse proportion to the gain.

Enrollment before shared caregiving

Divide the required number of usable family outcomes by the expected proportion entering eligible shared caregiving in time for assessment and by the expected outcome availability among eligible families. Account for separately enrolled adults who later form a dyad. Report partner changes, eligibility, and missing outcomes. Include participants without child outcomes in analyses of decisions and transitions.