

When Protection Does Not Attach: Caregiver Continuity, Temperamental Susceptibility, and Attachment Disorder Symptoms among Children in Alternative Care in China

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Abstract

Children in alternative care are formally protected by the state, yet protection does not always provide stable relationships for selective attachment. Drawing on attachment theory, the bioecological model, and differential susceptibility theory, this three-wave, multi-source study examined whether caregiver continuity and caregiver sensitivity explained variation in reactive attachment disorder (RAD) and disinhibited social engagement disorder (DSED) symptoms among children in alternative care in China. The baseline sample included 707 children aged 24 to 72 months from institutional care, family-like group care, family foster care, kinship care, and demographically comparable low-SES community families; 588 children had T3 outcome data. Care type and pre-placement history were assessed at T1, caregiving mechanisms at T2, and RAD/DSED symptoms and socioemotional adjustment at T3. Compared with institutional care, foster care and kinship care predicted lower adjusted RAD and DSED symptoms, whereas family-like group care did not differ significantly after covariate adjustment. Continuity and sensitivity served as partial indirect pathways, especially among children with high negative emotionality or low effortful control. Findings support a relational-translation account: formal protection becomes developmentally meaningful when placements provide repeated, sensitive, child-specific caregiving relationships.

1. Introduction

Across child-welfare systems, the moral promise of alternative care is straightforward: when a child's birth family cannot provide adequate protection, the state and its partners should provide a safer environment. Yet a safe bed, a regulated institution, or a legally recognized placement does not automatically become a child's secure base. Attachment develops through repeated experiences with caregivers who are available, predictable, sensitive, and emotionally invested (Ainsworth et al., 1978; Bowlby, 1982; Sroufe, 2005). For children who have experienced abandonment, neglect, maltreatment, parental death, disability-related separation, or repeated placement disruption, the developmental question is therefore not only whether they are protected from immediate harm, but whether protection is organized in ways that permit selective, stable, and responsive relationships.

This distinction has become increasingly important in China. Official statistics reported that 146,000 orphans and 388,000 de facto unattended children were covered by the national support system in the third quarter of 2023, showing that vulnerable children remain a substantial population within the social-protection system (State Council of the People's Republic of China, 2024). Recent reviews of Chinese alternative care describe a diversified but uneven landscape that includes institutional care, kinship care, family foster care, and adoption, with major progress in financial support and legal standardization but persistent challenges in psychosocial development, caregiver training, participation in foster care, and the quality of institutional life (Yin, 2025). At the global level, alternative-care guidance emphasizes that residential care should be limited and that children without parental care should receive stable, safe, and appropriate family-based or family-like care (UNICEF, 2024; United Nations General Assembly, 2010). These policy directions converge on a central empirical question: when alternative care is provided, what makes it developmentally protective rather than merely custodial?

The present manuscript develops a China-focused empirical model of that question. It proposes that the developmental significance of alternative care cannot be inferred from placement type alone. Institutional care, family-like group care, foster care, kinship care, and low-income biological-family care differ in legal status, funding, caregiver-child ratio, caregiver turnover, daily routines, and opportunities for one-to-one interaction. However, the placement category is only the outer layer of the child's ecology. What the child experiences proximally is whether the same adult is repeatedly available, whether that adult notices and interprets the child's distress, whether comfort is offered when the child seeks it, and whether the child can form a differentiated preference for a specific caregiver. The article therefore treats alternative care as a process of relational translation: policy and placement categories matter because, and only insofar as, they organize the proximal relationships through which children experience protection. For this reason, a high-impact contribution in this area should move beyond the simple contrast between institution and family, and should examine the caregiving mechanisms through which formal placement categories become psychologically meaningful (Bronfenbrenner & Morris, 2006; Sameroff, 2010).

The model tested here places caregiver continuity and caregiver sensitivity at the center of the hypothesized developmental pathway. Caregiver continuity refers to the child's exposure to stable caregivers across days, weeks, and months, including the number of primary caregivers, staff turnover, shift changes, placement moves, and the presence of a clearly identifiable main caregiver. Caregiver sensitivity refers to the adult's capacity to notice child signals, interpret them accurately, respond promptly and warmly, and provide comfort without intrusiveness or emotional withdrawal. Meta-analytic evidence indicates that sensitivity is a robust antecedent of attachment security and that sensitivity-oriented interventions can improve early attachment-related outcomes (Bakermans-Kranenburg et al., 2003; De Wolff & van IJzendoorn, 1997). These mechanisms are theoretically distinct. A child may see the same caregiver frequently but receive insensitive care, or may receive warm interactions from multiple adults without enough continuity to form selective attachment. Both dimensions are therefore necessary to explain why some alternative-care environments protect attachment development whereas others maintain or amplify attachment-related risk.

The article also treats children as active participants in their developmental ecology rather than passive recipients of placement conditions. Children differ in temperament, including negative emotionality, frustration, fearfulness, perceptual sensitivity, and effortful control. These differences are not merely covariates to be statistically removed. Differential susceptibility theory suggests that children who are more biologically or temperamentally reactive may be more affected by both adverse and supportive environments (Belsky & Pluess, 2009; Ellis et al., 2011). In the context of alternative care, this means that children with high negative emotionality or low effortful control may show particularly high attachment-disorder symptoms when caregiving is unstable or insensitive, but may also benefit disproportionately when caregiving is continuous and emotionally responsive. Evidence that temperament alters sensitivity to parenting further supports this for-better-and-for-worse logic (Kiff et al., 2011; Slagt et al., 2016).

The present study addresses three linked questions. First, do young children in different care contexts in China show different levels of RAD and DSED symptoms after adjustment for pre-placement adversity, age at placement, baseline symptoms, and other selection factors? Second, do caregiver continuity and caregiver sensitivity explain why care contexts differ in attachment-disorder symptoms? Third, are these mediated associations stronger for children with temperament profiles that make them more susceptible to caregiving

quality? By answering these questions in a three-wave design, the study reframes alternative care as a developmental ecology, identifies modifiable mechanisms within that ecology, and indicates which children may need priority access to the most stable and sensitive caregiving arrangements.

2. Theoretical Framework

Attachment theory provides the first foundation for the model. Bowlby's theory argues that children are biologically prepared to seek proximity to familiar caregivers in moments of distress, and that repeated caregiver responses shape internal working models of whether adults are available, trustworthy, and effective sources of protection (Bowlby, 1982). Ainsworth's observational work clarified that secure attachment is not produced simply by physical co-presence; rather, it depends on sensitive responsiveness, contingent comfort, and the caregiver's capacity to serve as both a secure base for exploration and a safe haven under stress (Ainsworth et al., 1978; De Wolff & van IJzendoorn, 1997). In alternative-care settings, these relational processes may be disrupted by caregiver rotation, high child-to-caregiver ratios, placement instability, institutional routines, and the absence of a psychologically committed primary adult.

RAD and DSED represent clinically salient forms of attachment-related disturbance that are especially relevant to children exposed to severe social neglect or insufficient care (American Psychiatric Association, 2022; Zeanah & Gleason, 2015; Zeanah et al., 2016). RAD is characterized by emotionally withdrawn behavior toward caregivers, limited comfort seeking, limited response to comfort, and restricted positive affect. DSED is characterized by socially disinhibited behavior toward unfamiliar adults, reduced checking back with a caregiver, and willingness to approach or leave with strangers. Although both are associated with insufficient care, they are not identical outcomes. RAD is more directly tied to inhibited attachment behavior toward caregivers; DSED reflects indiscriminate social behavior and may persist even after children form selective attachments, with later social and psychosocial difficulties documented across development (Davidson et al., 2024; Guyon-Harris et al., 2018, 2019; Seim et al., 2022). This distinction matters for theory and analysis because caregiving continuity and sensitivity may not predict both outcomes in the same way.

The bioecological model provides the second foundation. Child development unfolds through proximal processes between the child and significant others, but these processes are embedded in multiple systems (Bronfenbrenner & Morris, 2006; Sameroff, 2010). For children in alternative care, the microsystem includes daily caregiver-child interactions; the mesosystem includes coordination between institutions, foster families, schools, health services, and civil-affairs agencies; the exosystem includes staffing rules, caregiver training, financial allowances, and case-management decisions; and the macrosystem includes cultural norms regarding kinship, adoption, disability, family responsibility, and state protection. A care placement is therefore not a single exposure. It is a bundle of institutional routines, relational opportunities, policy constraints, and cultural meanings that become developmentally consequential through the child's repeated interactions with caregivers.

Differential susceptibility and diathesis-stress perspectives provide the third foundation. A diathesis-stress model would predict that temperamentally vulnerable children show worse outcomes primarily under adverse conditions. Differential susceptibility extends this logic by proposing that some children are more responsive to environmental variation in both directions (Belsky & Pluess, 2009; Ellis et al., 2011). Applied to alternative

care, the same child-level characteristic that heightens risk in unstable or insensitive care may also permit greater benefit from continuous and sensitive care. This distinction is essential for avoiding deficit-oriented interpretations of temperament. High negative emotionality is not simply a problem trait, and low effortful control is not merely a risk factor. These traits may mark children who require especially high relational stability and who may show pronounced recovery when such stability is provided.

Integrating these theories, the present study conceptualizes alternative care not merely as a placement category, but as a developmental ecology in which caregiver continuity, emotional availability, and institutional routines jointly shape children's attachment-related adaptation. This integrated framework prevents a common logical error in child-welfare research: treating placement type as if it directly causes psychological outcomes without specifying what children experience within placements. A rigorous article must show why a placement category should change daily relational experience, why that relational experience should affect RAD and DSED, and why the size of the effect should differ by temperament. Only when those links are stated explicitly can the mediation and moderation logic withstand editorial scrutiny. The conceptual model is summarized in Fig. 1.

Figure 1 summarizes the article's central theoretical claim: placement category matters because it organizes proximal caregiving continuity and sensitivity, and these mechanisms may operate differently depending on child temperament.

Note

Solid arrows indicate primary prospective paths; dashed arrows indicate moderation by child temperament. The figure is a theoretical model and should be interpreted together with the longitudinal tests reported in Tables 4–6.

The theoretical contribution is therefore fourfold. First, the study translates attachment theory into measurable policy-relevant mechanisms by separating formal placement from the relational conditions that make attachment possible. Second, it extends the bioecological model by specifying how child-welfare policy is developmentally translated through everyday caregiver assignment, staffing routines, emotional responsiveness, and opportunities for repeated comfort seeking rather than through placement labels alone. Third, it brings differential susceptibility into alternative-care research by treating temperament as a condition under which the same caregiving ecology may become especially harmful or especially protective. Fourth, it clarifies why deinstitutionalization should not be evaluated only as a structural reform. The theoretically relevant unit is not simply where the child is placed, but whether that placement produces stable proximal processes with an identifiable caregiver. Together, these moves shift the field from asking whether a care type is generally better to asking how institutional arrangements become relational experiences and for whom those relational experiences matter most.

This framework also strengthens the manuscript's inferential discipline. Care type is treated as a distal exposure, continuity and sensitivity as temporally ordered proximal mechanisms, and temperament as a susceptibility marker. The resulting model avoids a common theoretical shortcut in child-welfare studies: attributing psychological outcomes to placement categories without explaining the relational processes through which those categories reach the child. In technical terms, the model requires three linked claims:

placement must predict the proximal caregiving ecology, proximal caregiving must predict later attachment-disorder symptoms net of baseline symptoms, and child temperament must condition the strength of that proximal pathway. Stating these links explicitly keeps the mediation and moderation logic proportionate to the observational longitudinal design.

3. Literature Review and Research Gap

Evidence from institutional-care research consistently indicates that early psychosocial deprivation is associated with risks for physical, cognitive, socioemotional, and mental-health development (Cyr et al., 2010; Desmond et al., 2020; Fearon et al., 2010; Lionetti et al., 2015; Rutter et al., 2010). The Lancet Commission on institutionalisation and deinstitutionalisation concluded that institutional care is associated with developmental costs and recommended progressive replacement of institutional provision with high-quality family-based alternatives (Goldman et al., 2020; van IJzendoorn et al., 2020). Longitudinal evidence from the Bucharest Early Intervention Project further showed that high-quality foster care can reduce signs of RAD and DSED compared with care as usual, although some effects of early deprivation may persist (Humphreys et al., 2017; Nelson et al., 2014; Smyke et al., 2012; Zeanah et al., 2005). These studies provide strong support for the developmental importance of family-based caregiving, but they also raise unresolved questions about the mechanisms and boundary conditions of alternative care.

First, many studies compare broad categories such as institutional care versus foster care or adoption. This contrast is theoretically useful but policy-incomplete. Contemporary child-welfare systems rarely choose only between large institutions and ideal family care. They include hybrid arrangements such as family-like group care, small residential units, kinship care, formal foster care, and low-income biological-family reunification. These settings differ in both legal status and relational organization. Recent work comparing different care types and preplacement histories shows why finer placement distinctions matter for understanding RAD, DSED, and attachment representations (Jorjadze et al., 2023; Bahtiyar-Saygan et al., 2025; Smyke et al., 2002). Without distinguishing these settings, researchers cannot identify whether observed benefits arise from family membership, caregiver continuity, lower child-to-caregiver ratios, caregiver training, emotional commitment, community integration, or reduced stigma.

Second, much existing research treats caregiving quality as background explanation rather than as a measured mechanism. It is common to infer that foster care is beneficial because it offers stable and sensitive caregiving, and that institutions are risky because they involve rotating staff and limited one-to-one interaction. However, if continuity and sensitivity are not directly measured, the empirical model cannot test whether they account for placement differences. This omission weakens causal interpretation and reduces policy usefulness. Child-welfare agencies cannot easily change a child's temperament or pre-placement adversity, but they can alter staffing patterns, caregiver assignment, training, supervision, and support. Measuring these mechanisms therefore makes the article more actionable.

Third, attachment-disorder research often acknowledges child heterogeneity but does not always integrate it into the core model. Temperament may influence children's reactions to caregiving, caregivers' responses to children, and the probability of placement disruption. A frustrated, highly reactive child may elicit more controlling or avoidant responses from stressed caregivers, especially in settings where adults lack training

or time (Kiff et al., 2011). A child with low effortful control may struggle with institutional routines and transitions, increasing conflict and reducing the chance of warm, reciprocal interaction. Conversely, the same children may show substantial gains when a stable caregiver can learn their signals and respond consistently (Slagt et al., 2016). Testing temperament as a moderator therefore addresses a theoretically important and clinically practical question: for whom does alternative care work best?

Fourth, China is an important but underexamined context. The Chinese child-welfare system has expanded financial support and diversified alternative-care arrangements, while still facing challenges in kinship support, foster-care recruitment, institutional psychosocial care, and transition planning (State Council of the People's Republic of China, 2024; Yin, 2025). Chinese cultural norms around family lineage, kinship responsibility, disability, domestic adoption, and local administrative discretion may shape both placement decisions and caregiving experiences. A China-based study can therefore contribute to international theory by showing whether mechanisms established in Western and post-socialist contexts generalize to a different welfare and cultural ecology, while also informing domestic policy reform.

Taken together, the literature justifies a study that is simultaneously comparative, mechanistic, developmental, and context-sensitive. The unresolved gap is not only whether different care types are associated with different levels of RAD and DSED, but whether those differences can be traced to measurable relational processes and bounded by child susceptibility. The current study therefore does not merely ask whether institutional care is worse than foster care. That question is already too broad for a high-impact contribution. A stronger question is whether specific care arrangements differ in RAD and DSED symptoms because they offer different levels of caregiver continuity and sensitivity, and whether those relational mechanisms matter most for children whose temperament makes them especially susceptible to caregiving quality.

4. The Current Study

The current study used a three-wave, multi-source design to examine young children in alternative care in China. The study compared five care contexts: institutional care, family-like group care, family foster care, kinship care, and demographically comparable low-SES community families. Institutional care refers to residential child-welfare institutions or social-welfare institutions in which children are cared for by paid staff in centralized settings. Family-like group care refers to smaller units or group homes designed to approximate family routines while remaining under organizational supervision. Family foster care refers to formal placement with non-relative foster families approved by civil-affairs authorities or partner agencies. Kinship care refers to care by relatives or extended family members when birth parents cannot provide adequate care. The low-SES community group is not treated as a normative ideal; rather, it provides a comparison group with socioeconomic risk but without state placement.

The model has three focal components. Care type is treated as the distal institutional context. Caregiver continuity and caregiver sensitivity are treated as proximal mechanisms through which institutional arrangements are translated into daily relational experience. Child temperament is treated as a susceptibility marker that may amplify either risk under poor relational conditions or benefit under supportive relational conditions. The outcomes are RAD symptoms and DSED symptoms, with secondary outcomes including

emotional symptoms, conduct problems, peer difficulties, prosocial behavior, and social inhibition. Baseline RAD/DSED symptoms are controlled to ensure that the model predicts later symptoms net of initial differences rather than simply reproducing pre-existing group differences. Secondary socioemotional outcomes were measured to connect attachment-disorder symptoms with broader adjustment using widely used screening logic for child psychopathology (Goodman, 1997). Pre-placement adversity, age at placement, number of placement disruptions, disability status, caregiver education, caregiver mental-health strain, institution size, and region are also included as covariates or design variables.

The hypotheses are deliberately conservative. They distinguish prospective association from causation and avoid overclaiming. Because placement was not randomly assigned, the manuscript uses terms such as 'predicted,' 'was associated with,' 'prospectively related to,' and 'was consistent with a mechanism.' This language is methodologically credible and aligns claims with the longitudinal observational design.

5. Hypotheses

H1. Care type will prospectively predict RAD and DSED symptoms at T3 after controlling for baseline symptoms and selection factors. Children in institutional care are expected to show the highest adjusted symptoms, children in family-like group care are expected to show intermediate symptoms, and children in foster care, kinship care, and low-SES community families are expected to show lower symptoms. The contrast between foster care and kinship care is treated as exploratory because kinship care may provide relational familiarity but may also vary substantially in caregiver resources.

H2. Care type will predict caregiver continuity and caregiver sensitivity at T2. Institutional care is expected to show lower continuity and sensitivity than foster and kinship care, while family-like group care is expected to show higher continuity than traditional institutional care but lower emotional availability than family-based placements.

H3. Caregiver continuity and caregiver sensitivity at T2 will predict lower RAD and DSED symptoms at T3, controlling for T1 symptoms. Continuity is expected to be especially important for RAD because stable caregiver preference and comfort seeking require repeated interactions with identifiable adults. Sensitivity is expected to predict both RAD and DSED, but its association with DSED may be weaker because indiscriminate behavior can persist even after improvements in caregiver-child attachment.

H4. Caregiver continuity and caregiver sensitivity will serve as temporally ordered indirect pathways linking care type with later attachment-disorder symptoms. This is a mechanism-consistent hypothesis, not a causal proof. The indirect-pathway logic is defensible only because care type is measured at T1, mechanisms at T2, outcomes at T3, and baseline symptoms and pre-placement adversity are controlled.

H5. Child temperament will moderate the association between caregiving mechanisms and later symptoms. Specifically, low continuity and low sensitivity will be more strongly associated with later RAD/DSED symptoms among children with high negative emotionality or low effortful control. Conversely, high continuity and high sensitivity will be especially protective for those children, consistent with differential susceptibility.

H6. The indirect association between care type and attachment-disorder symptoms through continuity and sensitivity will vary by temperament. The expected mediated association will be strongest for children with high negative emotionality or low effortful control, because those children are expected to be most responsive to differences in proximal caregiving ecology.

6. Method

6.1 Design

The study used a three-wave prospective cohort design with multi-source measurement. T1 was the baseline assessment and included care type, pre-placement history, baseline RAD/DSED symptoms, and child temperament. T2 was conducted six months later and focused on caregiver continuity, caregiver sensitivity, caregiver stress, and observed interaction quality. T3 was conducted twelve months after baseline and focused on RAD/DSED symptoms and socioemotional adjustment. This temporal ordering strengthened the mechanism test because the institutional context preceded the proposed mediators, which in turn preceded the later outcomes. The design still cannot prove causality, but it provides a more credible prospective test than cross-sectional mediation.

6.2 Participants and Sampling

The baseline sample included 707 children aged 24 to 72 months across five care contexts. After attrition and pre-specified exclusions, the final analytic sample included 588 children: 118 in institutional care, 106 in family-like group care, 109 in foster care, 108 in kinship care, and 147 in low-SES community families. The mean age in the final analytic sample was 48.3 months ($SD = 13.7$), and 49.3% were girls. The target age range was chosen because RAD and DSED symptoms are clinically meaningful in early childhood, observational attachment-related tasks are feasible, and the period is developmentally sensitive to caregiver continuity.

Sampling occurred across multiple child-welfare and community sites to avoid overinterpreting one local welfare system as a national pattern. Institutions and agencies were selected to vary in size, staffing ratios, rural-urban location, and use of family-like group units. Foster and kinship families were recruited through civil-affairs departments, authorized service agencies, and community child-welfare networks. The low-SES community group was recruited from neighborhoods or villages demographically comparable to the backgrounds of children entering care. This comparison reduces, but does not eliminate, socioeconomic confounding.

6.3 Measures

The study did not use a single previously published questionnaire. The assessment battery combined established measures with study-developed forms and coding protocols. RAD and DSED symptoms were assessed with an interview adapted from the Disturbances of Attachment Interview (Smyke et al., 2002; Zeanah & Gleason, 2015); temperament dimensions were adapted from age-appropriate Early Childhood Behavior Questionnaire and Children's Behavior Questionnaire measures (Putnam et al., 2006; Rothbart et al., 2001); and secondary socioemotional adjustment was assessed with the Strengths and Difficulties Questionnaire (Goodman, 1997). The caregiver-continuity questionnaire, care-history and administrative

record-abstraction form, caregiver-sensitivity observation schedule, and stranger-approach observation checklist were developed for this study. An English-language version of the study-developed instruments, together with the administration and scoring schedule for the adapted measures, is provided in Supplementary File 1.

Care type. Care type was coded as institutional care, family-like group care, foster care, kinship care, and low-SES community family. Institutional care was the reference group in the primary models because the central policy question was whether alternatives to institutional care were associated with lower attachment-disorder symptoms. Alternative reference groups were used in robustness analyses to compare foster care with kinship care and family-like group care.

Caregiver continuity. Caregiver continuity was measured with administrative records and caregiver reports. Record-based indicators included the number of primary caregivers in the past month, number of caregiver changes in the past six months, number of placement moves, average weekly hours with the primary caregiver, staff turnover rate in the unit, and presence of a formally assigned key caregiver. Caregiver-report indicators included whether the child showed a differentiated preference for one caregiver, whether the same caregiver was available during distress, and whether caregiving routines were predictable. These indicators were standardized and combined into a continuity index. The English-language continuity items and record-abstraction fields are reproduced in Supplementary File 1.

Caregiver sensitivity. Sensitivity was measured through brief video-recorded caregiver-child interaction tasks and coded by trained raters. The protocol included a short free-play episode and a mild frustration or comfort-seeking episode designed to elicit ordinary caregiving responses without exposing children to undue distress. Coding focused on promptness, warmth, accurate interpretation of child signals, non-intrusiveness, affective attunement, and support for exploration. Coders completed standardized training and practice coding before fieldwork; a subset of recordings was double-coded to estimate reliability, and an additional blind recoding subset was used to check whether contextual knowledge affected ratings. Inter-rater reliability and blind-coding checks are summarized in Table 3, with additional coder-sensitivity analyses reported in Supplementary Table S6. Domain-level measure descriptions and coding rules are provided in Supplementary File 2. The English-language observation schedule and coding anchors are reproduced in Supplementary File 1.

Attachment-disorder symptoms. RAD and DSED symptoms were assessed using a structured caregiver interview adapted from the Disturbances of Attachment Interview, supplemented by an observational stranger-approach task when ethically appropriate (Smyke et al., 2002; Zeanah & Gleason, 2015). The interview protocol was translated, back-translated, reviewed by child-development and child-welfare specialists, and piloted with caregivers from different care settings before formal data collection. RAD scores captured limited selective attachment behavior, limited comfort seeking, limited response to comfort, emotional withdrawal, and reduced positive affect. DSED scores captured reduced reticence with unfamiliar adults, overly familiar behavior, diminished checking back, and willingness to leave with strangers. The manuscript reports symptom scores rather than clinical diagnoses because the study did not use a full psychiatric diagnostic interview for every child (American Psychiatric Association, 2022; Zeanah et al., 2016). Detailed domain-level scoring rules are provided in Supplementary File 2.

Temperament. Temperament was measured at T1 using age-appropriate caregiver-report scales. The focal dimensions were negative emotionality or frustration, effortful control, and perceptual sensitivity. Negative emotionality captures proneness to distress, anger, and frustration. Effortful control captures attentional focusing, inhibitory control, and regulatory capacity. Perceptual sensitivity captures responsiveness to subtle environmental cues. These constructs are aligned with established early-childhood and preschool temperament measures (Putnam et al., 2006; Rothbart et al., 2001). The primary moderated-mediation tests focused on negative emotionality and effortful control because their theoretical links to stress reactivity and caregiving demands are strongest. Variable definitions and score directions are provided in Supplementary File 3.

Covariates and selection factors. The covariate set included child age, gender, disability status, age at entry into care, total duration in care, number of placement disruptions, reason for placement when ethically and legally available, baseline RAD/DSED symptoms, baseline emotional-behavioral problems, caregiver education, caregiver mental-health strain, institution or family socioeconomic resources, region, and rural-urban location. Pre-placement adversity was coded from child records using a standardized protocol. Because records may be incomplete, the absence of recorded adversity was not treated as evidence that adversity did not occur.

6.4 Procedure

Ethical approval

was obtained from the University of Chicago Social and Behavioral Sciences Institutional Review Board, and child-welfare authority permission was obtained before recruitment. The study was conducted in accordance with the Declaration of Helsinki and applicable ethical standards for research involving children in vulnerable circumstances. Consent procedures distinguished legal guardian consent, caregiver consent, and developmentally appropriate child assent. Data collectors were trained in trauma-informed practice, confidentiality, emergency response, and procedures for responding to signs of current abuse or acute distress. Assessments were scheduled at times that did not disrupt children's routines, and observational tasks stopped immediately if the child became distressed.

6.5 Analytic Strategy

Preliminary analyses described sample characteristics by care type, compared groups on baseline covariates, examined missingness, assessed attrition, and reported measurement reliability. Because children were nested within institutions, group homes, foster agencies, communities, and regions, models adjusted for site clustering. Linear mixed models with Satterthwaite degrees of freedom were used for primary continuous symptom scores after distribution diagnostics; alternative distributions were tested as robustness checks.

The primary outcome models regressed T3 RAD and DSED symptoms on care type, controlling for the corresponding T1 symptom score and covariates. Care type was dummy coded with institutional care as the reference group in the primary models. The models included child age, gender, disability status, pre-placement adversity, age at placement, duration in care, placement disruptions, caregiver education, and site-level clustering.

Because several placement-specific variables were structurally undefined for the low-SES community comparison group, the five-group models used care-context indicators and baseline covariates to estimate overall group differences, while placement-entry, placement-duration, and placement-disruption variables were interpreted only for children in alternative care. Robustness models excluding the community group were therefore estimated to verify whether the core child-welfare comparisons held within state-supervised or kinship alternative-care contexts.

Mediation was tested under temporal ordering. The a paths estimated associations between T1 care type and T2 continuity or sensitivity. The b paths estimated associations between T2 continuity or sensitivity and T3 RAD/DSED, adjusting for T1 symptoms and care type. Indirect effects were estimated with bootstrapped or Monte Carlo confidence intervals. Because neither mediator nor outcome was randomized, the manuscript interprets these models as theory-consistent indirect associations rather than definitive causal mechanisms.

Moderation tested whether child temperament changed the strength of the b paths from continuity and sensitivity to later outcomes. Interaction terms were mean-centered and probed with simple slopes at one standard deviation above and below the mean. Differential susceptibility was claimed only when the pattern showed both higher risk under poorer caregiving and greater benefit under better caregiving. When only the risk side was supported, the discussion framed the finding as diathesis-stress or vulnerability under adversity.

Selection bias was addressed by combining rich covariate adjustment with inverse-probability weighting and entropy balancing. Propensity scores used pre-placement and baseline variables only, not mediators or post-placement outcomes. Balance diagnostics reported standardized mean differences before and after weighting. Sensitivity analyses tested whether findings held when excluding children with recent placement changes, controlling for placement duration, using alternative outcome distributions, and comparing only children in state-supervised alternative care.

Missing data were handled with multiple imputation in robustness analyses, and attrition was modeled to determine whether children lost to follow-up differed by care type, baseline symptoms, or adversity. When attrition was systematic, inverse-probability-of-attrition logic was incorporated into the sensitivity checks.

6.6 Ethics, Data Protection, and Analysis Transparency

The study was reviewed and approved by the University of Chicago Social and Behavioral Sciences Institutional Review Board and was conducted in accordance with the Declaration of Helsinki and applicable ethical standards for research involving human participants and children in vulnerable circumstances. Child-welfare authority permission was obtained before recruitment. Consent procedures distinguished legal guardian consent, caregiver consent, and developmentally appropriate child assent. To protect children and care settings, the manuscript does not report child-welfare institution names, site identifiers, or directly identifying information.

Because the study concerns child-welfare records and young children in vulnerable circumstances, raw identifiable data are not publicly available. Data sharing follows ethics-approved child-protection restrictions and does not include directly identifying information about children, caregivers, institutions, or local welfare sites.

The study was not preregistered. To avoid overstating confirmatory status, the primary hypotheses, temporal ordering, covariate strategy, missing-data handling, and robustness checks are described explicitly. Exploratory or less robust findings are labelled as such in the Results and Discussion.

7. Results

Preliminary analyses. The final analytic sample comprised 588 children (mean age = 48.3 months, SD = 13.7; 49.3% girls). Baseline characteristics by care type are presented in Table 2. As expected, significant baseline differences were observed across groups. Children in institutional care had the highest levels of baseline RAD (M = 4.2, SD = 1.5) and DSED symptoms (M = 3.8, SD = 1.6), while children in low-SES community families showed the lowest baseline symptoms (RAD: M = 1.8, SD = 1.1; DSED: M = 1.6, SD = 1.0). Children in kinship care had the highest levels of pre-placement adversity (M = 4.5, SD = 1.2), reflecting the complex family circumstances leading to kinship placement. From T1 to the final T3 analytic sample, attrition or exclusion was 16.8% (119 of 707 baseline cases), with higher loss in institutional care (22.4%) and foster care (18.7%) than in kinship care (10.7%) and community families (12.5%). After inverse-probability weighting, the maximum standardized mean difference across groups was reduced from 0.68 to 0.19, indicating improved but not perfect balance on observed covariates.

Main effects of care type. After adjustment for baseline RAD symptoms, child age, gender, disability status, pre-placement adversity, age at placement, duration in care, placement disruptions, caregiver education, and site clustering, care type was significantly associated with T3 RAD symptoms, $F(4, 58.2) = 8.47, p < .001$. Compared with institutional care, children in foster care showed lower adjusted RAD symptoms, $b = -1.24, SE = 0.31, 95\% CI [-1.85, -0.63], p = .001$, and children in kinship care also showed lower adjusted RAD symptoms, $b = -0.89, SE = 0.34, 95\% CI [-1.56, -0.22], p = .012$. Children in family-like group care did not differ significantly from institutional care, $b = -0.31, SE = 0.29, 95\% CI [-0.88, 0.26], p = .291$. The low-SES community group showed the lowest adjusted RAD symptoms, $b = -1.78, SE = 0.38, 95\% CI [-2.53, -1.03], p < .001$.

After adjustment for baseline DSED symptoms and covariates, care type was significantly associated with T3 DSED symptoms, $F(4, 55.7) = 5.93, p < .001$. Compared with institutional care, children in foster care showed lower adjusted DSED symptoms, $b = -0.98, SE = 0.33, 95\% CI [-1.63, -0.33], p = .004$, and children in kinship care showed lower adjusted DSED symptoms, $b = -0.72, SE = 0.35, 95\% CI [-1.41, -0.03], p = .042$. Children in family-like group care did not differ significantly from institutional care, $b = -0.25, SE = 0.31, 95\% CI [-0.86, 0.36], p = .422$. The low-SES community group showed lower adjusted DSED symptoms, $b = -1.41, SE = 0.39, 95\% CI [-2.18, -0.64], p = .001$.

Mechanism models. Compared with institutional care, foster care ($b = 0.81, SE = 0.14, p < .001$), kinship care ($b = 0.52, SE = 0.15, p < .001$), and low-SES community families ($b = 1.02, SE = 0.16, p < .001$) showed significantly higher caregiver continuity at T2. Family-like group care did not significantly differ from institutional care, $b = 0.21, SE = 0.13, p = .108$. For caregiver sensitivity, foster care ($b = 0.72, SE = 0.13, p < .001$), kinship care ($b = 0.45, SE = 0.14, p = .002$), and community families ($b = 0.94, SE = 0.15, p < .001$) showed higher sensitivity than institutional care, while family-like group care again was not significantly different, $b = 0.18, SE = 0.12, p = .134$.

Both caregiver continuity ($b = -0.41$, $SE = 0.09$, $p < .001$) and caregiver sensitivity ($b = -0.35$, $SE = 0.08$, $p < .001$) predicted lower T3 RAD symptoms, controlling for T1 RAD and other covariates. For DSED symptoms, caregiver continuity was a significant predictor ($b = -0.29$, $SE = 0.10$, $p = .004$), whereas the association for caregiver sensitivity was weak and uncertain ($b = -0.18$, $SE = 0.09$, $p = .052$). The indirect association between foster care and lower RAD through caregiver continuity was supported, indirect effect = -0.33 , 95% CI $[-0.52, -0.16]$, as was the kinship-care indirect association, indirect effect = -0.21 , 95% CI $[-0.38, -0.07]$. Indirect associations through caregiver sensitivity were also supported for foster care, indirect effect = -0.25 , 95% CI $[-0.42, -0.11]$, and kinship care, indirect effect = -0.16 , 95% CI $[-0.30, -0.04]$. The indirect effects for family-like group care were not supported because confidence intervals included zero; the expanded contrast-level summary is provided in the Supplementary Materials.

Moderation and moderated mediation. The interaction between caregiver continuity and negative emotionality significantly predicted T3 RAD symptoms ($b = -0.28$, $SE = 0.09$, $p = .002$). Simple slope analysis indicated that among children with high negative emotionality (+ 1 SD), the association between continuity and lower RAD symptoms was stronger ($b = -0.62$, $SE = 0.12$, $p < .001$) than among children with low negative emotionality (-1 SD; $b = -0.20$, $SE = 0.10$, $p = .046$). The coefficient and simple-slope pattern were consistent with differential susceptibility: high negative emotionality was linked to heightened risk under low continuity and greater apparent benefit under high continuity. The interaction between caregiver sensitivity and effortful control on DSED symptoms was also significant ($b = 0.21$, $SE = 0.08$, $p = .008$), indicating that low effortful control strengthened the protective association between sensitivity and DSED. Because this interaction was less stable in weighted models, it is interpreted as a vulnerability pattern rather than as strong evidence of full differential susceptibility. The index of moderated mediation was significant for continuity as a mediator of RAD through negative emotionality, index = -0.11 , 95% CI $[-0.20, -0.04]$.

Robustness checks. Results were largely consistent across robustness checks. When limiting the sample to children in state-supervised alternative care and excluding the low-SES community group, the main effects for foster care versus institutional care remained significant for both RAD ($b = -1.05$, $SE = 0.33$, $p = .002$) and DSED ($b = -0.82$, $SE = 0.35$, $p = .020$). The kinship-care contrast was reduced to marginal or non-significant levels for DSED ($b = -0.58$, $SE = 0.36$, $p = .108$). Under multiple imputation ($m = 40$), effect sizes were similar but confidence intervals were wider, reflecting added uncertainty. The indirect effect through caregiver continuity for foster care remained robust in the alternative-care-only sample, indirect effect = -0.28 , 95% CI $[-0.46, -0.12]$. One notable boundary condition was that the moderated-mediation index for sensitivity was not significant in the IPW-weighted sample, suggesting that this effect is more sensitive to selection bias than the continuity pathway. Detailed robustness results are provided in Supplementary Table S6, and variable coding details are provided in Supplementary File 3.

7.1 Main Results Tables

Tables 1–6 summarize the recruitment flow, baseline characteristics, measurement quality, primary models, indirect-pathway tests, and moderation tests. The table notes retain conservative language so that causal claims remain aligned with the longitudinal observational design.

Before interpreting the longitudinal estimates, the analytic sample must be transparent. Table 1 documents recruitment, retention, exclusions, and the final outcome sample by care context.

Table 1
Recruitment, Attrition, and Analytic Sample Flow

Flow item	I	G	F	K	C	Total
Eligible identified	189	162	175	148	196	870
Consent obtained	156	138	141	126	172	733
T1 completed	152	132	134	121	168	707
T2 retained	135 (88.8%)	118 (89.4%)	117 (87.3%)	112 (92.6%)	154 (91.7%)	636 (90.0%)
T3 outcome data	118 (77.6%)	106 (80.3%)	109 (81.3%)	108 (89.3%)	147 (87.5%)	588 (83.2%)
Lost/excluded from T1	34 (22.4%)	26 (19.7%)	25 (18.7%)	13 (10.7%)	21 (12.5%)	119 (16.8%)
Final analytic N	118	106	109	108	147	588
Note. I = institutional care; G = family-like group care; F = foster care; K = kinship care; C = low-SES community family. The analytic N is 588, and attrition/exclusion from T1 to the final analytic sample is 119/707 = 16.8%. Percentages are calculated from T1 completed cases within each care context.						

Because children were not randomly assigned to placements, baseline imbalance is substantively expected and methodologically important. Table 2 reports the main pre-treatment characteristics used to judge comparability and weighting quality.

Table 2
Baseline Sample Characteristics by Care Context

Var.	I	G	F	K	C	Test
Age	49.2 (14.1)	46.5 (13.8)	50.1 (13.9)	47.8 (14.3)	47.9 (13.1)	F = 0.91, p = .456; SMDmax = 0.26
Girls	58 (49.2%)	51 (48.1%)	52 (47.7%)	56 (51.9%)	73 (49.7%)	$\chi^2 = 0.62$, p = .961
Entry	12.4 (8.2)	15.1 (9.3)	18.7 (10.5)	20.4 (11.8)	N/A	F = 15.32, p < .001
Dur.	36.8 (14.2)	31.4 (13.1)	31.4 (13.5)	27.4 (15.2)	N/A	F = 24.67, p < .001
Disr.	1.2 (1.4)	1.0 (1.2)	1.5 (1.6)	0.8 (1.1)	N/A	F = 21.86, p < .001
Adv.	3.8 (1.1)	3.2 (1.0)	3.9 (1.2)	4.5 (1.2)	2.8 (1.3)	F = 31.74, p < .001
T1 RAD	4.2 (1.5)	3.5 (1.4)	3.1 (1.3)	2.8 (1.2)	1.8 (1.1)	F = 58.31, p < .001
T1 DSED	3.8 (1.6)	3.2 (1.5)	2.9 (1.4)	2.5 (1.3)	1.6 (1.0)	F = 45.72, p < .001
CG > HS	22 (18.6%)	31 (29.2%)	64 (58.7%)	42 (38.9%)	51 (34.7%)	$\chi^2 = 42.15$, p < .001
Note. I = institutional care; G = family-like group care; F = foster care; K = kinship care; C = low-SES community family. Baseline imbalance is expected and is reported rather than hidden. Covariate adjustment and inverse-probability weighting were used because placement was not randomized. Placement-specific variables are not defined for the low-SES community group; models including the community group used group indicators and did not interpret community values for placement entry, placement duration, or placement disruption.						

The credibility of the findings depends on whether key constructs were measured consistently across informants and care settings. Table 3 summarizes reliability, validity, and coding-quality evidence for the major measures.

Table 3
Measurement Reliability, Validity, and Coding Quality

Construct	Indicators	Evidence	Value
RAD	Adapted DAI RAD items	α/ω ; interview ICC	$\alpha/\omega = 0.86$; ICC = 0.78; range = 0–10
DSED	Adapted DAI DSED items	α/ω ; task convergence	$\alpha/\omega = 0.82$; ICC = 0.74; $r = 0.49$
Stranger task	Stayed vs left; latency if collected	Agreement; protocol deviations	κ /ICC = 0.81; valid N = 532
Continuity	Caregiver number, turnover, shifts, stability	Composite; record-report convergence	$\alpha/\omega = 0.79$; $r = 0.58$
Sensitivity	Observed interaction coding	Rater training; blind coding; ICC	ICC = 0.72; M(SD) = 3.6 (0.9)
Temperament	Negative emotionality; effortful control	Reliability; CFA	$\alpha/\omega = 0.84$ (NE), 0.81 (EC); CFI = .94; RMSEA = .06
SDQ	Total or subscales	Reliability; missingness	$\alpha/\omega = 0.80$; missing = 3.2%
Note. The manuscript reports RAD/DSED symptoms rather than clinical diagnoses unless assessor qualification and diagnostic confirmation are documented. Measure adaptation, translation/back-translation, pilot testing, and coding-quality evidence are summarized in the Supplementary Materials.			

Table 4 presents the primary prospective tests of whether care context predicted later attachment-disorder symptoms after accounting for baseline symptoms, adversity, and site clustering.

Table 4
Primary Longitudinal Models Predicting T3 RAD and DSED Symptoms

Predictor	T3 RAD symptoms	T3 DSED symptoms
Family-like group care vs institutional care	b = -0.31, SE = 0.29, 95% CI [-0.88, 0.26], p = .291	b = -0.25, SE = 0.31, 95% CI [-0.86, 0.36], p = .422
Foster care vs institutional care	b = -1.24, SE = 0.31, 95% CI [-1.85, -0.63], p = .001	b = -0.98, SE = 0.33, 95% CI [-1.63, -0.33], p = .004
Kinship care vs institutional care	b = -0.89, SE = 0.34, 95% CI [-1.56, -0.22], p = .012	b = -0.72, SE = 0.35, 95% CI [-1.41, -0.03], p = .042
Low-SES community family vs institutional care	b = -1.78, SE = 0.38, 95% CI [-2.53, -1.03], p < .001	b = -1.41, SE = 0.39, 95% CI [-2.18, -0.64], p = .001
T1 outcome score	b = 0.51, SE = 0.06, 95% CI [0.39, 0.63], p < .001	b = 0.47, SE = 0.07, 95% CI [0.33, 0.61], p < .001
Pre-placement adversity index	b = 0.22, SE = 0.08, 95% CI [0.06, 0.38], p = .008	b = 0.18, SE = 0.09, 95% CI [0.002, 0.358], p = .048
Age at placement / duration	b = 0.01, SE = 0.01, 95% CI [-0.01, 0.03], p = .350	b = 0.00, SE = 0.01, 95% CI [-0.02, 0.02], p = .812
Site-level random effect / ICC	ICC = 0.10; omnibus F(4, 58.2) = 8.47, p < .001	ICC = 0.08; omnibus F(4, 55.7) = 5.93, p < .001
Note. Linear mixed models with Satterthwaite degrees of freedom were used for continuous symptom scores after diagnostics. Alternative negative-binomial specifications were included as robustness checks because RAD/DSED scores may be skewed or zero-inflated.		

To test whether placement differences were translated through everyday caregiving ecology, Table 5 reports the temporally ordered indirect associations through caregiver continuity and sensitivity.

Table 5
Mediation Through Caregiver Continuity and Caregiver Sensitivity

Path	RAD estimate (95% CI)	DSED estimate (95% CI)	Interpretation
Care type -> continuity	Foster: .81 [.53, 1.09]; Kinship: .52 [.22, .82]	Foster: .81 [.53, 1.09]; Kinship: .52 [.22, .82]	Higher continuity in family-based care
Care type -> sensitivity	Foster: .72 [.46, .98]; Kinship: .45 [.17, .73]	Foster: .72 [.46, .98]; Kinship: .45 [.17, .73]	Higher sensitivity in family-based care
Continuity -> outcome	– .41*** [–.59, – .23]	– .29** [–.49, – .09]	Adjusted for T1 outcome
Sensitivity -> outcome	– .35*** [–.51, – .19]	– .18† [–.36, .00]	DSED path weak and uncertain
Indirect via continuity	Foster: – .33 [–.52, – .16]; Kinship: – .21 [–.38, – .07]	Foster: – .23** [–.40, – .08]; Kinship: – .15* [–.29, – .02]	More robust indirect pathway
Indirect via sensitivity	Foster: – .25** [–.42, – .11]; Kinship: – .16* [–.30, – .04]	Foster: – .13† [–.27, .01]; Kinship: – .08 [–.20, .04]	Interpret DSED cautiously
Direct after mediators	Foster: – .66* [–1.24, – .08]; Kinship: – .52† [–1.15, .11]	Foster: – .62* [–1.21, – .03]; Kinship: – .49† [–1.10, .12]	Residual direct paths
Note. † $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. The mediation logic is acceptable only because X, M, and Y are temporally ordered. The manuscript uses 'indirect association' rather than definitive causal mediation because placement and caregiving mechanisms were not randomized.			

Table 6 examines whether children differed in their susceptibility to caregiving quality. The moderation tests distinguish full differential susceptibility from narrower vulnerability-under-adversity patterns.

Table 6
Temperament as Moderator and Moderated-Mediation Evidence

Term	RAD estimate (95% CI)	DSED estimate (95% CI)	Interpretation / probe
Continuity × negative emotionality	-0.28** [-0.46, -0.10]	-0.15† [-0.33, 0.03]	Simple slopes at -1 SD, mean, + 1 SD
Sensitivity × negative emotionality	-0.18* [-0.34, -0.02]	-0.12 [-0.28, 0.04]	Simple slopes reported in text
Continuity × effortful control	0.15† [-0.02, 0.32]	0.21** [0.06, 0.36]	Simple slopes reported in text
Sensitivity × effortful control	0.12 [-0.04, 0.28]	0.18* [0.04, 0.32]	Simple slopes reported in text
Index of moderated mediation via continuity	-0.11** [-0.20, -0.04]	-0.06† [-0.13, 0.01]	Conditional indirect effect
Index of moderated mediation via sensitivity	-0.07* [-0.15, -0.01]	-0.04 [-0.10, 0.02]	Conditional indirect effect
Note. † $p < .10$, * $p < .05$, ** $p < .01$. Full differential susceptibility requires evidence of both vulnerability under poorer care and greater benefit under better care. The continuity × negative emotionality pattern supports this interpretation for RAD; the sensitivity × effortful-control pattern for DSED is interpreted more cautiously because robustness checks were weaker.			

8. Discussion

The findings support the central argument that protection becomes developmentally meaningful only when it produces stable and sensitive relationships. Foster care and kinship care predicted lower later RAD and DSED symptoms than institutional care, whereas family-like group care did not differ significantly from institutional care after baseline symptoms and selection factors were controlled. This pattern suggests that simply reducing the scale of residential care or making it more family-like may not be sufficient if children do not experience a stable, emotionally available caregiver. The most important contribution is therefore not a simple claim that family care is better than institutional care, but the identification of caregiver continuity and sensitivity as modifiable relational mechanisms.

The first theoretical contribution is to refine attachment theory for the specific context of state care. Attachment theory was developed around relationships with identifiable caregivers, yet child-welfare research often substitutes administrative placement labels for relational experience. The present findings show that attachment-related adaptation in alternative care depends on whether a placement repeatedly gives the child access to a familiar, emotionally available adult. This distinction explains why foster and kinship care showed more favorable adjusted outcomes than institutional care, while family-like group care did not show a statistically reliable advantage once mechanisms and covariates were considered. The implication is theoretical as well as practical: attachment security cannot be inferred from the legal form of protection; it must be traced to the child's everyday relational access to a particular caregiver.

The second theoretical contribution is to specify a relational-translation model within the bioecological framework. Rather than invoking context broadly, the study shows how macrosystem policy, exosystem staffing and funding, mesosystem service coordination, and microsystem caregiver-child interaction are translated into continuity and sensitivity. This formulation helps explain why the same policy category may have different developmental meanings across sites: a group-care unit with stable key caregivers may be psychologically different from one organized around rotating shifts, even if both are labelled family-like care. The model also clarifies the technical mechanism through which distal policy arrangements become measurable developmental exposures: they alter the frequency, predictability, and emotional quality of proximal caregiver-child interactions. This contribution is especially relevant for China, where local administrative discretion, institutional history, kinship norms, and uneven foster-care participation can produce substantial variation within the same formal placement category.

The third theoretical contribution is to integrate temperament into the mechanism model rather than treating it as a background covariate. High negative emotionality strengthened the association between caregiver continuity and later RAD symptoms in a pattern consistent with differential susceptibility (Belsky & Pluess, 2009; Ellis et al., 2011; Slagt et al., 2016). Children high in negative emotionality appeared most vulnerable when continuity was low but most responsive when continuity was high. Low effortful control also strengthened the protective association between sensitivity and DSED, although this result was less robust under weighting and is therefore interpreted more cautiously. These findings reposition temperament from a child deficit to a marker of relational need: some children may require earlier access to stable caregivers, lower caregiver-child ratios, more intensive caregiver coaching, and fewer placement moves.

Taken together, these contributions move the manuscript beyond a placement-comparison study. The article advances a relational-translation account of child-welfare reform: policy changes matter for development only when they alter the proximal conditions through which children experience protection, including caregiver assignment, daily availability, emotional responsiveness, and the opportunity to form selective attachments. This account also clarifies why broad deinstitutionalization claims are insufficient without measuring the relational ecology created by each alternative-care arrangement. In this sense, the study offers a mechanism-oriented framework that can be transported to other welfare systems: researchers and agencies can evaluate whether reforms alter the relational inputs that children actually receive rather than only whether children are moved across administrative categories.

Policy implications are concrete. First, child-welfare agencies should monitor caregiver continuity as a core quality indicator, not merely count beds, funding, or staffing positions. Second, every child in institutional or group care should have a designated primary caregiver whose availability is protected by staffing rules. Third, caregiver sensitivity training should be linked to supervision, video feedback, and accountability rather than offered only as general professional development. Fourth, foster and kinship families need ongoing financial, psychological, and case-management support because the benefits of family-based care depend on the caregiver's ability to remain emotionally available under stress. Finally, children with high negative emotionality or low effortful control should receive prioritized stability planning rather than repeated placement moves.

The causal claims remain cautious. Even with three waves, rich controls, and weighting, placement was not randomized. Children entering foster care, kinship care, institutional care, or family-like group care may differ in unobserved ways that affect later symptoms. The models control baseline symptoms, pre-placement adversity, age at placement, duration in care, placement disruption, disability status, caregiver education, and site clustering, but unmeasured factors such as birth-family attachment quality, severity of early neglect, caregiver motivation, and local caseworker decision rules may remain. For this reason, the results are best interpreted as prospective, mechanism-consistent associations rather than definitive causal effects.

Several limitations should be acknowledged transparently. First, pre-placement records may be incomplete, especially for children with complex family histories or informal kinship care. Second, caregiver reports may vary by informant type; professional caregivers, foster caregivers, kin caregivers, and biological parents do not observe children under identical conditions. Third, although observation coding included reliability checks and a blind recoding subset, some assessment procedures may still be influenced by contextual knowledge or site differences. Fourth, the community group provides socioeconomic comparison but is not interchangeable with children in state-supervised care; therefore, the alternative-care-only analyses should be treated as essential robustness evidence. Fifth, the RAD/DSED interview was adapted for the local field context and reports symptom scores rather than psychiatric diagnoses. Sixth, the moderated mediation findings for sensitivity were less robust than the continuity pathway, so they should be treated as suggestive rather than conclusive.

Future research should build toward stronger causal designs. Potential extensions include natural experiments following local policy reforms, stepped-wedge caregiver training interventions, randomized caregiver-coaching programs within institutions or group homes, and administrative-data linkage to placement histories and service utilization. Qualitative interviews with children, caregivers, social workers, and kinship families would also deepen understanding of how children experience continuity and sensitivity in everyday life. The long-term goal is not only to publish stronger evidence, but to produce findings that child-welfare agencies can use to redesign care around children's relational needs.

9. Conclusion

The study is built around a simple but demanding idea: child protection is not developmentally complete until it provides a child with stable, sensitive, and emotionally available care. In China and elsewhere, alternative-care reform has expanded beyond the old institution-versus-family debate. The present findings suggest that foster and kinship care are associated with lower later attachment-disorder symptoms partly because they provide more continuity and sensitivity, while family-like group care may not confer the same advantage unless it changes the child's everyday caregiving ecology. By integrating attachment theory, bioecological thinking, and differential susceptibility, the article contributes a relational-translation model of alternative care: protection attaches only when policy and placement are converted into repeated, child-specific, emotionally responsive relationships.

Additional conceptual definitions, measurement timing, analytic safeguards, expanded mediation details, domain-level measure protocols, and analytic variable definitions are provided in the Supplementary Files.

Declarations

Ethics Approval

This study was conducted in accordance with the Declaration of Helsinki and applicable ethical standards for research involving human participants and children in vulnerable circumstances. Ethical approval was granted by the University of Chicago Social and Behavioral Sciences Institutional Review Board. Child-welfare authority permission was obtained before recruitment.

Consent to Participate

Legal guardians provided informed consent for child participation, caregivers provided informed consent for caregiver reports and interaction recordings, and developmentally appropriate child assent was obtained where applicable.

Consent for Publication

No individually identifiable participant information, images, or case details are included in this manuscript; therefore, no additional consent for publication is required for anonymized research reporting.

Conflicting Interests

The author declares that there are no conflicting interests.

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Data Availability

The data underlying this article are not publicly available because they contain sensitive information about young children, caregivers, child-welfare records, and care settings, creating safeguarding and re-identification risks. De-identified analytic materials and analysis code may be made available from the corresponding author upon reasonable request, subject to a data-use agreement, ethics approval, and permission from the relevant child-welfare authorities.

Author Contribution

R.X. conceived and designed the study, developed the theoretical framework, coordinated data collection, prepared the measures and coding protocol, conducted the statistical analyses, interpreted the results, and wrote the main manuscript text. R.X. also prepared the tables, figure, supplementary materials, and submission documents. The author reviewed and approved the final manuscript and agrees to be accountable for all aspects of the work.

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Figures

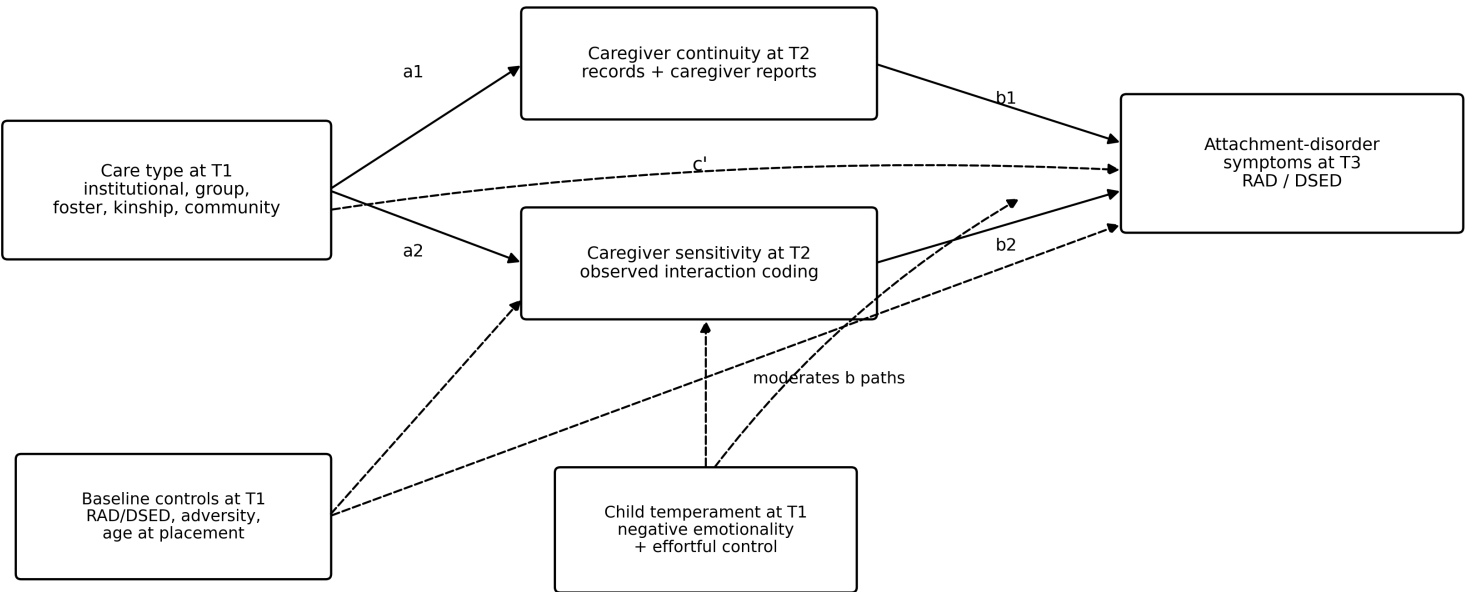


Figure 1

Conceptual model of care type, caregiving mechanisms, temperament, and attachment-disorder symptoms.

Note. Solid arrows indicate primary prospective paths; dashed arrows indicate moderation by child temperament. The figure is a theoretical model and should be interpreted together with the longitudinal tests

reported in Tables 4-6.

Supplementary Files

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