

Parent–Adolescent Communication, Peer Context, Autonomy, and Future-Related Thinking in Late-Adolescent Mental Health: A Targeted Narrative Review

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Abstract

Background: Parent–adolescent communication, peer relationships, autonomy, identity, and future-related thinking are often studied separately, despite their potential relevance to emotional adjustment during the transition to adulthood. Age-specific evidence for late adolescence and Saudi/Gulf settings remains limited. **Objective:** To critically synthesize evidence on these relational and developmental domains, distinguish association from temporal evidence, and identify a bounded Saudi/Gulf research and clinical agenda. **Methods:** This targeted narrative review retained a structured PubMed/MEDLINE search audit, final source-level eligibility re-screening, and purposive, citation-tracked source assembly. Three prespecified searches returned 9,443 records before deduplication. The first 20 records in PubMed’s default most-recent ESearch order were retained from each query as a 60-record search audit, not as a relevance-ranked inclusion screen. Twenty-two source records initially informed the synthesis; one out-of-scope source was excluded, leaving 21 original scholarly sources. A separately documented focused age-specific search then added two source-verified records, yielding 23 scholarly sources overall. **Findings:** Adolescent-rated parent–child communication quality and broader family relationship quality are consistently associated with mental-health outcomes, but much evidence is cross-sectional or based on one informant. Direct 15–19 evidence is narrow: one age-specific prospective synthesis addressed future-related thinking, one 15–19 relationship-profile study was cross-sectional, and one age-18 exposure study measured outcomes at age 21. Mixed-age samples must not be interpreted as age-specific evidence. Saudi and Gulf evidence provides important contextual and service-use information but does not yet provide a direct age-specific test of an integrated parent–peer–autonomy model. **Conclusion:** The review offers a hypothesis-generating framework and a transparent research agenda. Family-inclusive prevention may be useful when it is acceptable and safe, but the reviewed evidence does not support causal claims, diagnosis from appearance or friendship changes, or a stand-alone family intervention as treatment for mental disorders. **Keywords:** Adolescence; autonomy; family communication; future-related thinking; mental health; parent–adolescent relationships; peer relationships; Saudi Arabia.

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INTRODUCTION

Adolescence is commonly defined as the period from ages 10 to 19 years (World Health Organization, n.d.). The term *late adolescence* is not applied consistently across developmental traditions. To focus the present review on the developmental interval that includes age 17, late adolescence is operationalized here as ages 15–19 years. This is an analytic decision, not a claim that all developmental change occurs within a fixed age band. Sources are classified as age-specific only when their reported estimates are restricted to, or separately reported for, ages 15–19; as mixed-age overlap when the sample includes some participants

aged 15–19 but reports pooled findings; or as indirect developmental/contextual evidence when the target ages are absent or cannot be separated. Mixed-age overlap is not interpreted as age-specific evidence.

The transition from mid- to late adolescence includes changing expectations around autonomy, education or work, family roles, peer networks, and future plans. These changes do not have a single meaning. Parent–adolescent relationships are developmentally dynamic, and ordinary autonomy negotiation can coexist with enduring family connection (Smetana & Rote, 2019). The central clinical question is therefore not whether a young person has changed, but

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whether there are symptoms, functional impairment, safety concerns, harmful peer experiences, or barriers to seeking help.

Existing evidence supports attention to family relationships while cautioning against simple explanations. In a systematic review of 37 reports representing 26 unique studies, higher adolescent-rated parent–child communication quality was associated with fewer mental-health problems, but most reports were cross-sectional (Zapf *et al.*, 2024). In Saudi Arabia, poorer self-reported relationships with either mother or father were associated with greater odds of sadness/hopelessness and worry in a national school-student survey; the design cannot establish whether relationship problems preceded the symptom indicators or were partly shaped by them (Abou Abbas & AlBuhairan, 2017). These sources concern related but distinct constructs: communication quality, broader relationship quality, and symptom indicators should not be treated as interchangeable.

Mental-health outcomes also require separation. Depressive symptoms, anxiety symptoms, general psychological difficulties, diagnosed disorders, wellbeing, functioning, and help-seeking are not equivalent outcomes. Screening responses are not diagnoses, and a relationship variable that is associated with one outcome cannot automatically be assumed to affect another. This review consequently distinguishes outcome type, design, informant, and age applicability throughout.

Research Gap, Novelty, and Review Questions

Research gap

Gap 1: Conceptual fragmentation.

Parent communication, peer relationships, autonomy support, identity processes, and future-related thinking are commonly examined in separate literatures. Zapf *et al.* (2024) focused on dyadic parent–child communication and mental health; Delgado *et al.* (2022) focused on attachment and peer relationships; Tang *et al.* (2024) examined future-related thinking and internalizing symptoms. This separation makes it

difficult to assess how relational and developmental processes may coexist or interact.

Gap 2: Limited integrated longitudinal evidence.

Longitudinal studies provide more information about temporal ordering than cross-sectional studies, but they still do not prove causation. Parent–friend relationship perceptions may be reciprocally associated across adolescence (De Goede *et al.*, 2009), and negative future-related thinking and depressive symptoms show reciprocal temporal associations in an age-specific 16–19 synthesis (Tang *et al.*, 2024). However, age-specific longitudinal tests that integrate family communication, peer context, autonomy, identity, future-related thinking, and multiple mental-health outcomes remain uncommon.

Gap 3: Measurement and informant limitations.

Adolescent and parent reports often diverge. In a two-wave Japanese study, mother-reported autonomy support and adolescent-perceived controlling parenting showed different prospective associations (Tanaka *et al.*, 2023). In a six-wave Dutch study, stable between-family differences, rather than within-family annual change, accounted for associations between autonomy support and problem behavior (Vrolijk *et al.*, 2020). Such findings show why reporter, measure, and level of analysis cannot be treated as minor methodological details.

Gap 4: Saudi/Gulf contextual evidence.

The available regional evidence is important but not sufficient to determine how an integrated relational-developmental model operates among 15–19-year-olds. A scoping review of Arab-region parenting research found that 89% of 152 publications were cross-sectional and that only a minority were conducted in GCC countries (Ghandour *et al.*, 2024). Saudi and Gulf studies contribute school-survey, qualitative, prevalence, and measurement evidence, but they do not provide a longitudinal, multi-informant test of the proposed model.

Contribution relative to existing reviews

Table 1: Comparison with related reviews and the bounded contribution of the present review

Review	Purpose and population	Scope	Boundary or contribution
Zapf <i>et al.</i> (2024)	Adolescents ages 8–21; 37 reports representing 26 quantitative studies.	Adolescent-rated dyadic parent–child communication and mental-health outcomes.	Most reports were cross-sectional; does not integrate peer context, autonomy/identity, future-related thinking, or Saudi/Gulf evidence.
Ghandour <i>et al.</i> (2024)	Arab youth ages 10–24; 152 mapped publications.	Regional parenting evidence map across multiple youth outcomes.	Maps a broad regional field; search coverage ended April 2018 and does not provide an integrated late-adolescent relational model.
Tang <i>et al.</i> (2024)	Late-adolescent transition; 22 prospective studies, N = 10,682.	Future-related thinking and depressive/anxiety symptoms.	Provides the strongest temporal synthesis for future-related thinking but does not integrate family

Review	Purpose and population	Scope	Boundary or contribution
			communication, peer context, or Saudi/Gulf implementation questions.
Present targeted narrative review	Primary analytic focus ages 15–19; sources classified as age-specific, mixed-age overlap, or indirect developmental/contextual.	Family communication, peer context, autonomy/identity, future-related thinking, mental-health outcomes, and Saudi/Gulf evidence.	Integrates domains and applies explicit inference and age rules; it is not systematic, exhaustive, or a causal test.

Note. The present review's novelty is a bounded integrative synthesis and research agenda, not a claim of stronger causal evidence than prior reviews.

The contribution of the present review is therefore bounded and specific. It does not claim to replace prior reviews or establish a causal model. Its advance is threefold: it aligns evidence across family communication, peer context, autonomy/identity, and future-related thinking rather than treating these as separate literatures; it classifies each source by age applicability and inferential strength; and it converts the resulting gaps into falsifiable, Saudi/Gulf-relevant prospective hypotheses. The framework does not propose newly demonstrated relationships. Its scientific value lies in specifying which integrated propositions are supported only as plausible components, which require direct testing, and what future results would challenge them.

Review questions

1. Among adolescents aged 15–19 years, what is known about parent–adolescent communication or broader relationship quality in relation to depressive symptoms, anxiety symptoms, wellbeing, functioning, or help-seeking?
2. How are peer support, peer adversity, friendship quality, and parent–peer interplay associated with these outcomes, and what is known about temporal or reciprocal relationships?
3. What evidence connects autonomy support, identity processes, and future-related thinking with emotional adjustment, and which claims are age-specific, mixed-age overlap, or indirect for late adolescence?
4. What Saudi/Gulf evidence is available, what does it not establish, and what research is needed before clinical or policy conclusions are drawn?

METHODS

Review design

This was a targeted narrative review using structured search elements. It was not a systematic review, meta-analysis, or registered protocol. The purpose was to assemble and critically compare evidence relevant to the four review questions, with priority for late-adolescent, longitudinal, multi-informant, intervention, and Saudi/Gulf sources. The broad PubMed audit documents search execution; final source selection instead relies on a source-level eligibility re-screening and a separately documented focused age-specific

screen. This design can identify patterns and limitations, but it cannot provide an exhaustive effect estimate.

Search audit and record handling

PubMed/MEDLINE was searched on October 11, 2026. Three prespecified concept blocks covered: (S1) parent–adolescent or family communication and mental health; (S2) peers, autonomy, identity, future-related thinking, and mental health; and (S3) Saudi/Gulf adolescent relational context and mental health. Searches were limited to publications dated January 1, 2014 through October 11, 2026. Exact Boolean strings, PubMed field tags, dates, counts, and retained identifiers are supplied in Supplementary File 1.

The searches returned 1,710 records for S1, 7,641 for S2, and 92 for S3: 9,443 records before deduplication. The original query log did not record an explicit ESearch `sort` parameter. National Library of Medicine documentation states that PubMed ESearch defaults to most recent—the date a record was added to PubMed—rather than relevance (National Library of Medicine, n.d.). Accordingly, the 20 retained records from each block are now described as positions 1–20 in default most-recent order, not as relevance-ranked records. The three 20-record sets contained 60 unique PMIDs: there were no pairwise or three-way overlaps within this limited audit sample. Deduplication was not performed across the full 9,443 raw results.

The 60 records are a reproducible search-execution audit only. Their contemporaneous record-level inclusion/exclusion decisions were not retained and cannot be reconstructed reliably; they were therefore not used as the manuscript's study-selection frame. PsycINFO and Scopus were not searched because no institutional-access route or database connector was available for this review. The manuscript does not claim multi-database coverage, comprehensive screening, or systematic evidence retrieval.

Focused age-specific evidence enrichment

After the age-applicability limitation was identified, a distinct, focused PubMed/MEDLINE query was run on October 11, 2026 to look for relational studies that explicitly reported late-adolescent ages. The query combined parent–adolescent/parent–child relationship or communication terms, depression/anxiety/mental-

health terms, and the age terms “15–19” or “late adolescence.” It returned eight records in explicitly requested relevance order. Titles and abstracts for all eight records were screened against the review questions and strict age rule; two records were retained after source verification: Smith *et al.* (2024), which reported a 15–19-year-old sample, and Saminathen *et al.* (2026), which measured parental warmth and knowledge at age 18. The other six records were excluded because they were outside the relational-mental-health scope, did not provide a target-age-specific estimate, or did not address a relevant outcome. The exact query, returned identifiers, screening decisions, and reasons are documented in Supplementary File 1.

This focused update is an evidence-enrichment search, not a comprehensive rescreening of the original 9,443 results. It was conducted to improve direct age coverage and does not convert the review into a systematic review or provide a basis for estimating selection completeness.

Source assembly, eligibility, and age applicability

Before the focused update, the evidence set was assembled purposively through backward citation tracking from anchor reviews, title/DOI retrieval from publisher and PubMed Central records where accessible, and regional-source identification. The 21 retained original scholarly sources did not match any PMID in the documented 60-record PubMed audit sample. They are therefore identified as citation-tracked or targeted-retrieval sources rather than database-sampled

inclusions. The focused age-specific search added two database-selected sources after full source verification. The final set therefore contains 23 scholarly sources: 21 citation-tracked/targeted-retrieval sources and two focused-search sources. The 2009 De Goede *et al.* study, for example, was retained through backward citation tracking because it examined parent–friend relationship reciprocity in a cohort that included ages 16–20; it is classified as mixed-age overlap, older, nonregional, and non-mental-health evidence.

Purposive assembly was considered conceptually sufficient for the four question-driven domains, not statistically representative of the total literature. The selection was deliberately diversified across (a) parent communication/relationship quality, (b) peer and parent–peer processes, (c) autonomy, identity, and future-related thinking, and (d) Saudi/Gulf or Arab-region context; across reviews, longitudinal cohorts, cross-sectional studies, qualitative work, and service-context evidence; and across direct, mixed-age, and indirect age classes. Anchor reviews were used to identify construct breadth and methodological limitations, while targeted retrieval preserved landmark temporal studies and contextual regional sources. Every retained source was then mapped to at least one review question and retained only with an explicit age class, design, verified finding, and inference limit. This provides a transparent analytic sampling frame for a narrative synthesis, but it neither demonstrates exhaustive coverage nor permits prevalence, pooled-effect, or “best evidence” claims.

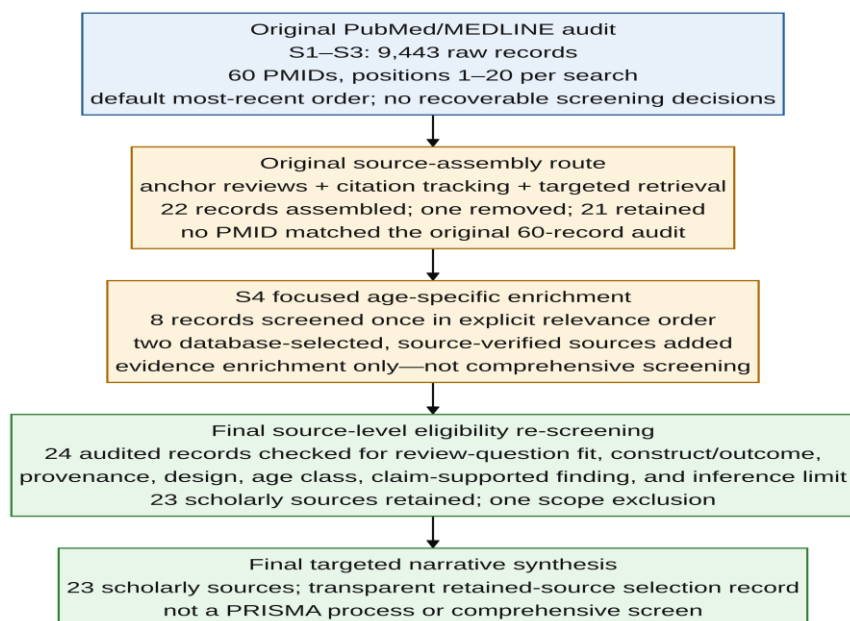


Figure 1: Documented PubMed search audit, final-source eligibility re-screening, and provenance

Note. Counts are raw PubMed/MEDLINE retrievals before deduplication and may overlap. The 60 original-audit PMIDs document search execution only; they did not constitute a relevance-ranked inclusion screen, and no citation-tracked/targeted-retrieval source matched that audit sample. Final source-level eligibility re-screening documents the retained-source decision process; it does not convert the original retrieval into comprehensive screening. This is a targeted narrative-review workflow, not a PRISMA flow diagram.

Figure 1 separates the 60-record PubMed search-execution audit from the independently assembled original source set and the subsequent focused age-specific evidence update. It makes clear that default-order retrieval is not relevance ranking, that no

recoverable record-level screening decisions exist for the original audit sample, and that the source-assembly route is citation tracking, targeted retrieval, and a transparently documented focused update rather than a PRISMA process.

Table 2: Structured PubMed search audit, focused age-specific enrichment, and source provenance

Search block	Key concepts and PubMed/MEDLINE fields	Raw retrieval and ordered audit sample	Role in this review
S1: Parent/family communication	Adolescent/youth/late-adolescence; parent–adolescent, parent–child, or family communication; mental health, depression, or anxiety. Title/abstract terms plus Parent–Child Relations [MeSH].	January 1, 2014–October 11, 2026; n = 1,710. Positions 1–20 retained in default most-recent order.	Search-execution audit only; not a relevance-ranked inclusion screen.
S2: Peer/developmental pathway	Adolescent/youth/late-adolescence; peer/friendship, autonomy, identity, or future orientation; mental health, depression, anxiety, or wellbeing. Title/abstract terms.	January 1, 2014–October 11, 2026; n = 7,641. Positions 1–20 retained in default most-recent order.	Search-execution audit only; not a relevance-ranked inclusion screen.
S3: Saudi/Gulf contextual pathway	Saudi Arabia/GCC; adolescent/youth; parent/family/peer; mental health, depression, or anxiety. Title/abstract terms.	January 1, 2014–October 11, 2026; n = 92. Positions 1–20 retained in default most-recent order.	Search-execution audit only; not a relevance-ranked inclusion screen.
S4: Focused age-specific enrichment	Parent–adolescent/parent–child relationship or communication; depression, anxiety, or mental health; “15–19” or “late adolescence.”	October 11, 2026; n = 8. Explicit relevance sort; all titles/abstracts screened once.	Evidence enrichment only: two database-selected sources retained after verification; not a comprehensive rerun.

Note. The original S1–S3 search total was 9,443 records before deduplication; full-result deduplication was not performed. The 60 retained original-audit PMIDs were all unique, with no pairwise or three-way overlap across the three 20-record sets. NCBI ESearch defaults to most-recent order (date added), not relevance, when no explicit sort is supplied. Record-level inclusion decisions were not retained for S1–S3. The separately documented S4 query screened eight records and added two age-relevant sources; it does not represent comprehensive screening. The final 23 scholarly sources comprise 21 citation-tracked/targeted-retrieval and two S4 database-selected sources. Exact strings, order, decisions, provenance, and source-use rationale are in Supplementary File 1.

Eligibility, screening, and extraction

English-language peer-reviewed reviews and empirical studies were eligible when they addressed at least one relational/developmental construct and an outcome relevant to the review questions. Age-specific evidence required estimates reported exclusively for, or separately extractable for, ages 15–19. Mixed-age overlap evidence included ages 15–19 in a broader sample but reported pooled estimates, and was used only to describe broader associations or context. Indirect developmental/contextual evidence did not provide a separable 15–19 estimate and was retained only when it clarified a mechanism, measurement issue, or contextual question. A sample range or mean that merely included some 15–19-year-olds was not sufficient for age-specific classification. Sources concerned only with childhood, without an interpretable relational/developmental construct, or outside the mental-health and transition-to-adulthood scope were excluded.

The 60 PubMed audit records document ordering and retrieval but do not provide a recoverable screening decision log. The original evidence file contained 22 source records; one was removed during scope correction because it did not address the review questions, leaving 21 citation-tracked/targeted-retrieval sources. The focused age-specific search then added two database-selected sources, yielding 23 scholarly sources in the final synthesis. Final source-level re-screening was then applied to all 24 audited source records: each record was mapped to at least one review question and checked for construct/outcome relevance, source provenance, design, age class, a claim-supported finding, and a material inference limit; the one out-of-scope record was excluded. For each retained source, the review charted provenance, setting, design, age-applicability class, sample/age, informant, exposure or construct, outcome, key finding, and major limitation. This source-level process makes the final purposive selection transparent and reproducible at the retained-source level, but it does not convert the broad 9,443-record search into a

systematic screen. Selection safeguards were question-to-source mapping, explicit age classification, source-verification checks, preservation of study limitations and null findings, and comparison across designs and settings rather than selection only of supportive studies. These safeguards reduce, but do not remove, selection bias because neither duplicate screening nor full-result screening was completed. Supplementary File 1 documents the original queries, focused update query, ordered records, overlap check, source provenance, source-use rationale, age class, and scope exclusions; Supplementary File 2 documents source-verification results.

No full-result duplicate screening, duplicate-record count, formal risk-of-bias assessment, or registered protocol was completed. Full-text exclusion reasons were not recorded for all raw PubMed results and are not reconstructed. The final source-level re-screening strengthens transparency of what was retained, excluded, and claimed, but does not resolve the selection limitations of the original broad retrieval. These limitations mean that this article is a targeted narrative review with a transparent search audit, not a systematic or scoping review.

Interpretation rules

Table 3: Design- and age-applicability interpretation rules applied in the narrative synthesis

Evidence feature	Permitted interpretation	Interpretation not supported
Cross-sectional association	A relational/developmental construct and an outcome co-occur in the sampled setting.	Temporal order, causation, or a treatment effect.
Prospective or longitudinal association	A construct predicts or is associated with a later outcome after the reported model adjustments.	A causal pathway; unmeasured confounding and reciprocal influence may remain.
Multiple-informant finding	Reporter agreement or disagreement is itself relevant to interpretation.	That one reporter supplies the objective or superior account.
Between-family versus within-family model	Stable family differences may explain an observed association.	That annual change in parenting or symptoms caused later change in the other.
Randomized parenting intervention	The tested intervention affected the measured outcome in its study population and follow-up period.	A general late-adolescent treatment effect, unless that population and outcome were tested.
Saudi/Gulf contextual source	The source informs setting, measurement, service context, or locally observed associations.	A region-wide cultural mechanism or a claim that generalizes beyond the sampled population.
Age-specific evidence	An estimate is restricted to, or separately reported for, ages 15–19.	That any adolescent sample including some 15–19-year-olds is age-specific.
Mixed-age overlap evidence	A pooled estimate comes from a broader sample that includes some ages 15–19.	An age-specific effect or conclusion for ages 15–19.
Indirect developmental/contextual evidence	The source clarifies a mechanism, measurement issue, or context outside a separable 15–19 estimate.	A late-adolescent effect estimate or age-specific clinical inference.

Note. These rules were used to prevent conflation of association, temporal ordering, diagnosis, intervention evidence, and age applicability.

Narrative evidence-confidence judgments

The confidence statements in Table 6 are structured narrative judgments, not GRADE certainty ratings and not a formal risk-of-bias assessment. For each domain-level conclusion and framework proposition, the source-verification record was used to consider six prespecified features: (1) design and temporal ordering, (2) consistency across sources, (3) relevance to ages 15–19 and to Saudi/Gulf settings, (4) measurement and informant quality, (5) confounding and alternative explanations, and (6) directness of the conclusion to the proposed framework. A conclusion was described as more supported only when multiple sources had broadly concordant component findings; it remained limited when direct age-specific/regional evidence, temporal ordering, measurement quality, or

confounding control was weak. “Very limited” or “untested” denotes that the integrated proposition itself lacks a direct study, even where one component has longitudinal evidence. These labels are intended to calibrate interpretation and research priorities; they do not estimate effect size, establish causality, or substitute for a systematic risk-of-bias appraisal.

Evidence Synthesis

Parent–adolescent communication and internalizing symptoms

The most consistent conclusion is associative: higher adolescent-rated parent–child communication quality and more positive family relationship quality are linked with fewer reported mental-health problems. Zapf et al. (2024) synthesized 37 reports from 26 unique

studies of adolescents aged 8–21. Better adolescent-rated dyadic communication was associated with fewer mental-health problems, but 25 reports were cross-sectional. The review's evidence therefore supports assessment of communication quality, not the conclusion that improving communication alone will prevent a disorder.

A prospective early-adolescent study reported that paternal open communication was associated with lower later anxious and depressive symptoms, while paternal co-rumination was associated with higher later depressive symptoms (Ioffe *et al.*, 2020). The study sampled participants with a mean age of 12.49 years; it is therefore classified as indirect developmental evidence under this review's 15–19 age-applicability rule, rather than as direct evidence about late adolescence. In the United States Add Health cohort, greater adolescent-reported family cohesion and absence of recent parent–child conflict were associated with lower depressive-symptom trajectories into adulthood; the exposures and outcome were self-reported, and duration of association differed by sex and measure (Chen & Harris, 2019). These studies support the relevance of family relationship quality, but neither establishes a single causal family pathway.

The focused age-specific update added two more directly applicable but limited sources. In a cross-sectional U.S. sample of 589 African American caregiver–adolescent dyads aged 15–19, latent profile analysis identified four parent–adolescent relationship profiles based on ethnic-racial socialization, general parenting practices, and relationship quality; adolescent depressive symptoms were correlated with profile membership (Smith *et al.*, 2024). The person-centered, cross-sectional design does not establish direction or identify a single component effect. In a Swedish cohort, parental warmth and knowledge were measured at age 18 and depressive/anxiety symptoms at age 21. After mutual adjustment and adjustment for earlier mental-health problems and measured covariates, parental warmth—but not parental knowledge—remained associated with lower probability of later symptoms (Saminathen *et al.*, 2026). This is direct age-18 exposure evidence with young-adult, rather than concurrent 15–19, outcomes; it does not establish a Saudi/Gulf effect or a causal mechanism.

The Saudi Jeeluna survey offers mixed-age Saudi contextual evidence: its school-student sample covered ages 10–19, but it did not report a separate 15–19 estimate. Poorer self-reported relationships with either parent were associated with higher odds of past-year sadness/hopelessness and worry severe enough to disrupt usual activities (Abou Abbas & AlBuhairan, 2017). The outcome items are symptom indicators rather than diagnoses, and the cross-sectional design precludes temporal interpretation. In a selected Saudi school

sample aged 12–16, higher perceived family support—but not friend or significant-other support—was independently associated with lower total difficulties in an adjusted cross-sectional model (Alshehri *et al.*, 2020). This finding should not be generalized to all Saudi adolescents or interpreted as evidence that friends are unimportant.

Peer relationships and mental health

Peer context is heterogeneous. Friendship support, intimacy, conflict, victimization, peer pressure, online interaction, and peer norms are distinct exposures. A systematic review of 19 studies found that secure parental attachment was associated with more supportive, trusting, intimate, and higher-quality peer relationships; its included literature was predominantly cross-sectional and did not synthesize clinical mental-health mediation analyses (Delgado *et al.*, 2022).

The temporal evidence does not support a simple parent-versus-peer competition. In a five-wave Dutch study, adolescent perceptions of parent and best-friend support, negative interaction, and power were reciprocally associated. The 16–20-year cohort had mixed-age overlap with the review range but did not report a separate 15–19 estimate; the study also did not measure mental-health outcomes (De Goede *et al.*, 2009). In a U.S. school-transition study of early adolescents, greater perceived maternal or paternal support was associated with fewer later depressive symptoms when best-friend support was low or average, after baseline adjustment (Ghosh *et al.*, 2024). This is an interaction in an early-adolescent self-report sample, not proof that parental support causes protection.

A three-wave Canadian pandemic-era cohort found that more positive change in parent relationship quality—but not change in peer relationship quality—was associated with lower later depressive symptoms, perceived stress, and emotion dysregulation; there was no statistically significant association with anxiety (Afriat *et al.*, 2023). The unusual pandemic context, separate parent and peer models, and mid-adolescent sample limit generalization. Taken together, the evidence supports direct assessment of both family and peer experience rather than treating either as a replacement for the other.

Parent–peer interactions

Parent–peer interplay may take several forms: shared relationship quality, compensation when one relationship is weak, moderation, and reciprocal influence. The reviewed studies illustrate these possibilities but do not establish a common mechanism. De Goede *et al.* (2009) support reciprocal perceptions of parent and friend relationship qualities in a Dutch cohort. Ghosh *et al.* (2024) support a compensatory pattern in which parent support was most strongly associated with lower later depressive symptoms when best-friend

support was not high. The constructs, ages, measures, and settings differ, so direct comparison is limited.

Future studies should test parent–peer interactions prospectively with adolescent and caregiver reports, repeated measures, and explicit confounder adjustment. Such work should also distinguish supportive friendships from victimization, coercion, substance-related risk, or other adverse peer experiences.

Autonomy support and identity development

Autonomy support, controlling parenting, identity exploration, and identity commitment are related but noninterchangeable constructs. In 408 Japanese mother–adolescent dyads with a mean adolescent age of 13.73 years, mothers’ reported autonomy support predicted higher later adolescent basic psychological-needs satisfaction, while adolescents perceived controlling parenting predicted higher later depressive symptoms (Tanaka *et al.*, 2023). These short-term, multi-informant associations are indirect for late adolescence and do not establish causality.

A six-wave Dutch study provides an important methodological correction. Stable between-family differences, rather than annual within-family deviations, explained the association between autonomy support and adolescent internalizing/externalizing problems (Vrolijk *et al.*, 2020). Adolescents with fewer problems tended to report more autonomy support from both parents, but within-family deviations in autonomy support did not predict later problem behavior, or vice versa. Thus, evidence of a stable family difference should not be converted into a claim that a year-to-year change in parenting caused symptom change.

Identity evidence adds a relational perspective without directly providing mental-health evidence. In a five-wave Dutch multi-informant study beginning at age 14, commitment and in-depth exploration predicted later

improvements in family relationships, while low maternal support predicted reconsideration of commitment, which in turn worsened paternal relationship quality (Crocetti *et al.*, 2017). The accessible report did not describe mental-health outcomes. It supports cautious consideration of identity–relationship links, not a clinical pathway at age 17.

Future-related thinking and mental health

Future-related thinking includes hope, optimism/pessimism, hopelessness, positive future expectations, and future orientation. It is not equivalent to career maturity or motivation. Tang *et al.* (2024) synthesized 22 prospective studies involving 10,682 participants and focused on the 16–19-year transition. More negative future-related thinking and depressive symptoms showed reciprocal longitudinal associations. Negative future-related thinking also predicted later anxiety symptoms, but evidence was insufficient to establish an anxiety-to-future-thinking reciprocal pathway. Construct heterogeneity, inconsistent confounder adjustment, and predominantly observational designs require noncausal interpretation.

A one-year Chinese university-student study found that better self-reported parent–adolescent communication was associated with later career maturity, with concurrently measured time-perspective variables statistically mediating the association (Bi & Wang, 2023). Its sample spanned ages 16.10–24.09, measured no mental-health outcome, and did not establish the temporal order of communication and time perspective. It therefore informs transition-related future planning, not a mental-health mechanism. Table 4 presents all 23 scholarly sources retained in this review and classifies each as age-specific, mixed-age overlap, or indirect developmental/contextual evidence; it is a complete source matrix rather than a selected set of illustrative studies.

Table 4: Complete evidence matrix: all 23 retained scholarly sources, age applicability, design, and inference limits

Source	Design / age class	Review-relevant finding	Primary inference limit
Smetana & Rote (2019)	Annual developmental review. Age class: Indirect developmental/contextual evidence; no 15–19 estimate.	Parent–adolescent relationship quality and autonomy negotiation change across adolescence.	Conceptual/developmental review; no single outcome estimate or Saudi/Gulf evidence.
Ghandour <i>et al.</i> (2024)	Scoping review of 152 Arab-region parenting publications. Age class: indirect developmental/contextual evidence; no direct late-adolescent estimate.	Mapped a large but predominantly cross-sectional regional literature; 89% of publications were cross-sectional.	Evidence map, not an integrated parent–peer causal test; regional heterogeneity.
Zapf <i>et al.</i> (2024)	Systematic review; 37 reports/26 studies; ages 8–21. Age class: Mixed-age overlap; no 15–19 stratum.	Higher adolescent-rated communication quality was associated with fewer mental-health problems.	25 reports were cross-sectional; adolescent-rated communication exposure; no causal conclusion.
Abou Abbas &	Saudi weighted school survey; n = 12,121; ages 10–19.	Poorer maternal or paternal relationship ratings were	Cross-sectional symptom indicators, not diagnoses; no

Source	Design / age class	Review-relevant finding	Primary inference limit
AlBuhairan (2017)	Age class: Mixed-age overlap; no separate 15–19 estimate.	associated with greater odds of sadness/hopelessness and worry.	direct communication or peer measure.
Ioffe et al. (2020)	Prospective U.S. study; mean age 12.49. Age class: Indirect developmental/contextual evidence.	Paternal open communication was associated with lower later anxiety/depression; paternal co-rumination with higher later depression.	Early-adolescent, observational, self-report evidence; not a late-adolescent causal effect.
Chen & Harris (2019)	U.S. Add Health longitudinal cohort; baseline ages 12–21 through adulthood. Age class: Mixed-age overlap; no 15–19 stratum.	Family cohesion and absence of recent parent–child conflict were associated with lower depressive-symptom trajectories.	Self-reported exposures/outcome; long-term association does not identify one causal family pathway.
Alshehri et al. (2020)	Selected Saudi school study; ages 12–16. Age class: Indirect developmental/contextual evidence.	Greater perceived family support was associated with lower total difficulties in an adjusted model.	Nonrepresentative, single-informant, cross-sectional sample; not nationally generalizable.
De Goede et al. (2009)	Five-wave Dutch cohort; 16–20-year cohort plus 12–16 cohort. Age class: Mixed-age overlap; no separate 15–19 estimate.	Perceived parent and best-friend relationship qualities were reciprocally associated over time.	No mental-health outcomes; adolescent self-report and older nonregional evidence.
Delgado et al. (2022)	Systematic review of parent attachment and peer relationships across adolescence. Age class: Indirect developmental/contextual evidence.	Secure parental attachment was associated with more supportive, trusting, intimate, higher-quality peer relationships.	Predominantly cross-sectional; did not synthesize clinical mental-health mediation evidence.
Ghosh et al. (2024)	Two-wave U.S. school-transition study; n = 368; mean age 13.60. Age class: Indirect developmental/contextual evidence.	Greater parent support was associated with fewer later depressive symptoms when friend support was low/average.	Early-adolescent self-report sample; observational interaction, not causal protection.
Afriat et al. (2023)	Three-wave Canadian cohort; ages 14–16 at baseline. Age class: Mixed-age overlap; no separate 15–19 estimate.	Positive change in parent relationship quality was associated with lower later depression, stress, and emotion dysregulation.	Pandemic-era, mid-adolescent cohort; separate parent/peer models; no causal or head-to-head inference.
Tanaka et al. (2023)	Two-wave Japanese mother–adolescent dyads; n = 408; mean age 13.73. Age class: Indirect developmental/contextual evidence.	Maternal-reported autonomy support predicted later needs satisfaction; adolescent-perceived controlling parenting predicted later depressive symptoms.	Short-term observational design; junior-high sample; no father or peer data.
Vrolijk et al. (2020)	Six-wave Dutch cohort; baseline mean age 13.03. Indirect for target ages.	Stable between-family differences, not annual within-family change, accounted for autonomy-support/problem-behavior associations.	Observational; informs level of inference rather than a causal developmental sequence.
Crocetti et al. (2017)	Five-wave Dutch multi-informant cohort beginning at age 14. Age class: Indirect developmental/contextual evidence.	Identity commitment/exploration and family relationship quality showed prospective associations.	Accessible report did not describe mental-health outcomes; nonregional mid-adolescent evidence.

Source	Design / age class	Review-relevant finding	Primary inference limit
Tang et al. (2024)	Systematic review/meta-analysis; 22 prospective studies; N = 10,682; ages 16–19. Age class: Age-specific outcome evidence.	Negative future-related thinking and depressive symptoms showed reciprocal temporal associations; future thinking predicted later anxiety.	Heterogeneous measures and confounder adjustment; insufficient evidence for anxiety-to-future-thinking reciprocity.
Bi & Wang (2023)	Two-wave Chinese university study; n = 442; ages 16.10–24.09. Age class: Mixed-age overlap; no separate 15–19 estimate.	Better communication was associated with later career maturity; time perspective was concurrently measured.	No mental-health outcome; university sample and concurrent mediator measurement.
Khalifa et al. (2018)	Saudi qualitative action-project study; 14 family dyads. Age class: Indirect developmental/contextual evidence; accessible authoritative record does not provide a separable 15–19 estimate.	Transition projects involved education/career, gender roles/marriage, and independence.	Qualitative, small Saudi sample; no mental-health outcome or effect estimate.
Chan et al. (2021)	Systematic review/meta-analysis of GCC child/adolescent mental-health outcome estimates. Indirect/contextual.	Outcome estimates varied substantially by recruitment setting and measurement instrument.	Not a parent/peer mechanism study; no single GCC prevalence estimate should be inferred.
Altwaijri et al. (2023)	Saudi national household survey; youth ages 15–30. Age class: Mixed-age overlap; no 15–19 stratum.	Lifetime diagnostic prevalence and treatment-use estimates inform service context.	Does not isolate age 17 or test family/peer mechanisms; lifetime diagnosis differs from current symptoms.
Yap et al. (2016)	Meta-analysis of parent-primary prevention trials from birth to age 18. Age class: Mixed-age overlap; no 15–19-specific effect.	Pooled benefits were very small for internalizing/depressive symptoms and small for anxiety symptoms.	Prevention evidence is not treatment evidence.
Cardamone-Breen et al. (2018)	Australian RCT; parents of adolescents ages 12–15. Age class: Mixed-age overlap at age 15 only; no 15–19 estimate.	Single-session web feedback improved parent-reported practices but not parent- or adolescent-reported symptoms at 3 months.	Short follow-up and early-adolescent sample; parenting behavior was self-reported.
Smith et al. (2024)	U.S. African American caregiver–adolescent dyads; n = 589; ages 15–19. Age class: Age-specific outcome evidence outcome evidence.	Four profiles based on ethnic-racial socialization, general parenting practices, and relationship quality were associated with depressive-symptom profile membership.	Cross-sectional profile analysis; direction and individual-component effects are not established; nonregional setting.
Saminathen et al. (2026)	Swedish two-wave cohort; n = 2,697; parental warmth/knowledge at age 18; symptoms at age 21. Age class: Age-specific exposure with young-adult outcome.	After mutual and covariate adjustment, parental warmth—but not knowledge—was associated with lower later depression/anxiety probability.	Outcome was measured at age 21; observational, nonregional, two-item exposures; no causal or concurrent 15–19 conclusion.

Note. This matrix accounts for all 23 retained scholarly sources; row inclusion does not indicate equal evidentiary weight. “Age-specific outcome evidence” requires estimates restricted to, or separately reported for, ages 15–19. “Age-specific exposure with young-adult outcome” applies when exposure is measured in the target range but the outcome is later. “Mixed-age overlap” means the pooled sample includes some ages 15–19 without a separate estimate. “Indirect developmental/contextual evidence” informs a construct, limitation, or context without a separable 15–19 estimate.

Saudi and Gulf Context: Evidence, Cultural Considerations, and Research Limitations

Saudi-specific, wider Gulf, and Arab-region evidence should not be treated as culturally homogeneous. The Jeeluna survey provides Saudi school-student evidence on parent-relationship ratings and symptom indicators (Abou Abbas & AlBuhairan, 2017). Alshehri *et al.* (2020) provide a nonrepresentative Saudi school-based, adolescent-report model of support and difficulties. Khalifa *et al.* (2018) provide qualitative Saudi transition-to-adulthood evidence from 14 family dyads; the study describes joint projects around education/career, gender roles/marriage, and independence but does not assess mental health. It is retained as contextual qualitative evidence, not as a direct age-specific estimate.

At the wider regional level, Ghandour *et al.* (2024) mapped 152 Arab-region parenting publications and found that 89% were cross-sectional. Its evidence-gap mapping recommended robust mixed-methods, evaluation, psychometric, youth-engaged, and broader-

outcome research; it should not be read as a Saudi-only or age-17-specific evidence base. Chan *et al.* (2021) found high heterogeneity in assessment-instrument-specific prevalence estimates across GCC child/adolescent studies, with substantial variation in recruitment settings and measures. This supports measurement caution, not a single GCC prevalence claim. Altwaijri *et al.* (2023) provide a national Saudi household-survey context for youth aged 15–30: lifetime diagnostic prevalence and treatment-use estimates are informative for service context but do not isolate 17-year-olds or examine family/peer mechanisms.

The appropriate interpretation is therefore modest. Family structures, educational transitions, gender-related expectations, negotiated autonomy, socioeconomic conditions, and access to confidential care may be relevant contextual factors, but the reviewed sources do not justify a uniform cultural explanation. Local validation of measures, qualitative work with adolescents and families, and prospective studies across school, work, and training settings are needed.

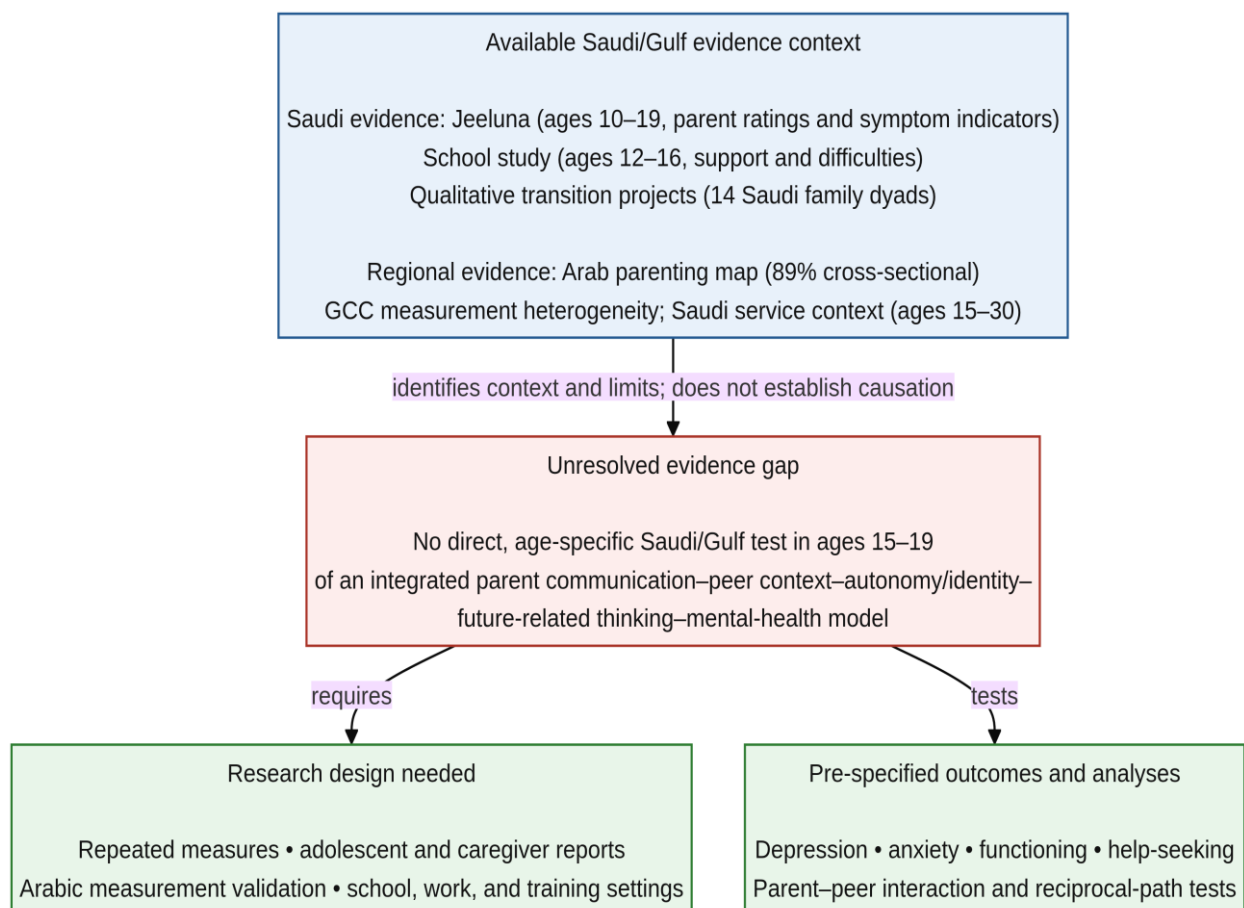


Figure 2: Saudi/Gulf evidence landscape, directional gaps, and research requirements

Note. Arrows show how available Saudi-specific and wider regional evidence identify—not resolve—the central evidence gap, which in turn motivates the specified research design and outcomes. The figure does not imply cultural homogeneity or causal inference.

Table 5: Saudi/Gulf evidence, interpretive caution, and priority research hypotheses

Evidence or hypothesis	Current contribution	Interpretive limit	Feasible next step
Saudi parent-relationship evidence	Jeeluna was a mixed-age 10–19 school survey linking poorer parent relationship ratings with symptom indicators.	No separate 15–19 estimate; school-student, cross-sectional self-report; no temporal parent–peer mechanism.	Prospective Saudi cohorts ages 15–19 with repeated relationship and symptom measures.
Saudi support evidence	Selected school study linked perceived family support with lower total difficulties.	Ages 12–16; selected disadvantaged school areas; single informant; not nationally representative.	Replicate in school, work, and training settings using multiple informants.
Saudi transition evidence	Qualitative Saudi family dyads described negotiated transition projects around education/career, gender roles/marriage, and independence.	Small nonprobability sample; no mental-health outcome, quantitative association, or separable 15–19 estimate in the accessible authoritative record.	Mixed-method studies linking transition experiences, identity, functioning, and help-seeking.
Arab/GCC evidence base	Arab parenting map was 89% cross-sectional; GCC outcome estimates varied by assessment instrument.	Regional settings and measures are heterogeneous; not a uniform Saudi/Gulf mechanism.	Validate Arabic measures and report country, setting, age, and informant before pooling.
H1: Parent–peer compensation	Untested integrated hypothesis: lower peer support may strengthen the association between lower parent relationship quality and 12-month depressive symptoms.	One early-adolescent component finding; no direct 15–19 Saudi/Gulf test.	Baseline/6/12-month cohort; PACS/NRI-type measures; PHQ-9/PHQ-A equivalent; mixed-effects interaction model.
H2: Autonomy/identity pathway	Untested integrated hypothesis: autonomy support and controlling parenting may predict identity commitment/reconsideration and 12-month depressive symptoms.	Component evidence is mainly nonregional and younger-adolescent.	Baseline/6/12-month multi-informant study; validated identity measure; random-intercept cross-lagged model.
H3: Future-related thinking pathway	Tested component temporal evidence: negative future-related thinking and depressive symptoms may be reciprocally associated in ages 15–19.	No Saudi/Gulf integrated test; anxiety reciprocity remains uncertain.	Baseline/6/12/18-month design; future-thinking/hopelessness, PHQ-9/PHQ-A, and GAD-7 equivalents; reciprocal-path model.
H4: Parent–friend reciprocity	Tested component relationship evidence: parent and friendship support, conflict, and power may predict one another across time.	Mixed-age temporal relational evidence; mental-health integration and regional test are absent.	Baseline/6/12/18-month multi-informant panel; relationship-quality measures; random-intercept cross-lagged model.
H5: Reporter agreement and help-seeking	Adolescent–caregiver discrepancy patterns may relate to symptom reporting and 12-month help-seeking.	Hypothesis; scale equivalence and local validity are required.	Baseline/6/12-month parallel reporter measures; help-seeking/referral outcome; agreement or latent-profile analysis.
H6: Implementation evaluation	A confidential adolescent-centered approach with optional safe family involvement may improve 90-day care engagement.	Proposed service question, not a validated regional protocol.	Baseline/3/6-month pragmatic cluster or phased implementation study; referral completion, symptoms, functioning, safety, and acceptability.

Note. H1–H6 are operational research proposals, not tested findings or clinical recommendations. Named instruments are candidate measures requiring Arabic adaptation and psychometric evaluation in the study setting.

Integrated Conceptual Framework

Figure 3 is a hypothesis-generating, testable framework rather than a causal diagram. Its contribution is not simply to place family, peers, development, and

mental health in one picture. It specifies four different propositions with distinct empirical implications: moderation, mediation, and two reciprocal processes. Evidence status is deliberately differentiated: H1 and H2

are untested integrated hypotheses supported only by selected component findings; H3 is tested component temporal evidence from a 16–19 prospective synthesis but remains untested within the integrated framework; and H4 is tested component relationship evidence from a mixed-age cohort but has no mental-health endpoint or regional test. Contextual factors—including age, gender-related context, family environment, socioeconomic conditions, education/work transitions, and access to care—are candidate confounders or effect modifiers rather than assumed explanations.

H1: Parent–peer moderation (compensation; untested integrated hypothesis).

Lower peer support or greater peer adversity is hypothesized to strengthen the association between lower parent relationship quality and later depressive symptoms. This is a moderation claim, not a claim that parent and peer effects are interchangeable. A longitudinal early-adolescent interaction finding supports a component-level plausibility argument (Ghosh *et al.*, 2024), but no direct 15–19-year Saudi/Gulf test of the integrated proposition exists. The proposition would be challenged if a preregistered parent-quality-by-peer-context interaction were null, directionally inconsistent, or eliminated after baseline symptoms and prespecified contextual covariates were modeled.

Operational definition.

The primary outcome is 12-month depressive-symptom severity, measured with a locally validated PHQ-9/PHQ-A-equivalent; anxiety, functioning, and help-seeking are secondary outcomes. Parent communication/relationship quality and peer support/adversity should be measured from adolescent and caregiver reports at baseline, 6 months, and 12 months. The primary test is a preregistered linear mixed-effects model for 12-month depressive symptoms containing the parent-quality $\times$ peer-context interaction, baseline symptom severity, and prespecified socioeconomic, family-structure, and education/work covariates.

H2: Autonomy–identity mediation (untested integrated hypothesis; component evidence only).

Perceived autonomy support and controlling parenting are hypothesized to relate to later depressive symptoms partly through identity commitment or reconsideration. Existing studies support component associations but do not test the complete age-specific mediation pathway (Crocetti *et al.*, 2017; Tanaka *et al.*, 2023). This proposition would be challenged if the temporal sequence is not supported, if identity does not predict later symptoms after baseline adjustment, or if associations remain only between families rather than within-person over time.

Operational definition.

Primary outcomes are identity commitment/reconsideration and 12-month depressive-symptom severity. Parent autonomy support/psychological control, identity processes, and symptoms should be measured at baseline, 6 months, and 12 months using locally validated adolescent and caregiver measures. The primary analysis is a preregistered three-wave random-intercept cross-lagged panel model (or equivalent structural-equation model) that separates stable between-person differences from within-person change; mediation is evaluated only if temporal ordering, model fit, and prespecified indirect-effect criteria are met.

H3: Future-related-thinking–depressive-symptom reciprocity (tested component temporal evidence; untested integrated hypothesis).

More negative future-related thinking and depressive symptoms are hypothesized to predict one another across time. This is the most strongly supported *component* of the framework because Tang *et al.* (2024) synthesized prospective evidence focused on ages 16–19; its integration with parent communication, peer context, and autonomy has not been tested. It remains an observational temporal proposition, not a causal claim. It would be challenged if repeated-measure models that separate stable between-person differences from within-person change showed no reciprocal lagged associations or a consistent one-way association only. The analogous anxiety reciprocity pathway remains speculative.

Operational definition.

Primary outcomes are future-related thinking/hopelessness and depressive-symptom severity; anxiety is a secondary, separately modeled outcome. Locally validated future-orientation or hopelessness and PHQ-9/PHQ-A-equivalent measures should be collected at baseline, 6, 12, and 18 months. The primary analysis is a preregistered random-intercept cross-lagged panel model comparing reciprocal depressive-symptom paths with one-way alternatives; anxiety paths require separate support and are not assumed to be reciprocal.

H4: Parent–friend relationship reciprocity (tested component relationship evidence; untested mental-health integration).

Parent and friendship support, conflict, and power perceptions are hypothesized to show reciprocal change over time, which may shape but is not assumed to mediate mental-health outcomes. A five-wave mixed-age Dutch cohort supports a component-level pattern of reciprocal relationship perceptions (De Goede *et al.*, 2009), but it did not measure mental health and does not establish a 15–19-year regional mechanism. This proposition would be challenged by repeated-measure models showing no cross-lagged parent–friend association, or a stable one-direction pattern that fails to replicate across reporters.

Operational definition.

Primary outcomes are parent–adolescent and friendship support, conflict, and power/relationship-quality scores; depressive symptoms, anxiety, and functioning are secondary outcomes and must be modeled separately from the relationship test. Parallel adolescent and caregiver relationship-quality measures

should be collected at baseline, 6, 12, and 18 months. The primary analysis is a preregistered random-intercept cross-lagged panel model, first within reporter and then across reporters where measurement invariance is adequate, to test reciprocal parent–friend paths without assuming a mental-health mediation pathway.

Candidate adjustment or effect modifiers for every proposition
Age • gender-related context • family environment • socioeconomic conditions
education/work transition • service access

Four distinct, nonsequential testable propositions

H1 • MODERATION

Evidence status: untested integrated hypothesis

Component: one early-adolescent interaction finding

Test: parent quality × peer context → later depressive symptoms

H2 • MEDIATION

Evidence status: untested integrated hypothesis

Component: autonomy–identity associations only

Test: autonomy/control → identity → later depressive symptoms

H3 • RECIPROCAL TEMPORAL ASSOCIATION

Evidence status: tested component temporal evidence; untested integrated hypothesis

Component: prospective synthesis in ages 16–19

Test: future-related thinking ↔ depressive symptoms

H4 • RECIPROCAL RELATIONSHIP ASSOCIATION

Evidence status: tested component relationship evidence; untested mental-health integration

Component: mixed-age temporal relationship cohort

Test: parent relationship quality ↔ friendship support/conflict/power

Interpretation: H1–H4 are parallel propositions; no sequential causal ordering is intended.
The manuscript specifies the study populations, evidence status, and falsification criteria for each.

Figure 3: Testable integrated framework: four parallel propositions

Note. H1 and H2 are untested integrated hypotheses supported by selected component findings. H3 is tested component temporal evidence from a 16–19 synthesis, but remains untested in the integrated framework. H4 is tested component relationship evidence from a mixed-age cohort, but its mental-health integration remains untested. The boxes are parallel propositions, not sequential causal stages; observed temporal associations remain noncausal.

Table 6: Narrative evidence-confidence matrix for the framework's domain-level conclusions

Conclusion / framework connection	Design and consistency	Population relevance and measurement	Confounding and central limitation	Narrative confidence
Parent communication/relationship quality and internalizing symptoms	Several association studies and a systematic review; consistency is broadly directional but designs are mainly cross-sectional.	Mostly adolescent-rated constructs; new direct-age sources are a U.S. cross-sectional 15–19 profile study and Swedish age-18 exposure study with outcomes at 21. Saudi evidence is mixed-age and cross-sectional.	Temporal order, shared-reporter bias, and residual confounding remain plausible.	More supported for association; still limited for causal, regional, or fully concurrent 15–19 effect.
H1: Parent–peer moderation (compensation)	Untested integrated hypothesis; one early-adolescent longitudinal interaction supports component-level plausibility.	No 15–19 Saudi/Gulf study; peer constructs vary across studies.	Interaction may change with covariate adjustment or measurement choice.	Untested integrated hypothesis; direct test required.
H2: Autonomy–identity mediation	Untested integrated hypothesis; prospective component associations exist, but no complete age-specific mediation test was retained.	Younger/nonregional samples; autonomy, identity, and symptom measures differ.	Stable between-family differences and temporal ordering may explain apparent pathways.	Untested integrated hypothesis; component evidence only.
H3: Future-related-thinking–depressive-symptom reciprocity	Tested component temporal evidence: 22 prospective studies focused on ages 16–19; reciprocal depression pathway is more consistent than anxiety reciprocity.	Age-specific synthesis, but future-thinking and symptom measures are heterogeneous.	Observational studies, uneven covariate control, and causal uncertainty remain.	Tested component temporal evidence; integrated pathway remains untested.
H4: Parent–friend relationship reciprocity	Tested component relationship evidence: one five-wave cohort found reciprocal relationship perceptions; no mental-health endpoint.	Mixed-age Dutch sample, adolescent self-report, and nonregional setting.	Stable relationship traits and shared reporter effects may account for cross-lagged associations.	Tested component relationship evidence; mental-health integration remains untested.
Saudi/Gulf application of the integrated framework	Regional evidence includes surveys, qualitative work, and reviews; designs are heterogeneous and mainly cross-sectional.	No integrated, multi-informant, age-specific Saudi/Gulf framework test; measure validity varies.	Cultural transferability, selection, reporting, and confounding are unresolved.	Very limited; contextual rationale only.

Note. This is a descriptive, domain-level narrative confidence assessment based on the retained evidence matrix and source-verification audit. It is not GRADE, CERQual, a formal risk-of-bias appraisal, or a quantitative certainty estimate. “More supported,” “limited,” and “very limited” summarize the joint pattern of design, consistency, population relevance, measurement, and confounding considerations; they do not grade individual studies.

Critical Discussion

The strongest synthesis is that relational quality matters, but its meaning depends on construct, informant, outcome, age, and design. Parent–child

communication quality, family cohesion, and parent relationship ratings are associated with internalizing outcomes across several sources. Yet the most comprehensive communication review was dominated

by cross-sectional reports, and Saudi evidence is cross-sectional. The appropriate conclusion is not that parent communication causes adolescent mental health, but that it is a relevant domain for assessment and a plausible prevention target requiring further evaluation.

The evidence also argues against a false choice between family and peers. Parent and friend relationships can be mutually related, and parental support may be most salient where friend support is limited. However, parent and peer constructs differ across studies, and the reviewed results do not show that peer relationships are unimportant when parent associations are stronger in a particular sample. Longitudinal parent–peer research should distinguish friendship quality, victimization, coercion, social support, and adverse norms.

The distinction between between-family and within-family findings is especially important. A stable association between autonomy support and fewer problems may reflect long-standing family, adolescent, social, or unmeasured contextual differences. A null within-family temporal effect does not make family relationships irrelevant; it cautions against assuming that a short-term change in one reported behavior will produce a predictable change in symptoms. Multiple reporters and repeated assessments are essential because adolescent and caregiver perceptions can diverge.

Future-related thinking provides the clearest age-specific late-adolescent longitudinal synthesis, but its constructs are heterogeneous. The focused update adds direct age-specific relationship evidence, yet it does not change the larger pattern: the Smith *et al.* (2024) study is cross-sectional and the Saminathen *et al.* (2026) study measures outcomes after age 19. Reciprocal associations with depressive symptoms are more supported than reciprocal associations with anxiety. This distinction should guide research and clinical discussion: negative future thinking may be a relevant assessment domain, but uncertainty about future plans is not by itself evidence of a mental disorder.

Saudi/Gulf transferability remains uncertain. The regional literature establishes that adolescent mental-health concerns and family-related correlates merit study, but it does not yet provide an integrated longitudinal model for late adolescence. The value of a contextual approach lies in testing local measurement validity, access barriers, family preferences, and transition settings—not in making unsupported cultural claims.

Clinical Implications

The reviewed evidence supports an adolescent-centered, family-aware assessment rather than a family-only intervention. It does not validate a service pathway, family programme, or clinical protocol for Saudi/Gulf late adolescents. Clinicians should directly and confidentially assess depressive and anxiety symptoms, functioning, self-harm or suicide risk, violence or coercion, sleep, substance use, peer adversity, educational/work participation, and barriers to care. Observable changes in appearance or peer affiliation alone should not be used as mental-health indicators.

Family involvement may be offered when it is safe, acceptable to the adolescent, clinically appropriate, and consistent with applicable confidentiality, consent, and safeguarding requirements. It can support listening, communication, and shared problem solving, but it should not replace indicated assessment, evidence-based treatment, or referral. Preventive parenting-intervention evidence is not late-adolescent-specific: Yap *et al.* (2016) found very small pooled effects for internalizing/depressive symptoms and small effects for anxiety symptoms across child samples from birth to age 18. In an Australian trial involving parents of 12–15-year-olds, a single-session web intervention improved parent self-reported practices but did not improve adolescent- or parent-reported anxiety or depressive symptoms at three months (Cardamone-Breen *et al.*, 2018). Figure 4 is a set of proposed, unvalidated considerations for local discussion and evaluation; it is not a clinical algorithm, decision rule, demonstrated intervention, or substitute for local protocols and professional judgment.

Table 7: Proposed clinical considerations: evidence status and implementation safeguards

Clinical domain	Evidence-informed action	Evidence status	Safeguards and outcomes to monitor
Adolescent-centered assessment	Ask directly about symptoms, functioning, safety, sleep, substance use, peer adversity, violence/coercion, and barriers to care.	Evidence-informed assessment principle; not a trial-tested protocol in this review.	Confidentiality, consent, safeguarding, symptom severity, functional impairment, and adolescent preferences.
Family involvement	Offer family-inclusive listening, communication, and shared problem solving when participation is safe and acceptable.	Indirect prevention evidence; not established as stand-alone treatment for mental disorders.	Voluntary participation, privacy, power imbalance, violence/coercion, and clinical appropriateness.

Clinical domain	Evidence-informed action	Evidence status	Safeguards and outcomes to monitor
Peer context	Assess support, victimization, pressure, online harm, and whether help can be sought safely.	Supported by relational evidence; mechanisms vary across peer constructs.	Avoid treating peer affiliation alone as risk; identify concrete harm, support, and functional impact.
Referral and follow-up	Use indicated mental-health assessment, treatment, safeguarding, and referral pathways when needed.	Standard clinical-care principle; not evaluated as a Saudi/Gulf pathway in this review.	Timeliness, service access, engagement, safety, symptoms, and functioning.
Regional adaptation	Co-design materials and evaluate confidential, family-inclusive approaches with adolescents, families, and services.	Proposed implementation approach requiring local evaluation.	Arabic measure validity, acceptability, equity, gender-related context, outcomes, and unintended harms.

Note. This table distinguishes evidence-supported relational considerations from untested implementation proposals. It is not a clinical algorithm, decision rule, or treatment protocol and must not replace local pathways or professional judgment.

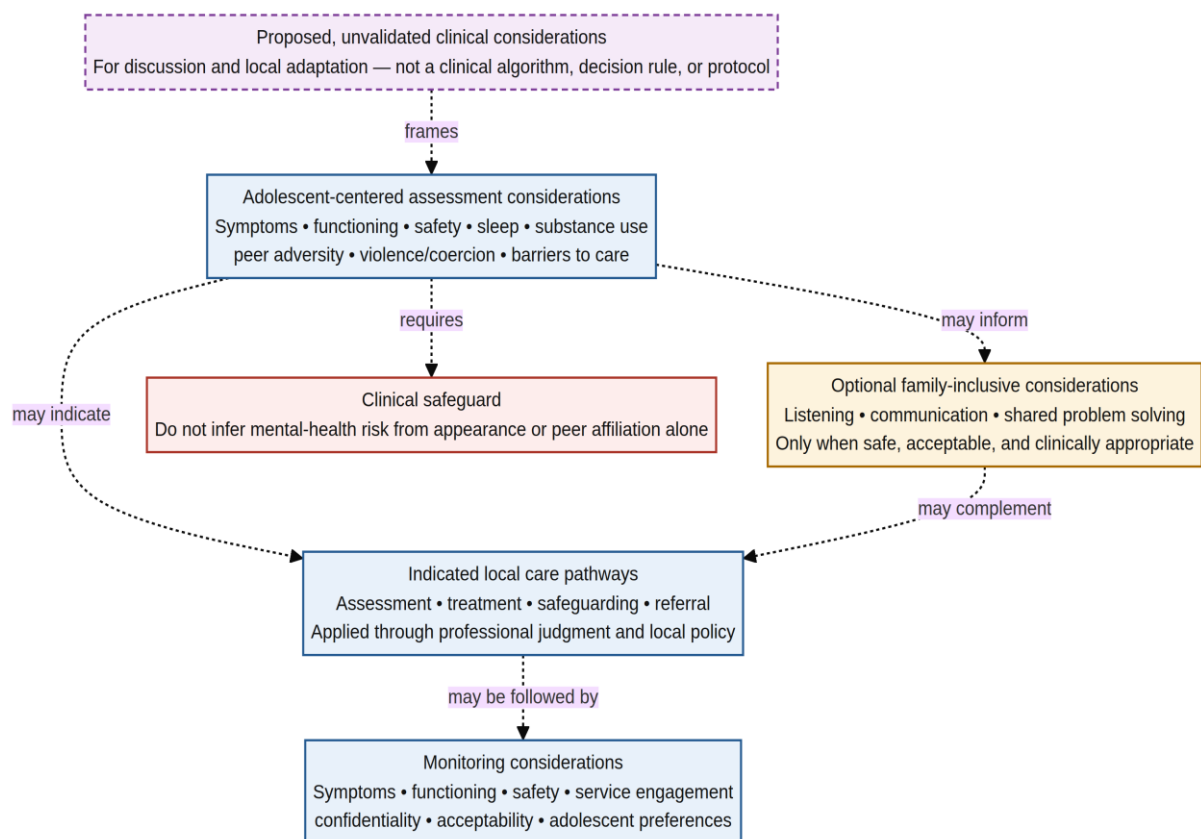


Figure 4: Proposed, unvalidated clinical considerations for local discussion

Note. This is a proposed, unvalidated set of clinical considerations, not a clinical algorithm, decision rule, or Saudi/Gulf protocol. Local pathways, professional judgment, confidentiality, and safeguarding requirements remain controlling.

Future Research Agenda

The following hypotheses are operational proposals, not established clinical recommendations. All candidate instruments require Arabic translation/adaptation and psychometric evaluation in the intended Saudi/Gulf settings before use.

1. Parent–peer compensation hypothesis.

Among Saudi adolescents aged 15–19, lower peer support will strengthen the association between lower parent relationship quality and later depressive

symptoms. Primary outcome: depressive-symptom severity at 12 months, measured with a locally validated PHQ-9/PHQ-A equivalent; secondary outcomes are anxiety symptoms (GAD-7 equivalent) and functional impairment. Candidate exposures: adolescent and caregiver versions of a parent–adolescent communication measure such as the Parent–Adolescent Communication Scale, and peer support/adversity measured with Network of Relationships Inventory domains plus a locally validated victimization measure. Timing: baseline, 6 months, and 12 months. Analysis:

linear mixed-effects models predicting 12-month depressive symptoms, with a parent-relationship-by-peer-support interaction, baseline symptom adjustment, and prespecified socioeconomic, family-structure, and education/work covariates.

2. Autonomy and identity hypothesis.

Perceived parental autonomy support will be associated with later identity commitment and lower depressive symptoms, whereas perceived controlling parenting will be associated with greater identity reconsideration and symptoms. Primary outcomes: identity commitment/reconsideration and 12-month depressive-symptom severity. Candidate instruments: adolescent and caregiver autonomy-support/controlling-parenting scales, the Utrecht-Management of Identity Commitments Scale or a culturally validated equivalent, and a locally validated PHQ-9/PHQ-A equivalent. Timing: baseline, 6 months, and 12 months. Analysis: a three-wave random-intercept cross-lagged panel model or preregistered structural-equation model that separates stable between-person differences from within-person change; mediation is tested only if temporal ordering and model fit support it.

3. Future-related thinking hypothesis.

More negative future-related thinking will predict later depressive symptoms, and depressive symptoms will predict later negative future-related thinking among 15–19-year-olds. Primary outcomes: future-related thinking/hopelessness and depressive symptoms; anxiety is a separate secondary outcome rather than an assumed reciprocal pathway. Candidate instruments: an Arabic-validated future-orientation or hopelessness scale, PHQ-9/PHQ-A equivalent, and GAD-7 equivalent. Timing: baseline, 6 months, 12 months, and 18 months to permit reciprocal-path tests. Analysis: random-intercept cross-lagged panel models with baseline adjustment, comparison of depression and anxiety paths, and sensitivity analyses for school/work transition and socioeconomic conditions.

4. Parent–friend relationship reciprocity hypothesis.

Parent relationship quality and friendship quality will predict one another across time, with reciprocal pathways tested separately from mental-health pathways. Primary outcomes: parent–adolescent and friendship support, conflict, and power/relationship-quality scores; secondary outcomes are depressive symptoms, anxiety symptoms, and functioning. Candidate instruments: adolescent and caregiver relationship-quality measures, Network of Relationships Inventory domains, and locally validated PHQ-9/PHQ-A and GAD-7 equivalents. Timing: baseline, 6 months, 12 months, and 18 months. Analysis: preregistered random-intercept cross-lagged panel models that distinguish stable between-person covariation from within-person cross-lagged associations; reporter-

specific models should precede any cross-informant synthesis.

5. Reporter-agreement and help-seeking hypothesis.

Adolescent–caregiver agreement on communication, autonomy support, and relationship quality will be incomplete, and discrepancy patterns will be associated with symptom reporting and help-seeking. Primary outcomes: agreement/discrepancy profiles at baseline and change in formal or informal help-seeking by 12 months; secondary outcomes are depressive symptoms, anxiety symptoms, and functioning. Candidate instruments: parallel adolescent/caregiver communication and autonomy-support scales, adolescent symptom measures, a locally adapted General Help-Seeking Questionnaire, and verified referral uptake where ethically and practically feasible. Timing: baseline, 6 months, and 12 months. Analysis: multilevel agreement models or latent-profile analysis of reporter discrepancies, followed by regression models predicting help-seeking and outcomes with preplanned moderation by gender-related context, school/work status, and socioeconomic conditions.

6. Implementation hypothesis.

An adolescent-centered service approach combining confidential assessment, optional safe family involvement, and clear referral may improve care engagement compared with usual practice. Primary outcome: completed appropriate referral or documented ongoing care within 90 days; secondary outcomes are 6-month change in symptom severity, functioning, perceived confidentiality, acceptability, and unintended harms. Candidate measures: locally validated PHQ-9/PHQ-A and GAD-7 equivalents, a brief functioning measure, service-use records, and adolescent acceptability/confidentiality scales. Timing: baseline, 3 months, and 6 months. Analysis: a preregistered pragmatic cluster trial or phased implementation evaluation using intention-to-treat mixed-effects or generalized-estimating-equation models that account for service clustering. This proposal requires formal ethics review, safeguarding governance, and local clinical leadership before testing.

LIMITATIONS

Review-level limitations

This targeted narrative review used one database for the documented structured search audit. The first 20 records from each PubMed/MEDLINE ESearch return were preserved in default most-recent order; they documented search execution but did not provide a recoverable record-level selection log or a systematic inclusion frame. A later eight-record focused age-specific query added two sources after transparent screening, but it was not a repeat of the original three-block search and cannot establish comprehensive coverage. The review did not register a protocol, perform

full-result duplicate screening, record all duplicate or full-text exclusions, conduct a formal risk-of-bias assessment, search PsycINFO or Scopus, or provide a comprehensive Arabic-language database search. The final set combined citation tracking, targeted retrieval, and two focused-search additions; backward reference tracking retained one older landmark study. Question-to-source mapping, explicit age classification, source verification, retention of null/qualifying findings, and cross-design comparison were used as selection safeguards, but cannot remove selection bias. These features limit reproducibility and completeness; the conclusions are therefore appropriately qualified as a critical narrative synthesis.

Evidence-level limitations

The evidence base includes many cross-sectional studies, single-informant measures, and heterogeneous age ranges. Direct age-specific evidence remains sparse: Tang et al. (2024) synthesize prospective studies focused on ages 16–19; Smith et al. (2024) provide a cross-sectional 15–19 relationship-profile study; and Saminathen et al. (2026) measure parental factors at age 18 but outcomes at age 21. These sources improve direct coverage but do not provide an integrated 15–19 family–peer–autonomy–future-thinking model or Saudi/Gulf test. Several broader samples include ages 15–19 without reporting separate estimates; neither these samples nor early/mid-adolescent studies provide age-specific evidence. Studies use different constructs and measures for communication, relationship quality, autonomy, future thinking, symptoms, and functioning. Observational temporal associations can still reflect residual confounding or bidirectional influence. Saudi/Gulf evidence includes school, qualitative, household-survey, and regional meta-analytic sources, but longitudinal, multi-informant, age-specific, and intervention evidence remains limited. These limitations prevent causal claims and limit transferability across settings.

CONCLUSION

The reviewed literature supports a focused conclusion: parent–adolescent communication, parent relationship quality, peer context, autonomy-related processes, and future-related thinking are interconnected domains relevant to late-adolescent emotional adjustment. The strength and meaning of the evidence differ across outcomes, ages, reporters, and designs. The direct 15–19 evidence boundary is narrow: the clearest age-specific temporal synthesis concerns future-related thinking and depressive symptoms; one direct family-relationship study is cross-sectional; and one age-18 exposure study assesses outcomes after age 19. No retained study tests the full parent–peer–autonomy–future-thinking model among Saudi/Gulf adolescents aged 15–19.

This review contributes an integrated, hypothesis-generating model, explicit age-applicability rules, and a transparent Saudi/Gulf research agenda. Future regional studies should recruit adolescents aged 15–19 across education and work settings, use repeated multi-informant measures, distinguish symptoms from diagnoses and functioning, test parent–peer interactions, and evaluate family-inclusive approaches with confidentiality and referral safeguards. Clinical implications are proportionate practice considerations for local evaluation, not evidence that a family-inclusive service approach is validated or effective in the region.

Required Declarations

Ethics approval and consent to participate: Not applicable. This narrative review analyzed published literature and did not collect human-participant data.

Consent for publication: Not applicable. The manuscript contains no identifiable individual information.

Author contributions

The lead author developed the review question, framework, methods, and first manuscript draft. All authors contributed to critical review of the evidence synthesis and manuscript, approved the final version, and accept accountability for the work. Author identities and full contribution details are provided in the identified submission file for the editor.

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Data availability

Data sharing is not applicable to this article because no primary dataset was generated or analyzed. The documented PubMed/MEDLINE search strings, dates, returned record counts, ordered audit identifiers, overlap check, source provenance, source-use rationale, age-applicability classifications, and final source-level eligibility record are included in Supplementary File 1. Source-verification results are included in Supplementary File 2. Both supplementary files accompany this manuscript as separate submission files and are not embedded within the main manuscript PDF.

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