
Emotional Intelligence: technical evidence appendix

The evidence, source notes, and scoring rationale behind the Emotional Intelligence review.

The Behavioral Scientist · Technical evidence appendix · 15 September 2026

Companion documents: Reader review and scoring rationale.

Book: Daniel Goleman, *Emotional Intelligence: Why It Can Matter More Than IQ*, tenth-anniversary text.

Reviewed content: 1995 main text and 2005 introduction; verified ebook product date 11 January 2012, ISBN 9780553903201.

Evidence cutoff: 15 September 2026.

Frozen selection: EI-SCOPE-20260915-v1.

Review record: The verification and correction sections below describe completed checks and remaining evidence limits. Preparation and AI review do not themselves establish independent human scientific sign-off.

80 min read

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The evidence behind the review

Category	Inputs	Exact percentage	Display
Scientific Accuracy	2, 3, 3	66.666...%	67%
Reference Accuracy	3, 2, 2	58.333...%	58%
Practical Value	3, 2, 3	66.666...%	67%
Overall	23/36	63.888...%	64%

Principal judgment: The book offers several useful, empirically supported ways to understand and develop emotional and social skills. Its comparative master-ability thesis has only partial support. Sustained educational programs and particular psychological interventions alongside medical care support substantial portions of its qualified educational and clinical claims. Important uncertainties remain about comparative importance, causal mechanisms, generalization, durability and the medical payoff forecast.

About this appendix. This document combines source-derived findings with editorial judgments. Its ratings are neither percentages of statements that are true nor estimates of an individual reader’s probability of benefit. A criticism of a reference’s use does not establish dishonesty, fabrication or misconduct. Source access varies and is identified below. The scientific reading and critiques were AI-assisted; independent human scientific review, human fact-checking sign-off and legal clearance are not documented.

Navigate: Reader review · Scoring rationale · Claim register

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Common claim register

The register retains all 28 families selected before detailed external research. A family can contain several different claim–source relationships. These are not 28 independent studies, a random reference sample, or a denominator for estimating the book’s error rate.

The navigation labels have the following meanings:

- **Supported:** the identified finding has appropriate support at the stated population, outcome and scope.
- **Partly supported:** a meaningful finding survives, with important qualifications.
- **Overstated:** an interpretation or extension exceeds the available support; it is not necessarily false.
- **Incorrect:** an identified factual or source–description discrepancy warrants correction.
- **Unresolved:** necessary material or detail was not verified; this is not an error finding.
- **Practical judgment:** advice or an interpretive rule, not a separately demonstrated intervention effect.

Labels apply to the specified subclaims, not indiscriminately to every sentence in a family. The three central scientific grades are separate from these navigation labels.

ID	Fixed family	Navigation judgment	Main reason
EI-A01	What counts as EI?	Partly supported; Overstated	Specified EI measures have positive associations, some added prediction and partial separation from cognitive ability. Their aggregation does not establish the book’s general enabling hierarchy.
EI-A02	IQ contribution and selected life-course cohorts	Partly supported; Unresolved	IQ predicts specified outcomes; selected-cohort illustrations do not allocate the remainder to EI. Several original comparisons remain unrecovered.
EI-A03	Marshmallow test and delinquency comparisons	Partly supported; Overstated; Unresolved	Conditional prediction and broader self-control evidence survive. Private SAT/IQ comparisons remain unverified; the delinquency interpretation was disputed.

ID	Fixed family	Navigation judgment	Main reason
EI-A04	Hope, realistic optimism and persistence	Partly supported; Unresolved historical comparisons	Hope has favorable predictive evidence. The recovered MetLife paper reports the 37% first-two-year sales contrast using retrospective sales records. Its quitting-rate table does not yield twofold quitting; the exact source chain behind the book's account remains unresolved.
EI-A05	Practice, positive affect and flow	Partly supported; Overstated	Practice, some mood manipulations and flow–performance associations matter. Emotional primacy, necessary flow and several historical figures are not established.
EI-A06	Rapid fear learning and neural hijacking	Supported for bounded findings; Overstated for broad anatomy	Animal conditioning and later human anatomy support relevant pathways. Neither proves every outburst is a neural takeover; a lesion counterexample limits universal amygdala necessity.
EI-A07	Self-awareness, alexithymia and somatic markers	Partly supported; Unresolved	Clinical observations and later decision-task disputes do not settle broad ordinary-person or necessary-mechanism claims. Original-source access is incomplete.
EI-A08	Anger, worry and ordinary melancholy	Supported for selected practices; Partly supported overall	Later experiments support avoiding ruminative venting and reducing arousal. Not every exercise, worry or clinical claim received equivalent verification.
EI-A09	Reading emotions and nonverbal communication	Incorrect; Overstated; Supported for a narrower association	The cited physiological-empathy study used observers watching strangers, not their spouses, and did not establish synchronization as necessary for empathy.
EI-A10	Empathy, morality and offending	Partly supported; Unresolved	Emotional understanding and moral action differ. The precise half-recidivism comparison is unrecovered; a related process study cannot replace it.
EI-A11	Contagion, rapport and social effectiveness	Supported in bounded form; Unresolved for extensions	A mood-convergence experiment is real; it does not establish universal dominance, rapport mechanisms or anecdotal de-escalation laws.
EI-A12	Marriage prediction, repair and safety	Supported number; Overstated generalization	The 94% figure matches development-sample classification. It is not independently validated individual prediction or proof that the proposed repair practices prevent divorce.
EI-A13	Constructive criticism and workplace prejudice	Supported in bounded form; Unresolved attribution details	Primary previews support criticism-style and repair effects. The opening anecdote, laboratory studies, workplace ratings and advice are distinct sources.

ID	Fixed family	Navigation judgment	Main reason
EI-A14	Team intelligence, star performance and leadership	Partly supported; Overstated	Group-process and EI associations have support, but the book's group-harmony ranking and contextual leadership comparison remain incompletely verified. A later collective-intelligence correction must be retained.
EI-A15	Psychoneuroimmunology and the common cold	Supported clinical association; Overstated mechanism	The cold percentages are approximately faithful and clinically measured. Stress was not assigned, and the study did not establish the claimed immune-suppression pathway.
EI-A16	Disease risk, hostility and smoking comparison	Partly supported; Overstated; Unresolved	Risk associations and some cardiac intervention benefits coexist with null trials. The smoking-sized preventive payoff remains a forecast, not a demonstrated return.
EI-A17	Isolation, support and physical health	Supported relevance; Partly supported intervention extension	Social relationships predict health and some support programs improve survival. Network structure, loneliness and different intervention types must remain distinct.
EI-A18	Breast-cancer support groups and survival	Supported original number; Partly supported current clinical extension	The original rounded means are correct; later specific trials did not reproduce overall survival benefit. A favorable 2026 broader synthesis materially qualifies a negative verdict.
EI-A19	Expressive writing and humane medical care	Partly supported; Incorrect for intervention description	Some clinical benefits exist. Writing outcomes vary; the hip-fracture account narrows a psychiatric screening/consultation service to depression therapy. Its nonrandom design limits causal inference; the book does not say it was randomized.
EI-A20	Ornish's multicomponent program	Supported package result; Overstated component attribution	Coronary narrowing improved in a small combined lifestyle trial. Relaxation's separate contribution, plaque-volume removal and survival were not demonstrated.
EI-A21	Parent coaching, readiness and maltreatment	Supported teaching core; Partly supported broader claims	Later randomized parent programs strengthen the practical case. Original family associations, comparative readiness claims and intergenerational risk need qualifications.
EI-A22	Temperament, neural plasticity and clinical relearning	Supported bounded change; Partly supported mechanism	Anxiety outcomes improved without a statistically detected effect on inhibition. Clinical benefit and brain correlates do not verify every proposed developmental mechanism.
EI-A23	Targeted aggression, depression and friendship programs	Partly supported; Overstated selected generalizations	Several targeted benefits are credible. Booster-specific, comparator-specific and population-specific limits matter; some exact figures remain unverified.

ID	Fixed family	Navigation judgment	Main reason
EI-A24	Emotional deficits and psychiatric/developmental risk	Incorrect source design; Overstated causal extension	The cited Leon 1993 paper was cross-sectional. Later prospective evidence supports vulnerability, not the book's complete causal chain.
EI-A25	Common prevention ingredients and school-wide transfer	Partly supported	Sustained packages can help, including some adult outcomes. Shared ingredients, mediator models and prevention knowledge do not establish every proposed mechanism or endpoint.
EI-A26	All six Appendix F program families	Partly supported, with program-specific strengths and gaps	All six remain assessed. Designs, raters, components, outcomes and follow-ups differ substantially.
EI-A27	Anniversary SEL meta-analysis and PATHS mechanism update	Supported later benefits; Overstated available mechanism; Unresolved original versions	Later school evidence is favorable. The 668-study calculations remain unavailable; published Riggs outcomes do not verify the introduction's described academic mechanism.
EI-A28	Childhood-decline narrative and its correction	Supported author correction; Overstated broader interpretation	The later survey supports Goleman's acknowledgment of improvement. These are behavior-problem measures, not a continuous global EI trend or proof of its cause.

01 Verdict in context

What the book is being judged as

Emotional Intelligence is a popular scientific synthesis and an argument for deliberate cultivation of emotional and social capacities. Its intended audience includes interested general readers, parents, educators and managers, with explicit extensions into medical care and public prevention. It is not merely a collection of inspirational anecdotes, and it is not a complete clinical treatment manual.

The review therefore asks two different questions. First, does the book's scientific explanation support the importance and benefits it attributes to emotional intelligence? Second, does its practical guidance contain methods that can help readers or institutions accomplish worthwhile goals? A weakness in the explanation does not automatically erase a demonstrated intervention benefit. A useful intervention does not automatically validate the explanation.

The strongest practical material concerns recognizing emotion without surrendering to it, avoiding destructive criticism, responding constructively to children, and organizing sustained emotional and social learning. The evidence for several of these practices is stronger now than the original book's evidence alone could establish.

The most important scientific limits concern the movement between different constructs, the interpretation of selected predictive comparisons, the conversion of observed relationships into causes, and the attribution of a program's effects to a particular mechanism. Some

reference problems are more concrete: the wrong participants or study design appear in the book's description of identifiable research.

Recommendation and confidence

Recommendation: read selectively for emotional regulation, parenting, interpersonal practice and the case for sustained education. Do not treat the book as a settled hierarchy of the abilities that determine success, or take its most dramatic medical and developmental examples as portable forecasts.

Confidence: moderate in the overall appraisal. Several consequential discrepancies and favorable findings are well established at the documented access level. Confidence is lower for historical comparisons that depend on unrecovered books, conference presentations, private analyses or unpublished manuscripts.

Ease of application: variable and descriptive, not scored. Changing a feedback conversation can be comparatively easy to begin. Implementing a school curriculum, preventing psychiatric problems in selected children, or providing structured psychological care involves training, time, supervision and an appropriate setting. Continuing support is not inherently a defect; its value depends on whether the resulting benefits justify its burden.

Clinical grade adjudication

The physical-health claim receives **3/4**. Appropriate intervention evidence supports much of the unchanged claim that medically meaningful benefits can occur alongside standard care, including survival benefits in some settings.

The smoking-cessation-sized preventive payoff remains unestablished and consequential. It is not discarded from the claim, treated as demonstrated because it is conditional, or converted into a requirement of universal clinical efficacy. Section 09 sets out the strongest case for both adjacent grades and explains why 3 is preferred.

02 Scope and method

Edition and source baseline

The reviewed text contains the **1995 main work** and the **2005 tenth-anniversary introduction**. The supplied title page identifies the tenth-anniversary edition. Publisher format metadata in the supplied source supplement verifies an **11 January 2012 ebook product date**, ISBN 9780553903201. That product date does not establish a newly researched 2012 main text.

The publisher's product description and sample checked during preparation referred to different anniversary material. They were not substituted for the supplied edition. The relevant publisher metadata record is:

Penguin Random House format record for ISBN 9780553903201

The source–book copyright and publishing–history anchors are `text00001.html ¶004–007, ¶017–021`.

Reading coverage

The scoping–stage record documents a complete reading of the supplied book extraction: all 39 retained non–advertisement source documents, including the introduction, prologue, 16 chapters, appendices A–F, Resources, acknowledgments and Notes. The 433 numbered paragraphs in the Notes source are an extraction count, not a count of independent studies or distinct references.

The title–page image and sole scientific illustration were inspected in the preceding research stage. The illustration in chapter 2 depicts visual threat input, thalamic and cortical routes, the amygdala and bodily mobilization. It is an explanatory schematic, not experimental imaging evidence. Its caption is `text00007.html ¶036`; the source credit is `text00001.html ¶012–015`.

Four third–party advertisement inserts were excluded from the supplied extraction and reported as logged. The end–page marketing logo contributes no scientific evidence. The original XHTML print–style anchors were not independently converted into conventional page references.

This appendix draws on the reader review, source critiques and accumulated research records. Preparation of the appendix did not constitute another complete reading of the book or a fresh full–paper reading of every external source. The verification section identifies the completed checks and remaining limits.

Locator convention

Every locator of the form `text00018.html ¶104` identifies a stable paragraph in the supplied extraction. It is **not a print page**. `text00035.html` contains the numbered Notes material. Complete chapter ranges and targeted claim ranges appear in Sections 04–06.

Claim selection and audit design

The three central claims and 28 audit families were recorded under **EI–SCOPE–20260915–v1** before detailed external research. Their wording remains unchanged.

Selection was purposive. It sought the organizing thesis, the principal teaching promise, a consequential clinical extension, and significant source relationships across the entire book. Favorable examples were deliberately retained, particularly A06, A08, A13, A15, A23, A26 and A28.

This is not a randomized citation audit, registered systematic review, independent scientific preregistration or estimate of the prevalence of errors. Multiple reports from one cohort are not independent replications. Multiple outcomes from one program are not independent confirmations of every component of EI.

The supplementary neuroscience strand is assessed within the central claims, chapter assessment and family audit. It is not a fourth hidden scientific score.

Evidence distinctions

The assessment keeps five questions separate:

1. **Construct validity:** what capacity, trait, self-perception or behavior was measured?
2. **Prediction:** what outcome did it predict, and what did it add beyond specified alternatives?
3. **Causality:** what design identifies the effect of changing the proposed cause?
4. **Intervention benefit:** what happened when a specified program was offered or delivered?
5. **Mechanism and generalization:** why did that program help, and what follows for other settings, outcomes or people?

A successful multicomponent intervention is relevant when the book advocates sustained work across families, classrooms and communities. It is not disqualified because it contains more than emotional instruction. A separate component-identification problem arises only when the book attributes the result specifically or uniquely to an ingredient or neural mechanism that was not isolated.

Likewise, a clinical claim that psychological care can help in some settings does not require proof of a single universal EI treatment. It still requires appropriate evidence for the claimed kinds of benefit.

Access terminology

The following labels describe the source access documented in the coordinated preparation:

- **M — Main text:** the article body was read; this does not automatically include every reference, supplement or dataset.
- **S — Selected primary sections:** named methods, results, tables or discussion passages were inspected.
- **A — Abstract:** the complete publisher-, author- or PubMed-supplied abstract was examined.
- **P — Preview:** a specified original first page, publisher preview or indexed excerpt was examined.
- **I — Identity only:** bibliographic or notice identity was established without reading the substantive argument.
- **U — Unrecovered:** necessary original content was not obtained.

Unless explicitly stated otherwise, these labels summarize source access documented by the research records. They do not mean that every original was reopened during each revision. Initial source checks and later checks by other AI readers are distinguished where material; later access does not retroactively enlarge an earlier check.

Search and computation boundaries

The coordinated research used primary publishers, author repositories, PubMed/Europe PMC, source-linked papers, exact-title and DOI searches, and selected correction, retraction and response checks through **15 September 2026**. It was not an exhaustive search of every database or every original endnote.

This appendix draws on accumulated research records rather than a new broad literature search. Those records were critically assessed: the Fast Track summary includes favorable and adverse results, access upgrades remain source-specific, and the clinical grade was assessed against the frozen proposition. The verification section identifies the source checks and access limits.

Simple arithmetic involving the scores and selected reported counts was checked. No participant-level analysis, repaired meta-analysis, independent model estimation or execution of the Robinson–Zell code was performed.

03 Strongest fair reading

Goleman argues that academic ability alone does not prepare people to manage frustration, understand others, sustain effort or maintain relationships. These capacities deserve explicit cultivation. He uses emotional intelligence to organize related research that had often been discussed separately.

The strongest reasonable version of this argument is neither “feelings should replace thought” nor “IQ is irrelevant.” The book seeks intelligent coordination of emotion and thought. Its teaching examples generally involve repeated practice, adult modeling, feedback and supportive environments rather than a single lesson or positive affirmation.

Several qualifications are central to the review.

IQ and EI. The original chapter admits that EI’s explanatory share is unknown. The anniversary introduction explicitly rejects assigning EI the residual 80% of success left unexplained by an IQ estimate. It names family resources, temperament and luck among other influences. It also rejects universal superiority to IQ and acknowledges the cognitive requirements of demanding professions. These qualifications occur at `text00009.html ¶008–019`, `¶056–062` and `text00003.html ¶024–038`.

Multiple models. The introduction distinguishes ability, well-being-oriented and competency-oriented EI models. The original chapter openly uses ego-resilience as a surrogate. This prevents an accusation of concealed substitution, although it does not itself establish that the constructs are equivalent. The book should not be criticized simply for failing to make every useful social skill one psychometric factor.

Emotion regulation. The book’s aim is appropriate emotion, not emotional suppression or permanent calm. It distinguishes ordinary sadness and worry from conditions requiring professional treatment, and it includes conditions under which expression, reappraisal and

action differ. See [text00004.html ¶028](#) and [text00011.html ¶010–018, ¶023–051](#).

Parenting and temperament. Repeated patterns matter more than a single parental mistake. Genes and temperament constrain development without fixing every outcome. Adults can learn, treatment need not erase all vulnerability, and several neural explanations are presented tentatively. See [text00020.html ¶011–022](#), [text00021.html ¶039–040, ¶065–069](#), and [text00022.html ¶024, ¶035–039, ¶049–055](#).

Education. Goleman’s preferred approach starts early, fits development, continues through school, and connects home, school and community. He anticipates teacher discomfort, training requirements, curriculum opportunity costs, poor programs and children’s privacy objections. Recognizing those objections is not the same as providing a complete consent or confidentiality framework. See [text00025.html ¶129–139](#).

Social causes. Emotional difficulties are not offered as the sole explanation for poverty, psychiatric illness or crime. The book recognizes biological, family, political and economic contributions and admits that some prevention programs fail or cause problems. See [text00024.html ¶125–141](#).

Clinical care. Chapter 11 explicitly says benefits do not occur in every case or condition, rejects happiness as a cure and rejects blaming patients for disease. It distinguishes immune-marker changes from clinical importance and permits behavioral explanations for health associations. Its conclusion makes more humane care the minimum and faster recovery a possibility for some. See [text00018.html ¶014–017, ¶029, ¶040–043, ¶060, ¶069, ¶073, ¶104–110](#).

Self-correction. The anniversary introduction acknowledges that later child-problem data improved and that the earlier social explanation therefore no longer fits neatly. Resources also names *Emotional Intelligence: Science and Myth* as a thoughtful critique. These are real counterweights to a portrayal of the book as uniformly dismissive of contrary evidence.

Those qualifications do not remove the book’s ambitious claims. It still presents EI as a general enabling capacity, offers broad educational and adult benefits, makes strong neural statements, and proposes potentially large preventive medical returns. Both the qualifications and the extensions are assessed.

04 Unchanged central claims and assessments

EI-C1 — Comparative importance and breadth

Frozen proposition

Emotional intelligence is a set of largely IQ-independent emotional and social capacities that acts as a meta-ability, helping determine how effectively people use their other abilities and conferring advantages across life’s domains. It can matter as much as or more than IQ, especially for emotional and relational outcomes and for distinguishing outstanding leadership among already intellectually selected professionals.

Extraction anchors

- text00009.html ¶011: EI can be as powerful as, and sometimes more powerful than, IQ; its explanatory share is unknown.
- text00009.html ¶018: emotional aptitude is described as a meta-ability governing use of other skills.
- text00009.html ¶048–062: practical domains, IQ/EI profiles and the disclosed ego-resilience surrogate.
- text00003.html ¶024–038: rejection of the 80% myth, qualification of academic comparisons, selected-professional leadership claim and competence distinction.
- text00012.html ¶011, ¶022, ¶080: emotional regulation and motivation extended into a general enabling role.

Supporting evidence

Specified EI measures relate positively to academic performance, workplace outcomes and broader functioning. Some add prediction beyond intelligence and personality. MacCann et al. found an additional 1.7 percentage points of academic-performance variance for ability EI after those controls. O’Boyle et al. reported favorable increments for some self-report and mixed streams in work-performance models.

The cognitive-ability correlations summarized in A01 support partial separation between these EI measures and conventional cognitive tests. This is distinct from—and receives credit separately from—added prediction of an outcome after IQ is controlled.

Robinson and Zell’s 2026 synthesis provides substantial favorable evidence across measurement classes. Its overall association of approximately .28 survives several dependence-sensitive analyses, and positive associations remain across different predictor/outcome measurement methods. The result is not adequately described as entirely a same-questionnaire artifact.

Constituent capacities also have their own evidence: repeated childhood self-control measures predict adult outcomes, hope adds some academic prediction, and social coordination is associated with group performance. These findings are relevant rather than disposable simply because they are not all measured by one EQ test.

Limits and competing explanations

The umbrella covers performance abilities, self-perceptions, personality characteristics, motivation and work habits. The observed separation from cognitive ability supports a distinct contribution, but correlations involving these different constructs do not establish the book’s broader comparative and enabling explanation. The issue is not a demand for a single factor; it is whether the particular evidence warrants those claims.

Joseph et al.’s expanded-control analysis illustrates overlap: mixed EI’s positive supervisor-performance correlation remained, while its adjusted coefficient fell close to zero after including ability EI and several self-perception and personality variables. That does not disprove every EI measure, but it changes the interpretation of broad mixed inventories.

Selected–professional comparisons also require care. Restricted cognitive range can reduce observed IQ discrimination without establishing that EI predicts outstanding leadership as well as or better than IQ within that selected group. Bell Labs’ interview–derived work strategies and leadership competency lists are not direct estimates of a validated EI measure outperforming independently measured IQ on new leadership outcomes.

The comparison with IQ for emotional and relational outcomes remains incompletely verified in this preparation. Academic relative–importance findings cannot be extended to romantic relationships, and a broad flourishing correlation is not that head–to–head comparison.

Assessment

C1 = 2 — Partly supported.

Meaningful predictive contributions exist, including adjusted and cross–method findings. Direct correlations also support partial separation from conventional cognitive ability. These findings support distinct contributions from emotional and social capacities. The evidence examined does not establish the general enabling role across domains or the stated EI–versus–IQ comparisons for emotional and relational outcomes and outstanding leadership among intellectually selected professionals. Different definitions require matching findings to the capacity measured; personality overlap alone does not count against this frozen claim. Those comparative and explanatory gaps concern central parts of the proposition, so support remains partial.

Primary evidence and limits: A01–A05, A07 and A14.

EI-C2 — Teachability, development and broad prevention

Frozen proposition

Emotional competencies can be taught and improved, particularly through sustained, developmentally appropriate learning at home and school. Well–designed emotional–literacy or social–and–emotional–learning education can improve academic performance and behavior, reduce a broad range of later problems, and make better adult relationships, work and citizenship more likely. Repeated experience shaping emotional habits and developing brain circuitry is the book’s proposed explanation for these benefits.

Extraction anchors

- text00004.html ¶017–027: teaching and developmental promise.
- text00020.html ¶019–022: parent coaching and its claimed range of benefits.
- text00022.html ¶029–056: temperament, experience and plasticity.
- text00024.html ¶124–141: prevention ingredients and partial remedies.
- text00025.html ¶129–139, ¶152–188: sustained implementation, school outcomes and anticipated adult benefit.

- text00003.html ¶012–015: anniversary school synthesis and proposed neurocognitive explanation.

Supporting evidence

The evidence includes direct tests, not only correlations. Randomized parent programs improve some parenting practices and child emotional and behavioral outcomes. Targeted interventions can reduce diagnosed affective-disorder incidence, improve some aggressive children's later outcomes, or prevent anxiety without a demonstrable change in temperament.

School syntheses support positive average effects on several intended outcomes. Durlak's 2011 academic estimate came from 35 studies within a wider 213-study corpus. Ha's 2025 outcome-specific analysis reports a smaller but favorable academic average, $g=.101$, using 40 studies from the older Cipriano corpus. These are modest, heterogeneous benefits rather than either no benefit or universal transformation.

Some sustained programs reach adult outcomes. Seattle's follow-up is encouraging but quasi-experimental. Fast Track supplies randomized-package evidence at age 25, mixed age-31 total effects and a favorable, model-based intergenerational component-path result in 2026.

These extensive packages are relevant to Goleman's proposal for coordinated, continuing work with children, parents, schools and communities. Their complexity does not make them irrelevant.

Limits and competing explanations

Different curricula target different combinations of instruction, classroom management, family support, social skills and emotional knowledge. Their effects cannot be assigned automatically to an isolated emotional component. That is a limit on a specific mechanism attribution, not a requirement that all useful programs contain only one ingredient.

Follow-up evidence varies by outcome and time. The maximum follow-up in a synthesis is not the duration of all included studies. Positive subgroup or mediator findings are not substitutes for the complete randomized assignment contrast. The age-31 Fast Track results are mixed, not uniformly null and not uniformly favorable.

The proposed neural explanation is less directly established than the teaching benefits. The published Riggs study measured inhibitory control, verbal fluency and teacher-rated behavior. It did not measure academic attainment, a dedicated working-memory test or brain imaging. Its results cannot verify the introduction's particular academic-neural account. The exact 2005 manuscript remains unavailable.

The claim's "can" and "well-designed" qualifications must remain operative. It is not assessed as a promise that ordinary brief lessons reliably transform every child or that every program improves every outcome.

Assessment

C2 = 3 — Substantially supported, with qualifications.

Appropriate evidence supports much of the sustained teaching, behavioral, academic and targeted-prevention proposition, with some evidence extending into adulthood. The incomplete coverage of adult relationships, work and citizenship, heterogeneous program results, and limited verification of the proposed neural explanation prevent full credit.

Primary evidence and limits: A08, A21–A28, with prediction-versus-intervention distinctions from A03–A05.

EI-C3 — Physical disease, recovery and medical care

Frozen proposition

Improving the management of chronic distress and the quality of emotional support can deliver medically meaningful disease-prevention, recovery and, in some settings, survival benefits alongside standard medical care—not merely greater comfort. Goleman proposes that reducing chronic harmful emotions could potentially have a medical payoff comparable to helping heavy smokers quit.

Extraction anchors

- text00004.html ¶023 and text00003.html ¶028–029: medical importance in the book’s framing.
- text00018.html ¶014–017: qualified additional medical benefit and explicit rejection of positive-thinking cures and patient blame.
- text00018.html ¶032–045, ¶048–083: risk evidence and clinical interpretation.
- text00018.html ¶088–089: original metastatic-breast-cancer survival account and comparison with medication.
- text00018.html ¶100: Ornish’s combined program.
- text00018.html ¶103–110: prevention, emotional care and the conditional smoking-sized payoff.

Supporting evidence

Stress and social relationships have credible physiological and clinical associations. More importantly for the intervention claim, some randomized psychological programs improve physical outcomes.

RCPP and SUPRIM report fewer recurrent cardiac events under substantial behavioral or cognitive-behavioral programs. ENHANCED reports fewer secondary composite clinical events when stress management was added to cardiac rehabilitation. Patient-clinician and preoperative-preparation trials support some clinical benefits beyond satisfaction alone.

The 2021 psychosocial-support synthesis and 2026 cancer synthesis report favorable pooled survival findings. The latter includes structured psychological interventions across cancers and stages, with a main death hazard ratio of .80. This evidence prevents reducing the entire clinical case to comfort or claiming that survival benefit has been ruled out.

Limits and competing explanations

ENRICHED improved depression and perceived support without improving its main death/recurrent-infarction outcome. Several later metastatic-breast-cancer trials did not reproduce the original overall survival advantage. These findings constrain expectations; they do not negate a claim explicitly allowing benefit only in some settings.

The support syntheses are heterogeneous. Smith's program-type difference concerns fixed-time survival; its separate time-to-event analysis, based mostly on social/emotional interventions, also favors support. The 2026 cancer synthesis has bias and heterogeneity concerns, and some alternative statistical adjustments for possible publication bias are inconclusive.

Broad primary prevention through emotional education remains less established than treatment and secondary prevention in existing illness. The resemblance between risk associations for smoking and distress does not establish equal returns from interventions. The smoking statement is an important, untested forecast.

The frozen clinical proposition does not require a unitary EI capacity to be the sole active ingredient. Studies of distress management and structured support can bear on that proposition without validating the book's entire EI construct. Component uncertainty still matters wherever a particular effect is attributed specifically to relaxation, emotional expression or a named biological route.

Assessment

C3 = 3 — Substantially supported, with qualifications.

Direct intervention evidence supports much of the qualified clinical core, including meaningful physical benefit and bounded survival effects. The broader primary-prevention extension and smoking-sized forecast prevent full credit.

A grade of 2 is defensible if the broad preventive forecast is given greater weight within the compound claim. This appendix prefers 3 because the direct support reaches several of the claim's substantive clinical clauses, not merely a peripheral comfort benefit. Section 09 gives the full adjacent-grade adjudication.

Primary evidence and limits: A15–A20.

05 All-chapter and appendix assessment

Complete ranges below describe the book coverage. External source verification within those ranges is uneven and is specified in Section 06; complete chapter reading is not a claim that every sentence received a full primary-source audit.

Section and complete extraction range	Strengths and consequential assessment
Tenth-anniversary introduction — text00003.html ¶001–048	Its explicit rejection of the 80% myth and unrestricted EI superiority materially improves the strongest fair reading. The childhood-trend correction deserves credit. Its selected-leadership claim remains stronger than the comparative evidence verified here. The 668-study source is unrecovered, and the available Riggs publication does not verify the described academic/working-memory mechanism.
Aristotle's Challenge — text00004.html ¶001–028	Clearly states the value of appropriate rather than absent emotion and introduces teaching as a practical aim. The global social diagnosis, moral language and chain-smoking analogy are broader than the evidence established by the examples. The opening stories are illustrations, not measurements of a societal EI decline.
1. What Are Emotions For? — text00006.html ¶001–055	Presents action readiness and interaction between feeling and thought in an accessible form. Culture is acknowledged. The layered evolutionary narrative and species-wide assertions in ¶043–054 were not independently checked species by species; they should not be presented as verified modern comparative neuroscience. The source notes acknowledge uncertainty concerning distinct physiological signatures.
2. Anatomy of an Emotional Hijacking — text00007.html ¶001–077	Bounded animal-conditioning evidence and later human anatomical findings support relevant pathways. The diagram is explanatory, not experimental. “All passion” depending on the amygdala at ¶016 is too sweeping in light of the lesion counterexample. Notes preserve residual feeling, animal-to-human uncertainty and distributed circuitry. Specific frontal “off-switch” and autobiographical-outburst explanations are not all verified.
3. When Smart Is Dumb — text00009.html ¶001–062	Correctly distinguishes intellectual achievement from guaranteed success in every domain. It admits an unknown EI share and discloses its surrogate. The 20% framing does not identify the residual, and selected cohorts do not establish a broad EI hierarchy. Current EI prediction earns credit without validating all of the meta-ability interpretation.
4. Know Thyself — text00010.html ¶001–047	Nonreactive awareness is usefully distinguished from becoming immersed in emotion. Clinical deficits, attention to feelings, emotional knowledge and regulation should remain distinct. Several original sources are inaccessible, and the later somatic-marker dispute does not settle every ordinary-person claim. Difficulty identifying emotion is not a moral verdict.
5. Passion's Slaves — text00011.html ¶001–105	The critique of indiscriminate catharsis, attention to arousal and professional-care boundaries are practical strengths. Later anger evidence supports selected recommendations. It does not validate every exercise recommendation or every explanation of worry, melancholy and repressive coping. Clinical depression is not treated here as merely a bad mood to regulate alone.
6. The Master Aptitude — text00012.html ¶001–080	Self-control, hope, realistic optimism, practice and flow each have relevant evidence. The problem is their conversion into a general emotional master aptitude. Waiting-task predictions are conditional; several striking comparisons depend on private sources. The practice comparison is substantially faithful and is not a universal 10,000-hour rule. The MetLife and candle-task source relationships require qualification.
7. The Roots of Empathy — text00013.html ¶001–066	Takes emotion recognition and relationships seriously and acknowledges multiple routes to antisocial behavior. The physiological-empathy description changes the observed relationship and overstates necessity. The “90%” claim concerns emotional messages, not all communication. The half-recidivism figure is unresolved, not disproved.
8. The Social Arts — text00014.html ¶001–091	Includes useful distinctions between social effectiveness, authenticity and integrity. Mood convergence has bounded experimental support. Display rules, rapport, group entry and de-escalation are not one experimentally established mechanism. The train story remains an acknowledged anecdote, not evidence of a general law.

Section and complete extraction range	Strengths and consequential assessment
9. Intimate Enemies — text00016.html ¶001–091	Behavior-focused complaints, repair and de-escalation offer useful guidance. The 94% number is faithfully rounded but comes from development-sample classification. Gender generalizations, physiological thresholds and the prevention of divorce require more than predictive associations. The endnote distinguishing calculated violence prevents treating ordinary conflict advice as a prescription for victims to manage abuse.
10. Managing with Heart — text00017.html ¶001–075	Constructive criticism and repair have primary support, with laboratory and rating limits. The opening engineer anecdote is attributed to Weisinger, not Baron's experiment. Workplace prejudice claims should distinguish stated norms from durable change. Bell Labs and the group-performance material do not establish the specified EI-versus-IQ leadership comparison or the book's strongest group-harmony ranking.
11. Mind and Medicine — text00018.html ¶001–110	Strong anti-blame and anti-cure-all qualifications coexist with ambitious clinical claims. Risk associations, selected physical benefits and favorable recent survival syntheses deserve substantive credit. Exact trial outcomes, nonrandom comparisons, component attribution, primary versus secondary prevention and the smoking forecast remain essential limits.
12. The Family Crucible — text00020.html ¶001–062	Later randomized parent programs strengthen the practical coaching case. The original family study was small and observational, and its authors acknowledge reverse causation. Readiness comparisons do not establish an EQ-over-academics hierarchy. Maltreatment increases risk without making an abusive future inevitable.
13. Trauma and Emotional Relearning — text00021.html ¶001–069	Clinical relearning is possible, and modern treatment evidence supports that direction. Safety, pacing, professional context and persistent vulnerability are recognized. Neither therapy benefit nor responder-associated imaging validates every neurotransmitter claim or establishes one required therapeutic sequence.
14. Temperament Is Not Destiny — text00022.html ¶001–056	Combines stability with developmental opportunity rather than claiming unlimited malleability. Anxiety-prevention trials show that consequences can improve without significant measured inhibition change. Exact historical temperament percentages and some critical-window claims remain unverified. The brain explanation must not inherit certainty from behavioral benefit.
15. The Cost of Emotional Illiteracy — text00024.html ¶001–142	Includes real targeted prevention evidence and recognizes ineffective programs and other causes. Important problems include the Leon design mismatch, generalizing booster-specific classroom effects, and categorical substance-use language. Symptoms, diagnoses, risk markers and causal prevention are different endpoints.
16. Schooling the Emotions — text00025.html ¶001–200	Sustained, developmentally appropriate and coordinated implementation is a substantive strength. Later research supports average school benefits, with heterogeneous outcomes and some longer follow-up. The program-specific evidence is not uniform. The Head Start/High-Scope attribution remains insufficiently specified. Privacy concerns are recognized but not fully operationalized into protections.
Appendix A: What Is Emotion? — text00026.html ¶001–015	Useful distinctions among emotions, moods, temperaments and disorders. The author acknowledges disagreement about basic-emotion classification. This terminology section is not itself validation of the EI construct or a settled biological taxonomy.
Appendix B: Hallmarks of the Emotional Mind — text00027.html ¶001–027	Gives a coherent interpretive model of speed, association and state-dependent processing, including slower thought-generated emotions. Notes disclose adaptation of other authors' ideas. It should be read as a synthesis rather than a complete experimentally verified two-system architecture.
Appendix C: The Neural Circuitry of Fear — text00028.html ¶001–015	Supplies a detailed pathway narrative and acknowledges incomplete mapping. The lesion case and illustrated pathway are different evidence types. Later findings support important amygdala involvement while limiting universal necessity and a single-route account.

Section and complete extraction range	Strengths and consequential assessment
Appendix D: Prevention ingredients — text00029.html ¶001–023	Identifies emotional, cognitive and behavioral elements worth considering. Shared presence across programs is not randomized proof of active ingredients. Program effectiveness remains compatible with several interacting routes.
Appendix E: Self Science — text00030.html ¶001–017	Specifies what the curriculum teaches, including communication, decision-making and safe disclosure. Curriculum content establishes the intervention's intended components, not its efficacy or superiority.
Appendix F: SEL results — text00031.html ¶001–103	All six families receive separate assessment in A26. The listing contains controlled, randomized, quasi-experimental and perception-based evidence, with varying raters and follow-ups. It is neither six interchangeable randomized replications nor six unsupported anecdotes.
Resources — text00032.html ¶001–053	Includes a recommendation of Matthews, Zeidner and Roberts's critical book at ¶052–053. This is evidence that criticism is acknowledged, not that every criticism has been answered. Contact and program information from this historical edition is not certified current.
Notes and remaining matter — text00035.html ¶001–433 and other retained front/back documents	Notes supply important caveats and distinguish some interviews, unpublished work and publications. Their existence improves traceability but does not independently establish descriptive fidelity, inference or present reliability. Non-scientific publisher matter was not treated as evidentiary support.

06 Substantive claim and source audit

EI-A01 — What counts as EI?

Book: text00009.html ¶045–062; text00003.html ¶023–038.

Source notes: text00035.html ¶057–060, ¶006.

Role: central measurement and attribution test for C1.

Original framework and disclosed substitution

Salovey and Mayer (1990), *Emotional Intelligence*, was accessed at **A** level. It proposes recognition, regulation and use of emotion as hypothesized skills. It does not by itself validate Goleman's exact five-domain collection or the comparative and enabling claims made for those capacities.

Mayer, Caruso and Salovey (2016), *The Ability Model of Emotional Intelligence: Principles and Updates*, was accessed at **A** level as a later conceptual update. It helps distinguish emotion-related reasoning from broader personality or competency inventories; it must not be back-projected as the measurement used in earlier constituent studies.

Goleman explicitly calls ego-resilience a surrogate at text00009.html ¶057; the associated unpublished Block source is not a direct EI-versus-IQ test. This is an acknowledged substitution, not evidence of concealed measurement. Original Bar-On and Block materials remain incompletely accessed.

Added prediction and overlap

O'Boyle et al. (2011), S, distinguishes performance-based ability EI, self- or peer-report within an ability framework, and mixed measures. Its assembled correlation-matrix analysis reports incremental R^2 beyond cognition and the Big Five of .004, .052 and .068, respectively; the first was nonsignificant. The harmonic-mean $n=68$ used in the matrix analysis is not the total number of people in the literature.

Table 3 also reports correlations between the EI streams and cognitive ability. The observed correlations are .264 for performance-based ability EI, .070 for self- or peer-report measures within an ability framework, and .050 for mixed measures; the corresponding estimates corrected for measurement unreliability are .315, .083 and .060. These findings support partial separation from conventional cognitive tests, especially for report-based measures. They do not establish complete statistical independence or settle which measure better predicts leadership or relationship outcomes.

Access update, 15 September 2026: the initial audit recorded relevant methods, Tables 5–6 and related discussion. The combined critique and independent follow-up added visual inspection of Table 3, rechecked Table 6 visually, and examined the relevant methods and surrounding results. This does not imply an independent matrix reconstruction or a complete audit of every study in the synthesis.

Joseph et al. (2015), A, reports a corrected mixed-EI/supervisor-performance correlation of $\rho=.29$. Mixed EI overlaps strongly with a collection including ability EI, self-efficacy, self-rated performance, conscientiousness, emotional stability, extraversion and general mental ability: multiple $R=.79$. Its adjusted performance coefficient is $\beta=-.02$ after that expanded set.

These are different questions. The latter does not establish that the raw relationship vanished, that only IQ and the Big Five explained it, or that all EI measures lack value.

MacCann et al. (2020; online 2019), A, includes 42,529 participants, 1,246 extracted effects and 158 citations. Overall corrected academic association is $\rho=.20$; ability, self-rated and mixed estimates are .24, .12 and .19. Their additional variance beyond intelligence and the Big Five is 1.7, 0.7 and 2.3 percentage points. Understanding and management branches show larger increments, 3.9 and 3.6 points. EI ranks behind intelligence and conscientiousness in the reported relative-importance analysis.

An increment in variance accounted for is not a percentage improvement in grades caused by training.

Robinson and Zell 2026: favorable evidence with substantive method checks

Robinson and Zell (2026) synthesized 62 meta-analyses and 161 meta-analytic effects. The main article's abstract, significance statement and source information were accessed; its full main text remained paywalled. Coordinated readers examined the complete 44-page public supplement's text, tables, references and captions, visually checked selected table pages, and inspected the search log, codebook, selected code and the complete deposited primary and bias output documents. Figures S1–S10 were not visually inspected. The code was not executed.

The primary reported association is $r=.28$, 95% CI [.25,.31]. The inspected author output gives $r=.2792$, $SE=.0132$, $t(61)=21.1987$ and CI [.2529,.3056], with total $I^2=95.98\%$. These are reported outputs, not reproduced estimates.

The association remains approximately .28 under robust variance estimation and correlated-effects sensitivity assumptions. Ability EI is .21 [.16,.26], trait EI .29 [.23,.35], and mixed EI .31 [.27,.35]. “Combined” aggregates two or more model classes and is not a synonym for mixed EI.

Cross-method findings remain favorable: subjective EI with objective outcomes is .19 [.10,.27], and objective EI with subjective outcomes is .16 [.13,.19]. Both-subjective and both-objective categories are .32 [.27,.37] and .26 [.18,.34]. These differences also reflect different outcomes and populations; they do not experimentally identify method bias.

The author outputs sum 3,316 study records and 1,064,932 participant records. They do not establish that all are unique studies or people. Overlap was screened and restricted, not eliminated. The search log applied a less-than-25% overlap criterion; retained articles still share samples. Dependence modeling does not substitute for person-level deduplication.

“Flourishing” includes cognition, personality, optimism and self-efficacy as well as school, work and health outcomes. The pooled coefficient therefore mixes prediction with relationships to neighboring constructs. It is not a general IQ/personality-adjusted effect.

Publication-bias clearance is not established. The supplement reports larger effects among less precise estimates. The later inspected auxiliary output contains incompatible-vector-length warnings when article-level random effects are added to effect-level vectors, and ignored misspelled options. These observations warrant caution about those diagnostics. They do not, without repair and rerunning, quantify a changed result or refute the separately fitted primary association.

An apparent “ $z=1.98$, $p=.010$ ” discrepancy is not a demonstrated mathematical error: the inspected output identifies 1.98 as a coefficient, with $SE=.761$ and test statistic 2.60, $p=.0103$. Minor inconsistencies in overlap counts and a sensitivity estimate also do not overturn the main rounded finding.

Relevant materials: publisher supplement; author repository.

Current methodological critique

Davis and Mahoney (2026), S, rated 73 reviews found through an April 2024 search. Fifty-nine received low/reject SIGN ratings. That is not an estimate that 81% of primary EI findings are false. Intervention-focused and second-order reviews were excluded, and the search predates Robinson and Zell; it is not a direct quality audit of that 2026 synthesis.

Assessment: positive measured associations and some added prediction are supported. The broad enabling and comparative framework remains only partly supported. Neither total dismissal nor general confirmation is warranted.

El-A02 — IQ's contribution and selected life-course cohorts

Book: text00009.html ¶008–019; text00003.html ¶024–034.

Source-note frame: text00035.html ¶047–051, with adjacent cohort references.

Role: comparative importance and the interpretation of selected populations.

Howard Gardner's 1995 essay, *Cracking Open the IQ Box*, was inspected in the relevant original passage. Its numerical discussion concerns correlations with societal position and summarizes other work. It is not an original EI study or a universal allocation of success.

The book's Harvard, Somerville and valedictorian examples involve selected cohorts and particular outcomes. The underlying Vaillant book, Felsman–Vaillant chapter and Arnold account were not recovered sufficiently to verify every number. Their access gaps are not errors.

A selected-group result cannot determine how much a predictor matters across a wider population. Nor does academic distinction being insufficient for distinction everywhere measure EI's contribution.

Strenze (2007), S, supplies favorable longitudinal evidence for IQ. Corrected weighted correlations are .56 with education, .43 with occupational status and .20 with income; uncorrected values are .48, .36 and .16. The respective samples comprise 59/84,828, 45/72,290 and 31/58,758 study samples/participants. These are specified socioeconomic outcomes, not every form of fulfillment or causal percentages of a person's success. Family background and grades also predict outcomes.

The anniversary introduction's rejection of residual-80%-equals-EI is explicit. The appropriate concern is the ensuing comparison and breadth, not a claim Goleman rejects.

Assessment: the book is right that IQ is not a guarantee and that other influences matter. The residual does not identify EI, selected-cohort illustrations do not establish a broad hierarchy, and several exact historical comparisons remain unresolved.

El-A03 — Marshmallow test and delinquency comparisons

Book: text00012.html ¶013–022; text00024.html ¶034.

Notes: text00035.html ¶103–106, ¶353.

Role: predictive magnitude, construct interpretation and prediction-to-training inference.

Shoda, Mischel and Peake (1990), S with Table 4 visually checked, reports 185 follow-up respondents, 94 with SAT information. In the highlighted exposed-reward/spontaneous-strategy condition, n=35; waiting correlates .42 with verbal SAT and .57 with quantitative SAT. The authors report wide intervals, [.10,.66] and [.29,.76], and other task conditions do not show the same pattern.

The book discloses the Stanford setting and attentional strategies. Its tercile arithmetic is correct:

$(610 + 652) - (524 + 528) = 210$ SAT points.

The group means and the “twice as well as preschool IQ” comparison depend partly on Peake’s analysis and personal communication. The published table does not independently verify those exact comparisons or identify the metric behind “twice.” They remain unresolved, not fabricated.

Watts, Duncan and Quan (2018), S, analyzes 918 children in SECCYD, with 552 in the focal subgroup whose mothers lacked a degree. Its seven-minute task differs from the original procedure. The age-15 achievement coefficient in that subgroup changes from .236 without controls to .081 after early/background controls and .050 after concurrent cognitive/behavioral controls; corresponding p values are <.001, .016 and .140. Behavioral associations are small. Ceiling effects and observational uncertainty matter.

Michaelson and Munakata (2020), S, uses the same source cohort but an independently preregistered 2017 analysis, different outcome composites and a binary completion measure. Problem behavior retains an association after its preschool level is controlled, $\beta = -.18$, $p = .01$; academic and social associations do not. This is not a new independent cohort or merely an analysis invented after the Watts result.

Sperber et al. (2024), S, follows 702 SECCYD participants to age 26. Most adjusted outcomes are nonsignificant. Bivariate education and BMI correlations are .17 and $-.17$. An exploratory binary waiting model retains a BMI association, $\beta = -.17$, $p = .03$. A blanket “nothing predicted anything” statement would therefore be wrong.

Moffitt et al. (2011), S, uses a composite of nine self-control measures across ages 3–11 in the Dunedin cohort. Adult health, finances and offending remain associated with childhood self-control after accounting for IQ and social class. A separate British E-Risk analysis of 509 same-sex dizygotic twin pairs links age-5 self-control differences to smoking, school performance and antisocial behavior at age 12, adding a family-background comparison for those child outcomes. These observations strengthen the relevance of self-control without estimating the effect of teaching it.

The delinquency comparison also has a dispute. Block (1995) offered an alternative interpretation of an existing dataset; Lynam and Moffitt’s reply contested it. Abstracts and original text portions were examined, not a complete model reconstruction. The book’s ranking should not be presented as an uncontested new cohort finding.

Assessment: conditional prediction and broader self-control evidence are meaningful. Neither proves that teaching waiting produces the later SAT difference or that one brief test diagnoses a general master ability. Exact private comparisons remain unresolved.

EI-A04 — Hope, realistic optimism and persistence

Book: text00012.html ¶039–058.

Notes: text00035.html ¶115–120.

Role: favorable constituent evidence and exact predictive comparisons.

Snyder et al. (1991), **A/P**, develops hope as agency and pathways. It is a self-report construct, not an emotion-reasoning performance test. The book's exact freshman-grade/SAT comparison and its 500-student optimism comparison remain interview/trade-book trails not independently reconstructed.

Day et al. (2010), **M**, follows 129 UK undergraduates for three years using degree outcomes, Raven matrices, personality and prior achievement. Adding two hope facets yields $\Delta R^2 = .03$, $p = .041$. Hope-degree correlations are .23 and .31, compared with .46 for Raven and .52 for prior grades. The facets correlate .80. This is favorable added prediction, not superiority to intelligence or an optimism-training trial.

Marques, Gallagher and Lopez (2017), **A**, synthesizes 45 studies and 9,250 participants and reports a moderate positive academic relationship. The exact pooled coefficient and complete bias tables were not inspected.

Seligman et al. (1990), **A**, studies varsity swimmers after false negative performance feedback. Pessimistic explanatory style predicts deterioration, while the optimistic group does not show the same worsening. Feedback was manipulated; optimism was not randomly assigned. The study does not show that optimists always improve.

MetLife: a consequential source distinction

Seligman and Schulman (1986), **S**, separates two studies. Its cross-sectional study, reported as including 94 experienced agents, gives a 37% sales contrast for the agents' first two years of service. Explanatory style was measured after they had acquired sales experience, so this result does not establish prediction from optimism measured at hiring. The prospective study obtained production records for 103 new agents and reports first-year differences of 14% excluding dropouts and 20% including zero sales, both nonsignificant. Some second-half-year and extreme-group comparisons were favorable.

Among 101 agents with status information:

- Optimistic group: 28 retained, 24 left; leaving rate $24/52 = 46.15\%$.
- Pessimistic group: 14 retained, 35 left; leaving rate $35/49 = 71.43\%$.
- Ratio of leaving rates: approximately 1.55.
- Ratio of retention rates: approximately 1.88.

A roughly doubled retention rate is not a doubled quitting rate. The recovered paper supports the book's 37% first-two-year sales number; the book does not explicitly label that comparison prospective. Its MetLife paragraphs (text00012.html ¶054–055) carry no separate note identifying the 1986 article. The surrounding notes lead to *Learned Optimism* and Goleman's Seligman interview (text00035.html ¶117, ¶119), which have not been fully checked. The later special-hire figures of 21% and 57% also remain unrecovered. The quitting-rate discrepancy therefore applies to the recovered paper; the book's complete source chain has not been reconstructed.

Access update, 15 September 2026: the initial audit inspected both studies' methods/results, Tables 1–6 and the causal caveat. During the combined critique and independent follow-up, the relevant methods and results were rechecked and printed pages 833–836 were visually inspected, including the sales and status tables. No later company-analysis audit or individual-data reconstruction was performed.

Goleman's practical qualifications are important: optimism should be realistic, goals and routes can change, and competence contributes to self-efficacy. He is not simply recommending unqualified positive thinking.

Assessment: favorable predictive evidence for hope and explanatory style survives, and the recovered article supports the 37% historical sales association. Exact superiority claims, quitting comparisons and later special-hire figures require their own source verification. Prediction is not the effect of teaching optimism.

EI-A05 — Practice, positive affect and flow

Book: [text00012.html ¶008–010, ¶033–036, ¶059–079](#).

Notes: [text00035.html ¶100–102, ¶114, ¶121–128](#).

Role: expertise, cognitive-state effects and the claimed master aptitude.

Ericsson and Charness (1994), S, reports top violinists accumulating more than 10,000 hours by age 20, approximately 2,500 more than the next group. The book's approximate 10,000/7,500 comparison is substantially faithful. It does not state a universal 10,000-hour rule. The source also emphasizes teachers, family support, access and rest; those are not evidence that an EI faculty is the predominant cause.

The later Macnamara, Hambrick and Oswald meta-analysis must be read with its 2018 correction, examined in full. Corrected overall $r=.38$, CI [.33,.42], corresponds to 14% variance; corrected domain values are games 24%, music 23%, sports 20%, education 5% and professions 1%. These are correlations under particular definitions, not causal proportions of talent.

Macnamara and Maitra's 2019 replication, accessed at abstract/result-summary level, and the responses by Ericsson and Harwell, Hambrick et al., and Ericsson preserve an unresolved definition and method dispute. Only selected arguments were read. The appendix does not settle the entire debate.

Isen, Daubman and Nowicki (1987), A/P, describes film/candy mood manipulations and creative-problem tasks. It matches the book's funny-film/candle example more closely than the 1991 clinical problem-solving source identified in the attached note. Exact cell counts and full protocols were not recovered.

Baas et al. (2008), A, reports positive-versus-neutral mood $r=.15$ across the relevant creativity comparisons. Negative-versus-neutral and positive-versus-negative contrasts are nonsignificant; activation and motivational orientation matter. This does not establish that positive emotion improves all thinking, or validate casual substitution of clinical hypomania for ordinary positive affect.

Harris et al. (online 2021; issue 2023), S, reports a flow–performance correlation of .31 [.24,.38] in sport, gaming and simulation work. Performance is objectively assessed, while flow is self-reported. Direction remains unresolved. Flow accompanying success is not proof that flow is necessary for learning or mastery.

Exact historical homework, flow-hour, artist-follow-up and neural-flow claims remain unverified. Broad racial or cultural explanations in the chapter do not acquire validation from these separate studies.

Assessment: several constituent findings are useful and favorable. Their interpretation as evidence of emotional primacy or a general prerequisite exceeds what was measured.

EI-A06 — Rapid fear learning and neural hijacking

Book: text00007.html ¶015–016, ¶029–036, ¶050–077; text00028.html ¶003–015.

Notes: text00035.html ¶023–027, ¶034–043.

Role: preselected favorable neuroscience anchor and test of broader anatomical claims.

LeDoux, Sakaguchi and Reis (1984), accessed through the author-posted abstract/bibliographic record, supports a bounded animal finding: auditory-cortex lesions did not abolish conditioned autonomic and behavioral responses to simple acoustic stimuli. Related 1986 work reports that damage involving auditory thalamic neurons disrupted conditioned responses while unconditioned responses remained. These are not complete independent methods audits.

The findings support subcortical participation in conditioned defensive responses. They do not directly measure human conscious fear, prove that a particular autobiographical outburst originated in the amygdala, or establish the relative speed of every cortical and subcortical route.

Pessoa and Adolphs (2010), S, is a critical perspective concerning dominant visual low-road interpretations in primates and simplistic assumptions about cortical slowness. It is not an experiment demonstrating that subcortical pathways do not exist.

Rafal and Koller (2025), A/P, uses probabilistic tractography in 19 people and identifies streamlines consistent with subcortical visual and auditory routes to the amygdala. This favorable anatomical evidence deserves credit. Tractography does not by itself establish speed, necessity or the cause of an ordinary angry response.

Feinstein et al. (2013), **M including online methods**, examined three women with bilateral amygdala damage and 12 matched controls using air and 35% carbon-dioxide challenges. All three lesion patients and three controls met panic criteria. Physiological analyses had still smaller patient denominators. The result limits universal amygdala necessity for fear/panic; it does not erase its role in externally cued threat learning.

The book's "all passion depends on it" at text00007.html ¶016 is too sweeping. Its notes nonetheless acknowledge residual feeling, speculative human extrapolation and a distributed emotional brain. Those caveats must remain beside the criticism.

Assessment: bounded pathways and conditioning findings are supported. A universal seat of passion and a demonstrated neural explanation for every regretted outburst are not.

EI-A07 — Self-awareness, alexithymia and somatic markers

Book: [text00010.html](#) ¶008–017, ¶027–047.

Notes: [text00035.html](#) ¶062–075.

Role: clinical observations, ordinary decision-making and proposed mechanisms.

The original self-awareness, alexithymia and Damasio source trails include unpublished material, clinical papers and the 1994 book *Descartes' Error*. Several were not recovered in full. This prevents certification of each clinical or ordinary-person generalization, not a finding that the material is false.

The later Iowa Gambling Task dispute addresses a more specific mechanism. Maia and McClelland (2004), S, assigned 40 undergraduates to original versus more detailed questioning. More sensitive questioning revealed reportable knowledge consistent with advantageous choices; overall performance did not significantly differ, $p=.28$. The authors did not establish that this knowledge caused the choices or exclude all nonconscious contributions.

Bechara et al. (2005), S, replied that emotional signals and unconsciousness are distinct, that healthy-student questioning did not test the relevant lesion and skin-conductance evidence, and that working-memory impairment can also affect performance. The complete ensuing reply was not read.

This is later evidence concerning a task and theoretical interpretation, not an original study Goleman could have cited in 1995. It neither validates every somatic-marker claim nor disproves emotion-assisted decision-making generally.

Assessment: distinguish emotional attention, emotional knowledge, regulation and clinical deficits. The broad claim that awareness is a necessary foundation for all good decisions remains incompletely established by the inspected sources. No moral or personality diagnosis follows from a person's difficulty identifying feelings.

EI-A08 — Anger, worry and ordinary melancholy

Book: [text00011.html](#) ¶010–018, ¶023–051, ¶054–095.

Notes: [text00035.html](#) ¶078–094.

Role: preselected favorable practical anchor.

The book rejects indiscriminate catharsis and gives alternatives involving arousal, reappraisal and constructive action. It also distinguishes everyday distress from conditions requiring professional care.

Bushman (2002), S, analyzed 600 participants across rumination-plus-punching, distraction-plus-punching and no-punching control conditions. Rumination versus control produced greater immediate anger, $d=.31$, and laboratory aggressive responding, $d=.30$.

Distraction-versus-control differences were not significant. The ordering of means does not establish that all pairwise contrasts differed.

Kjærvik and Bushman (2024), **A and limited indexed sections**, synthesizes 154 reports, 184 independent samples and 10,189 participants. Arousal-decreasing activities have pooled $g = -.63 [-.82, -.43]$; arousal-increasing activities average $-.02 [-.13, .09]$, with heterogeneous findings.

This supports cooling strategies and challenges ruminative venting. It is not evidence that every form of emotional expression or exercise is harmful, and a standardized effect is not a percentage of anger eliminated. The book's active-exercise recommendations cannot automatically inherit the stronger arousal-reduction result.

The original Tice/Baumeister, Zillmann, Mallick–McCandless, Tavis, Borkovec, Nolen–Hoeksema and clinical-source relationships were not all checked at equivalent depth. Those gaps remain distinct from the favorable later evidence.

Assessment: several consequential practices have appropriate support. The whole chapter's clinical and mechanistic explanations are not certified by the anger literature alone.

EI-A09 — Reading emotions and physiological empathy

Book: [text00013.html](#) ¶004–010, ¶034–042.

Notes: [text00035.html](#) ¶130–132, ¶141–143.

Role: recognition measures and a specific descriptive/inferential discrepancy.

The “90%” statement at ¶010 concerns emotional messages, not all human communication. No directly attached source verifies a universal percentage. Original PONS and Nowicki/Duke instrument claims remain incompletely checked.

The more consequential verified relationship is Levenson and Ruef (1992), **S with principal tables visually checked**. Thirty-one married adults observed strangers from selected couple recordings. They were not watching their own spouses, as the book describes.

A composite physiological-linkage measure correlated with accuracy for negative emotion, not positive emotion. Skin conductance and pulse-transmission measures were more consistent individual correlates; heart-rate matching was not established as necessary. The researchers did not show that observers experienced the same emotion as the targets or that physiological linkage caused accurate understanding.

The authors' 1997 chapter, *Physiological Aspects of Emotional Knowledge and Rapport*, **S**, distinguishes marital interaction, reliving one's own interaction and the stranger-observer experiment. It does not repair the book's precise own-spouse description of the cited study.

Assessment: the book changes the participants' relationship and turns an association into a necessity claim. These are, respectively, a descriptive discrepancy and an inferential overstatement. The real negative-emotion association remains favorable evidence. No allegation of invented research or knowledge of the discrepancy follows.

El-A10 — Empathy, morality and offending

Book: text00013.html ¶044–066; text00014.html ¶015; text00025.html ¶190–193.

Notes: text00035.html ¶144–150.

Role: behavioral and ethical extension.

The book's claim that a treatment program halved subsequent offending traces to Goleman's 1992 reporting based on Pithers. The full report, comparison details and exact rates were not recovered.

Pithers (1994), A, evaluates a group component intended to increase offenders' empathy. It reports empathy change while recidivism follow-up was still underway. It is related evidence, not verification of the book's earlier numerical claim.

Barnett and Mann (2013; online 2012), A, critically examines empathy theory and victim-empathy treatment. It is a conceptual review, not a trial demonstrating that the programs are ineffective.

A full-program reduction in offending would be relevant evidence for treatment. It would not isolate empathy's contribution or prove that all effective treatment requires the same mechanism. Recognition accuracy, concern, inhibition and ethical action are distinguishable.

The book itself acknowledges manipulation, calculated violence, multiple causes of offending and the absence of a simple criminal-gene inference. Those boundaries prevent treating poor emotion recognition, alexithymia or neurodivergence as evidence of dangerousness.

Assessment: the moral and treatment extension is only partly supported by the inspected material. The half-rate comparison remains unresolved. It is neither validated by the process paper nor disproved by inability to retrieve the earlier source.

El-A11 — Contagion, rapport and social effectiveness

Book: text00014.html ¶019–052, ¶065–091.

Notes: text00035.html ¶152–166.

Role: experimental mood transfer versus broader social interpretation.

Sullins (1991), A, paired relatively expressive and less expressive individuals in same-sex dyads and measured mood before and after a silent waiting period. More expressive partners influenced the others more than the reverse; social comparison was implicated in part.

This is bounded evidence for mood convergence. It does not show that the most forceful person always determines a group's mood or establish a universal automatic-mimicry mechanism.

The book's rapport, social-entry and display-rule sources were not all recovered as complete original reports. Their conceptual distinctions remain worth preserving: emotional display can be culturally regulated, social effectiveness can be used manipulatively, and approval-seeking can conflict with authentic relationships.

The Dobson train story is presented as an anecdote with permission and a source note identifying a published retelling. Its value is illustrative, not a controlled demonstration that a particular de-escalation strategy generally works.

Assessment: a favorable experimental component survives. Broader causal and practical extensions require more evidence than the mood study and anecdote supply.

El-A12 — Marriage prediction, repair and safety

Book: [text00016.html](#) ¶004–019, ¶024–035, ¶045–091.

Notes: [text00035.html](#) ¶169–197.

Role: predictive accuracy, generalization and practical transfer.

Buehlman, Gottman and Katz (1992), S, recruited 56 families through advertising, all with a young child; 52 were followed for three years. The classification table contains 47 complete cases: 37 of 40 intact marriages and 7 of 7 divorces were correctly classified.

$44/47 = 93.62\%$, which supports the book's rounded 94%.

The same data were used to develop the predictor combination. The inspected methods do not describe a separate held-out validation sample. The figure is not a 94% divorce probability for each couple.

Heyman and Smith Slep (2001), A/P, illustrates base-rate and overfitting issues in a different, cross-sectional archival dataset. Its approximately 29% positive predictive value is not a reanalysis of this 94% study.

The Gottman research FAQ acknowledges an initial post-hoc study and points to later prospective work. That response is relevant. Recurring associations in new samples and successful validation of a fixed prediction rule are still different questions; this appendix does not claim to have adjudicated every later model.

Uhlich et al. (2026), A, reports balanced accuracies of 64% and 71% in two larger longitudinal samples. Different outcomes, horizons, predictors and metrics prevent using those figures as a direct numerical rebuttal of Gottman.

The book's cohort-based divorce projection should not be confused with an annual divorce rate. Its conflict advice also does not itself establish prevention of divorce. Broad sex generalizations and physiological thresholds remain less securely checked.

Safety: [text00035.html](#) ¶186 distinguishes calculated from impulsive violence. Ordinary de-escalation advice must not become a claim that victims can or should manage an abusive partner's behavior.

Assessment: faithful numerical reporting coexists with limited predictive validation and uncertain prescription-level transfer.

El-A13 — Constructive criticism and workplace prejudice

Book: text00017.html ¶1014–060.

Notes: text00035.html ¶202–214.

Role: preselected favorable feedback evidence and workplace generalization.

The engineer/vice-president anecdote is attributed to **Hendrie Weisinger**, with *The Critical Edge* named in the note at ¶1202. It is separate from Baron's experiments.

Baron (1988), **APA-supplied P/A**, reports studies of 83 undergraduates, 106 undergraduates and 108 employees. Destructive versus specific, considerate criticism produces more anger/tension and less cooperative stated responses; another experiment finds lower goal-setting and self-efficacy. Employees rate poor criticism as an important source of conflict.

Baron (1990), **APA-supplied P/A**, tests remedies after destructive criticism. Apology and explanations reducing inferred harmful intent are comparatively helpful; expressing irritation worsens responses. Manager/employee judgments are ratings, not a randomized field productivity trial.

The book's shampoo-ad task resembles the earlier experimental description, while its note identifies the 1990 article and an interview. Full methods and the interview remain unavailable. It is therefore inappropriate to declare the 1990 citation definitively wrong solely because a neighboring 1988 paper is easier to match. Levinson's advice is a further source, not a tested script in every detail.

Blanchard, Lilly and Vaughn (1991), **A**, reports campus experiments in which strong social norms influence expressed antiracist opinions, including private responses. This supports a bounded norm effect, not enduring implicit-attitude change or general workplace-training efficacy. A contemporaneous Daubman reply was identified but not read in full.

Assessment: constructive criticism and repair deserve direct-evidence credit. The study settings and measured outcomes remain narrower than company productivity, and the exact task attribution remains unresolved rather than definitively erroneous.

El-A14 — Team intelligence, star performance and leadership

Book: text00017.html ¶061–075; text00003.html ¶018, ¶031–038.

Notes: text00035.html ¶216–218.

Role: group processes and the selected-professional comparison.

Williams and Sternberg (1988), **A**, relates cognitive and social-cognitive measures to group product quality. The abstract does not establish harmony as the single most important determinant.

Kelley and Caplan (1993), *How Bell Labs Creates Star Performers*, **A/P**, concerns work strategies and a described training initiative. The book itself says its initial cognitive and social tests did not clearly distinguish stars, then turns to detailed interviews about initiative, networks and

coordination. The full original results and causal training identification were not audited.

Woolley et al. (2010), **A**, provides favorable later evidence: two studies involving 699 people support a group-performance factor associated with social sensitivity and conversational turn-taking. In those studies, the group factor was not strongly correlated with average or maximum individual intelligence. This is not causal proof of a hiring or group-composition rule.

Riedl et al. (2021), **A/P**, combines 22 studies, 5,279 people and 1,356 groups. Its 2022 correction, read in full, changes average variance extracted from 44% to 19.6% after a software error and clarifies models/loadings. The authors retain their main conclusions. The correction is material, not a retraction.

Rowe, Hattie and Munro (2024), **A**, finds a better-fitting two-factor account in a small 85-student study. It is a meaningful measurement qualification, not enough alone to reject the wider literature.

Harms and Credé (2010), **A**, reports an EI/leadership association of .59 when the same source rates both and .12 with different sources. Different methods also alter measurement and agreement; this is not a pure experimental estimate of bias.

Assessment: social processes and specified EI measures have relevant positive evidence. The book's strongest group-harmony ranking and the contextual EI-versus-IQ comparison for outstanding leadership remain incompletely verified here. Competency-list percentages are not percentages of explained leadership success.

EI-A15 — Psychoneuroimmunology and the common cold

Book: [text00018.html](#) ¶019–033, ¶050–053.

Notes: [text00035.html](#) ¶220–224, ¶241–244.

Role: preselected favorable clinical finding and mechanism check.

Ader and Cohen (1975), **A**, reports conditioned immune effects after saccharin was paired with cyclophosphamide in rats. The measured endpoint was antibody response to sheep erythrocytes, not the exact T-cell/death discovery narrative. The full discovery history was not recovered, so no detailed historical error allegation is made.

Cohen, Tyrrell and Smith (1991), **M with an original figure page visually checked in the coordinated audit**, analyzed 394 healthy virus-exposed adults; 26 received saline. Stress was measured before controlled exposure. Clinical colds required confirmed infection plus clinical signs.

The book's approximately 27% versus 47% contrast is faithful. Adjusted high-versus-low stress-quartile odds ratios were 2.16 [1.11,4.23] for colds and 5.81 [2.12,15.91] for infection. Confirmation of infection and covariate adjustment make this more informative than a survey of reported symptoms.

Stress was not randomly assigned. Measured cell counts and baseline antibody levels did not explain the association; functional immunity was not assessed. The study therefore did not establish that stress caused the excess colds through immune suppression, as the book's phrasing suggests. The research unit was at Salisbury, not Sheffield.

Segerstrom and Miller (2004), A/P, distinguishes acute from chronic stress effects across more than 300 human studies. A universal suppression story is too simple.

Cohen et al. (2012), A, adds supportive mechanistic evidence from two viral-challenge studies, involving 276 and 79 adults, concerning glucocorticoid resistance and inflammatory regulation. It does not test the proposed lifetime medical benefit of childhood EI education.

Assessment: biological interaction and the clinical susceptibility association are supported. The 1991 mechanism and later intervention payoff are stronger inferences. Criticism should preserve the genuine clinical measurement and subsequent mechanistic progress.

EI-A16 — Disease risk, hostility, cardiac intervention and smoking

Book: text00018.html ¶032–045, ¶048–073, ¶104.

Notes: text00035.html ¶225–240, ¶245–254.

Role: magnitude, causality and clinical benefit.

Risk and acute physiological evidence

Friedman and Booth-Kewley (1987), P/A, discusses a disease-prone personality across several conditions. Its first page qualifies much noncoronary evidence as weak. The exact 101-study count, pooled scale and conversion to a blanket doubled disease risk were not verified from full tables. The book's spelling "Boothby-Kewley" is incorrect, but this bibliographic slip does not invalidate the research.

Ironson et al. (1992), A/P, studies 18 coronary patients and nine controls during anger recall and other tasks. An approximately five-percentage-point ejection-fraction fall in patients is consistent with the book. This is an acute physiological result, not a recurrence or survival trial; the abstract does not establish that every patient had survived a first infarction.

Mittleman et al. (1995), A, uses a case-crossover design in 1,623 patients. Thirty-nine reported anger in the preceding two hours; the relative risk of infarction onset during that window was 2.3 [1.7,3.2]. Recall and time-varying coexposures remain possible. The endpoint is myocardial infarction, not cardiac arrest. The book's earlier 1994 source version remains unrecovered.

Chida and Steptoe (2009), A, reports prospective associations of anger/hostility with incidence and prognosis: HR 1.19 [1.05,1.35] in initially healthy cohorts and 1.24 [1.08,1.42] with established disease. Publication-bias indications qualify the estimates. They cannot replace an unknown scale from the different 1987 synthesis.

Interventions: positive and negative findings

Study	Design and access	Relevant outcome	Interpretation
RCCP, Friedman et al. 1986	A plus later trial-profile sections. Of 1,013 patients, 862 were randomized: 592 additional Type-A counseling, 270 cardiac counseling; 151 formed a nonrandom no-group comparison.	At 4.5 years, recurrence 12.9%, 21.2% and 28.2%, respectively.	Favorable randomized contrast: 8.3 percentage points, about 39% relative reduction. It does not independently verify or refute the book's 44% from a different 1990 presentation/follow-up.
ENRICHED, 2003	A. Multicenter RCT, 2,481 recent-infarction patients; CBT, with medication when indicated, versus usual care.	Depression/support improved; mean 29-month event-free survival 75.8% versus 75.9%.	No significant improvement in death/recurrent infarction despite psychological improvement. Control improvement also matters.
SUPRIM, Gulliksson et al. 2011	A. 362 coronary patients; twenty two-hour CBT sessions over one year versus usual care.	Mean 94-month follow-up: recurrent CVD HR .59 [.42,.83]; recurrent infarction .55 [.36,.85]; all-cause mortality .72 [.40,1.30].	Meaningful favorable recurrence evidence; mortality remains uncertain.
ENHANCED, Blumenthal et al. 2016	A plus design/endpoints/intervention sections. 151 patients randomized to rehabilitation with or without added stress management.	Median 3.2-year secondary composite events 18% versus 33%; HR .49 [.25,.95].	Supports additional clinical value. The composite includes procedures and hospitalizations; stress, not events, was primary. The extra no-rehabilitation group was not randomized.

The 2017 Richards et al. Cochrane review, **S**, includes 35 trials and 10,703 participants. Total mortality RR=.90 [.77,1.05] is nonsignificant; cardiac mortality RR=.79 [.63,.98] is favorable but low certainty, with small-study-bias indications. Psychological symptoms improve, while nonfatal infarction and revascularization are not significantly reduced.

The 2024 Ski et al. Cochrane review, **A**, asks a different, narrower depression/anxiety treatment question. Its mortality and major-event estimates are imprecise, based on only three and four studies respectively. It is not a consecutive update of the identical 2017 corpus.

Smoking comparison

The book's ¶104 statement is conditional. It does not report a direct trial comparing emotional management with smoking cessation. Nevertheless, similar risk associations do not identify similar intervention returns. Population, exposure, baseline risk, outcome and follow-up need alignment before comparing effects.

Assessment: some clinical benefit beyond comfort is supported. Broad disease-risk multipliers and the smoking-sized preventive return are not established. The missing Friedman tables and Thoresen presentation remain unresolved, not adverse evidence by themselves.

EI-A17 — Isolation, support and physical health

Book: [text00018.html](#) ¶1076–083.

Notes: [text00035.html](#) ¶1256–262.

Role: social exposure, prognosis and intervention evidence.

House, Landis and Umberson (1988), S, reviews prospective structural–relationship evidence. Risks vary by cohort, sex and adjustment; the exact combined 37,000–person and smoking–versus–isolation arithmetic was not fully verified. The body’s 1987 date differs from the actual 1988 publication and correct endnote.

Holt–Lunstad, Smith and Layton (2010), S, combines 148 studies and 308,849 people, with mean follow–up 7.5 years. Survival OR=1.50 [1.42,1.59], with I²=81%. Complex integration measures have stronger associations than living alone. This is not a randomized trial of prescribed friendship or a 50% increase in lifespan.

Wang et al. (2023), A, includes 90 prospective cohorts and 2,205,199 people. Pooled mortality associations are 1.32 [1.26,1.39] for isolation and 1.14 [1.08,1.20] for loneliness. This limits the book’s restriction of the risk to felt disconnection: objective structure and subjective experience are not interchangeable.

Smith et al. (2021), S, covers 106 randomized trials and 40,280 patients. Across 87 fixed–time studies, survival OR=1.20 [1.09,1.31]. A reviewer–requested, nonprespecified comparison found OR=1.35 [1.20,1.52] in 31 behavior–focused studies and 1.01 [.87,1.16] in 56 social/emotional–focused studies; between–subgroup Q=9.3, p=.002.

A partly different set of 22 time–to–event studies also favored support: benefit–oriented HR=1.29 [1.12,1.49]. Eighteen focused on social/emotional outcomes; no corresponding program–type subgroup test was performed. Three studies contributed to both sets, so 87+22–3=106. Neither estimate measures percentage extra lifespan, and the HR must not be averaged directly with death–oriented HRs below one. Active information/class controls weakened the HR results. Studies consisting solely of one–on–one psychotherapy were excluded; multicomponent psychosocial interventions were eligible. Hospice and palliative–care interventions were also excluded.

These findings support the health relevance of relationships and some clinical support programs. They also caution against assuming that improving any social measure produces the same benefit, or that all favorable support results isolate emotional support itself.

Assessment: substantial relevance, with specific intervention support and important subtype limits. Solitude is not identical to loneliness; risk prediction is not a prescription’s effect.

EI-A18 — Breast-cancer support groups and survival

Book: text00018.html ¶060, ¶069, ¶088–089.
Note: text00035.html ¶265.
Role: historical numerical fidelity, replication and current clinical evidence.

Source	Actual access and comparison	Result
Spiegel et al. 1989	A; original full paper unrecovered. Eighty-six women, 50 supportive group therapy plus self-hypnosis and 36 controls; routine oncology in both.	Mean survival 36.6 months, SD 37.6, versus 18.9, SD 10.8. Ten-year follow-up documented 83 deaths; three remained alive. The book’s 37/19 rounding is faithful.

Source	Actual access and comparison	Result
Goodwin et al. 2001	A plus indexed primary methods/results/discussion; 235 women, 158 intervention and 77 controls. Survival primary.	Median 17.9 versus 17.6 months; unadjusted death HR 1.06 [.78,1.45], adjusted 1.23 [.88,1.72]. Mood/pain benefits, especially in initially distressed patients.
Kissane et al. 2007	A; 227 consenting women, 147 group therapy plus relaxation classes and 80 classes alone.	Median 24.0 versus 18.3 months, but unadjusted HR .92 [.69,1.26] and adjusted 1.06 [.74,1.51]. No reliable overall survival benefit; psychological benefits occurred.
Spiegel et al. 2007	A plus selected indexed methods/discussion, not a complete paper audit. 125 women, 64 group therapy/education and 61 education.	No overall survival benefit; medians 30.7 versus 33.3 months. A favorable ER-negative subgroup of 25 was exploratory. The authors acknowledge failure to replicate overall survival.

These are distinct trials despite repeated authors. Their means, medians, controls, populations and medical eras are not interchangeable. An exploratory subgroup is not an established indication.

The 2026 Asakawa-Haas et al. synthesis, **A plus selected methods/results/discussion**, materially strengthens the favorable side. It includes 32 randomized trials and 5,704 participants across cancers, stages and structured psychological interventions. The search extends to 17 October 2025.

The main death HR is .80 [.71,.90], $I^2=48\%$, with prediction interval [.49,1.29]. A separate 16-trial estimate of median survival difference is +3.9 months [-.7,8.5]. These are not equivalent estimands or a 20% increase in lifespan.

Primary, sensitivity and multiverse analyses generally favor benefit. All included trials had some or high bias concerns in the review’s assessment. PET/PEESE estimates cross the null; the authors argue that those methods are less suitable under the observed conditions. The full supplements and primary datasets were not independently audited or reanalyzed here.

Spiegel’s coauthorship is disclosed. It is not itself a refutation of the synthesis. Nor does a favorable pooled result establish near-doubling in metastatic breast cancer or superiority to medicines. The original trial did not compare psychosocial care against medication as rival treatments.

Assessment: the original number is faithful; the specific generalized survival expectation is not dependable. Current broader survival evidence is favorable enough that “psychological care never extends survival” would be wrong. Standard medical care and the book’s anti-blame caveats remain central.

EI-A19 — Expressive writing and humane medical care

Book: text00018.html ¶085–087, ¶095–099, ¶101–110.

Notes: text00035.html ¶263–271, ¶273–276.

Role: clinical benefit, endpoint distinctions and a specific design correction.

Pennebaker and Beall (1986), **original P/A**, studied 46 undergraduates writing over four consecutive days. The abstract specifically associates writing about both the facts and emotions surrounding a traumatic event with greater immediate negative mood and blood pressure, followed by fewer health-center visits over six months. The authors described the findings as preliminary. This does not establish the same result for every trauma-writing condition, and health-service utilization is not equivalent to disease prevented.

Pennebaker (1993), **A**, has the title of the book's cited 1992 presentation and discusses disclosure, language and health. That supports a connection to the narrative-building account, not proof that every liver-enzyme, immune or absence figure is verified or that the 1992 and 1993 texts are identical.

Jensen-Johansen et al. (2018), **A**, randomized 507 Danish breast-cancer patients to expressive or neutral writing. Main physical-symptom and GP-visit/call outcomes at three and nine months did not show the expected group-by-time effect.

Abu-Odah et al. (2023/2024), **A**, reports 34 studies and 4,316 participants, with some fatigue/physical-quality-of-life benefit but no pooled anxiety, depression or global-quality-of-life benefit. A malformed abstract interval is not reproduced as a valid statistic.

Han et al. (2026), **entire one-page P/A only**, reports favorable psychosocial and quality-of-life outcomes across 24 studies, 22 pooled, with a search through August 2023. Exact effects, bias tables and overlap with earlier trials were not inspected. A null physical-symptom/GP-use result does not directly contradict a favorable anxiety or quality-of-life synthesis. Neither establishes cancer survival.

Hip-fracture care

Strain et al. (1991), **A**, compared baseline-year referral practice with an experimental-year psychiatric liaison service among 452 older hip-fracture patients at two hospitals. Length of stay changed 20.7→18.5 days and 15.5→13.8 days; estimated savings were \$166,926 and \$97,361.

The book reports a real site's savings and an approximately two-day shorter stay, but narrows the broader psychiatric screening/consultation service to therapy for depression. It does not claim random assignment. The before/after design limits causal attribution: secular changes in care and differences between the patient groups remain possible explanations.

Broader care evidence

Powell et al. (2016), **A plus authors' plain-language summary; not a full-report audit**, synthesizes 105 randomized preoperative-preparation studies with 10,302 participants. Low-certainty evidence suggests less pain/negative emotion and about half a day shorter stay. Discharge timing is not identical to biological healing speed.

Kelley et al. (2014), **S**, includes 13 randomized relationship interventions and finds a small clinical effect, $d=.11$, $p=.02$. Satisfaction/adherence-only outcomes were excluded, but eight interventions contained other components. Its correction concerns affiliations/funding, not the effect.

Howick et al. (2018), **A plus full erratum**, includes 29, not 28, trials and 6,017 participants. Seven empathy trials yield a small pooled pain/anxiety/satisfaction effect, $SMD = -.18$ $[-.32, -.03]$. The mixed endpoint set is not a disease-survival result.

Assessment: meaningful care benefits exist independently of longevity. Writing is outcome-specific, and the historical physiological claims remain partly untraced. The hip-fracture intervention description needs correction; the nonrandom design limits the causal inference.

El-A20 — Ornish's multicomponent program

Book: text00018.html ¶100.

Note: text00035.html ¶272, citing the 1991 trade book.

Role: objective package result and component attribution.

Ornish et al. (1990), **M with extracted tables, not a complete visual table audit**, included 48 participants: 28 intensive lifestyle and 20 usual care. Forty-one repeated angiography at one year.

The intervention combined an approximately 10%-fat vegetarian diet, exercise, smoking cessation, extensive group support and daily stress management. Mean coronary diameter stenosis changed from 40.0% to 37.8% in the intervention group and 42.7% to 46.1% in controls. The endpoint is lumen narrowing, not direct plaque-volume measurement. Lesions are nested within patients and cannot be counted as independent participants.

Randomized invitation preceded consent, creating a selection issue. The sample was small and selected. The authors leave component contributions unresolved. Goleman explicitly mentions the diet, so an allegation that he hid it would be wrong. Relaxation's special contribution is nonetheless unisolated. The trial excluded patients scheduled for bypass. That does not by itself refute the book's description that disease was severe enough to warrant bypass, but the inspected eligibility criteria do not establish that description for every participant.

Ornish et al. (1998), **S**, follows the same trial. Thirty-five participants supplied five-year angiograms. Mean stenosis change was -3.07 versus $+11.77$ percentage points. Twenty-five versus 45 events include repeat hospitalizations and procedures, not 25 versus 45 distinct patients; deaths were two versus one. The trial does not demonstrate a survival advantage. The formal correction adds an omitted author.

Aldana et al. (2007), **A**, randomized 93 coronary patients to an Ornish program or traditional rehabilitation. Neither group significantly reduced carotid intima-media thickness, although several other outcomes favored the intensive program. Carotid wall thickness differs from coronary stenosis; this is not an exact failed replication.

Assessment: the original objective package result deserves credit. Relaxation alone removing plaque, eliminating surgery or extending survival is not established. Component uncertainty restricts attribution, not the existence of the measured package benefit.

El-A21 — Parent coaching, school readiness and maltreatment

Book: [text00020.html](#) ¶011–045, ¶048–062; related neural extension [text00022.html](#) ¶052–053.

Notes: [text00035.html](#) ¶279–291.

Role: parent practices, developmental prediction and reciprocal causes.

Parent coaching

Hooven, Gottman and Katz (1995), **S**, is the located publication corresponding to the book's forthcoming-sounding source. Fifty-six advertisement-recruited families were studied, with 53 contacted three years later. Children were approximately five and eight, rather than five and nine. The study is observational and exploratory; its authors explicitly do not treat the structural model as a causal test.

The 1996 author reply, **S**, reports reverse-direction models equally compatible with the data. Easier child regulation could facilitate coaching. That does not show that parenting is irrelevant; it identifies causal ambiguity.

Havighurst et al. (2010), **A**, randomized 216 parents to Tuning in to Kids or waitlist. Six sessions and two boosters produced improvements in observed emotion discussion, child emotional knowledge and some behavior ratings over six months.

Wilson, Havighurst and Harley (2012; online 2011), **A**, tests community delivery in 128 families. Parent-reported child behavior improved comparatively; some teacher and social-competence improvements occurred in both groups. Raters should not be collapsed into universal confirmation.

England-Mason et al. (2023), **A**, synthesizes 15 randomized trials with untreated controls. Reported effects include $g=.50$ for emotion-socialization practices, $.44$ for child emotional competence and $.34$ for behavioral adjustment. This strengthens the causal practical case without proving active-comparator superiority, the original neural pathway or adult success.

Readiness

The *Heart Start* report behind [text00020.html](#) ¶028–038 was not retrieved. Duncan et al. (2007), **A plus corrected-paper record**, finds across six longitudinal datasets that early math, reading and attention are strong predictors of later achievement; socioemotional measures generally do not add prediction after controls. This challenges a broad comparative readiness claim, not all benefits of social skills. Its correction concerns a supplement DOI, not findings.

Maltreatment

Main and George (1985), **A**, describes 10 abused and 10 matched toddlers. The book's use of nine per group is not declared an error: opportunity-level observation availability could differ from recruitment, and the full analysis was not recovered.

Widom (1989), **A**, emphasizes that most maltreated children do not become delinquent, criminal or violent. Widom, Czaja and DuMont (2015), **A**, finds transmission estimates differ by type and informant and may involve surveillance/detection bias. The book's own note

acknowledges debate.

Assessment: parent teaching receives substantial favorable credit. Original correlations do not establish one-way causation, comparative readiness superiority or inevitable transmission of abuse.

EI-A22 — Temperament, plasticity and clinical relearning

Book: text00021.html ¶019–069; text00022.html ¶003–056.

Notes: text00035.html ¶295–333.

Role: change, constraints and biological explanations.

Exact historical Kagan/Arcus percentages and some frontal-asymmetry claims were not independently recovered. They remain unresolved rather than false.

Clauss and Blackford (2012), A, reports a strong association between childhood inhibition and later social anxiety, OR=7.59. This is not a probability that every shy child becomes ill.

Rapee et al. (2005) and the 2010 follow-up, both A, provide favorable randomized evidence. A six-session parent program reduced anxiety diagnoses without significant measured inhibition/withdrawal change. The three-year report enrolled 146 inhibited preschoolers; 121 supplied data at two or more occasions. The trial supports improvement in anxiety without a detected effect on the inhibition measures; this does not establish that temperament remained unchanged.

Baxter et al. (1992), A, reports reduced normalized right-caudate metabolism among OCD treatment responders after fluoxetine or behavior therapy. This is a biological correlate of improvement, not a trial proving equal efficacy of the treatments or validating every cortical-inhibition explanation.

Hoppen et al. (2023), A, synthesizes 157 randomized trials of psychological treatments for adult PTSD, involving 11,565 patients. Treatments with and without trauma focus can help; trauma-focused CBT has some advantages and somewhat greater dropout. Heterogeneity and network inconsistency limit precise rankings.

Goleman recognizes persistent vulnerability, adult learning, genetic constraints and speculation. Clinical success supports the possibility of relearning without establishing every 1995 neurotransmitter account, a unique therapeutic sequence or ordinary-school EI efficacy.

Assessment: substantial support for bounded change and effective learning; less complete support for specific developmental-neural explanations.

EI-A23 — Targeted aggression, depression and friendship programs

Book: text00024.html ¶040–043, ¶074–080, ¶105–108.

Notes: text00035.html ¶347–348, ¶370–371, ¶381–382.

Role: preselected favorable prevention examples with outcome and population boundaries.

The nearby notes ¶347–348 concern aggression mechanisms, not the three-year Lochman intervention. The intervention account includes an interview at [text00024.html](#) ¶041; ¶043 has no distinct source note. The Penn threshold figures also lack a distinct note here; ¶371 cites Garber's "Cognitions."

Anger coping

Lochman (1992), A, reports lower drug/alcohol involvement and higher self-esteem/problem-solving at three years among anger-coping participants. Overall delinquency and classroom-behavior effects were absent; a booster subgroup retained some classroom improvement.

The book's unqualified classroom-improvement description overgeneralizes this published result. That does not erase favorable substance-related and self-esteem outcomes. Exact allocation, subgroup size and dose comparisons remain incompletely checked.

Depression prevention

Clarke et al. (1995), A, screened 1,652 adolescents, excluded current affective diagnoses and randomized 150 to 15 group sessions or usual care. Twelve-month affective-disorder incidence was 14.5% versus 25.7%: an 11.2-percentage-point difference, approximately 44% relative reduction.

That supports the book's rounded diagnostic direction and magnitude. It does not verify its eight-session duration or every recovery figure. The cited 1993 presentation remains unavailable and is not silently replaced.

Jaycox et al. (1994), S, reports symptom and some behavioral benefits in a more complicated school/volunteer comparison, not clean individual randomization. Gillham et al. (1995), A, reports two-year symptom prevention. Exact book threshold percentages were not recovered from full tables.

Brunwasser, Gillham and Kim (2009), A, reports small Penn Resiliency Program symptom effects against no intervention, $d=.11-.21$, but limited evidence of superiority to active controls. Bastounis et al. (2016), A, restricted to universal randomized PRP/derivative trials, finds no convincing benefit on its selected outcomes. Different inclusion rules and populations prevent declaring the earlier targeted trial erased.

Friendship coaching

The original Asher/Williams practical item and unpublished Nowicki manuscript remain unrecovered. Oden and Asher's 1977 study, accessed through its original-publication record and abstract, establishes an experimental tradition, not the book's exact 50–60% or normalization figures.

Kjøbli and Ogden (2014), A/P, randomized 198 children with conduct problems to individual social-skills training or usual practice. One of ten outcomes favored training immediately; none did at six months. This different population and program limits broad transfer rather than refuting all coaching for nonaggressive isolated children.

Assessment: real targeted benefits coexist with booster, comparator and population limits. Symptom prevention, diagnosed-disorder incidence and classroom conduct are not interchangeable.

EI-A24 — Emotional deficits and psychiatric/developmental risk

Book: text00024.html ¶050–073, ¶083–089, ¶097–103, ¶110–128, ¶142.

Notes: text00035.html ¶357–399.

Role: prediction, causal interpretation and clinical scope.

The clearest discrepancy is the eating-disorder account. At ¶084–086, Goleman praises prospective research and describes a causal chain. The attached source is Leon et al. (1993), A, which explicitly reports cross-sectional first-year findings in 937 girls.

The outcome is a risk-status composite involving diagnostic criteria and weight/attitudinal information, not prospective incidence of diagnosed anorexia or bulimia. Body dissatisfaction, negative emotionality and poor interoceptive awareness correlate with risk. That design cannot establish temporal ordering or show that other mechanisms are irrelevant.

Leon et al. (1995), A, does report three-wave prospective findings in 852 girls and 815 boys. Prior risk is the strongest predictor. After adjustment, poor interoceptive awareness and race predict later disordered eating in girls; only prior risk remains significant for boys.

Goleman's interview could have included newer findings, so the discrepancy does not establish invention. Even the prospective paper does not identify the complete causal chain or test the clinical treatment recommendation experimentally.

The other depression, rejection and substance-use source relationships were read in the book but not all independently reaudited at primary-data level. Exact dropout, opioid and biochemical subgroup counts remain unverified.

The book recognizes reciprocal effects and biological, social and economic causes. Its statement that acquiring emotional skills removes the impetus for substance use at ¶122 is stronger than that multi-cause framework and the mixed intervention evidence support.

Assessment: regulation and relationships are relevant risk-related constructs. A unified demonstrated EI cause of heterogeneous psychiatric conditions is not established. The Leon design discrepancy is specific; the unverified statistics are not treated as errors.

EI-A25 — Common prevention ingredients and transfer

Book: text00024.html ¶129–141; text00025.html ¶015–021, ¶097, ¶129–139, ¶153–188; appendices D/E.

Notes: text00035.html ¶401–410.

Role: program packages, proposed ingredients and adult outcomes.

Common elements and prevention endpoints

The W. T. Grant list identifies common emotional, cognitive and behavioral elements. Shared presence in successful programs is not a randomized dismantling test. That limits claims about causal ingredients; it does not invalidate the programs.

Finkelhor, Asdigian and Dziuba-Leatherman (1995), A, surveyed 2,000 youths aged 10–16 and their caregivers. More comprehensive school programs were associated with greater knowledge, recommended self-protection and disclosure, but not lower assault severity; exposed children reported more injuries during sexual assaults. Comprehensive parental instruction was also associated with knowledge, recommended responses and disclosure, and with lower assault severity. These associations do not establish causality. The follow-up more than a year later, A, followed 1,457 children and did not find reduced victimization, injury or upset associated with more comprehensive school prevention, while retaining some favorable response/disclosure associations.

Walsh et al. (2015), A, synthesizes 24 trials with 5,802 participants. Knowledge and protective-behavior benefits are supported; disclosure evidence is less certain after clustering is considered. Actual long-term abuse prevention is inadequately measured.

The book's disclosure comparison must not become a claim that abuse incidence fell by the same factor. Prevention knowledge and prevention of victimization are different outcomes.

Head Start/High-Scope source limit

At [text00025.html](#) ¶097, the book attributes several adult benefits to well-run Head Start. The note at [text00035.html](#) ¶410 identifies only an April 1993 High/Scope Educational Research Foundation report.

That citation does not identify the exact program, cohort or report title sufficiently to certify the attribution here. The original report was not recovered. It would be improper either to validate the Head Start claim from the foundation's name or to silently substitute a familiar Perry Preschool report and declare the issue resolved. This remains a specific source/program-identity gap.

Fast Track at age 25

Dodge et al. (2015; online 2014), A, reports follow-up of 891 high-risk children assigned by school to a ten-year program or control. The program combined parent training/home visits, social-skills groups, peer work, academic tutoring and classroom curricula.

At age 25, at least one psychiatric problem occurred in 59% versus 69%, with reported adjusted OR=.59 [.43,.81] and NNT=8. The raw ten-percentage-point difference and model-derived NNT should not be treated as identical calculations. Some crime, risky-sex and well-being outcomes also favored intervention.

The 2015 correction, read in full by the coordinated checker, corrects authorship. It reports no change to data, analyses or findings.

Fast Track at age 31: corrected full pattern

McCabe et al. (online 2024; issue 2025), **S plus complete Supplementary Table S4**, is a follow-up of the same randomized cohort, not a new trial. The original groups were 445 and 446; 657 provided age-31 survey data. Models used baseline covariates, school-clustered standard errors and full-information maximum likelihood.

The abstract's blanket nonsignificance summary does not match the Results and tables.

Endpoint/model component	Direct effect, Table 2	Total effect, Supplement S4
Full-time employment	OR .55 [.36,.79]	OR .68 [.49,.94]
Substance crime: structural-zero component	OR .54 [.28,.82]	OR .66 [.44,.98]
Substance crime: severity-weighted count component	IRR .80 [.57,1.07]	IRR .76 [.59,.96]
Violent crime: severity-weighted count component	IRR .82 [.61,1.17]	IRR .76 [.58,.99]

Most other total estimates are nonsignificant. The favorable count-component totals and adverse employment/zero-component totals must both be reported.

Zero-inflated Poisson components answer different questions. Lower modeled severity-weighted counts can coexist with a less favorable structural-zero component. IRR=.76 is not evidence that 24% fewer people committed a crime. Nor does a structural-zero parameter equal a simple observed proportion of nonoffenders.

Direct models adjust for childhood/adolescent competencies; total models do not make those mediator adjustments. Favorable indirect paths through interpersonal skills are reported, but mediators were not independently randomized. They do not identify one component's causal contribution or replace the total assignment contrast.

These are nominal tests across many outcomes without multiplicity adjustment. Covariate control and bootstrapping do not supply familywise error control. The authors note equal prevalence of any employment in both arms and suggest parenting/part-time work as a possible explanation of the full-time difference. That is a hypothesis, not established mediation.

Conclusion: mixed adult results, not universal persistence, universal disappearance or general harm.

Fast Track's 2026 intergenerational evidence

Gorla, Rothenberg and Godwin (2026), **S plus complete Appendix S1 and selected visual checking**, studies a selected parenting follow-up. There were 374 age-34 survey respondents, 191 intervention and 183 control; 403 participants with some relevant data entered path models.

The researchers formally compared constrained and freely estimated paths between arms. The G1 partner-violence→G2 parent-to-child-violence path differed across five related models, χ^2 differences 12.95–13.77, one degree of freedom, all reported $p < .01$. This is not

merely significance in one arm and nonsignificance in the other.

The total-difficulties model's reported standardized component coefficients were .39 in controls and -.09 in intervention participants. Complete indirect-effect products through parenting were not separately contrasted between arms in the inspected analysis. Their differing within-arm significance is not such a test.

The same cohort and overlapping measures underlie the five models. G1 exposure partly overlaps intervention years; G2 violence and G3 difficulties share a contemporaneous parent reporter; parenting follow-up is selected; and covariates were selected after an under-identification problem. Model-fit statistics do not establish causality. No broad mean reduction in every violence or child-health measure follows.

Assessment: substantial packages can produce worthwhile and sometimes durable benefits. The evidence is relevant to Goleman's sustained, multisystem proposal. It leaves uncertainty about which components produced the benefits and does not verify every adult and intergenerational expectation.

EI-A26 — All six Appendix F program families

The complete Appendix F range is [text00031.html](#) ¶003–103. Every program below remains in the audit. Original source access and later evidence are distinguished.

A26a. Child Development Project — ¶003–018

The cited Schaps/Battistich 1991 and Solomon et al. 1992 chapters were traced but not fully recovered.

Solomon et al. (1988), **A**, describes three program and three comparison schools followed as a cohort moved from kindergarten through fourth grade. Classroom observations favored supportive, friendly and spontaneous prosocial behavior.

Solomon et al. (2000), **author-upload A**, describes 12 intervention and 12 comparison schools in six districts. Favorable personal/social/ethical findings were concentrated in five schools showing implementation progress; academic benefits occurred in two schools with performance-based assessment and a consistent reform setting.

Assessment: favorable outcomes exist, but implementation-selected and local-assessment results do not establish a uniform program effect. Literature instruction, cooperative learning, classroom management, community and parent involvement are part of the package. Unread original chapters prevent certification of every appendix bullet.

A26b. PATHS — ¶019–046

Greenberg et al. (1995), **A**, reports 30 randomized classrooms and 286 second/third graders, approximately 30% in special-needs classrooms. Emotional vocabulary, conceptual understanding and efficacy beliefs improved.

The cited 1992 Fast Track source is a model paper, not a completed trial demonstrating every listed outcome. The 1993 deaf-program book remains unrecovered. A 1998 deaf-program report, accessed at abstract level, includes 57 children in 11 classrooms with a quasi-experimental waitlist comparison. Social cognition/competence improved, without teacher/parent psychopathology benefit in the normative sample.

Schonfeld et al. (2015), A, reports academic-proficiency benefits at some grades in 24 randomized schools and a 705-pupil stable cohort.

Hennessey and Humphrey (online 2019; issue 2020), **A plus selected methods**, reports no positive attainment effect in a 45-school English trial. Year-5 and year-6 analyses involved 1,705 and 1,631 pupils. Implementation profiles did not explain attainment effects.

Assessment: emotional-learning benefits and some academic findings are credible. Proficiency thresholds, continuous attainment, raters, populations and comparison practice differ. The English result is not proof that all PATHS outcomes fail. Riggs's narrower mechanism evidence is assessed in A27.

A26c. Seattle Social Development Project — ¶047–060

The cited Hawkins 1991 source was not fully obtained. O'Donnell et al. (1995), A, reports a six-year package of teaching practices, parent training and child skills. School commitment/participation improved; substance-initiation benefits were reported for girls and work/social skills for boys. The book's 1994 date differs from the final publication.

Hawkins et al. (2008), *Effects of Social Development Intervention in Childhood 15 Years Later*, S, follows 598 participants at ages 24/27, with 93% retention for the compared conditions. Initial classroom randomization expanded into nonrandom school conditions, making this follow-up quasi-experimental.

Some attainment, mental-health and sexual-health indices favored intervention. Adult substance-use and crime benefits were not significant in that report. A favorable socioeconomic composite does not mean every education or income endpoint improved.

Assessment: a meaningful favorable durability case, with package, design and outcome limits.

A26d. Yale–New Haven Social Competence Promotion — ¶061–076

The cited Elias/Weissberg chapter remains unrecovered.

Caplan et al. (1992), A, examines 282 sixth/seventh graders in a 20-session program combining stress management, self-esteem, problem-solving, substance information, assertiveness and social networks.

Reported benefits include interpersonal problem-solving, anxiety coping, teacher-rated conflict resolution, impulse control and popularity, self-reported efficacy and some substance-use intentions/excessive drinking outcomes. Full allocation and attrition procedures were not verified.

Assessment: a favorable controlled-program example with proximal and rater-specific outcomes. No separately verified adult follow-up of this exact trial was recovered.

A26e. Resolving Conflict Creatively Program — ¶077–087

The original May 1990 Metis evaluation of 1988–89 remains unrecovered. The book itself describes teacher ratings before and after participation, without specifying a control group. It should not be relabeled a randomized trial.

Aber et al. (1998), S, studies 5,053 pupils in grades 2–6 across 11 schools and two waves. High-lesson profiles show slower worsening on some aggression-related processes; a low-lesson/high-training profile sometimes fares worse. Profiles were not randomly assigned, and the authors acknowledge implementation confounding.

Aber, Brown and Jones (2003), publisher-supplied A, extends the trajectory evidence across four waves and two years in 11,160 pupils.

Assessment: later evidence is stronger than the original perception summary alone, but it remains conditional and quasi-experimental. Aggression-related cognitions and trajectories are not demonstrated prevention of future violent crime. High training/low delivery does not establish that training causes harm.

A26f. Improving Social Awareness–Social Problem Solving — ¶088–103

Elias et al. (1991), A, reports a two-year elementary-school program followed six years later with controls. Social-adjustment and psychopathology findings are favorable but modest overall, with notable gender differences.

The full article and Elias/Clabby 1992 book remain unavailable. A sample of 420 and nonrandom-allocation details appeared only in secondary summaries and are not certified here.

Assessment: retain the favorable long-follow-up finding and the original authors' qualifications. Do not assert equivalent benefits for every pupil or treat generic SEL syntheses as direct replications of this program.

Overall A26 judgment

The evidence is heterogeneous and contains genuine strengths. Neither “six fully verified randomized replications” nor “six uncontrolled anecdotes” describes the appendix. Each program's accessible results receive credit at their actual scope; unresolved bullets and original chapters remain unresolved.

EI-A27 — Anniversary meta-analysis and PATHS mechanism

Book: text00003.html ¶012–015.

Notes: text00035.html ¶003–004.

Role: anniversary numerical claims, modern school evidence and the specific neural interpretation.

The unrecovered 2005 presentation

The note identifies Durlak and Weissberg's August 2005 presentation, *A Major Meta-analysis of Positive Youth Development Programs*, and a separate Weissberg address.

The book reports a meta-analysis of 668 evaluation studies. It says that, in participating schools, up to 50% of children showed improved achievement scores and up to 38% improved their grade-point averages. Those are stated proportions of children, not the size of score increases. It also reports an average 28% decline in misbehavior incidents, 44% fewer suspensions, 27% fewer other disciplinary actions, and 63% of students showing significantly more positive behavior.

The original presentation and calculations were not recovered. The book specifies the type of denominator for its 50%, 38% and 63% statements, but we have not verified the underlying samples, comparison conditions, definitions of improvement or calculation methods. Later citations establish the presentation's existence; they do not verify those details. Student proportions, percentage changes in disciplinary incidents and effect-size or percentile measures from later syntheses are not interchangeable. The empirical figures remain unverified; this access gap does not make them false.

Later syntheses: favorable evidence, different corpora

Source	Access, corpus and principal results	Interpretation
Durlak et al. 2011	S: methods, Tables 1–3, bias checks and limits. 213 controlled studies through 2007; 270,034 pupils; 99 randomized and 114 nonrandomized. Skills ES=.57; attitudes .23; positive behavior .24; conduct .22; distress .24; academic .27 [.15,.39], from 35 studies.	Supports average benefits. The academic result is not based on 213 academic studies. Its 11-percentile-point translation is not an 11% grade rise or 11% of children improving. Only 33 studies had follow-up of at least six months.
Taylor et al. 2017	S: methods/tables/results/limits. 82 programs, 97,406 students; follow-up six months–18 years. Fifty programs had study-average follow-up under one year. Academic follow-up ES=.33 [.17,.49], from eight studies, mean 195 weeks.	Favorable follow-up evidence, not 82 eighteen-year demonstrations. Most outcomes were self-reported. Nonsignificant demographic moderators do not prove equality, especially with missing identity data.
Cipriano et al. 2023	A: 424 reviewed studies, 53 countries, 252 interventions, 575,361 pupils; 2008–2020 corpus; favorable outcomes with heterogeneity.	Full exact effect tables were not inspected. A 2024 secondary analysis, accessed in methods excerpts, clarifies that 258 studies supplied effect-size statistics.
Ha et al. 2025	S: publisher methods, results, Tables 1 and 3–6 and limits. Secondary analysis of the Cipriano corpus: 40 studies, 30 programs, 12 countries, 33,737 pupils, 76 effects; 29 randomized and 11 quasi-experiments. $g=.101$ [.035,.168], $p=.004$; $I^2=85.64\%$.	Favorable academic evidence from 2008–2020 studies, not 40 new 2025 trials. Follow-up achievement was not analyzed.
Jones et al., online 2025/issue 2026	A: 158 articles, 97 trials, 32 CASEL-selected programs. Specific benefit evidence was sparse for named racial/ethnic groups.	A reporting/transportability limitation, not evidence that untested groups do not benefit.

Ha's analysis uses dependence-sensitive methods and caps three large positive outliers at two standard deviations. Its nonsignificant Egger test, $p=.309$, is not proof of absent bias. Grade and test averages are favorable, as are literacy and math; smaller middle/high-school subsets are nonsignificant. One subgroup significant and another nonsignificant does not establish a difference between them.

Ha's introduction describes Durlak's .57 too broadly; the original Durlak table identifies .57 as the social-emotional-skills estimate. This appendix uses each source for its own inspected result rather than importing that secondary description. The 8 and 17 October 2025 publisher versions were not compared.

SAFE and implementation moderators in these syntheses are not randomized component tests. The differing averages across reviews do not demonstrate temporal decline in one invariant treatment effect.

Riggs: what was actually measured

The book cites a **2005 under-review manuscript**, not merely personal communication. The published version is Riggs et al. (2006), S, with methods/results/Tables 2–3 and limitations examined.

Four Seattle schools, two per condition, were randomized; 318 second/third graders were analyzed. Posttest measures included Stroop inhibitory control and categorical verbal fluency; teachers rated internalizing/externalizing behavior at the one-year follow-up. The program improved tasks and ratings; inhibitory control statistically partially mediated the behavioral results.

The paper did **not** measure academic attainment, a dedicated working-memory outcome or brain imaging. Its limitations call for further work linking task changes to cortical structure/function. Four randomized schools provide fewer independent intervention units than 318 independently randomized pupils.

The introduction instead describes academic gains attributable to attention and working memory and infers a key neural mechanism. The accessible publication does not verify that account. The original 2005 manuscript remains unavailable, so a definitive reconstruction of what changed between manuscript and publication is not claimed.

Assessment: later school benefits deserve substantial credit. The original 668-study calculations remain unresolved; the available mechanistic source supports a narrower conclusion than the introduction.

EI-A28 — Childhood decline and the author's correction

Book: text00004.html ¶026; text00024.html ¶006–021; text00003.html ¶039–044.

Notes: text00035.html ¶007, ¶337–345, particularly ¶343.

Role: preselected favorable self-correction and trend interpretation.

Achenbach and Howell (1993), A, compares 1989 reports with earlier parent and teacher cohorts. Average problems increase and competence decreases, with small broad effects. The book's endnote dates the paper 1989 rather than 1993.

Achenbach, Dumenci and Rescorla (2003), A, compares parental reports from 1976, 1989 and 1999. Competence improves and problems fall by 1999, though problems remain above the 1976 level.

This supports Goleman's anniversary acknowledgment of improvement and his uncertainty about the explanation. The measures are behavior-problem/competence scales, not direct national EQ tests. They do not establish a worldwide continuous decline, an SEL-caused reversal, or the technological explanation discussed in the book.

Exact threshold and doubling calculations remain unverified without the complete tables.

Assessment: credit the author's explicit update. Preserve the historical trend evidence while distinguishing measurement, time interval and causal speculation.

07 Practical use, trade-offs and safeguards

What earns practical credit

The Practical Value score concerns the advice as supplied, not a safer replacement written by the reviewer.

Emotion regulation: the book offers more than "control yourself." Its distinctions among arousal, rumination, reappraisal and constructive action have support from selected experiments and later syntheses. Those findings are strongest for the actual practices and outcomes tested, not every exercise or clinical claim in the chapter.

Feedback and repair: specifying the problem, avoiding character attacks and repairing harm have relevant primary evidence. The evidence is narrower than proof that every script improves company productivity, but it is not merely an anecdotal recommendation.

Parenting: coaching practices have later randomized support, including observed or rated child outcomes. This provides practical credit even though the original observational family model did not identify one-way causation.

Schools and prevention: several programs improve intended outcomes. Goleman's emphasis on developmental fit, teacher preparation and continuing reinforcement is reasonably aligned with how successful programs were delivered.

Clinical care: psychological support can improve meaningful outcomes, and some physical benefits are supported. Relief of pain, depression or distress is worthwhile even when mortality is unchanged. The book's rejection of patient blame and endorsement of ordinary medical treatment materially reduce foreseeable misuse.

Where the evidence-to-reader bridge is weaker

A person reading a book does not automatically receive the intervention tested in a trial. Programs may require trained delivery, repeated sessions, home support, specific eligibility criteria or infrastructure. These conditions are not defects by definition; they are part of what must be reproduced or adapted thoughtfully.

Evidence for a population mean does not identify the best intervention for a particular person. Nor does a good score on an EI inventory demonstrate the ability to handle a specific workplace, family or clinical situation.

Durability is also outcome-specific. Some benefits appear years later; others do not, and some require boosters or continuing practice. The longest follow-up in a review is not a guarantee for every included program.

Burdens and autonomy

The relevant costs include time, money, training, classroom displacement, emotional discomfort and access to qualified support. This preparation did not establish a comprehensive cost-effectiveness ratio or measure a book-attributable harm rate.

The book recognizes educational privacy concerns. It does not provide a complete consent, confidentiality or data-protection procedure. Recognition receives credit as a qualification, not as proof that privacy is resolved.

Its moral language also needs restraint. Difficulty identifying feelings is not evidence of lesser humanity. Skill at reading others is not equivalent to concern for them. The book's own examples of manipulation and calculated aggression partly acknowledge this.

For relationship advice, ordinary conflict and coercive violence are different circumstances. An application that assigns victims responsibility for regulating another person's violence would exceed the book's more careful note and is not endorsed here.

Proposed application, distinct from book evidence

A useful application is to specify a recurring situation and the behavior that needs changing: a manager communicating revisions, a parent setting a limit, or a school reducing a defined pattern of conflict. Select a practice with relevant evidence, attend to the environment, and evaluate the outcome and burden.

This is an editorial application rule, not a tested personalization system. It does not earn the book additional points. It also does not imply that every problem should be solved by more self-regulation; workload, safety, access, incentives and the suitability of the goal may matter more.

08 Evidence over time

Original-era evidence versus later findings

The book's 1995 main text and 2005 introduction must not be treated as if all content was newly researched for the 2012 ebook product.

Period	Evidence or change	Consequence for this review
Before/around 1995	Animal-conditioning findings, early family and school work, Baron criticism studies, original cold challenge, RCPP and Spiegel's initial trial	Provides real original support. Some source-description and inference problems can be assessed against these contemporaneous publications without relying on later replication.
Around the original publication	Leon 1993 cross-sectional report and 1995 prospective follow-up; Clarke's 1993 presentation and 1995 publication	Versions must be distinguished. A later publication may support a narrower account without proving exactly what an earlier interview or presentation contained.
2001–2005	Goodwin's metastatic-breast-cancer trial, ENRICHED, Achenbach's later trend comparison, the 2005 conference synthesis and under-review Riggs manuscript	Some limiting evidence existed by the introduction. This does not prove Goleman personally knew each finding or rewrote all main-text claims in 2005. The trend correction is explicitly incorporated.
2006–2011	Published Riggs study, later cancer trials, school/work EI syntheses, self-control and social-support evidence, SUPRIM	Strengthens some practical claims while narrowing source-specific mechanisms and survival expectations.
2013–2018	Amygdala-lesion counterexample, parenting and school follow-ups, clinician-care synthesis, ENHANCED, waiting-task reassessment, practice and care errata	Changes present interpretation. A correction to a later synthesis is not an error Goleman could have anticipated.
2019–2024	English PATHS attainment results, broad 2023 SEL review, parent-program synthesis, social-disconnection cohorts, anger synthesis, adult waiting-task follow-up, age-31 Fast Track publication	Provides favorable and contrary current evidence; outcome definitions and shared cohorts remain important.
2025–15 September 2026	Human tractography, Ha's secondary academic analysis, Robinson–Zell synthesis and inspected methods, Davis–Mahoney review-quality study, favorable cancer synthesis and intergenerational Fast Track analysis	Prevents reliance only on familiar older critiques. Publication recency does not make all underlying studies recent or independent.

Source descriptions that can be corrected now

The strangers-versus-spouses distinction, Leon's cited cross-sectional design, the narrowing of the hip-fracture intervention to depression therapy, and the cardiac-arrest/infarction distinction are specific source issues.

Other cases remain version-bound. The published Riggs paper does not support the described academic mechanism, but the exact 2005 manuscript is unavailable. MetLife's recovered 1986 paper does not reproduce every later company figure. The original 668-study presentation and several private numerical comparisons remain unresolved.

Corrections do not all have the same meaning

A changed empirical result, an authorship correction, a bibliographic repair and a substantive author disagreement are different events.

- The practice meta-analysis's 2018 correction changes numerical estimates.
- Riedl's 2022 correction changes average variance extracted and clarifies models.
- Fast Track's 2015 correction changes authorship, not results.
- Kelley's correction concerns affiliations/funding.

- Howick’s erratum changes the eligible-trial count to 29.
- Ornish’s author correction adds an omitted author.
- Duncan’s correction concerns the supplement DOI.

No retraction of the principal affirmative sources was identified in the inspected, bounded records. That is not an exhaustive no-retraction guarantee or scientific clearance. Unrelated notices involving papers that cite a source were not attributed to the source itself.

Review-side corrections

The age-31 Fast Track assessment uses Supplement S4. This appendix reports both favorable and adverse totals, not merely adverse direct effects alongside favorable indirect paths.

Ha’s newer access shows an outcome-specific secondary analysis of the older Cipriano corpus. It is not 40 additional independent recent trials.

The clinical score is revised through explicit scope adjudication, not by changing the frozen claim or adding a new rule.

09 Nine-input scoring rationale and calculations

The house method uses three integer inputs per category, each from 0 to 4. These are editorial anchors, not measured quantities. The same substantive issue can inform more than one dimension, but it does not receive automatic or unexplained multiple deductions.

Scientific Accuracy

C1 = 2 — Partly supported

Why not lower: specified EI measures and several constituent skills have meaningful positive associations, cross-method support and some added prediction. Direct correlations also support partial separation from conventional cognitive ability. The broad category is not evidentially empty.

Why not 3: the evidence examined does not yet establish the broadly enabling role claimed across domains or the stated EI-versus-IQ comparisons for emotional and relational outcomes and outstanding leadership among intellectually selected professionals. The remaining gaps concern central parts of the comparative explanation.

Scope preserved: the judgment does not require a single factor for every useful skill, independence from personality, or superiority over every competing predictor. It does not falsely assign EI the residual 80%.

C2 = 3 — Substantially supported, with qualifications

Why 3: randomized parent, targeted-prevention and school programs support teachability and several intended behavioral and academic outcomes. Sustained packages provide some adult evidence. This is support for much of the qualified proposition, not only one proximal

skill.

Why not 4: evidence for the stated adult relationships, work and citizenship outcomes is uneven, and the specific proposed neural explanation is not directly established. These material gaps keep the claim short of full support.

Scope preserved: multicomponent, continuing programs fit the book's multisystem proposal. No penalty is imposed simply because they require effort, and no stronger "brief ordinary lessons reliably work" criterion is substituted.

C3 = 3 — Substantially supported, with qualifications

Why 3: direct tests of structured psychological care support meaningful physical outcomes, including recurrent cardiac events, and current syntheses support survival benefit in some settings. The claim expressly permits setting-specific benefit alongside standard care.

Why not 4: the broad primary-prevention extension and smoking-cessation-sized forecast remain unestablished. Important null trials and pooled-evidence uncertainty bound the strength of the clinical conclusion. The original cancer near-doubling is assessed as a source and narrative claim in A18; it is not an additional numerical condition in the frozen C3 proposition.

Scope preserved: the grade does not require a universal EI therapy or proof that one emotional component is solely responsible. Neither does it treat a conditional forecast as automatically supported.

Adjudication: the strongest case for C3=2

Chapter 11's ¶103–104 makes broad prevention a principal implication, not an incidental aside. It proposes emotional education for children, retirees and people under severe social pressure. The smoking analogy helps motivate that recommendation and recurs in earlier risk comparisons.

Much favorable intervention evidence concerns existing disease, secondary prevention or structured care. It does not establish a comparable lifetime disease-prevention return from broad emotional education. If that extension is given dominant weight within the compound proposition, 2 is a defensible anchor.

Why this appendix nevertheless prefers C3=3

The frozen claim also centrally asserts additional medical benefit from distress management and support, with survival in some settings. The direct evidence reaches these substantive clauses across more than one clinical setting. It is not merely support for a peripheral comfort proposition.

The smoking forecast remains important and unverified. The grade treats it as a material limit preventing full support, rather than replacing the full qualified claim with that forecast alone. This is consistent with awarding C2 substantial support despite uncertainties in adult transfer and its proposed neural explanation.

The relative weighting of the compound clauses was not numerically prespecified. This is an explicit, judgment-sensitive adjudication, not a mechanical consequence of the number of positive papers. A reasonable neighboring-grade disagreement should remain visible.

Scientific calculation:

$$100 \times (2 + 3 + 3) / 12 = 200/3 = 66.666...\% \rightarrow 67\%.$$

Reference Accuracy

R1 Traceability = 3 — Mostly accurate

Many consequential sources are identifiable, including the cold study, original cancer trial, parent study, school reports, criticism studies and physiological-empathy paper. Notes distinguish some interviews and unpublished sources.

There are incomplete or imprecise trails, occasional dates/names and source-program ambiguity. These prevent top credit. Identifiable but inaccessible originals are not themselves traceability failures, and unrecovered conference calculations are not scored as fabrication.

R2 Description = 2 — Mixed

Faithful material includes the rounded cancer means, approximate cold rates, original practice comparison and 94% classification arithmetic. These counterexamples matter.

Consequential problems include the empathy participants, Leon's cited design, the hip-fracture intervention description and selected outcome generalizations. The available Riggs publication does not verify the described outcomes, with the original-version limit preserved.

This balance fits mixed rather than mostly accurate or fundamental failure. It is not a count-based estimate of how often the book is wrong.

R3 Inference = 2 — Mixed

The book often supplies genuine caveats and correctly bounded findings. It also repeatedly moves from association to causal explanation, selected prediction to broad hierarchy, package effects to a particular ingredient, or a measured endpoint to a larger outcome.

Description and inference are distinct here: identifying the wrong observed relationship damages descriptive accuracy; making synchronization necessary for empathy exceeds even the correctly described association. These are explained dimensions, not automatic duplicate penalties.

Reference calculation:

$$100 \times (3 + 2 + 2) / 12 = 175/3 = 58.333...\% \rightarrow 58\%.$$

Practical Value

V1 Intended benefit = 3

Several consequential recommendations have direct supporting evidence: constructive feedback, selected regulation practices, parent teaching, structured school programs and psychological care.

No trial of reading the book is required to credit a faithfully described established method. However, the full range of predicted adult and clinical returns is not comparably supported, and adjacent clinical treatment does not validate every ordinary EI exercise.

V2 Applicability and durability = 2

The book provides real implementation guidance and appropriate acknowledgment of continuing practice. Some interventions have meaningful longer follow-up.

Important gaps remain in population transfer, comparator choice, program selection, required resources and durable outcomes. Some findings depend on boosters or selected implementation settings; others do not persist uniformly. These limits concern the evidence-to-reader bridge, not the mere existence of effort or ongoing support.

V3 Net value and safeguards = 3

Professional-care guidance, rejection of patient blame, acceptance of emotion rather than suppression, teacher preparation and recognition of developmental fit provide meaningful boundaries.

Privacy objections are recognized but not resolved by a detailed protection procedure. Moralizing language and broad medical forecasts also create foreseeable interpretive risks. Those limits prevent top credit.

The reviewer's additional safeguards and behavior-selection proposals do not earn the book points. No measured harm rate or comprehensive cost-effectiveness estimate is asserted.

Practical calculation:

$$100 \times (3 + 2 + 3) / 12 = 200/3 = 66.666\ldots\% \rightarrow 67\%.$$

Overall, missingness and sensitivity

Inputs: 2,3,3 / 3,2,2 / 3,2,3.

Total: 23 of 36.

$$\text{Overall: } 100 \times 23/36 = 575/9 = 63.888\ldots\% \rightarrow 64\%.$$

The equivalent calculation averages the three unrounded category percentages. It does not average their displayed whole numbers.

No NR or NA is required for these bounded judgments. Several individual sources remain unresolved, but sufficient accessible material supports each criterion. Were a whole required criterion unrateable, the method would withhold its category and the overall rating rather than substitute zero or average the remaining criteria.

A one-grade change shifts its category by $8\frac{1}{3}$ percentage points and the overall by $100/36 \approx 2.78$ points. With $C_3=2$ and all else unchanged, the overall would be $22/36 = 61.111\ldots\%$, displayed 61%.

These are sensitivity amounts, not confidence intervals. Equal weighting is an editorial convention, and the categories are not independent statistical estimates.

10 Actual verification, access and remaining limits

What this preparation establishes

The complete supplied-text reading and frozen claim selection preceded the reader essay. AI source readers checked the essay against the book and external sources. The verification and evidence notes distinguish these checks from independent human scientific review.

AI-assisted verification included assessment of the research records, checks of selected book passages and simple arithmetic, followed by source-specific audits. None of these checks retroactively upgrades an earlier abstract-level check into a full-paper reading.

Targeted source checks following the combined critique

Source-specific checks completed by 15 September 2026. AI source readers checked disputed passages and proposed additions against the book and primary sources:

- **MetLife and cognitive ability:** Seligman–Schulman's relevant methods and sales/status tables, and O'Boyle's Table 3 cognitive-ability correlations, were inspected and visually checked. These checks distinguish the retrospective 37% sales comparison from prospective prediction, and EI–cognitive-ability correlations from incremental outcome prediction. They do not reconstruct later company analyses or independently rerun the correlation matrices.
- **Writing and medical care:** the complete APA abstract for Pennebaker–Beall was reread in the author-posted record. It supports the facts-plus-emotions condition; the full original trial and all between-condition comparisons were not audited. Strain's complete abstract and Smith's relevant primary eligibility passage were rechecked for the intervention-description and solely-individual-psychotherapy distinctions.
- **Childhood and education:** the complete Rapee 2005 and 2010 abstracts were reread. Their nonsignificant inhibition findings do not establish unchanged temperament. The anniversary introduction and its note were checked directly for the proportions of children whose achievement scores or grades improved; the underlying 2005 presentation remains unrecovered.

These are source-specific additions to access, not retroactive claims about earlier reading or fresh complete audits of each source.

Source-specific access that particularly matters

Source or group	Documented access	What is not claimed
Robinson–Zell 2026	Main abstract/significance/source information; complete supplement text, tables, references and captions; selected visual table checks; search log/codebook; selected code; complete deposited primary/bias output documents. Figures S1–S10 were not visually inspected.	Full main-paper reading, participant-level deduplication, code execution, repaired bias analysis or independent reproduction
MacCann 2020; Joseph 2015	Complete primary abstracts	Full included-study bias audits or independent reconstruction of the regression models
Baron 1988/1990	APA-supplied original first-page previews/abstracts	Complete methods, exact shampoo-task attribution or a randomized firm-productivity result
Original LeDoux work	Author bibliography, original abstract and indexed primary summaries	Complete original animal-method audit or direct proof of human conscious-fear timing
Feinstein 2013	Main article, online methods, analyses and captions in the coordinated audit	Large representative lesion evidence or verification of every physiological endpoint in all three patients
Levenson–Ruef 1992	Methods, analyses, principal tables and substantial discussion; selected tables visually checked	Every reference or final discussion sentence; proof that no related unpublished work existed
Hooven 1995	Selected scanned/OCR primary pages and author reply	Unchecked OCR coefficients or a causal family experiment
Cohen 1991	Main text through discussion, with original figure page visually checked	Complete correspondence resolution or functional-immune mechanism demonstration
Friedman–Booth-Kewley 1987	Preview/abstract	Full 101-study denominator, pooled scale or double-risk reconstruction
Spiegel 1989	Complete abstract	Original protocol/baseline audit or detailed allegations based on unseen methods
Goodwin 2001; Spiegel 2007	Complete abstracts plus selected indexed primary sections	Complete original-paper audits
Asakawa-Haas 2026	Abstract and selected methods/results/discussion	Full supplement review, independent HR reconstruction or rerunning the multiverse
Ha 2025	Publisher methods/results/tables/limitations in the later check	New independent trial corpus, follow-up analysis or comparison of the two October versions
McCabe age 31	Methods, analyses, Table 2, results/discussion and complete S4	Participant-level model rerun or multiplicity-adjusted findings
Gorla 2026	Selected main methods/results/discussion, complete Appendix S1, Table S2, selected S1 comparisons and Figure 2	All figures/cells, a whole-indirect-effect contrast not reported, or causal identification from fit statistics
Han 2026 writing synthesis	Entire one-page preview/abstract	Full inclusion table, exact effects or verified overlap with earlier writing trials
Appendix F originals	Program-specific abstracts/sections and later reports, with gaps identified separately	Certification of every historical result bullet

Material unresolved items

The unresolved cases include:

- The original 2005 668–study presentation and its percentage calculations.

- The exact 2005 Riggs manuscript.
- Private marshmallow tercile and IQ-comparison analyses.
- Several original selected-cohort, hope, optimism and flow comparisons.
- Later MetLife special-hire analyses.
- The original Pithers interview-based recidivism comparison.
- Full original source tables for the 1987 disease-prone-personality synthesis.
- The exact 1990 Thoresen 44% recurrence presentation.
- Several expressive-writing physiological endpoints and conference versions.
- The original Metis RCCP evaluation and several Appendix F chapters/books.
- Original friendship-coaching numerical comparisons.
- The exact High/Scope report and program behind the Head Start attribution.
- Full manuscript/version comparisons and some original-author correspondence.

These gaps constrain individual conclusions. They are not labeled false and do not automatically make a whole criterion NR.

Bibliographic and notice checks

An initial source-identity pass checked 64 unique external links in the reader plus three specified additions. That pass did not establish complete verification of every appendix link. It confirmed the intended original DOI identities and corrected Hennessey/Humphrey authorship attribution.

A failed follow-up download of the Robinson-Zell supplement was not treated as evidence that the supplement did not exist; its retained publisher-linked artifact had already been inspected. Likewise, metadata timestamps were distinguished from original print publication.

Notice searches were targeted and dated. Metadata establishing that a comment exists does not establish its argument or whether the author's reply resolves it.

Statistical and production limits

The arithmetic checks are not statistical reanalyses. No causal mediation model, treatment effect, pooled estimate, publication-bias correction or raw dataset was independently rerun for this appendix.

No inter-rater reliability calculation is claimed for the editorial grades. Independent AI critique is not independent human peer review.

The reader and appendix underwent AI source and editorial audits during preparation. Working HTML and PDF previews were also checked. The verification and correction sections identify completed checks and remaining source-access limits. The preparation record does not establish independent human scientific review.

11 References and source identity guide

The linked sources in Section 06 are the operative claim–source references: they identify the finding, access level and limitation beside its use. This section consolidates the main bibliographic identities and version relationships. It does not count them as additional evidence.

Book, method and primary source baseline

- **Goleman, Daniel.** *Emotional Intelligence: Why It Can Matter More Than IQ*. Main text copyright 1995; tenth-anniversary introduction copyright 2005; ebook ISBN 9780553903201 issued 11 January 2012. Supplied complete extraction and two relevant images.
- **The Behavioral Scientist.** *How We Review Books*, Edition 3.0, supplied method dated 8 September 2026. Three equally weighted categories and nine integer inputs; not the official Red Pen Reviews method.
- **EI-SCOPE-20260915-v1.** Recorded pre-research selection of three central claims and 28 purposive families. The full propositions are reproduced in Section 04.
- **Source supplement.** Publisher format verification, distinction from current anniversary copy, illustration identification and extraction–locator convention. Publisher record linked in Section 02.

Measurement, prediction and performance

Reference	Primary identity and relevant access
Salovey & Mayer, 1990	<i>Emotional Intelligence</i> . DOI. A.
Mayer, Caruso & Salovey, 2016	<i>The Ability Model of Emotional Intelligence: Principles and Updates</i> . DOI. A.
Joseph et al., 2015	<i>Why Does Self-Reported Emotional Intelligence Predict Job Performance?</i> DOI. A.
O'Boyle et al., 2011	<i>The Relation Between Emotional Intelligence and Job Performance</i> . DOI. S: initial methods/Tables 5–6 access; Table 3 and Table 6 visually checked with relevant text during the combined critique and independent follow-up, 15 September 2026.
MacCann et al., 2020	<i>Emotional Intelligence Predicts Academic Performance</i> . DOI. A; online 2019 distinguished.
Robinson & Zell, 2026	<i>Robust Associations of Emotional Intelligence with Human Flourishing</i> . DOI. Main abstract plus extensive supplementary/output access, not full main text.
Davis & Mahoney, 2026	<i>A Meta-Synthesis of Emotional Intelligence Research: Methodological Rigour and Linguistic Hype in Systematic Reviews and Meta-Analyses</i> . DOI. S.
Strenze, 2007	<i>Intelligence and Socioeconomic Success</i> . DOI. S.
Shoda, Mischel & Peake, 1990	<i>Predicting Adolescent Cognitive and Self-Regulatory Competencies from Preschool Delay of Gratification</i> . DOI. S, selected visual table check.
Watts, Duncan & Quan, 2018	<i>Revisiting the Marshmallow Test</i> . DOI. S.

Reference	Primary identity and relevant access
Michaelson & Munakata, 2020	<i>Same Data Set, Different Conclusions</i> . DOI. S; independent 2017 preregistration.
Sperber et al., 2024	<i>Delay of Gratification and Adult Outcomes</i> . DOI. S; same later source cohort.
Moffitt et al., 2011	<i>A Gradient of Childhood Self-Control Predicts Health, Wealth, and Public Safety</i> . DOI. S.
Snyder et al., 1991	<i>The Will and the Ways</i> . DOI. A/P.
Day et al., 2010	<i>Hope Uniquely Predicts Objective Academic Achievement Above Intelligence, Personality, and Previous Academic Achievement</i> . DOI. M.
Marques et al., 2017	<i>Hope- and Academic-Related Outcomes</i> . DOI. A.
Seligman et al., 1990	<i>Explanatory Style as a Mechanism of Disappointing Athletic Performance</i> . DOI. A.
Seligman & Schulman, 1986	<i>Explanatory Style as a Predictor of Productivity and Quitting Among Life Insurance Sales Agents</i> . DOI. S: initial methods/results and Tables 1–6 access; relevant text rechecked and printed pp.833–836 visually inspected during the combined critique and independent follow-up, 15 September 2026. Later company analyses remain unverified.
Ericsson & Charness, 1994	<i>Expert Performance: Its Structure and Acquisition</i> . DOI. S.
Macnamara et al., 2014/2018	<i>Deliberate Practice and Performance...</i> Article; numerical correction. A plus full correction.
Isen, Daubman & Nowicki, 1987	<i>Positive Affect Facilitates Creative Problem Solving</i> . DOI. A/P.
Baas et al., 2008	<i>A Meta-Analysis of 25 Years of Mood-Creativity Research</i> . DOI. A.
Harris et al., online 2021/issue 2023	<i>A Systematic Review and Meta-Analysis of the Relationship Between Flow States and Performance</i> . DOI. S.

Neuroscience, empathy, relationships and organizations

Reference	Primary identity and relevant access
LeDoux et al., 1984	<i>Subcortical Efferent Projections of the Medial Geniculate Nucleus Mediate Emotional Responses Conditioned to Acoustic Stimuli</i> . DOI. A/author record.
Pessoa & Adolphs, 2010	<i>Emotion Processing and the Amygdala: From a “Low Road” to “Many Roads”</i> . DOI. S; perspective, not experiment.
Rafal & Koller, 2025	Human auditory/visual subcortical pathway tractography report. DOI. A/P.
Feinstein et al., 2013	<i>Fear and Panic in Humans with Bilateral Amygdala Damage</i> . DOI. M and online methods.
Maia & McClelland, 2004	<i>A Reexamination of the Evidence for the Somatic Marker Hypothesis</i> . DOI. S.
Bechara et al., 2005	Reply concerning the somatic-marker interpretation. DOI. S.
Bushman, 2002	<i>Does Venting Anger Feed or Extinguish the Flame?</i> DOI. S.
Kjærvik & Bushman, 2024	<i>A Meta-Analytic Review of Anger Management Activities That Increase or Decrease Arousal</i> . DOI. A/P.
Levenson & Ruef, 1992	<i>Empathy: A Physiological Substrate</i> . DOI. S with selected visual table checks.

Reference	Primary identity and relevant access
Pithers, 1994	Process evaluation of an empathy-focused offender-treatment component. DOI. A.
Barnett & Mann, 2013	Empathy and sexual-offending conceptual review. DOI. A.
Sullins, 1991	Emotional-contagion dyad experiment. DOI. A.
Buehlman, Gottman & Katz, 1992	<i>How a Couple Views Their Past Predicts Their Future</i> . DOI. S.
Heyman & Smith Slep, 2001	Divorce-prediction methodological critique using a different dataset. DOI. A/P.
Uhlich et al., 2026	Longitudinal relationship-prediction study. DOI. A.
Baron, 1988; 1990	Destructive criticism; remedial interventions. Original P/A, not full methods.
Blanchard, Lilly & Vaughn, 1991	Antiracist social-norm experiments. DOI. A.
Williams & Sternberg, 1988	<i>Group Intelligence: Why Some Groups Are Better Than Others</i> . DOI. A.
Kelley & Caplan, 1993	<i>How Bell Labs Creates Star Performers</i> . PubMed. A/P.
Woolley et al., 2010	<i>Evidence for a Collective Intelligence Factor in the Performance of Human Groups</i> . DOI. A.
Riedl et al., 2021/2022	<i>Quantifying Collective Intelligence in Human Groups</i> . Article; correction. A/P plus full notice.
Rowe, Hattie & Munro, 2024	<i>High-Performing Teams: Is Collective Intelligence the Answer?</i> DOI. A.
Harms & Credé, 2010	<i>Emotional Intelligence and Transformational and Transactional Leadership</i> . DOI. A.

Medical evidence

Reference	Primary identity and relevant access
Ader & Cohen, 1975	<i>Behaviorally Conditioned Immunosuppression</i> . DOI. A.
Cohen, Tyrrell & Smith, 1991	<i>Psychological Stress and Susceptibility to the Common Cold</i> . DOI. M, selected visual check.
Segerstrom & Miller, 2004	<i>Psychological Stress and the Human Immune System</i> . DOI. A/P.
Cohen et al., 2012	<i>Chronic Stress, Glucocorticoid Receptor Resistance, Inflammation, and Disease Risk</i> . DOI. A.
Friedman & Booth-Kewley, 1987	<i>The "Disease-Prone Personality"</i> . DOI. P/A; original tables unavailable.
Ironson et al., 1992	<i>Effects of Anger on Left Ventricular Ejection Fraction in Coronary Artery Disease</i> . DOI. A/P.
Mittleman et al., 1995	<i>Triggering of Acute Myocardial Infarction Onset by Episodes of Anger</i> . DOI. A.
Chida & Steptoe, 2009	Anger/hostility and prospective coronary outcomes. DOI. A.
Friedman et al., 1986	Type-A behavior intervention and cardiac recurrences. DOI. A plus trial-profile sections.
ENRICHD Investigators, 2003	Depression/social-support treatment after myocardial infarction. DOI. A.
Gulliksson et al., 2011	SUPRIM recurrent cardiovascular-event trial. DOI. A.
Blumenthal et al., 2016	<i>Enhancing Cardiac Rehabilitation with Stress Management Training</i> . DOI. A/S.
Richards et al., 2017	<i>Psychological Interventions for Coronary Heart Disease</i> . DOI. S.

Reference	Primary identity and relevant access
Ski et al., 2024	Psychological treatment of depression/anxiety in cardiac populations. DOI. A; different review question.
House, Landis & Umberson, 1988	<i>Social Relationships and Health</i> . DOI. S.
Holt-Lunstad, Smith & Layton, 2010	<i>Social Relationships and Mortality Risk</i> . DOI. S.
Wang et al., 2023	Ninety-cohort isolation/loneliness/mortality synthesis. DOI. A.
Smith et al., 2021	Psychosocial-support interventions and survival, 106 RCTs. DOI. S.
Spiegel et al., 1989	<i>Effect of Psychosocial Treatment on Survival of Patients with Metastatic Breast Cancer</i> . DOI. A.
Goodwin et al., 2001	Group psychosocial support and metastatic-breast-cancer survival. DOI. A/P.
Kissane et al., 2007	Supportive-expressive group therapy, survival and psychosocial outcomes. DOI. A.
Spiegel et al., 2007	Randomized prospective supportive-expressive therapy replication. DOI. A/P.
Asakawa-Haas et al., 2026	Cancer psychosocial-intervention meta-analysis and multiverse analysis. DOI. S; no complete supplement audit.
Pennebaker & Beall, 1986	<i>Confronting a Traumatic Event</i> . DOI. P/A.
Pennebaker, 1993	<i>Putting Stress into Words</i> . DOI. A.
Jensen-Johansen et al., 2018	Expressive writing and physical-health outcomes in breast cancer. DOI. A.
Abu-Odah et al., 2023/2024	Expressive-writing synthesis in cancer/palliative care. DOI. A.
Han et al., 2026	Expressive-writing psychosocial/quality-of-life synthesis in breast cancer. DOI. One-page P/A only.
Strain et al., 1991	Psychiatric consultation-liaison and hip-fracture cost offsets. DOI. A.
Powell et al., 2016	Psychological preparation and postoperative outcomes. DOI. A plus authors' plain-language summary; not a full-report audit.
Kelley et al., 2014	Patient-clinician relationship and healthcare outcomes. Article; correction. S plus full notice.
Howick et al., 2018	Empathic/positive communication in healthcare. Article; erratum. A plus full notice.
Ornish et al., 1990; 1998	One-year Lifestyle Heart Trial; five-year follow-up. M/S; same trial.
Aldana et al., 2007	Intensive lifestyle program and carotid intima-media thickness. DOI. A.

Development, prevention and schooling

Reference	Primary identity and relevant access
Hooven, Gottman & Katz, 1995	<i>Parental Meta-Emotion Structure Predicts Family and Child Outcomes</i> . DOI. S.
Katz, Gottman & Hooven, 1996	Reply to Cowan and Eisenberg on meta-emotion/family functioning. Author-hosted text. S.
Havighurst et al., 2010	Tuning in to Kids randomized parenting trial. DOI. A.
Wilson et al., 2012	Community delivery of emotion-focused parenting. DOI. A.
England-Mason et al., 2023	Randomized emotion-socialization parenting-program synthesis. DOI. A.

Reference	Primary identity and relevant access
Duncan et al., 2007	<i>School Readiness and Later Achievement</i> . DOI. A and correction record.
Main & George, 1985	Abused and comparison toddlers' responses to distress. DOI. A.
Widom, 1989; Widom et al., 2015	Cycle-of-violence study; intergenerational follow-up. A.
Clauss & Blackford, 2012	Behavioral inhibition and later social-anxiety synthesis. DOI. A.
Rapee et al., 2005; 2010	Preschool anxiety prevention; three-year follow-up. A.
Baxter et al., 1992	OCD treatment-response metabolism study. DOI. A.
Hoppen et al., 2023	Randomized PTSD-treatment synthesis. DOI. A.
Lochman, 1992	Cognitive-behavioral intervention with aggressive boys, three-year follow-up. DOI. A.
Clarke et al., 1995	Adolescent affective-disorder prevention trial. DOI. A; the cited 1993 presentation remains unrecovered.
Jaycox et al., 1994; Gillham et al., 1995	School depression-prevention study; follow-up. S/A.
Brunwasser et al., 2009; Bastounis et al., 2016	PRP broader synthesis; universal randomized-program synthesis. A; different scopes.
Kjøbli & Ogden, 2014	Individual social-skills trial in children with conduct problems. DOI. A/P.
Leon et al., 1993; 1995	Cross-sectional risk study; prospective follow-up. A.
Finkelhor et al., 1995	Victimization-prevention survey; one-year follow-up. A.
Walsh et al., 2015	School-based child-sexual-abuse prevention review. DOI. A.
Dodge et al., 2015	Fast Track age-25 outcomes. Article; authorship correction. A plus full notice.
McCabe et al., online 2024/issue 2025	Fast Track age-31 outcomes and mechanisms. DOI. S plus complete S4.
Gorla, Rothenberg & Godwin, 2026	Fast Track intergenerational violence/parenting analysis. DOI. S plus specified supplement and visual checks.
Solomon et al., 1988; 2000	Child Development Project evidence. 1988; 2000. A.
Greenberg et al., 1995	<i>Promoting Emotional Competence in School-Aged Children</i> . DOI. A; original print year 1995.
Schonfeld et al., 2015	PATHS academic-proficiency trial. DOI. A.
Hennessey & Humphrey, online 2019/issue 2020	<i>Can Social and Emotional Learning Improve Children's Academic Progress?</i> DOI. A/S.
O'Donnell et al., 1995	Seattle long-term elementary intervention. DOI. A.
Hawkins et al., 2008	<i>Effects of Social Development Intervention in Childhood 15 Years Later</i> . Original report. S.
Caplan et al., 1992	Inner-city/suburban social-competence promotion. DOI. A.
Aber et al., 1998; Aber, Brown & Jones, 2003	RCCP evaluations. 1998 original; 2003 publisher abstract. S/A.
Elias et al., 1991	<i>The Promotion of Social Competence: Longitudinal Study of a Preventive School-Based Program</i> . DOI. A.
Durlak et al., 2011	<i>The Impact of Enhancing Students' Social and Emotional Learning</i> . DOI. S.
Taylor et al., 2017	<i>Promoting Positive Youth Development Through School-Based SEL: Follow-Up Effects</i> . DOI. S.
Cipriano et al., 2023; 2024	Contemporary SEL synthesis; U.S. secondary analysis. A/P.

Reference	Primary identity and relevant access
Ha et al., 2025	<i>Disentangling the Effects of SEL Programs on Student Academic Achievement Across Grades 1–12</i> . DOI. S; older-corpus secondary analysis.
Jones et al., online 2025/issue 2026	Program evidence and racial/ethnic generalizability review. DOI. A.
Riggs et al., 2006	<i>The Mediation Role of Neurocognition in the Behavioral Outcomes of a Social-Emotional Prevention Program</i> . DOI. S; original 2005 manuscript unavailable.
Achenbach & Howell, 1993; Achenbach et al., 2003	Earlier trend comparison; 23-year comparison. A.

Additional replies, conceptual sources and unrecovered historical leads are identified in their family records. An identity-only source is not used as empirical confirmation of unseen results.

12 Notices, interests and correction record

Authorship and responsibility

This review and appendix were prepared for Jason Hreha / The Behavioral Scientist with AI-assisted research, drafting, source critique and editorial review. These checks do not constitute independent human peer review.

The commissioning editor is **Jason Hreha**. The publisher is **The Behavioral Scientist / Haystack Group LLC**, as identified in the supplied house materials. That publisher identity is reported from those materials, not from a newly performed legal or corporate-status check.

First-person judgments in the reader are editorial assessments. They do not claim personal experience, searches or original statistical analyses.

Interests

The supplied materials identify Hreha’s commercially available behavior-change book *Real Change* and behavioral-science services. These create potentially competing intellectual and commercial interests.

Other financial or personal relationships, sponsorship, funding, review-copy arrangements and affiliate interests have not been comprehensively established. No blanket declaration of their absence is made. Jason supplied the reviewed book. The research records and evidence bundle were produced during the AI-assisted preparation.

Goleman and researchers whose work supports or critiques EI can have intellectual or professional involvement in the field. Specific involvement, such as Spiegel’s coauthorship of the 2026 cancer synthesis and some included trials, is disclosed where relevant; it is not treated as disproof without a methodological reason.

Meaning of the ratings and criticism

The ratings apply the supplied house rubric to the identified claims and examined relationships. They are not percentages of truth, probabilities of individual benefit, scientific consensus estimates, safety certifications or judgments of the author's character.

A source can be correctly identified yet inaccurately described. A finding can be accurately described yet used to support an excessive inference. A useful intervention can survive an inaccurate explanation. These distinctions govern the criticism.

Missing access is not evidence of fabrication. Later contrary evidence does not establish what the author knew earlier. Minor corrections and substantive retractions are not interchangeable.

This adaptation is not affiliated with or endorsed by Red Pen Reviews, and its scores are not directly interchangeable with that organization's scores.

Professional and publication limits

This appendix provides scientific discussion and editorial judgment, not individualized medical, psychological, educational, legal or financial advice. It does not prescribe treatment, diagnose a reader, or replace professional assessment.

No independent human scientific sign-off, human fact-checking completion or legal clearance is documented. No response from Goleman to this review is claimed. No new request for comment was sent for this review.

Source-specific verification limits and unresolved originals are disclosed in Section 10. The verification and correction sections distinguish completed checks from remaining source-access limits.

Formal notices and responses retained

Source	Notice or response	Recorded consequence
Macnamara et al. practice synthesis	2018 correction, full notice/table read	Corrected numerical estimates used; no retraction.
Riedl et al. collective intelligence	2022 correction, full notice read	Average variance extracted changed to 19.6%; main conclusions retained by authors.
Dodge et al. Fast Track	2015 correction, full notice read	Author list corrected; no change to findings reported.
Kelley et al. clinician relationships	2014 correction, full notice read	Affiliation/funding correction, not an altered pooled result.
Howick et al. communication	2018 erratum, full notice read	Eligible-trial count corrected to 29.
Ornish et al. 1998	Author correction, full notice read	Omitted author added; cardiac findings unchanged by the notice.
Duncan et al. readiness	Correction in the corrected-paper record	Supplemental DOI repaired, not substantive results.
Block/Lynam–Moffitt	Comment and reply, original portions read	Disputed interpretation, not an uncontested new dataset or retraction.

Source	Notice or response	Recorded consequence
Somatic-marker dispute	Maia–McClelland and Bechara et al. reply, selected sections	Both interpretations retained within their task and population limits.
Parent meta-emotion	1996 author reply, selected sections	Reverse-direction ambiguity explicitly acknowledged by original researchers.
Gottman prediction	Research FAQ, inspected	Later prospective-research response considered; not blanket validation of every fixed rule.
Cohen 1991	1992 correspondence, opening/reply identity only	Baseline-antibody objection and reply existence recorded; full resolution not claimed.
SUPRIM	2011 correspondence/reply, identity only	Favorable trial not described as criticism-free; unseen exchange not adjudicated.
Goodwin 2001	2002 correspondence/replies, identity established	No inference about the complete exchange from metadata alone.
Spiegel trials	Original letter and later comment/reply, largely identity-level	Trial results used directly; unseen arguments not treated as resolved.
Robinson–Zell, Davis–Mahoney and Asakawa-Haas	Publisher/update records checked in supplied preparation	“Current” status is not scientific clearance. Auxiliary-code concerns are not formal retractions or independently quantified invalidation.

Targeted searches for selected additional sources did not identify formal retractions in the retrieved records. This does not certify that every source lacks a notice or criticism.

Claim selection and substantive assessment

Claim selection. Complete supplied-text reading and the EI-SCOPE-20260915-v1 claim/family selection were recorded before detailed external research. No central proposition has been altered for this appendix.

Edition identity. The 2012 date is verified ebook product metadata. The reviewed content consists of the 1995 main text and 2005 introduction.

Balance and attribution. The assessment includes both favorable and adverse Fast Track age-31 findings, distinguishes source attributions and access levels, and evaluates the claims at their recorded scope.

Evidence distinctions and clinical grade. The assessment preserves the age-31 total-effect pattern, source-specific Ha and Robinson–Zell access limits, and the distinction between formal component-path testing and whole-indirect-effect comparison in the 2026 Fast Track report. The adjacent clinical grades were considered against the unchanged proposition: C3 receives 3/4, giving 64% overall. The grade rests on the evidence and scope, not a target total.

Source checks. AI domain audits and editorial source checks examined source distinctions and survival-analysis details while preserving the frozen scoring scope. Completed checks and remaining limits are documented in Section 10.

Corrections procedure

Factual corrections or substantive responses should identify the book edition, stable passage locator, disputed statement, supporting primary source and proposed change. They should be directed to the commissioning editor.

A factual repair, newly available evidence and a changed editorial judgment should be recorded separately. A missing response is not agreement. Material revisions should preserve earlier versions and explain any resulting score change under the same criterion anchors.

The present conclusion is therefore explicit but revisable: the book's practical and qualified clinical directions deserve substantial credit; its broad comparative theory and several source uses require correction or restraint. The evidence record, not the desired verdict or prior score, governs subsequent revision.