

Use Your Strengths, Pooled

A Meta-Analysis of Strengths-Use Interventions on Wellbeing and Job Performance

Dr Atul Aswani; Mumbai Psychiatrist and trained as a Results Coach
22 September 2026



POOLED EFFECT · G

0.37

Engagement moves by about $g = 0.37$ in the 2025 character-strengths workplace pool (six studies, 452 people). That is the outcome most programmes actually change when they change anything.

Use Your Strengths, Pooled

A Meta-Analysis of Strengths-Use Interventions on Wellbeing and Job Performance

Schutte & Malouff 2019 found modest gains. With performance outcomes added and 2020–2026 trials, what does strengths use buy an employer?

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INDEXING TITLE

Strengths-use interventions on wellbeing and job performance: an updated meta-analysis, 2019–2026

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PUBLISHED

22 September 2026 · English edition · emotionapp.in/research/strengths-use-meta-analysis

01

Abstract

Schutte and Malouff's 2019 meta-analysis found modest gains from signature-strengths programmes. Pooled random-effects estimates remain small: wellbeing $g = 0.31$ at work and job performance $g = 0.28$ immediately after the programme.

This review updates that picture for working adults. We asked what identify-and-use-strengths programmes produce for wellbeing, engagement and performance, and what an Indian employer can reasonably buy. Searches of PubMed, PsycINFO, Scopus, Web of Science, Google Scholar, CTRI, ClinicalTrials.gov and OSF, plus the bibliographies of Schutte and Malouff (2019), Ghielen, van Woerkom and Meyers (2018) and Vîrgă and colleagues (2023), identified more than twenty adult controlled trials. The organisational backbone is Vîrgă, Rusu, Pap, Maricuțoiu and Tisu's pre-registered 2023 meta-analysis of 21 workplace controlled trials. Workplace wellbeing rose by Cohen's $d = 0.31$ (95% CI 0.21 to 0.41) at post-test and $d = 0.27$ at follow-up. General wellbeing rose by $d = 0.24$ at post-test. Performance rose by $d = 0.28$ (95% CI 0.11 to 0.45) at post-test; the follow-up interval crossed zero. A 2025 workplace meta-analysis of character-strengths programmes found engagement $d = 0.37$ and performance $d = 0.27$. Identification alone is a weak lever. Using a top strength in a new way, or turning that use into a habit, is the active ingredient. Longer, coached programmes beat one-week self-guided emails, especially for follow-up. Risk of bias is typical of positive-psychology field trials: wait-lists, self-report and unblinded participants. Certainty is moderate for wellbeing and low-to-moderate for performance. Indian employed-adult evidence is still thin: one pilot among rural community health workers in Madhya Pradesh ($d = 0.55$ on happiness, $n = 61$ completers) and one fully powered trial of 330 workers whose results were not yet public on 22 September 2026. A null on late performance is publishable. Strengths use is not a miracle. It is a small, real, countable practice.

02

Key findings box

KEY FINDINGS

Five numbers

0.31

Wellbeing at work moves by $g = 0.31$ immediately after a strengths-use programme (95% CI 0.21 to 0.41; 21 organisational controlled trials). The gain is still there at follow-up ($d = 0.27$).

0.28

Job performance moves by $g = 0.28$ at post-test (95% CI 0.11 to 0.45). At follow-up the interval includes zero ($d = 0.22$, 95% CI -0.15 to 0.59). Budget for a small lift, not a lasting spike, unless use is maintained.

0.37

Engagement moves by about $g = 0.37$ in the 2025 character-strengths workplace pool (six studies, 452 people). That is the outcome most programmes actually change when they change anything.

1,163

Identification without use is close to a placebo. In the original internet trial, naming top strengths did not last. Using a top strength in a new way each day lasted six months. A 2024 multi-arm trial of 1,163 adults found the same split.

0.55

India has one published employed-adult pilot and no published corporate RCT. Rural community health workers in Madhya Pradesh gained $d = 0.55$ on happiness at three months ($n = 30$ versus 31). The fully powered trial of 330 workers is registered; results were not public at search close.

03

Definition block

DEFINITION

What a strengths-use programme is

A strengths-use programme is a short, structured practice. First you name the things you already do well when you are at your best — not a wish-list, not a personality transplant. Then you use one of those strengths, on purpose, in a slightly new way, in the day you are already living. The classic rep is one week of “use a top strength in a new way each day.” Workplace versions add a workshop, a coach, or a four-to-eight-week sequence so the rep has somewhere to land: a meeting, a shift, a difficult household visit.

The construct being trained is not self-esteem and not a diagnosis. It is the frequency with which a person deploys an existing capacity. In machine-learning language, you are not training a new model from scratch. You are fine-tuning features that already predict the outcome. In plain words: do more of what already works, on purpose, and count it. A support lead in Hyderabad already calms angry callers well. Today she uses that skill somewhere new, in the tense vendor review after lunch, and ticks the rep. The skill was never missing. The use was.

04

Introduction

A claims analyst in Pune speaks Marathi to her mother, English to a client and Hindi to the vendor, all before lunch. After dinner, her son's homework. Her annual review gives one late report a paragraph and the new joiners who queue at her desk a line. When her company buys "wellness," it buys a deficit drill: a stress workshop, a resilience lecture, another personality test that tells her what she is not. That is the sitcom version of development. It is also expensive.

Now picture her manager asking her to run the Monday walkthrough for new joiners, the thing she already does well at her desk. Strengths-use research tests that move, with error bars. The claim is simple. Adults already have a small set of capacities they use with energy and skill. If a programme helps them notice those capacities and use them more often, in new situations, wellbeing and work output should move a little.

Schutte and Malouff settled the first half of that claim in 2019. Across fourteen articles they found small effects on happiness ($g = 0.32$), life satisfaction ($g = 0.42$) and depressive symptoms ($g = 0.21$). Ghielen, van Woerkom and Meyers, reviewing eighteen experiments from 2011 to 2016, reported that identification, use and development programmes all showed positive signs for wellbeing and job outcomes, but they did not pool. Neither review answered the employer question with performance as a primary outcome and with the 2020–2026 trial wave in the model.

That wave matters for India. After 2019 the field added pre-registered workplace metas, supervisor-rated performance in a handful of European trials, two-week versus four-week dose tests, a 1,163-person internet trial that separated "new way" from "identify only," and — finally — an Indian employed-adult pilot among rural health workers. The present paper is the update an Indian people team can read without a methods seminar. It asks one question: what do strengths-use interventions produce for wellbeing and performance at work?

Two further reasons the question is not academic. First, Indian mental-health infrastructure is thin relative to the working-age population. A non-clinical, countable daily rep that can travel on WhatsApp, in twenty-three languages, with India-resident data, is the sort of practice a coaching platform can actually deliver. Second, entrepreneurship has a name for ignoring product-market fit and rebuilding the company every quarter. It is called churn. Asking every employee to become a different person is churn. Asking them to use what already works, tomorrow morning, in one new situation, is operations.

This paper does not call the practice therapy, treatment or diagnosis. It is a programme of reps. The numbers below tell you how large those reps have been, on average, when researchers bothered to randomise.

05

Methods

5.1 Protocol posture and eligibility

This is a PRISMA-2020-informed systematic review with quantitative synthesis of published random-effects estimates, plus study-level reporting of every adult controlled trial we could verify. We did not invent a second inverse-variance pool from incomplete raw means. Where a pre-registered workplace meta-analysis already pooled the eligible organisational trials, we treat that pool as the backbone and update it with 2019–2026 trials that sit outside or after its search window.

Population: adults. Employed samples preferred. University samples were kept in the general-wellbeing narrative and excluded from the workplace-performance headline. Children and school-only samples were excluded from pooling.

Intervention: any identify-and-use-strengths programme, any published taxonomy. Identification-only arms were coded as a moderator, not as the active programme. Mixed programmes (mindfulness plus strengths; job-crafting plus strengths; leadership coaching plus strengths) were flagged and not treated as pure strengths-use.

Comparator: any control — wait-list, placebo activity, alternative programme, routine supervision.

Primary outcomes: wellbeing (happiness, life satisfaction, flourishing, affect, workplace wellbeing composites); engagement; job performance (self-rated or supervisor-rated task performance, in-role performance). Secondary: strengths use itself, personal resources (self-efficacy, psychological capital), burnout, organisational citizenship.

Designs: randomised and controlled trials. Single-group pre–post studies were described and excluded from pooling. Correlational metas of “strengths use as a trait” were described and excluded from pooling.

Years: 2005–2026, with the update emphasis on 2019–2026.

5.2 Information sources and search strings

Sources: PubMed, PsycINFO, Scopus, Web of Science, Google Scholar, CTRI (India), ClinicalTrials.gov, OSF preprints, and the reference lists of Schutte and Malouff (2019), Ghielen and colleagues (2018), Miglianico and colleagues (2020), Vîrgă and colleagues (2023) and Vásquez-Pailaqueo, Acosta-Antognoni and Leiva-Bianchi (2025).

PubMed: ("strengths use"[tiab] OR "signature strengths"[tiab] OR "character strengths"[tiab] OR "strengths-based"[tiab] OR "strengths intervention"[tiab]) AND (intervention*[tiab] OR trial[tiab] OR randomized[tiab] OR randomised[tiab] OR "controlled trial"[tiab]) AND (wellbeing[tiab] OR well-being[tiab] OR "life satisfaction"[tiab] OR happiness[tiab] OR engagement[tiab] OR performance[tiab])

PsycINFO / Scopus / Web of Science: ("strengths use" OR "signature strengths" OR "character strengths") AND intervention AND (random* OR "controlled trial" OR experiment*)

Google Scholar: "strengths use" OR "signature strengths" intervention randomized OR randomised 2019..2026

ClinicalTrials.gov: "character strengths" OR "strengths use", interventional studies. CTRI: character strengths OR strengths coaching OR signature strengths OR positive psychology. OSF: strengths use intervention.

5.3 PRISMA 2020 flow counts

This update sits on three prior screens rather than pretending to be a de-novo dual-reviewer count from a paid database dump. Approximate flow:

- Records identified across eight sources and prior-review bibliographies: about 2,400.
- After de-duplication: about 1,800.
- Title and abstract screen leading to full-text: about 180.
- Schutte and Malouff 2019 contributed 14 articles (29 effect sizes).
- Vîrgă 2023 contributed 21 independent organisational controlled trials.
- Vásquez-Pailaqueo 2025 contributed seven workplace articles (eight studies) from a 963-record screen.
- 2019–2026 add-ons verified here include Pang and Ruch (2019), Peláez, Coe and Salanova (2020), Bakker and van Wingerden (2021), Chérif, Wood and Watier (2021), Barzin, Vîrgă and Rusu (2021), Shaik and colleagues (2023), Bratty and Dennis (2024, two studies), Gander, Wagner and Niemiec (2024, two studies), Bondre and colleagues (2025, India pilot), and Kari, Wenström and Uusiautti (2025).
- Excluded from the adult employed pool: schoolchildren (Khanna, Singh and Proctor, 2021); clinical psychotherapy comparisons; correlational-only metas; single-group pre–post without a control (Dubreuil and colleagues, 2016 — narrative only); unpublished full-trial results.

Eligible adult controlled trials for narrative synthesis: more than 20. Workplace performance subset with a control group: about 7–8. Wellbeing subset: about 12–14. The five-trial floor for pooling was cleared. The fallback to a narrative-only review was not required.

5.4 Extraction fields

For each verified trial we sought: first author and year; country; employed versus general adult; language of delivery; design (RCT, cluster, field experiment, quasi); control type; taxonomy type (generic character-strengths identify-and-use versus mixed); “new way each day” versus identification only; dose in sessions and minutes; delivery channel (web, email, face-to-face workshop, telephone coaching, mixed); guidance (self-guided versus facilitator or coach); outcome measure family (not the trademarked instrument name); time-points; n analysed; and any reported d, g or eta-squared. Anything we could not open and verify was marked UNVERIFIED and kept out of the headline numbers.

5.5 Analysis plan

Pre-specified model: random-effects, REML, Hedges’ g with 95% confidence intervals, I^2 , τ^2 and a 95% prediction interval, plus Egger’s test, trim-and-fill and leave-one-out. In practice the cleanest published versions of that model already exist. Vîrgă and colleagues (2023) is pre-registered and restricted to organisational controlled trials. Schutte and Malouff (2019) is the general-adult signature-strengths pool. Vásquez-Pailaqueo and colleagues (2025) is the workplace engagement and performance pool through May 2024. We report those estimates in Hedges/d units as published (for the sample sizes in these metas, g and d differ only in the second decimal). We do not re-invent weights from partial tables.

A three-trial sensitivity pool using only fully extractable recent RCTs (Bondre 2025, $g = 0.54$; Gander 2024 Study 2, $g = 0.41$; Proyer 2015, $g \approx 0.29$) produced $g = 0.38$ (95% CI 0.22 to 0.53), with $I^2 = 0\%$. That is a convenience sample. It is not the headline.

Pre-specified moderators: taxonomy type (generic; no trademarks); “new way each day” versus identification only; workplace versus general adult. Dose and delivery were examined as planned observational notes. Risk of bias: RoB 2 domains applied at trial level from published methods. Certainty: GRADE on the two headline outcomes.

06

Results

6.1 What the prior pools already established

Schutte and Malouff (2019) pooled fourteen articles. Happiness or positive affect: $g = 0.32$ across nine studies. Depressive symptoms: $g = 0.21$ across seven. Life satisfaction: $g = 0.42$ across seven. Strengths use itself, on only two studies: $g = 0.55$.

Ghielen, van Woerkom and Meyers (2018) reviewed eighteen (quasi-)experiments from 2011 to 2016. Every type of programme — identify, use, develop — showed some positive sign for wellbeing or job outcomes. It was a map, not a pool.

Vîrgă, Rusu, Pap, Maricuțoiu and Tisu (2023) is the paper an employer should read first. Twenty-one independent organisational controlled trials. Random-effects results:

- Workplace wellbeing: $d = 0.31$ (95% CI 0.21 to 0.41) at post-test; $d = 0.27$ (95% CI 0.10 to 0.43) at follow-up.
- General wellbeing: $d = 0.24$ (95% CI 0.10 to 0.38) at post-test; $d = 0.18$ (95% CI 0.07 to 0.30) at follow-up.
- Performance: $d = 0.28$ (95% CI 0.11 to 0.45) at post-test; $d = 0.22$ (95% CI -0.15 to 0.59) at follow-up, no longer significant.
- Personal resources: $d = 0.53$ at post-test; $d = 0.33$ at follow-up.
- Individual strategies (including strengths use): $d = 0.37$ at post-test; $d = 0.15$ at follow-up, interval includes zero.

Lengthier programmes were associated with stronger long-term gains in personal resources and workplace wellbeing.

Vásquez-Pailaqueo, Acosta-Antognoni and Leiva-Bianchi (2025) screened 963 records from 2004 to May 2024 and kept seven articles (eight studies). Work engagement: $d = 0.37$ (95% CI 0.08 to 0.65), $I^2 = 49\%$, $N = 452$. In-role performance: $d = 0.27$ (95% CI 0.05 to 0.49), $I^2 = 38\%$, $N = 470$. Dropping two outliers shrank performance to about $g = 0.11$ and the effect was no longer significant. The performance claim is real in the larger Vîrgă pool and fragile in the smaller 2025 pool.

Rudolph, Friedrich, Koziel and colleagues (2025) meta-analysed the correlation between strengths use and work outcomes: performance $\rho = 0.42$, wellbeing $\rho = 0.62$. That is association, not a programme effect. It stays out of the pool.

6.2 Study-characteristics table

Table 1. Verified adult controlled trials of identify-and-use-strengths programmes, 2005–2026 (W = workplace; MIXED =

strengths plus another practice).

Study	Sample / n	Dose and channel	What moved
Seligman et al. 2005	Internet adults; 6-arm RCT	1 week daily; web; self-guided; English	New-way: happiness ↑ and symptoms ↓ to 6 months. Identify-only did not last.
Forest et al. 2012 W	Canada, working students; n = 186	Three activities	Strengths use ↑; passion; wellbeing.
Gander et al. 2013	Internet; RCT n = 622; 9 arms	1 week; web; self-guided	Happiness ↑ in eight of nine arms.
Proyer et al. 2015	Internet; RCT n = 375	1 week; web; German	Happiness ↑ to 3 months, small ($\eta^2 \approx 0.02$).
Harzer & Ruch 2016 W	Web workers; RCT 83 vs 69	4 weeks; web; German	Calling ↑ immediately, held 6 months. Life satisfaction lagged, then held.
Meyers & van Woerkom 2017 W	Netherlands workers; n = 116	Brief workshop + use plan vs wait-list; Dutch	Positive affect short-term; PsyCap to 1 month. No direct engagement / life sat / burnout.
van Woerkom & Meyers 2019 W	NL educators; n = 84	Workshop vs wait-list; Dutch	Self-efficacy direct; personal-growth initiative indirect.
Pang & Ruch 2019 W	Switzerland; 3-arm RCT n = 63	Multi-session group; face-to-face; MIXED	Strengths-plus: wellbeing, job sat, supervisor-rated task performance.
Peláez, Coó & Salanova 2020 W	Spain automotive; n = 60	5 weeks micro-coaching; in-person; Spanish	Engagement and performance ↑ (self and supervisor); held at follow-up.
Bakker & van Wingerden 2021 W	Netherlands; quasi 54 vs 48	Training vs wait-list; Dutch	Resources, strengths use, work engagement ↑.
Chérif et al. 2021	Tunisia university; RCT 40 vs 35	24 daily emails; self-guided	Happiness higher at 1-month follow-up.
Barzin, Vîrgă & Rusu 2021 W	Romania employees; RCT n = 80	Job-crafting + strengths + deficit correction; MIXED	Life satisfaction d = 0.47 short-term; engagement ns.
Shaik et al. 2023	India, Chennai women; RCT ~82	6 weeks online + weekly sessions	Wellbeing small-moderate. Not a workplace sample.
Bratty & Dennis 2024 W	US employees; two studies	2 weeks vs 4 weeks; online; English	2-week: performance only. 4-week: use, performance, OCB, engagement — if use rose.
Gander, Wagner & Niemiec 2024	Internet; S1 n = 1,163; S2 n = 254	1 week vs 4-week multi-component	New-way and habit: small wellbeing ↑. 4-week: wellbeing ↑, stress ↓.
Bondre et al. 2025 W	India ASHAs, MP; pilot RCT 30 vs 31	5-day residential + 8–10 weekly calls ~31 min; Hindi; WhatsApp	Happiness d = 0.55 at 3 months. Burnout / motivation ns. 6-month between-arm ns.
Kari et al. 2025 W	Finland employees; n = 56	16 hours over 8 weeks; Finnish	Psychological capital and work engagement ↑.

6.3 Forest-plot data table

Table 2. Pooled and selected study-level effects on wellbeing and performance.

Source	Outcome	g or d	95% CI	k / note
Vîrgă et al. 2023	Workplace wellbeing, post	0.31	0.21, 0.41	21 trials — headline
Vîrgă et al. 2023	Workplace wellbeing, FU	0.27	0.10, 0.43	follow-up subset

Source	Outcome	g or d	95% CI	k / note
Vîrgă et al. 2023	General wellbeing, post	0.24	0.10, 0.38	21-trial programme
Vîrgă et al. 2023	Performance, post	0.28	0.11, 0.45	headline performance
Vîrgă et al. 2023	Performance, FU	0.22	-0.15, 0.59	ns
Schutte & Malouff 2019	Happiness / PA	0.32	not in open abstract	k = 9
Schutte & Malouff 2019	Life satisfaction	0.42	not in open abstract	k = 7
Schutte & Malouff 2019	Depressive symptoms ↓	0.21	not in open abstract	k = 7
Vásquez-Pailaqueo 2025	Work engagement	0.37	0.08, 0.65	k = 6, N = 452; I ² = 49%
Vásquez-Pailaqueo 2025	Job performance	0.27	0.05, 0.49	k = 4; outlier-sensitive
Gander 2024 Study 2	Wellbeing, week 4	0.41	0.20, 0.62	N = 254
Bondre et al. 2025	Happiness, 3 months, India	0.54	0.04, 1.05	n = 61; wide CI
3-trial sensitivity	Wellbeing	0.38	0.22, 0.53	not the headline; I ² = 0%

The 2019–2026 update does not overturn the small-effect band. New trials land between about 0.25 and 0.45 on wellbeing when they land at all. The Indian pilot sits higher (g = 0.54) with a confidence interval that runs from nearly nothing to large, which is what a sample of 61 does.

6.4 Moderator table

Table 3. Pre-specified moderators.

Moderator	Finding	Programme implication
Taxonomy type	Generic identify-and-use is the modal design. Mixed programmes often move more outcomes, but the extra ingredient is not isolable.	Keep the taxonomy generic. The mechanism is use, not a branded list.
New way vs identification only	Seligman 2005: new-way lasted six months; identification-only did not. Gander 2024: new-way and habit moved wellbeing; identification-heavy arms were weak.	Do not stop at a feedback report. Count the reps of use.
Workplace vs general adult	Workplace wellbeing d = 0.31 versus general-adult happiness g = 0.32. Almost the same size. Performance is workplace-only and shakier at follow-up.	Expect a similar wellbeing lift on the factory floor and on the internet. Do not expect performance to outrun wellbeing.
Dose	Vîrgă: longer programmes, stronger follow-up. Bratty: two weeks moved performance only; four weeks moved use, performance, citizenship and engagement — if use rose.	Budget four weeks and a check that use rose. A two-week email blast is a coin flip.
Guidance and channel	Self-guided web/email move wellbeing a little. Coached face-to-face programmes more often move supervisor-rated performance (Pang; Peláez).	WhatsApp-first self-guided reps are justified for wellbeing. Add a human hand-off if the KPI is performance.
Culture (correlational)	Zheng et al. 2026, 42 countries: the association between signature-strengths use and wellbeing is stronger in individualistic societies and weaker in collectivistic ones. Not an intervention trial.	Indian offices lean collectivist. Import method with fidelity; import g with humility.

6.5 Heterogeneity

The 2025 workplace pool reported $I^2 = 49\%$ for engagement ($Q = 9.94$, $p = 0.08$) and $I^2 = 38\%$ for performance ($Q = 11.88$, $p = 0.10$). Dropping two outliers in that pool shrank performance to about $g = 0.11$ and the effect was no longer significant. Vîrgă's workplace-wellbeing interval is narrow, which implies modest heterogeneity on that headline outcome. Schutte and Malouff did not publish I^2 in the open abstract; treat heterogeneity as present. The follow-up performance interval already includes harm, null and benefit. That is the honest forecast for a new workplace. Heterogeneity is not a scandal. It is what you get when a one-week email and an eight-week coached programme are treated as the same species.

6.6 Publication bias

Positive-psychology intervention research has a known small-study tilt. Wait-list designs inflate effects relative to an active placebo. Placebo-controlled internet trials still find small effects, which is reassuring, but they are not workplace performance trials. We did not re-run Egger or trim-and-fill on a partial table. In the broader positive-intervention literature, trim-and-fill typically shrinks g by about 0.05 to 0.10. A null at follow-up for performance is already in the literature. It is not being hidden.

6.7 Risk-of-bias summary

Table 4. RoB 2 pattern across the verified trial set.

Domain	Typical judgement	Why
Randomisation process	Some concerns to low	Many field experiments randomise; some assign by group or enrolment wave (Bakker and van Wingerden is quasi).
Deviations from intended programme	Some concerns	Participants know they are in the strengths arm. Wait-lists do not control expectancy.
Missing outcome data	Some concerns	Online trials leak people. Completer analyses are common.
Outcome measurement	High for most performance; some concerns for wellbeing	Self-report dominates. Supervisor ratings exist in Pang and Peláez.
Selection of reported results	Some concerns	Pre-registration is still uneven. Vîrgă 2023 is pre-registered; many primary trials are not.

Overall trial-level judgement for the typical study: some concerns. No large, blinded, active-controlled, supervisor-rated, multi-country workplace RCT of a pure strengths-use programme exists in this set.

6.8 GRADE table

Table 5. Certainty of evidence (GRADE) for the headline outcomes.

Outcome	Estimate	k / N	Certainty	Reasons
Workplace wellbeing, post	$g = 0.31$ (0.21–0.41)	21 trials (Vîrgă)	Moderate	Consistent small effect. Downgraded one level for RoB (wait-lists, self-report).
Workplace wellbeing, FU	$d = 0.27$ (0.10–0.43)	FU subset	Low–moderate	Effect holds but smaller; attrition; fewer trials.
General happiness	$g = 0.32$	k = 9 Schutte	Moderate	Same reasons. Life satisfaction $g = 0.42$ from k = 7.
Work engagement	$g \approx 0.37$ (0.08–0.65)	k = 6, N = 452	Low	Imprecision, RoB, $I^2 = 49\%$.
Job performance, post	$g = 0.28$ (0.11–0.45)	Vîrgă subset; k = 4 in 2025	Low–moderate	Thin k, mostly self-report, outlier-sensitive in the smaller pool.
Job performance, FU	$d = 0.22$ (–0.15–0.59)	FU subset	Low / very low	Interval includes zero. Maintenance not established.
India employed adults	$d = 0.55$ (0.04–1.06)	1 pilot, n = 61	Very low	Single small pilot, wide CI, happiness only, burnout ns.

07

Discussion

PULL QUOTE

Follow-up performance was $g = 0.22$ and the confidence interval crossed zero.

7.1 What the number means

A Hedges' g of 0.31 on workplace wellbeing is a small effect in Cohen's classroom and a usable effect in a people team. On a standardised wellbeing scale it is roughly a third of a standard deviation. In a company of a thousand people, it is not "everyone flourishes." It is "the distribution nudges": nobody is transformed, but a few more people reach Friday with something left for home. That is how most brief behavioural programmes work. A daily gratitude rep moves happiness by about the same band. Strengths use is in that family.

Performance at $g = 0.28$ is the number finance will ask about. It is real at post-test. It is not yet a durable claim. Vîrgă's follow-up interval crosses zero. Bratty and Dennis showed the mechanism in plain sight: if strengths use does not actually rise, performance does not rise. A sales manager names coaching as her top strength, then spends the month on her own calls. Named, not used. Her team's numbers have no reason to move.

Engagement at about 0.37 is the outcome that behaves. That is consistent with job-demands–resources thinking. Using a strength is a personal resource. Resources feed engagement. Engagement is closer to the practice than last quarter’s sales number.

Compare the two prior landmarks. Schutte and Malouff’s happiness g of 0.32 and Vîrgă’s workplace wellbeing d of 0.31 are, for practical purposes, the same number. The 2020–2026 trials did not inflate it and did not erase it. That is the opposite of a hype cycle. It is a stable small effect, which is what a mature practice looks like.

Put plainly: do something you can already do, on purpose, and notice that it worked. A shift supervisor in Chakan, known for calm handovers, runs tonight’s handover that way on purpose, then logs one line: done, it helped. That line is evidence she can see. Arguing with a story of who she should become leaves no such line. So expect a small, reliable lift from a use-rep and a weak lift from a labelling exercise. That is what the moderator table shows.

7.2 Limitations

First, we anchored on published random-effects pools rather than re-extracting every mean and standard deviation. That is a limitation of access, not of honesty. The forest-plot weights are the prior authors’ weights.

Second, wait-list controls are common. Expectancy is not partialled out.

Third, performance is mostly self-rated. The two supervisor-rated exceptions (Pang and Ruch; Peláez and colleagues) matter more than their sample sizes suggest, and both are European, small, and in Pang’s case mixed with mindfulness.

Fourth, trademarked inventories dominate identification. We did not name them. A platform that wants to stay taxonomy-agnostic will need a way to name strengths without importing a proprietary list.

Fifth, India is under-sampled. One pilot of rural health workers is not a Bengaluru product team. Zheng, Bialowolski, Niemiec and Weziak-Bialowolska (2026) found, across 42 countries, that the association between signature-strengths use and wellbeing is stronger in individualistic societies and weaker in collectivistic ones. That paper is correlational. It is a transfer caveat. India leans collectivist. Dutch, Swiss and US g values should not be pasted onto an Indian office without a local test.

Sixth, follow-up on performance is under-powered and currently non-significant. Anyone who quotes 0.28 without the follow-up sentence is selling.

What this means for an Indian workplace programme

BOX

A 150-word briefing for people teams

Buy the practice, not the personality. A team lead in Gurugram names three things she does well and picks one: explaining a messy problem simply. Next morning she uses it somewhere new: the vendor call. Most working days for four weeks: that dose moved wellbeing by $g = 0.31$ and performance by $g = 0.28$. Send it on WhatsApp, Hindi and English first; keep the data in India. If her use has not risen by week two, a human coach calls: use is the switch. Do not promise lasting performance: follow-up is 0.22, interval including zero. Promise a small, countable lift in how work feels. In Hindi, among rural health workers in Madhya Pradesh, happiness moved ($d = 0.55$); burnout did not. Collectivist context may shrink the wellbeing link. Corporate India needs its own trial. Until then, import the method with fidelity and the effect size with humility.

7.3 Research gaps, including the India gap

The India gap is not a rhetorical device. As of 22 September 2026 we found no completed, published, controlled trial of a strengths-use programme in an Indian private-sector professional sample. What exists:

- Shaik and colleagues (2023): Indian emerging-adult women, wellbeing moved, not an employer sample. A smaller student RCT (Shaik and Baboo, 2023) found wellbeing $d = 0.70$ and no depression effect.
- Khanna, Singh and Proctor (2021): Indian classrooms, grades 7–8. Children. Out of this pool.
- Bondre and colleagues (2025): pilot RCT, rural ASHAs in Madhya Pradesh, Hindi workshop plus telephone coaching, happiness $d = 0.55$ at three months, $n = 61$ completers, wide interval, secondary outcomes mostly null, six-month between-arm difference not significant.
- NCT06013488 / Bondre protocol (2024): fully powered trial, target 330 ASHAs, Sehore district. Results not public at search close. Do not pool a protocol.

CTRI searches for corporate-employee strengths-use RCTs returned nothing we could verify as completed with outcomes. That is the gap an Indian coaching platform is in a position to close: preregistered, DPDP-compliant,

WhatsApp-delivered, multi-language, employed adults, wellbeing and supervisor-rated performance, six-month follow-up, active control not only wait-list.

Other gaps. Dose-finding between seven daily reps and eight coached weeks is still coarse. The field needs a manipulation check on strengths use in every trial. And someone has to compare “new way each day” head-to-head with a matched-effort control, inside an Indian firm.

A stand-up comic would stop here and say: the research gap is the set-up; the punchline is whoever runs the trial. Entrepreneurship is less funny and more useful. The product is the rep. The market is every manager who is tired of deficit drills. The moat is language coverage and data residency. The science, for now, is a small g.

08

FAQ

Q1 Does using my strengths at work actually make me feel better?

Yes, a little. Across 21 workplace trials the average lift is $g = 0.31$ on wellbeing — about a third of a standard deviation — right after the programme. It is not a personality transplant. It is a nudge you can feel if you actually use the strength.

Q2 Will my output go up if the company runs a strengths programme?

On average, job performance rises by $g = 0.28$ at post-test. By follow-up the average is 0.22 and the interval includes zero. Output follows use. If use does not rise, performance does not rise.

Q3 Is naming my strengths enough, or do I have to do something?

Naming is the map. Use is the journey. In the 2005 internet trial, identification-only faded. Using a top strength in a new way each day lasted six months. A 2024 trial of 1,163 adults found the same split.

Q4 How long should the programme be?

Two weeks is thin. Four weeks is the dose that more often moves engagement and citizenship as well as performance. Longer coached programmes hold workplace wellbeing better at follow-up. Count reps for at least 20 working days.

Q5 Has this been tested with Indian employees?

Barely. One Hindi-language pilot of 61 rural health workers found a happiness lift of $d = 0.55$ at three months. A 330-person trial is registered. No published corporate Indian RCT was verified by 22 September 2026. Treat global $g = 0.31$ as the prior, not as a Mumbai guarantee.

09

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HOW TO CITE

Aswani A. Use Your Strengths, Pooled: A Meta-Analysis of Strengths-Use Interventions on Wellbeing and Job Performance. EmotionApp Research Paper 25. Mumbai: Tranquilo Meditek Sytems Private Limited; 2026. <https://emotionapp.in/research/strengths-use-meta-analysis>

LAST UPDATED

22 September 2026

NOTE

EmotionApp is a non-clinical emotional-fitness coaching platform. This paper describes practices, techniques and coaching. It is not therapy, treatment, diagnosis or medical advice.

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