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# Twelve-month suicidal ideation in Bangladesh after the first pandemic year: an updated systematic review and random-effects meta-analysis

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**Systematic Review**

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# Twelve-month suicidal ideation in Bangladesh after the first pandemic year: an updated systematic review and random-effects meta-analysis

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## Abstract

**Background:** Bangladesh lacks continuous, high-quality, nationally representative suicide-mortality data, and post-pandemic surveys of suicidal ideation vary substantially in population and measurement. This review synthesized past-12-month suicidal ideation in non-clinical Bangladeshi samples surveyed from 2021 onward.

Methods: PubMed, PubMed Central (PMC), and a multidisciplinary academic discovery index were searched to 1 September 2026; supplementary sources and reference lists were checked. Peer-reviewed studies reporting an extractable past-12-month ideation numerator and denominator were eligible. Risk of bias was assessed with the nine-item Joanna Briggs Institute prevalence checklist. Logit proportions were pooled using DerSimonian–Laird random effects. Prediction intervals, subgroups, restriction to lower-risk studies, leave-one-out analysis, small-study effects, and restricted maximum-likelihood Hartung–Knapp–Sidik–Jonkman sensitivity analysis were examined.

Results: Twelve cross-sectional studies included 9985 participants and 1474 events. Pooled prevalence was 13.2% (95% confidence interval [CI] 9.5%–18.1%; 95% prediction interval 4.0%–36.1%; $I^2$ = 97.5%). Education-linked and mixed/non-student estimates were 15.4% (95% CI 10.7%–21.8%) and 9.4% (95% CI 4.3%–19.6%), respectively. Four lower-risk studies pooled to 7.0% (95% CI 2.6%–17.8%). The restricted maximum-likelihood Hartung–Knapp estimate was 13.2% (95% CI 8.3%–20.2%; prediction interval 2.4%–48.7%).

**Conclusions:** Past-year suicidal ideation was common in sampled populations, but extreme heterogeneity prevents national or temporal interpretation. Repeated probability surveillance using invariant measures is needed.

**Keywords:** Bangladesh; COVID-19; suicidal ideation; prevalence; systematic review; meta-analysis; PRISMA

# Background

Suicide is a major public-health outcome, but its measurement in Bangladesh remains fragmented. A rural denominator study observed 38 suicide deaths during 1155546 person-years in seven subdistricts in 2013 (3.29 per 100000 person-years) [1]. A later psychological-autopsy study of 100 deaths and 100 controls in Dhaka found psychiatric disorder in 61% of suicide deaths versus 13% of controls and a recent life event in 91% versus 24% [2].

Coronavirus disease 2019 (COVID-19) brought economic disruption, bereavement, interrupted education, isolation and fear. Mamun's early review included four online surveys conducted from April to July 2020, with suicidal-ideation or related estimates from 5.0% to 19.0%, plus five media-derived case studies or series; it did not pool prevalence or perform a formal risk-of-bias appraisal [3]. Shimu and colleagues later synthesized six studies of Bangladeshi medical students and reported 25% lifetime suicidal ideation, but did not estimate post-2020 annual prevalence across broader populations [4]. Arafat and colleagues conducted the first Bangladesh-wide prevalence meta-analysis, combining 34 studies published during 2015–2024; 20 studies yielded 15.0% 12-month suicidal thoughts with $I^2$ = 98.1% [5]. A 2025 South Asia-wide review also pooled multiple countries and recall periods but did not provide the present Bangladesh-specific, post-2020 estimand [6].

This study is **not** the first meta-analysis of suicidal behavior in Bangladesh, and quality restriction and student-only sensitivity analysis already appear in Arafat et al. Its narrower and updated contribution is different: to the best of the documented search, it is the first Bangladesh synthesis to define eligibility jointly by data-collection date after 2020, non-clinical sampling, an explicit past-12-month ideation outcome, and separate numerator/denominator availability, while reporting a study-level nine-item JBI matrix, a prediction interval, leave-one-out influence analysis and a robust REML–Hartung–Knapp sensitivity analysis. It extends the search to 1 September 2026 and adds four eligible studies to the original working set, including two studies with

2024 fieldwork published in 2026 [7,8]. This is therefore best framed as a methodologically focused update and re-analysis, not a de novo national prevalence review.

**Table 1. Comparison with prior reviews**

| Prior review | What it already established | What remains distinct here |
|---|---|---|
| Mamun 2021 [3] | Nine early-pandemic reports; four surveys and five case/case-series reports; narrative range rather than pooled prevalence | Post-2020 fieldwork, exact annual ideation, formal JBI appraisal and meta-analysis |
| Shimu et al. 2025 [4] | Six medical-student studies; JBI appraisal; pooled lifetime ideation, plan and attempt | Multiple non-clinical populations, annual rather than lifetime estimand, prediction and influence analyses |
| Arafat et al. 2025 [5] | First national meta-analysis; 34 studies; lifetime, annual and point outcomes; quality and student sensitivities; moderators | Eligibility by **fieldwork** after 2020, exclusion of clinical samples, item-level JBI matrix, evidence through September 2026, prediction intervals and leave-one-out analysis |
| Imran et al. 2025 [6] | South Asia-wide prevalence synthesis across outcomes and recall periods | Bangladesh-specific post-2020 annual ideation estimand |

As summarized in Table 1, the objectives were to identify peer-reviewed non-clinical studies with data collected from 2021 onward; estimate past-12-month suicidal ideation; quantify between-study dispersion; test population, data-year, quality and influence patterns; and delineate what the evidence cannot show about suicide mortality or population trends.

# Methods

## Protocol, reporting and review question

The review follows the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement [9]. No prospective protocol was registered; analyses added during manuscript upgrading are explicitly identified. The review question was: among people in non-clinical Bangladeshi samples surveyed from 2021 onward, what is the pooled prevalence of suicidal ideation referring to the preceding 12 months?

## Information sources and search strategy

The original search used PubMed, PMC, and a multidisciplinary academic discovery index. On 1 September 2026, an update searched the same sources using multiple population and outcome variants; checked Google Scholar-indexed results, World Health Organization (WHO) Bangladesh and global suicide pages, and

medRxiv/preprint records; and backward-searched Bangladesh-specific and South Asia reviews [3–6]. Preprints were used to locate later peer-reviewed articles but were not pooled.

Core concepts were Bangladesh AND (suicide, suicidal ideation, suicidal thought, suicide attempt, suicidal behavior, self-harm) AND, in targeted searches, (past year, 12 month, COVID-19, student, adolescent, disability, community, nurse, physician). Representative strings were:

("Bangladesh" AND ("suicide" OR "suicidal ideation" OR "suicidal thought" OR "suicidal behavior"))

("Bangladesh" AND ("past year" OR "12 month") AND ("suicidal ideation" OR "suicidality"))

("Bangladesh" AND ("student" OR "adolescent" OR "disability" OR "community") AND "suicidal ideation")

The update produced 132 unique URL-level records and 120 article-level records after DOI/title deduplication. Twelve reports were assessed in full and four newly eligible studies were added. No additional Bangladesh-specific 2025–2026 systematic review with the same post-2020, past-year, non-clinical estimand was identified. Search exports and extracted full texts are archived with the analysis.

## Eligibility criteria

Eligible studies were peer-reviewed primary quantitative studies; sampled people living in Bangladesh outside samples selected because of clinical diagnosis or treatment; collected data from 1 January 2021 onward; used a cross-sectional, cohort or surveillance design; measured suicidal thoughts/ideation over approximately the preceding 12 months; and reported an exact numerator and denominator or a percentage permitting unique reconstruction.

Studies were excluded if they reported only lifetime or short-period ideation, a composite suicide-risk score without an extractable annual ideation item, combined ideation/plan/attempt outcomes, clinical patient samples, case reports, qualitative findings, media-derived deaths without a population denominator, overlapping data, pre-2021 fieldwork, or non-peer-reviewed results. Publication year was never substituted for fieldwork year.

## Study selection and extraction

Digital object identifier (DOI), title and uniform resource locator (URL) were used for deduplication. Titles/abstracts and then full texts were screened. Extracted variables were author, publication year, journal, DOI/URL, fieldwork dates, region, population, sampling, design, measure, recall period, events, denominator

and reported prevalence. Two numerators in the original set were uniquely reconstructed from exact reported percentages; one newly added numerator (71/508) was read directly from the article's table rather than reconstructed.

## Outcome definition and data harmonization

The target outcome was any self-reported suicidal thought or ideation referring to approximately the preceding 12 months. This restriction was adopted before pooling to reduce the incompatibility created by mixing lifetime, past-month, past-week and current outcomes. Where a study reported several suicidality constructs, only the separable ideation item was eligible; plans, attempts, composite risk classifications and mortality outcomes were not combined with ideation. Measures were retained in their reported form rather than treated as diagnostically equivalent. Direct serious-consideration questions, Suicidal Behaviors Questionnaire–Revised (SBQ-R) item 2 and the Montgomery–Åsberg Depression Rating Scale (MADRS) suicidality item all contributed to the same broad construct, but their wording and response thresholds were recognized as potential sources of heterogeneity.

For each report, the event count and analysis denominator were checked against the stated percentage. Counts reported directly in tables or text were preferred. Reconstruction from a rounded percentage was used only when one integer numerator was uniquely compatible with the reported denominator and percentage; reconstructed values were flagged in Table 2. Population labels were assigned from the sampling frame rather than inferred from age alone. Data year referred to fieldwork, not publication, because delayed publication could otherwise misclassify the period represented by a study.

## Risk of bias

The nine-item Joanna Briggs Institute (JBI) checklist for prevalence studies assessed sample frame, recruitment, sample size, setting, coverage, outcome validity, consistency, analysis and response management [10]. "Yes" scored one and "no/unclear" zero; 7–9, 4–6 and 0–3 were labelled lower, moderate and higher risk. Scores were not exclusion criteria. The cluster-sampled 2024 rural-adolescent study was downgraded for analysis that did not explicitly model clustering; convenience online studies were downgraded for frame, selection and response concerns.

### Statistical analysis

The primary model used logit-transformed proportions, within-study variance 1/x+1/(n-x), DerSimonian–Laird (DL) between-study variance and inverse-variance random effects. Individual intervals were Wilson intervals. Heterogeneity was summarized with Q, $I^2$, $\tau^2$ and a 95% prediction interval.

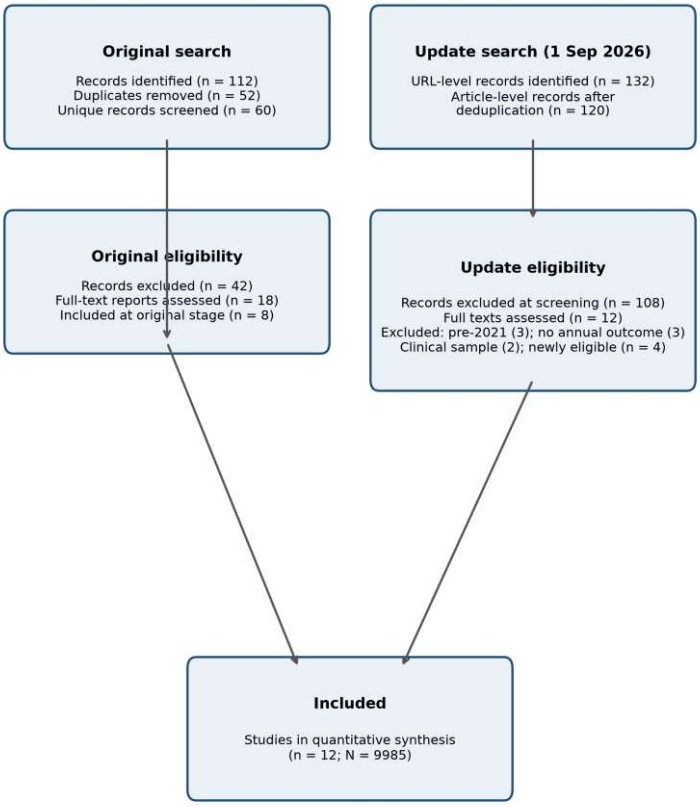
Prespecified subgroups compared education-linked with mixed/non-student samples and data-collection years. Sensitivity analyses retained JBI scores of at least seven and omitted studies sequentially. A post hoc robustness analysis used restricted maximum likelihood and Hartung–Knapp–Sidik–Jonkman (REML–HKSJ) inference for $\tau^2$. Funnel asymmetry was examined visually and by Egger regression; its result was interpreted as small-study-effect evidence rather than proof of publication bias. Analyses used Python 3.14, NumPy, SciPy, pandas and Matplotlib. Perplexity Computer (an artificial intelligence (AI)-assisted tool; version not displayed in the interface; Perplexity AI, Inc.) was used for literature searching, cross-checking extracted aggregate data, supporting statistical-analysis code and drafting. All search decisions, eligibility judgments, extracted values, code, outputs, citations and manuscript text were personally reviewed and verified by the author, who accepts full responsibility for the work. The analytic dataset, machine-readable results and analysis code are provided as Additional files 1–3.

## Results

### Study selection

The original search identified 112 records, removed 52 duplicates, screened 60 records and assessed 18 full texts, yielding eight studies. The 1 September 2026 update identified 132 URL-level records, consolidated these to 120 article-level records, and assessed 12 reports in full. Four additional studies were eligible: two 2022 studies missed by the original workflow and two 2026 publications reporting 2024 fieldwork. Update-stage exclusions were pre-2021 fieldwork (three), no extractable 12-month ideation (three) and clinical sampling (two). Twelve studies entered the quantitative synthesis (Figure 1).

**Figure 1. PRISMA 2020 flow diagram for the original search and 1 September 2026 update.**




## Study characteristics

All studies were cross-sectional; fieldwork occurred in 2021, 2022 or 2024. Eight samples were education-linked, two concerned people with disabilities, one sampled rural community adults and one mainly sampled young adults. Measures ranged from direct questions to SBQ-R item 2 and a MADRS suicidality item (Table 2).

**Table 2. Characteristics of included studies**

| Study | Journal; fieldwork year | Population and sampling | Past-year measure | Events/N | JBI |
|---|---|---|---|---|---|
| Mamun et al. [11] | Risk Management and Healthcare Policy; 2021 | Online young adults; snowball | MADRS suicidality item | 62*/756 | 4 |
| Ali et al. [12] | Health Science Reports; 2021 | Rehabilitation students; email recruitment | Direct serious-consideration question | 119/731 | 6 |
| Roy et al. [13] | BJPsych Open; 2021 | People with disabilities; snowball | COVID-related direct question | 98/410 | 5 |
| Mamun et al. [14] | PLOS ONE; 2022 | Rural adults; cluster/household sampling | Direct question | 19/490 | 8 |

| Study | Journal; fieldwork year | Population and sampling | Past-year measure | Events/N | JBI |
|---|---|---|---|---|---|
| Nahrin et al. [15] | BJPsych Open; 2022 | University entrance candidates; convenience | Direct question | 219/1523 | 5 |
| Roy et al. [16] | Public Health Nutrition; 2022 | Six public universities; convenience | Past-12-month item | 218/1480 | 6 |
| Koly et al. [17] | BMC Psychology; 2022 | People with disabilities; PPS sampling | SBQ-R item 2 | 32*/355 | 8 |
| Al-Mamun et al. [18] | Global Mental Health; 2022 | School adolescents; two-stage cluster | Direct question | 48/1496 | 8 |
| Arafat et al. [19] | Frontiers in Psychiatry; 2022 | University/medical students; convenience | Direct question | 339/1207 | 5 |
| Arafat et al. [20] | Mental Illness; 2022 | University/medical students; convenience | Direct item accompanying validation survey | 151/529 | 4 |
| Tamanna et al. [7] | SAGE Open Medicine; 2024 | Rural school adolescents; two-stage cluster | SBQ-R item 2 | 98/500 | 8 |
| Huraira et al. [8] | PLOS ONE; 2024 | Students/recent graduates; convenience/snowball | Direct serious-consideration question | 71/508 | 4 |

*Reconstructed: 8.2% of 756 is uniquely compatible with 62 events; 9.01% of 355 with 32 events. PPS = probability proportional to size.

## Risk of bias

Table 3 shows that scores ranged from 4 to 8: four studies were lower risk and eight moderate risk. Recurring limitations were non-probability recruitment, non-national frames, single-item ascertainment and poorly characterized non-response.

**Table 3. Item-level JBI prevalence risk-of-bias appraisal**

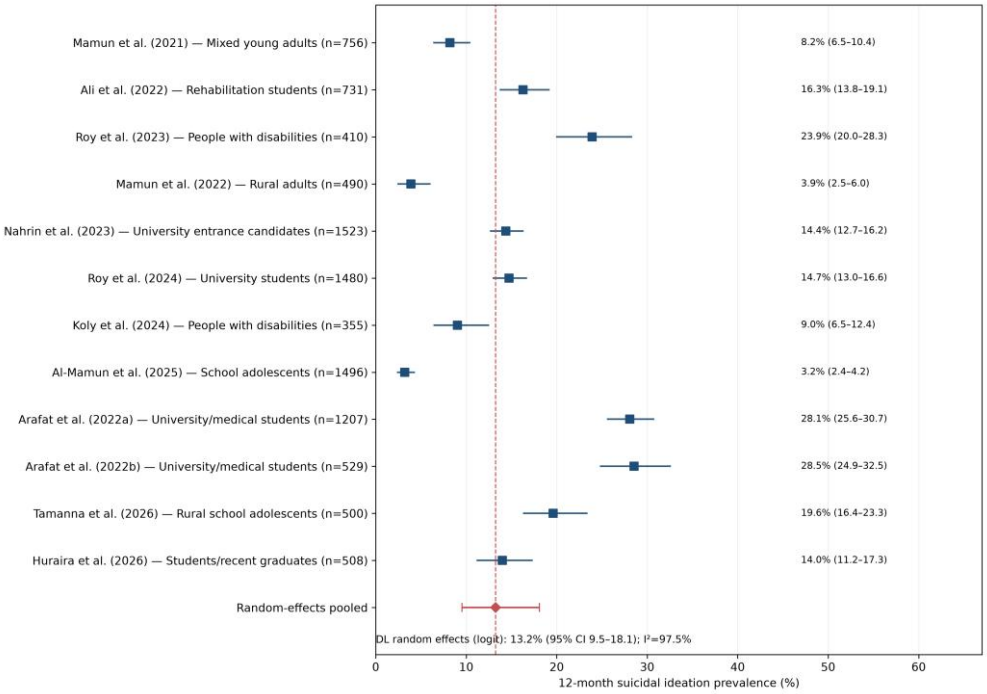
| Study | Frame | Sampling | Size | Setting | Coverage | Valid outcome | Consistent | Analysis | Response | Total |
|---|---|---|---|---|---|---|---|---|---|---|
| Mamun 2021 | N | N | Y | Y | N | U | Y | Y | N | 4 |
| Ali 2022 | U | U | Y | Y | Y | Y | Y | Y | N | 6 |
| Roy 2023 | N | N | Y | Y | U | U | Y | Y | Y | 5 |
| Mamun 2022 | U | Y | Y | Y | Y | Y | Y | Y | Y | 8 |
| Nahrin 2023 | N | N | Y | Y | Y | U | Y | Y | Y | 5 |
| Roy 2024 | N | N | Y | Y | Y | Y | Y | Y | U | 6 |
| Koly 2024 | Y | Y | Y | Y | Y | Y | Y | Y | U | 8 |
| Al-Mamun 2025 | U | Y | Y | Y | Y | Y | Y | Y | Y | 8 |
| Arafat 2022a | N | N | Y | Y | Y | U | Y | Y | U | 5 |
| Arafat 2022b | N | N | Y | Y | N | U | Y | Y | N | 4 |
| Tamanna 2026 | Y | Y | Y | Y | Y | Y | Y | N | Y | 8 |
| Huraira 2026 | N | N | Y | Y | N | U | Y | Y | N | 4 |

Y = yes; N = no; U = unclear.

## Primary synthesis

Across 12 studies, 1474 of 9985 participants reported past-year suicidal ideation. Random-effects prevalence was 13.2% (95% CI 9.5%–18.1%). The 95% prediction interval was 4.0%–36.1%. As shown in Figure 2, heterogeneity was extreme: Q = 438.88, 11 degrees of freedom (df), $p < 0.001$; $I^2 = 97.5\%$; $\tau^2 = 0.410$.

**Figure 2. Random-effects forest plot of past-12-month suicidal ideation.**




## Subgroups and sensitivity analyses

Education-linked samples pooled to 15.4% (95% CI 10.7%–21.8%; $I^2 = 97.8\%$); mixed/non-student samples pooled to 9.4% (95% CI 4.3%–19.6%; $I^2 = 96.7\%$). The difference was not statistically significant (Qb = 1.86, p = 0.172).

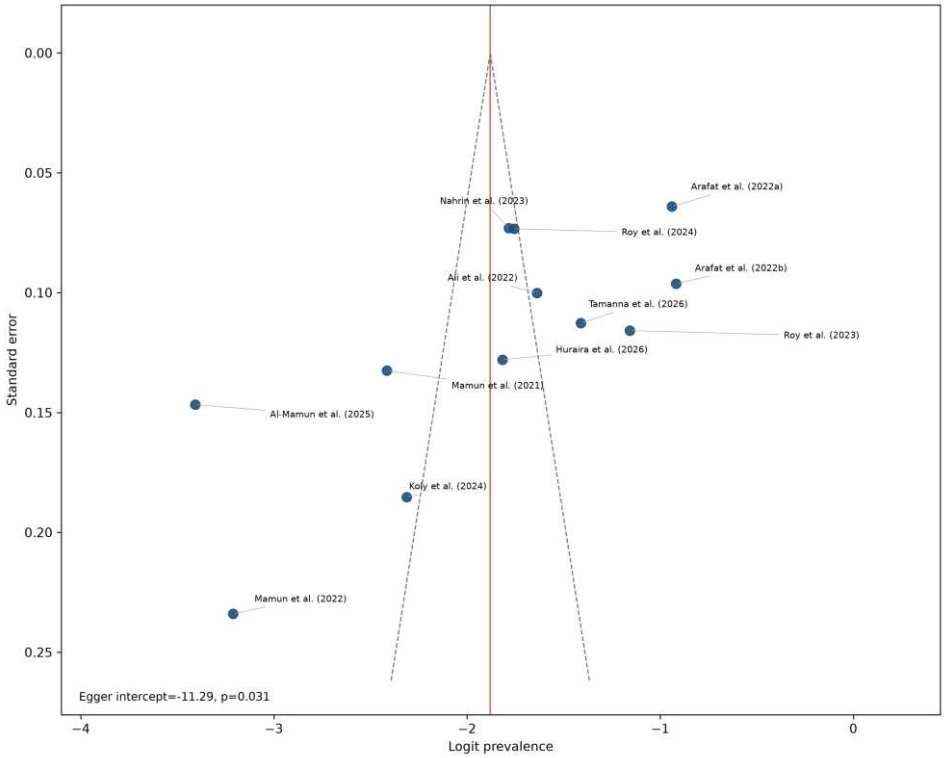
Estimates were 15.0% for 2021 data (95% CI 8.4%–25.5%), 11.6% for 2022 (95% CI 6.9%–18.9%) and 16.7% for 2024 (95% CI 11.8%–22.9%); the between-year test was not significant (Qb = 0.80, p = 0.669). Population and instrument changes preclude interpreting these cross-sectional strata as a time trend.

The four lower-risk studies pooled to 7.0% (95% CI 2.6%–17.8%; $I^2 = 97.8\%$). Leave-one-out estimates ranged from 12.3% to 15.0%, with $I^2$ from 96.4% to 97.7%. The REML–Hartung–Knapp analysis gave 13.2% (95% CI 8.3%–20.2%; prediction interval 2.4%–48.7%), reinforcing that uncertainty is much larger than the primary confidence interval alone suggests.

## Small-study effects

Egger's intercept was −11.29 (standard error (SE) 4.49; p = 0.031). The funnel plot (Figure 3) was asymmetric, but the pattern can reflect selection, population mix, measurement differences or publication processes; it is not specific evidence of publication bias.

**Figure 3. Funnel plot for past-12-month suicidal ideation.**




## Outcomes not pooled

Past-year attempts were reported in some included studies, but sparse and heterogeneous ascertainment precluded a comparably coherent synthesis. Suicide mortality was not pooled because the available evidence combines person-time mortality studies, psychological autopsy and media case series with incompatible denominators. A 2026 analysis of nationally representative injury surveys compared deaths in 2002 and 2015, not a post-pandemic continuous series [21]. A media analysis identified 984 news-reported student deaths in 2022–2023 but lacked an enrolled-student denominator [22].

# Discussion

## Principal findings

Approximately one in eight participants in selected non-clinical Bangladeshi samples with 2021–2024 fieldwork reported past-year suicidal ideation. The more important result is dispersion: the prediction interval spanned 4.0%–36.1%, and the lower-risk estimate was approximately half the main estimate. Neither 13.2% nor any year subgroup is a national rate.

The estimate is lower than the 15.0% annual prevalence in Arafat et al., whose synthesis included pre-pandemic and clinical samples and searched to November 2024 [5]. The overlap is substantial: both reviews use random-effects pooling, quality restriction, student analyses and funnel methods. The present review's added value is consequently its post-2020 fieldwork rule, non-clinical estimand, updated 2026 evidence, JBI item-level transparency, prediction intervals and influence/robust-inference analyses—not the existence of a Bangladesh prevalence meta-analysis itself.

The two 2022 validation-oriented surveys had the highest annual estimates, 28.1% and 28.5% [19,20]. Their convenience samples and direct items warrant caution. Conversely, three of four lower-risk studies reported 3.2%–9.0%, while the probability-selected rural adolescent study reported 19.6% in 2024 [7]. Methodological quality alone therefore does not explain heterogeneity; population, geography, age and instrument also matter.

## Interpretation and implications

The non-significant student/non-student contrast is not evidence of equivalence. "Education-linked" includes school pupils, rehabilitation trainees, university candidates, current students and recent graduates; the comparator combines young adults, rural adults and people with disabilities. Likewise, year strata are not repeated observations of a stable sampling frame.

Bangladesh needs repeated probability surveys using an invariant validated instrument and identical recall periods. A minimum surveillance core should report ideation, plan and attempt separately; lifetime, 12-month and recent outcomes separately; raw and survey-weighted denominators; response; age, sex, disability, education and geography; and referral procedures for participants endorsing risk. Mortality surveillance

requires stable civil-registration, police, forensic or sentinel ascertainment and age-standardized denominators.

## Clinical and service implications

The pooled estimate describes the frequency of reported ideation in the included samples; it is not a diagnostic threshold and does not estimate an individual's probability of attempting or dying by suicide. For practising psychiatrists, the central implication is therefore interpretive rather than a directive to apply 13.2% as a national benchmark. The prediction interval shows that a study using a different population, recruitment route or instrument could reasonably obtain a substantially different prevalence. Local clinical services should interpret community and student survey figures alongside case mix, wording, recall period, response rates and referral pathways rather than assuming that differences represent true changes in underlying risk.

The results nevertheless support preparedness for meaningful demand. Ideation can identify distress and a need for further assessment even though it does not map directly onto attempts or mortality. Psychiatric services, university counselling programmes, disability services and primary-care referral networks should avoid collapsing thoughts, plans and attempts into a single "suicidality" category. Separate recording permits more appropriate triage and clearer communication about urgency. Survey programmes should also specify what happened when respondents endorsed acute risk, because ascertainment without a safety and referral process has limited clinical value.

The lower pooled estimate in methodologically stronger studies should not be interpreted as proof that high estimates are false. One lower-risk probability-selected adolescent study still reported a relatively high prevalence, indicating that population and instrument can matter even when sampling is stronger. Conversely, selected online samples may reach groups with substantial distress while remaining unsuitable for population prevalence estimation. Clinical planning therefore needs two complementary evidence streams: representative surveillance to estimate population burden and service-based data to characterize severity, comorbidity, help-seeking and care needs among people who present.

## Surveillance and research priorities

A credible national surveillance design would use repeated probability samples drawn from a stable frame, with deliberate coverage of adolescents, adults outside education, rural and urban populations, people with disabilities and groups often missed by online recruitment. The same validated core questions, response

options and 12-month reference period should be retained across waves. Reports should provide unweighted counts, survey-weighted estimates, sampling strata and clusters, non-response information and prespecified subgroup analyses. Stable methods would make changes over time more interpretable than comparisons among unrelated cross-sectional samples. Survey reports should publish exact question wording and denominator definitions, document language adaptation, distinguish missing answers from negative responses, and report whether estimates account for the sampling design. Linkage to service-capacity indicators could help planners judge whether changes in identified need are matched by accessible assessment and follow-up, while preserving confidentiality and avoiding the identification of respondents or small communities.

Future reviews should be prospectively registered before screening or extraction, search major subscription databases where access is available, preserve complete deduplication and exclusion logs, and use at least two independent reviewers for screening, extraction and risk-of-bias judgments. Protocols should specify the outcome hierarchy, treatment of rounded percentages, handling of overlapping samples, subgroup definitions and preferred estimator before results are known. With enough comparable studies, meta-regression could examine measurement and sampling characteristics, but the present evidence base is too small and heterogeneous for extensive moderator modelling without a high risk of unstable findings. Individual-participant-data collaboration could eventually harmonize age, sex or gender, disability, education and geographic variables, provided consent, governance and disclosure-risk requirements are met.

Patient and public involvement was not undertaken in this evidence synthesis. Future surveillance and review updates would benefit from involvement of people with lived experience, clinicians and community stakeholders when selecting outcome wording, safety procedures and dissemination formats. Such involvement may improve acceptability and help ensure that prevalence reporting is linked to feasible pathways for assessment and support rather than presented as an isolated statistic.

## Strengths and limitations

Strengths include a sharply defined estimand, explicit fieldwork-year eligibility, updated searching through September 2026, separation of clinical and non-clinical populations, full-text outcome verification, transparent reconstructed counts, an item-level JBI matrix, prediction intervals, subgroup and quality analyses, leave-one-out analysis, and reproducible code. The search found four eligible studies absent from the original working set, demonstrating the value of broader terms and citation checking.

Limitations remain material. The review was not prospectively registered. Subscription-only Embase, PsycINFO, Scopus and Web of Science interfaces were not directly searched in the update, and screening/appraisal were not duplicated by two independent reviewers. The original record-level search log was unavailable; the update flow is therefore reported as a separate search phase rather than merged into an artificial single count. All studies were cross-sectional, most used selected samples, and measures were not identical. Two counts were reconstructed from rounded percentages. JBI scoring contains judgment and does not convert selected samples into representative ones. DerSimonian–Laird intervals can be overconfident with few heterogeneous studies, although REML–Hartung–Knapp sensitivity produced a compatible center with wider uncertainty. Subgroups were ecological and underpowered. Funnel asymmetry is difficult to interpret amid extreme heterogeneity. Finally, ideation does not identify trends in suicide mortality.

## Conclusions

Twelve studies with 9985 participants yielded 13.2% pooled past-year suicidal ideation after the first pandemic year, but extreme heterogeneity, a 4.0%–36.1% prediction interval and a 7.0% lower-risk estimate preclude a national-prevalence interpretation. The defensible novelty is a focused, updated and uncertainty-forward re-analysis—not the first Bangladesh suicide meta-analysis. Harmonized probability surveillance and reliable mortality registration are the next priorities.

## List of abbreviations

AI: artificial intelligence

CI: confidence interval

COVID-19: coronavirus disease 2019

df: degrees of freedom

DL: DerSimonian–Laird

DOI: digital object identifier

JBI: Joanna Briggs Institute

MADRS: Montgomery–Åsberg Depression Rating Scale

PMC: PubMed Central

PPS: probability proportional to size

PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses

REML–HKSJ: restricted maximum likelihood–Hartung–Knapp–Sidik–Jonkman

SBQ-R: Suicidal Behaviors Questionnaire–Revised

SE: standard error

URL: uniform resource locator

WHO: World Health Organization

# Declarations

## Ethics approval and consent to participate

Not applicable. No human participants or primary data were recruited or collected by the author; only published aggregate data were analyzed.

## Consent for publication

Not applicable. The manuscript contains no individual person's data, images, or case details.

## Availability of data and materials

The analytic CSV, machine-readable JSON results, statistical-analysis code and figures are provided as supplementary material. They are also available from the corresponding author, M.AM.R., upon reasonable request.

## Competing interests

M.AM.R. declares that he has no competing interests.

## Funding

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## Authors' contributions

M.AM.R. conceived the study, performed the systematic search, screening, data extraction, risk-of-bias appraisal and statistical analysis, and drafted and revised the manuscript. M.AM.R. created the visualizations, read and approved the final manuscript, and is accountable for all aspects of the work.

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