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# Evaluating a Digital Literacy Program for Misinformation Discernment and Digital Civil-Rights Awareness Among Vocational-School Adolescents

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**Abstract**

Digital literacy programs are increasingly used to strengthen misinformation discernment and responsible participation in digital environments, yet their evaluation may be distorted when participants begin with near-perfect knowledge scores. This study evaluated the immediate outcomes of a digital literacy program delivered to vocational-school-aged adolescents in Nambo and Lulut, Bogor Regency, Indonesia. The program combined conceptual instruction, case-based identification of misinformation, guided use of verification tools, discussion of digital rights and responsibilities, and an invitation to participate as digital-literacy agents. A one-group pretest-posttest evaluation was conducted on 18 June 2026. Of 20 pretest and 20 posttest responses, 19 valid identity-matched pairs were analyzed. Participants were 15-18 years old (M = 16.42, SD = 0.69). The ten-item assessment measured knowledge of digital literacy, misinformation, source verification, digital civil rights, hate-speech responses, personal-data protection, and digital ethics. Mean scores increased from 98.42 (SD = 6.88) to 99.47 (SD = 2.29). The Wilcoxon signed-rank test did not indicate a statistically significant aggregate change ($p = .317$, $r = .229$), largely because 94.7% of participants had already achieved the maximum score at pretest. Eighteen scores remained at 100, whereas one participant improved from 70 to 90. Item-level gains occurred in appropriate responses to hate speech and understanding the purpose of digital literacy. The program is therefore best interpreted as maintaining high competence while producing targeted improvement, rather than generating a broad score increase. Future evaluations should use more demanding performance tasks, delayed posttests, and behavior-based indicators to improve measurement sensitivity and establish retention and transfer.

**Keywords:** digital literacy; educational program evaluation; misinformation; digital citizenship; ceiling effect

## 1. Introduction

Digital communication has expanded young people's opportunities to learn, build relationships, and participate in civic life. The same environment, however, exposes users to misinformation, disinformation, manipulated images, hate speech, privacy violations, and impulsive content sharing. These risks are particularly consequential for adolescents, whose digital practices increasingly shape learning, peer interaction, identity formation, and participation in public discussion. Digital literacy is therefore not adequately defined as operational skill with devices. It also requires the ability to evaluate information, protect personal data, communicate ethically, and understand rights and responsibilities in networked spaces (UNESCO, 2024; Vuorikari et al., 2022).

Educational interventions are a common response to misinformation. Evidence indicates that media-literacy instruction can improve credibility assessment, although effects vary according to intervention design, baseline competence, participant characteristics, and the outcome measure used (Ali & Qazi, 2023; Guess et al., 2020; Lu et al., 2024). Brief programs may produce immediate gains, but the persistence of those gains depends on the instructional approach and the timing of follow-up measurement (Berger et al., 2025). These findings make evaluation design central to determining whether a program genuinely changes knowledge and behavior or merely produces a short-term testing effect.

The present program was implemented in the Nambo community area of Klapanunggal District, Bogor Regency, Indonesia, through a vocational-education setting. Its substantive goals were to strengthen participants' capacity to recognize misinformation, verify sources, use simple fact-checking tools, respond ethically to harmful content, protect personal information, and understand digital civil

rights. The intervention also encouraged participants to become local digital-literacy agents. These goals connect digital literacy with digital citizenship, because responsible digital participation involves not only information-processing competence but also civic judgment, legal awareness, community care, and ethical action (Ribble, 2015; Vajen et al., 2023; Webster, 2025).

A methodological problem emerged during evaluation: almost all participants obtained the maximum score before the intervention. When baseline scores are concentrated at the upper limit, conventional mean comparisons have little capacity to detect change. An intervention may consolidate knowledge, correct a small number of misconceptions, or improve practical judgment without producing a substantial numerical increase. A ceiling effect therefore creates both a statistical limitation and an interpretive risk. Evaluators who focus only on mean gain may incorrectly conclude that the program had no value, whereas evaluators who report only a higher posttest mean may overstate effectiveness.

This study addresses that problem by treating the program as an educational evaluation rather than as a descriptive community-service report. It examines the quality of the paired data, the magnitude and distribution of pretest-posttest change, item-level patterns, statistical uncertainty, and the implications of a ceiling effect for assessment design. The study asks three questions: (1) What immediate changes occurred in participants' digital-literacy knowledge after the program? (2) How should non-significant aggregate results be interpreted when baseline scores are extremely high? and (3) What improvements are required for more valid evaluation of digital-literacy programs? The study contributes to educational assessment and program evaluation by demonstrating an evidence-based interpretation of maintenance plus targeted improvement and by identifying measurement strategies for high-baseline groups.

## 2. Literature review

### a. Digital literacy and misinformation discernment

Digital literacy combines information access, critical evaluation, communication, safety, content use, and problem solving. DigComp 2.2 organizes digital competence into five areas: information and data literacy; communication and collaboration; digital content creation; safety; and problem solving (Vuorikari et al., 2022). For misinformation education, the most directly relevant competencies include identifying the origin of a claim, comparing sources, recognizing misleading presentation, evaluating evidence, protecting personal data, and deciding whether content should be shared.

International evidence generally supports the value of media-literacy interventions while showing substantial heterogeneity. A meta-analysis by Lu et al. (2024) found that media-literacy interventions can improve fake-news credibility assessment, but the magnitude of benefit depends on intervention and sample characteristics. Experimental studies also suggest that generic messages may have limited effects, whereas interventions supplemented with personalized feedback can better correct specific errors (Ali & Qazi, 2023). Similarly, a brief digital media-literacy intervention improved discernment between mainstream and false news in the United States and India (Guess et al., 2020). These findings imply that programs should diagnose learner needs, provide active practice, and evaluate more than declarative definitions.

### b. Digital citizenship and civil-rights awareness

Digital citizenship extends digital competence into ethical, legal, and civic dimensions. Ribble's (2015) framework includes digital etiquette, law, rights and responsibilities, security, and literacy. More recent scholarship emphasizes that digital citizenship is dynamic and context dependent, encompassing considered technology use, community-oriented responsibility, and civic participation (Webster, 2025). Comparative research further shows that digital-citizenship education must balance freedom of expression, democratic participation, responsibility, and identity rather than reducing citizenship to technical safety rules (Vajen et al., 2023).

Within this perspective, misinformation education is also civil-rights education. Participants need to understand how false claims, hate speech, privacy breaches, and irresponsible sharing can harm individuals and communities. They also need practical ways to respond without reproducing harm: verifying before sharing, correcting claims respectfully, reporting dangerous content, protecting vulnerable individuals, and preserving lawful expression. A digital-literacy program therefore has cognitive, affective, behavioral, and civic outcomes, even when a short knowledge test measures only a subset of those outcomes (Fajri et al., 2022; UNESCO, 2024).

### c. Program evaluation and sensitivity to change

One-group pretest-posttest designs are frequently used in educational programs because they are feasible and make individual change visible. Their interpretation nevertheless requires caution. Without a comparison group, observed change cannot be attributed exclusively to the intervention, and using the same instrument within a short interval may introduce memory or testing effects (Bonate, 2000; Dimitrov & Rumrill, 2003). In addition, a test must be sufficiently difficult and discriminating to detect the expected form of learning.

Sensitivity to change is particularly limited when participants begin near the maximum possible score. In such circumstances, statistical significance is constrained by the small amount of available score variance, and the absence of a significant mean difference should not automatically be equated with the absence of learning. Evaluation should examine score distributions, individual trajectories, item-level performance, effect sizes, confidence intervals, and authentic task performance. For programs intended to change real-world verification behavior, delayed assessment and performance-based tasks are more informative than immediate repetition of easy multiple-choice items.

## 3. Research Method

The study used a one-group pretest-posttest program-evaluation design. The intervention and both assessments were conducted on 18 June 2026 in the Nambo/Bantarjati and Lulut communities of Klapanunggal District, Bogor Regency, West Java, Indonesia, through a vocational-education setting. The design was intended to evaluate immediate learning outcomes rather than long-term retention or stable behavioral change.

The source dataset contained 20 pretest responses and 20 posttest responses. Records were matched using available identity information and checked for internal consistency. Nineteen valid pairs were retained for complete-case paired analysis. One pretest respondent had no matching posttest record, and one posttest response was excluded because its identity, age, and contact information were implausible and did not match the baseline data. The valid paired sample consisted of participants aged 15-18 years ($M = 16.42$, $SD = 0.69$). Fourteen participants (73.7%) were recorded in the Nambo/Bantarjati category and five (26.3%) in the Desa/Kampung Lulut category. Individual records were pseudonymized as P01-P19.

The educational program followed five integrated components. First, participants received an introduction to digital literacy, misinformation, digital ethics, and digital civil rights. Second, they worked with practical examples to identify misleading or false information. Third, facilitators demonstrated accessible verification strategies, including source checking, fact-checking searches, and reverse-image-search principles. Fourth, participants discussed safe and ethical responses to hate speech, privacy risks, and provocative content. Fifth, the program promoted continued engagement through the proposed formation of digital-literacy agents. The intervention was designed as an educational and preventive model linking knowledge, verification skills, and responsible citizenship.

A ten-item knowledge assessment was administered before and immediately after the program. The items covered: the definition of digital literacy; the definition of misinformation or hoaxes; source verification; characteristics of misinformation; digital civil rights; social consequences of misinformation; appropriate responses to hate speech; personal-data protection; digital-citizenship ethics; and the purpose of digital literacy. Each correct response contributed ten points to a total score ranging from 0 to 100. The posttest also recorded participants' stated willingness to serve as digital-literacy agents. Because the same items were used at both time points and most were answered correctly at baseline, the instrument primarily measured immediate knowledge confirmation and had limited capacity to assess advanced verification performance.

Analysis used valid matched pairs only. Descriptive statistics included the mean, median, mode, sample standard deviation, minimum, maximum, absolute change, relative change, and item-level accuracy. The distribution of difference scores (posttest minus pretest) was examined using the Shapiro-Wilk test. Because the differences were highly non-normal and contained many zero values, the Wilcoxon signed-rank test was selected as the primary inferential test. The Wilcoxon effect size was expressed as $r$. A paired-samples $t$ test, Cohen's $d_z$, and an exact sign test were used as sensitivity analyses. Uncertainty was summarized with a conventional 95% confidence interval for the mean difference and a percentile bootstrap interval. Statistical significance was evaluated at $\alpha = .05$. Subgroup comparisons were treated as descriptive because the groups were small and unequal.

Direct identifiers were excluded from the article, and individual-level results are reported only through pseudonyms. The available documentation did not include a formal ethics-approval number or a complete consent statement; these details must be added by the authors before journal submission. The analysis does not claim outcomes for which documentary evidence was unavailable, such as verified mastery of fact-checking tools, formal establishment of an agent group, or long-term behavioral change.

## 4. Results And Discussion

### a. Participant flow and characteristics

The matched-pair retention rate was 95.0% relative to the 20 pretest responses. This rate indicates good completion of the immediate evaluation, but it should not be interpreted as the overall attendance rate because a formal attendance list was not included in the analyzed documentation. Most participants were 16 years old (52.6%), followed by 17 years old (36.8%); one participant was 15 and one was 18.

***Table 1. Participant characteristics and data screening***

| Component | Result |
|---|---|
| Pretest responses | 20 |
| Posttest responses | 20 |
| Valid matched pairs | 19 |
| Unmatched pretest record | 1 |
| Excluded posttest record | 1 |
| Age range | 15-18 years |
| Mean age (SD) | 16.42 (0.69) |
| Nambo/Bantarjati | 14 (73.7%) |
| Desa/Kampung Lulut | 5 (26.3%) |

### b. Score changes

At the aggregate level, the mean score increased by 1.05 points, from 98.42 to 99.47. The median and mode were 100 at both time points. The standard deviation declined from 6.88 to 2.29, indicating that posttest scores were more homogeneous. No participant's score decreased. Eighteen participants scored 100 at both time points, while P18 improved from 70 to 90. Thus, the group mean increased because of one substantial individual improvement rather than a broad upward shift across the sample.

***Table 2. Descriptive statistics for pretest, posttest, and change scores***

| Statistic | Pretest | Posttest | Change |
|---|---|---|---|
| N | 19 | 19 | 19 |
| Mean | 98.42 | 99.47 | 1.05 |
| Median | 100.00 | 100.00 | 0.00 |
| Mode | 100.00 | 100.00 | 0.00 |
| Standard deviation | 6.88 | 2.29 | 4.59 |
| Minimum | 70 | 90 | 0 |
| Maximum | 100 | 100 | 20 |

### c. Inferential analysis

The difference scores were strongly non-normal (Shapiro-Wilk $W = 0.244$, $p < .001$). The Wilcoxon signed-rank test did not identify a statistically significant aggregate change ($W = 0$, $z$ approximately -1.00, $p = .317$), and the effect size was small ($r = .229$). The paired-samples t test reached the same conclusion, $t(18) = 1.00$, $p = .331$, Cohen's $dz = .229$. The 95% confidence interval for the mean change ranged from -1.16 to 3.26 points, and the bootstrap interval ranged from 0.00 to 3.16. The exact sign test was also non-significant ($p = 1.000$), reflecting one positive change and 18 ties.

***Table 3. Inferential tests, effect sizes, and uncertainty***

| Analysis | Result | Interpretation |
|---|---|---|
| Shapiro-Wilk test of change | $W = 0.244$; $p < .001$ | Non-normal distribution |
| Wilcoxon signed-rank test | $W = 0$; $z \approx -1.00$; $p = .317$ | Not statistically significant |
| Wilcoxon effect size | $r = .229$ | Small |

| Analysis | Result | Interpretation |
|---|---|---|
| Paired-samples t test | t(18) = 1.00; p = .331 | Sensitivity result unchanged |
| Cohen's dz | d = .229 | Small |
| 95% CI for mean change | -1.16 to 3.26 | Includes zero |
| Bootstrap 95% CI | 0.00 to 3.16 | Zero at lower bound |
| Exact sign test | p = 1.000 | One positive change; 18 ties |

**d. Item-level outcomes**

Item-level performance was already very high before the intervention. Eight items remained at 100% accuracy at both time points. Accuracy for the item on appropriate responses to hate speech increased from 94.7% to 100%, and accuracy for the purpose of digital literacy increased from 94.7% to 100%. The item concerning characteristics of misinformation remained at 94.7%, indicating one persistent area for instructional follow-up.

***Table 4. Item-level accuracy before and after the program***

| Item | Competency | Pretest correct | Posttest correct | Change (pp) |
|---|---|---|---|---|
| I1 | Definition of digital literacy | 100.0% | 100.0% | 0.0 |
| I2 | Definition of misinformation | 100.0% | 100.0% | 0.0 |
| I3 | Source verification | 100.0% | 100.0% | 0.0 |
| I4 | Characteristics of misinformation | 94.7% | 94.7% | 0.0 |
| I5 | Digital civil rights | 100.0% | 100.0% | 0.0 |
| I6 | Social consequences of misinformation | 100.0% | 100.0% | 0.0 |
| I7 | Response to hate speech | 94.7% | 100.0% | +5.3 |
| I8 | Personal-data protection | 100.0% | 100.0% | 0.0 |
| I9 | Digital-citizenship ethics | 100.0% | 100.0% | 0.0 |
| I10 | Purpose of digital literacy | 94.7% | 100.0% | +5.3 |

**e. Ceiling effect and attitudinal outcome**

The baseline mean of 98.42 on a 100-point scale created a severe ceiling effect. From that baseline, the largest possible relative increase in the group mean was only approximately 1.60%. Consequently, the original program target of a 30% mean-score increase was mathematically incompatible with the observed starting point. All posttest respondents stated willingness to participate as digital-literacy agents. This result is best interpreted as an immediate attitudinal commitment, not as verified civic behavior or evidence that a formal agent group was established.

## 5. Discussion

The evaluation produced a clear but nuanced result. The program did not generate a statistically significant aggregate score increase, yet it maintained very high knowledge among most participants, reduced score dispersion, corrected two item-level gaps, and produced a 20-point gain for the participant with the lowest baseline score. The most defensible interpretation is therefore maintenance plus targeted improvement. This interpretation follows the observed distribution rather than relying only on the direction of the mean difference.

The non-significant result is primarily an assessment problem rather than evidence of program failure. With 18 of 19 participants already at the maximum score, the instrument offered almost no numerical space for improvement. The test was dominated by declarative and definitional content, whereas the intervention also addressed practical verification, ethical judgment, personal-data protection, and civic responsibility. A learner may become more careful in tracing sources or responding to harmful content without obtaining a score above 100. The instrument therefore underrepresented the intended outcomes and lacked sensitivity for a high-performing sample.

This pattern is consistent with international evidence showing that the effectiveness of misinformation education depends on program design and outcome measurement. Meta-analytic

findings support a positive average effect of media-literacy interventions but also show heterogeneity across contexts (Lu et al., 2024). Experimental studies indicate that generic instruction does not necessarily benefit all participants equally and that personalized feedback may be especially useful for correcting specific misconceptions (Ali & Qazi, 2023). The present finding that the largest change occurred for the participant with the lowest baseline score supports a differentiated approach: participants who already demonstrate conceptual mastery need advanced performance tasks, whereas participants with identified gaps need targeted feedback and guided practice.

The timing of measurement also limits interpretation. The median interval between pretest and posttest completion was 24.16 minutes, with a range of 23.38-25.83 minutes. The posttest therefore captured immediate learning or recall, not retention. Brief media-literacy interventions can have immediate and short-term effects, but persistence must be demonstrated through later measurement (Berger et al., 2025). A delayed posttest after two to four weeks and a behavioral follow-up after one to three months would provide stronger evidence of durable learning and transfer.

From a digital-citizenship perspective, the item gains related to hate-speech response and the purpose of digital literacy are educationally meaningful. These topics connect information competence with etiquette, rights, responsibilities, safety, and community care (Ribble, 2015; Webster, 2025). The unanimous stated willingness to become digital-literacy agents may indicate an emerging civic disposition. Nevertheless, intention should not be conflated with action. Future evaluation should verify whether participants conduct fact checks, correct misinformation respectfully, support peers, protect personal data, and participate in community education.

The study also demonstrates why program targets must be calibrated to baseline performance and instrument range. A 30% mean increase was impossible when the baseline mean was 98.42. More valid success criteria would include the proportion of participants who correctly complete a fact-verification task, the quality of evidence used to justify a judgment, the reduction of errors among participants below mastery, retention at delayed posttest, and documented civic action. These indicators would align program goals with observable performance and avoid rewarding or penalizing a program because of an arbitrary percentage target.

For assessment design, the next instrument should combine difficult multiple-choice items with scenarios, open-ended reasoning, and authentic tasks. Participants could be asked to locate the original source of a claim, compare independent sources through lateral reading, conduct a reverse image search, identify a mismatch between an image and its alleged date or location, distinguish satire from deceptive content, and formulate an ethical response to hate speech. Scoring rubrics should evaluate accuracy, reasoning quality, source credibility, safety, and respect for rights. Content validity should be reviewed by experts, and item difficulty, discrimination, and reliability should be examined before program-scale use.

Several limitations constrain the study. First, the one-group design does not establish causal attribution. Second, the sample was small and limited to adolescents aged 15-18, restricting generalizability to the broader village population. Third, the same short and easy test was repeated within approximately 24 minutes, increasing the risk of testing and memory effects. Fourth, demographic information was limited; gender, education level, previous digital-literacy exposure, device access, and internet-use patterns were unavailable. Fifth, the dataset did not include performance records for verification tools, formal documentation of the agent group, or delayed behavioral outcomes. These limitations reduce internal and external validity and should guide the design of a stronger subsequent evaluation.

## 6. Conclusion

This study evaluated the immediate outcomes of a digital-literacy program for vocational-school-aged adolescents in rural Bogor. Among 19 valid matched pairs, mean knowledge scores increased from 98.42 to 99.47, score variability decreased, no participant declined, and one participant improved by 20 points. The aggregate change was not statistically significant, principally because 94.7% of participants had already reached the maximum score at pretest. The program should therefore be interpreted as maintaining high competence and producing targeted improvement rather than as generating a broad increase across participants. The evaluation also shows that an easy immediate posttest cannot adequately represent advanced verification skills, retention, or civic behavior. Future studies should use baseline-calibrated targets, performance-based assessment, delayed posttests, behavior indicators, and stronger comparison designs. These changes would provide more valid

evidence about whether digital-literacy education improves misinformation discernment, protects digital civil rights, and supports responsible civic participation.

## Declarations

**Ethics statement:** All trader identities are anonymised. Interview quotations are reported through codes and are paraphrased or translated without changing substantive meaning.

**Data availability:** The aggregated data used in this manuscript are derived from field questionnaire and interview summaries. Raw respondent-level data should be shared only after anonymisation and author approval.

**Conflict of interest:** The authors should declare any conflict of interest. If none, write: The authors declare no conflict of interest.

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