AFFECTIVE DIGITAL LITERACY AND CITIZENSHIP: A QUASI-EXPERIMENTAL STUDY IN PRIMARY SCHOOL

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ABSTRACT
Introduction. We evaluated a primary-school programme integrating media education and social-emotional learning. Method. Quasi-experimental pre-test/post-test/follow-up design; four intact classes (N = 96) received a 24-hour programme over 12 weeks, while two control classes followed standard teaching. Measures included digital critical thinking (DCTS-C), digital emotional awareness (DEAQ), and algorithmic manipulation recognition (AMRT). Results. ANCOVA models adjusting for baseline scores showed higher post-test scores in the experimental group across all outcomes. Sensitivity analyses adjusting for clustering showed that only digital critical thinking remained statistically robust across plausible intraclass correlation ranges. The T0–T1 change scores of critical thinking and emotional awareness correlated moderately (r = .42, p = .003). Discussion. The intervention shows promising gains in digital critical thinking and emotional awareness. However, with four clusters, findings are promising rather than causally conclusive, highlighting the need for larger cluster-randomized trials.

KEYWORDS
Affective digital literacy; media education; social-emotional learning; primary school; quasi-experimental design.

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1. Introduction

The contemporary debate concerning the relationship between digital technologies, education, and citizenship, in recent years, has been addressed by multiple and different disciplinary traditions: media pedagogy, the sociology of education, developmental psychology, and, more recently, the ethics of artificial intelligence. Despite this plurality of perspectives, there remains a relatively underrepresented research area: that which investigates the affective and emotional dimension of the digital experience of children and pre-adolescents; in particular, the capacity of the latter to recognize and resist the forms of emotional manipulation conveyed by the algorithms of social media and digital platforms.

Floridi (2015, 2019) highlights how we now live in an "onlife" condition, in which the distances between the physical and the informational world are increasingly minimal, to the point of redefining both the modalities of access to knowledge and the identity, relational, and cognitive practices of individuals. In this ecosystem, algorithmic systems play an active and non-neutral role: they select, filter, and amplify content based on predictive models of user behaviour, with effects that concern both access to information and the emotional and affective sphere (Pariser, 2011; Zuboff, 2019). In the digital world, the attention economy feeds on emotional aspects: content that arouses fear, indignation, enthusiasm, or nostalgia receives greater algorithmic exposure because it is capable of generating more intense affective responses and of prolonging the time spent on the platform (Lazer et al., 2018).

From the point of view of the school institution, it is possible to trace two paths: the first concerns that of national and European educational policies, from the National Digital School Plan (MIUR, 2015) to the DigComp framework, from version 2.1 (Carretero et al., 2017) to version 3.0 (Cosgrove & Cachia, 2025).

These regulatory references insistently invoke the development of critical digital competences as a transversal objective of school curricula. The second aspect partially contradicts what is established by European regulation, since the available empirical research suggests that the effective implementation of educational paths aimed at the critical understanding of digital media remains fragmentary, predominantly oriented toward technical-instrumental aspects and scarcely attentive to the emotional and relational dimension of the digital experience (Buckingham, 2019; Ranieri, 2024; Rivoltella, 2019). A particularly relevant gap is therefore highlighted in primary school, where most Media Education interventions focus on online safety (cyberbullying, privacy protection) and not on the development of competences for the critical analysis of algorithmic mechanisms and their affective implications. This deficiency was already made manifest by Mascheroni and Ólafsson (2014), and subsequently confirmed by OFCOM (2023), according to which school-age children are active users of digital platforms even before the age of ten, in family contexts in which digital media are a pervasive and often contested presence (Livingstone & Blum-Ross, 2020), which makes it urgent to anticipate digital affective literacy paths well before adolescence.

The contribution pursues three general aims, which are translated into the specific research questions presented in Section 3.1:

To what extent can the construct of "digital affective literacy" be defined in a theoretically coherent way, integrating contributions from Media Education, digital citizenship, and socio-emotional education?

Is it possible to design and implement a structured instructional intervention, calibrated to the developmental age of primary school students, that produces measurable improvements in digital critical thinking and emotional awareness regarding algorithmic manipulation mechanisms?

What relationship exists, in empirical terms, between the cognitive and affective dimensions of digital literacy, and do the effects of the intervention vary according to variables such as gender, home access to technology, or family socioeconomic status?

2. Theoretical framework

2.1 From Media Education to digital citizenship: a critical genealogy

Media Education has undergone, over the last fifty years, radical transformations that reflect changes in the communicative ecosystem and reference pedagogical theories. Doni (2015) states that the history of approaches to Media Education can be read as a progressive overcoming of the idea that considered media as agents of cultural decline from which to protect young people, moving toward models increasingly centered on the critical agency of users and on the enhancement of participatory skills. This transition was neither linear nor free of tensions. Suffice it to think of the "popular arts" model of the 1960s and 1970s, which struggled to relate to the pervasive and transversal nature of mass media (Buckingham, 2003), leading up to the semiotic and sociocultural perspective, which recognizes the value of media as active symbolic systems in the construction of social reality (and not mere channels for transmitting content). In this context, Media Education assumes an autonomous and theoretically founded disciplinary profile (Rivoltella, 2005).

Soriani's (2018) article offers a particularly accurate analysis of the subsequent transition from Media Education to Media Literacy and from the latter to Media and Information Literacy (MIL), documenting how UNESCO progressively broadened the semantic field of media education to include dimensions of active citizenship, democratic participation, and socio-relational skills; more recently, UNESCO's guidance on artificial intelligence in education has

extended this citizenship-oriented perspective to AI (UNESCO, 2021). The 2011 Fez Declaration and the 2014 Paris Declaration mark the affirmation of an integrated model in which technical competence is inseparable from the responsible exercise of civic rights and duties in the digital world. As Tomasello (2023) observes, the transition from Media Literacy to digital citizenship introduces a qualitatively new complexity: it is no longer just a matter of knowing how to read and produce media content, but of understanding the automated mechanisms that allow access to information and political participation. In this sense, numerous contributions (Chiusaroli, 2026; Selwyn, 2022; Williamson et al., 2023) state that Artificial Intelligence education cannot be reduced to the transmission of technical skills, but must aim to form subjects capable of critically questioning algorithmic systems, recognizing their limitations and ethical implications, and acting responsibly in the digital society.

Unfortunately, there is a sharp discrepancy between this emancipatory vision of digital education and the available empirical data on the actual implementation of national policies. In this regard, Soriani's (2018) analysis of the national school plans of five European countries reveals that most government interventions stop at the level of technological infrastructure and technical teacher training, without systematically addressing the social, relational, and critical dimensions of digital citizenship. Regarding the Italian school system, the National Digital School Plan is certainly an emblematic example, since it recognizes the importance of the educational relationship and the participatory implications of technologies, but resources are predominantly focused on logistical and cognitive aspects, neglecting the affective-relational dimension. This gap reflects a structural tension in the way educational policies address the digital domain: on the one hand, the need for certifiable and measurable skills (which privileges technical and cognitive dimensions); on the

other hand, the difficulty of translating constructs such as digital empathy, emotional awareness, or critical thinking into empirical indicators. This hypothesis is also reinforced by the fact that, although reference European frameworks, from DigComp to the Council of Europe's proposal for Digital Citizenship Education, dedicate increasing space to competences such as "Ethics and Empathy" or "Health and Wellbeing", in the school context these often remain at the margins of curricula.

2.2 Platform capitalism and algorithmic manipulation: educational implications

The understanding of algorithmic manipulation mechanisms is explained in detail by Zuboff (2019), who introduced the concept of "surveillance capitalism". With it, the author describes the economic model of major digital platforms, based on the systematic collection of users' behavioural data, their transformation into predictive profiles, and the sale of the latter as "raw material" to influence behaviour. In this scheme, users' attention (particularly their emotional responses) becomes a commodity; therefore, recommendation algorithms are designed to maximize emotional engagement, not necessarily the well-being or critical autonomy of users. Pariser (2011) has argued that content personalization algorithms are capable of filtering and amplifying users' pre-existing beliefs and emotions, so as to reduce exposure to different perspectives and foster forms of cognitive and affective polarization. Selwyn (2019) deepened the educational implications of these dynamics, showing how schools are progressively permeated by logics that risk subordinating educational goals to the metrics of algorithmic engagement. Ragnedda and Ruiu (2020) proposed the concept of "digital capital" to analyze inequalities in access to and use of digital technologies. The authors highlight how digital capital is strongly correlated with economic, cultural, and social

capital, an aspect that amplifies pre-existing inequalities, not only in providing access to tools, but also in distributing the skills necessary to use them in a critical and autonomous way.

For primary education, these dynamics assume a specific relevance since children between 8 and 12 years of age find themselves in a phase of cognitive and affective development in which the capacity for abstract thought, necessary to understand algorithmic mechanisms, is still forming (Piaget & Inhelder, 1966) but basic emotional structures are already fully active. This latter aspect creates a particular vulnerability to the emotional persuasion strategies of digital media: children react intensely to algorithmically selected content, but still possess limited cognitive resources to recognize and critically analyze them. In this regard, Crone and Dahl (2012) highlight how the limbic system, responsible for emotional responses, reaches full maturation before the prefrontal cortex, responsible for cognitive control and critical thinking, effectively creating a window of vulnerability that is particularly relevant for digital affective education. As Hobbs (2010) argues, an effective media education for children must integrate the emotional dimension of the media experience as an object of reflection and learning. Concretely, this means helping children recognize their own emotional responses to digital content, question the reasons behind such responses, and develop emotional regulation strategies that allow them to maintain critical autonomy even in the presence of affectively intense stimuli.

2.3 Artificial intelligence: an educational challenge between opportunities and risks

Artificial intelligence introduces a series of qualitatively new challenges into the educational ecosystem. As Chiusaroli (2026) points out, AI is not simply an additional technological tool, but a "sociotechnical actor", taking up the sense

of Latour's (2005) Actor-Network Theory, which actively participates in the construction of social, educational, and cognitive practices. This perspective invites reflection on a more complex vision of artificial intelligence concerning its agency and human responsibilities in its design and governance. From the point of view of educational intervention, what has been said translates into a series of concrete issues that affect both "what" and "how" to teach. On the content level, Panciroli and Rivoltella (2023) have argued that an AI education that is authentic and educational must include:

a) the understanding of data collection and utilization mechanisms;

b) the functioning logic of recommendation algorithms;

c) the ethical implications of automated decision-making systems;

d) the forms of critical resistance that citizens can exercise.

On the methodological level, Malavasi (2019) proposes a pedagogy of artificial intelligence that combines technological, ethical, and environmental dimensions, avoiding both techno-optimistic naiveties and technophobic pessimisms. Ranieri (2024) has proposed a lucid analysis of the tensions that manifest in the educational field concerning AI in Italian schools. The author documents how poor teacher training, fragmented curricula, and the lack of appropriate teaching tools make a systematic implementation of AI difficult. Toci et al. (2024) have shown that the perception Italian teachers have toward AI (opportunity or threat) depends particularly on the level of digital competence and the organizational culture of their school.

The present work uses artificial intelligence as an object of study in primary school, not as a technical understanding of machine learning algorithms (objectively inaccessible to 8-10-year-old children), but through concrete experiences and age-appropriate metaphorical languages: "the phone that

learns what you like"; "suggestions that change because the machine observes you"; "the difference between what you search for and what you are shown". AI manifests itself, therefore, through metacognitive reflections accessible even to primary school children.

2.4 Digital affective literacy: toward an integrated theoretical proposal

The concept of digital affective literacy proposed in this contribution stems from the integration of three distinct theoretical bodies:

a) critical Media Education in the tradition of Masterman (1985) and Buckingham (2003, 2019), including critical media literacy (Kellner & Share, 2007);

b) socio-emotional education (Social-Emotional Learning, SEL) in the tradition of emotional intelligence research (Goleman, 1995; Salovey & Mayer, 1990), of Zins et al. (2004) and of CASEL (2013, 2020);

c) research on the relationship between emotion and cognition in the digital context.

Therefore, it is not possible to speak, in a strict sense, of a new autonomous construct, but rather of a specification of the broader field of critical digital citizenship, emphasizing, particularly, the affective and relational dimensions of the digital experience.

The CASEL (Collaborative for Academic, Social, and Emotional Learning) framework identifies five core competences of socio-emotional education: self-awareness, self-management, social awareness, relationship skills, and responsible decision-making (CASEL, 2020; Durlak et al., 2011). The present work aims to decline each of these competences in specific relation to the digital experience, generating a two-dimensional framework in which the SEL

dimension and the Media Literacy dimension intersect and mutually reinforce each other (see Table 1).

Table 1: Definition of the fundamental skills of socio-emotional education

Fundamental skills of social-emotional education	**Declination**
self-knowledge	includes the ability to recognize one's own emotional responses to digital content and to question their origin
self-management	includes the ability to adjust screen time and media exposure quality
social awareness	includes understanding group dynamics in digital contexts and recognizing social influence strategies
interpersonal skills	include the ability to communicate responsibly online
responsible decision-making	includes the ability to critically evaluate sources, recognize fake news, and resist the emotional levers of algorithmic manipulation

As can be seen from Table 1, the operationalization of these competences allows for the translation of the theoretical concepts of SEL into situated educational objectives. It is worth noting that, in the past, Hobbs (2010, 2011, 2016) developed a Digital and Media Literacy framework that already incorporates affective elements, including the ability to reflect on one's own conduct and communication by applying ethical principles and social responsibility. However, as Buckingham (2019) observed, the affective dimensions of the media experience frequently remain at the margins of Media Education curricula, which privilege cognitive-analytical aspects. The present contribution aims to fill this gap, both on a theoretical level, by articulating a more explicit model of the interconnection between cognition and emotion in the digital experience, and on an empirical level, by operationalizing the construct into measurement instruments usable in educational research.

An effective digital affective literacy must aim for the conscious recognition of emotional responses and their integration into reflective decision-making processes. What has been said can be framed, drawing on Bandura's (2001) social cognitive account of human agency, as a cognitive-affective form of agency: the capacity to act in the world, including the digital world, through an integrated process of cognitive evaluation and emotional regulation. It is necessary to specify that the field of digital affective literacy is still theoretically fragmented and empirically poorly consolidated. Available research is predominantly conducted with samples of adolescents and adults, and evidence on the effectiveness of specific interventions in primary school is limited. This makes the production of new empirical evidence, such as that which the present study aims to generate, particularly important, while remaining aware of the unavoidable limitations of an exploratory study.

1. Method

3.1 Research questions and hypotheses

The present work aims to answer the following research questions:

RQ1: Does the experimental instructional intervention produce statistically significant improvements in the level of digital critical thinking of the students in the experimental group compared to the control group?

RQ2: Does the intervention produce significant improvements in the emotional awareness of students in digital contexts?

RQ3: Does the intervention produce significant improvements in the capacity to recognize algorithmic manipulation strategies?

RQ4: Are there differences in effect sizes as a function of gender, family socioeconomic status, and home access to technology?

RQ5: Are the differences between the groups observed at the post-test still present three months after the end of the intervention?

Hypotheses H1–H3 predict that, controlling for pre-test scores, the experimental group obtains higher post-test scores than the control group in digital critical thinking (H1), emotional awareness (H2) and recognition of algorithmic manipulation (H3). RQ4 and RQ5 are treated as exploratory questions, without directional hypotheses: in particular, since a non-significant interaction test cannot demonstrate that moderation is absent, no hypothesis of absence of moderation was formulated. H1–H3 are founded on the SEL literature (Durlak et al., 2011) and on Media Education research (Hobbs, 2010), which document positive effects of similar interventions on comparable populations.

3.2 Participants and allocation of classes

The study involved 96 students (47 girls, 49 boys) attending four intact primary school classes (two fourth-grade classes and two fifth-grade classes) of a primary school in the province of Naples, in a medium-sized urban context, in the school year 2024/2025. Allocation took place at class level: the four classes were assigned by convenience, in agreement with school leaders and in compliance with organizational constraints (no random procedure was used), to either the experimental condition (n = 48; 26 girls, 22 boys; mean age = 9.8 years, SD = 0.70) and two to the control condition (n = 48; 21 girls, 27 boys; mean age = 9.85 years, SD = 0.70). Data collection occurred across three time points within the same academic year: baseline (T0: November 2024), post-test (T1: February 2025, immediately following the 12-week intervention), and follow-up (T2: May 2025, three months post-intervention, prior to end-of-year class transitions). Teacher interviews were conducted in June 2025. Table 2 reports the demographic and class structure.

Table 3: Composition of the four classes (anonymous class codes)

Class	Grade	Condition	*n* (girls/boys)	Age, *M* (*SD*)	Teachers	T2 data (*n*)
E1	4th	Experimental	24 (14/10)	9.4 (0.42)	Class teacher and specialist teacher (trained); 15 and 12 years of experience	21
E2	5th	Experimental	24 (12/12)	10.3 (0.45)	Class teacher and specialist teacher (trained); 18 and 22 years of experience	19
C1	4th	Control	24 (10/14)	9.3 (0.40)	Class teachers (no training; usual curriculum); 2 teachers, 14 and 10 years of experience	19
C2	5th	Control	24 (11/13)	10.4 (0.48)	Class teachers (no training; usual curriculum); 2 teachers, 20 and 16 years of experience	19
Total	–	Experimental	48 (26/22)	9.85 (0.63)	–	40
Total	–	Control	48 (21/27)	9.85 (0.70)	–	38

The inclusion criteria required:

a) regular attendance of the class of origin;

b) signed availability from parents, or legal guardians, via informed consent;

c) the child's assent to take part in the data collection (see Section 3.3).

No exclusion criteria based on pre-existing digital competence levels were applied, so as not to exclude students on the basis of their initial level of competence. Families were informed of the study's objectives through a plenary meeting and a written document; informed consent was acquired in writing from all parents or legal guardians.

The socioeconomic profile of the families was collected through a questionnaire administered to parents or legal guardians and identified only by the student's alphanumeric code, so that the answers could be linked to the student's scores without recording names, in order to evaluate parental education level, occupation, and home access to digital technologies. Sixty-eight percent (68%) of the households possessed at least one smartphone device for each family member, 72% had a stable home internet connection, and 44% had at least one computer or tablet dedicated to their children. Thirty-five percent (35%) of the students declared that they owned their own personal smartphone. The construction of the moderator variables derived from this questionnaire is described in Section 4.4.

3.3 Ethical considerations

The study was conducted in accordance with the ethical guidelines for educational research of the British Educational Research Association (BERA, 2018) and with the European and Italian legislation on the protection of personal data (Decreto legislativo 30 giugno 2003, n. 196, 2003, as amended by Legislative Decree 101/2018; Regulation (EU) 2016/679, 2016). The project was

approved by the school's collegial bodies and authorized by the head teacher following the Teachers' Council.

The study did not involve clinical procedures, pharmacological interventions, or sensitive data beyond standard school records, and formal review by a university ethics committee was not required under current Italian and EU legislation (D.lgs. 196/2003 as amended by D.lgs. 101/2018; Regulation EU 2016/679); all procedures were conducted in compliance with the BERA (2018) ethical guidelines for educational research.

Written informed consent was obtained from all parents or legal guardians after a plenary meeting and the delivery of an information sheet describing the aims and procedures of the study, the processing of the data and the right to withdraw at any time without consequences. The children's assent was also sought: before the pre-test, a researcher explained the purpose of the activities to each class in age-appropriate language, clarifying that the questionnaires and tasks were not school tests and would not be graded, and that each child could decline to answer or stop at any time. Written assent was collected on simplified forms. None of the children declined or withdrew.

Confidentiality during the activities was protected by ground rules agreed with each experimental class at the beginning of Module 1: children were invited to share online experiences without naming classmates or other identifiable people and without disclosing information they did not wish to share, and not to report outside the classroom what peers had said. The digital materials used were simulated or pre-selected by the teachers, and no personal accounts or devices of the children were used. Teachers were asked to refer any disclosure suggesting a risk to a child (e.g., cyberbullying or contact with strangers) to the school's safeguarding procedures.

Data were pseudonymized at collection: each child received an alphanumeric code, and the key linking codes to names was stored separately from the dataset and was accessible only to the principal investigator. Results are reported only in aggregate form, and qualitative excerpts are identified by anonymous codes (Section 3.6).

3.4 Measures

The three instruments used to measure the dependent variables were all developed ad hoc by the authors. The works cited below are the theoretical sources used to define the constructs, not pre-existing measurement instruments; for each instrument we report development, piloting and psychometric analyses.

a) Digital Critical Thinking Scale for Children (DCTS-C). The DCTS-C is a new scale developed for the present study and is not the adaptation of a pre-existing instrument. Its content domains were derived from conceptual frameworks of digital and media literacy that identify the access, analysis and evaluation of media messages as core competences (Hobbs, 2010; Hobbs & Jensen, 2009); these works are theoretical sources and do not contain a measurement scale. Development followed the phases recommended for new scales (Boateng et al., 2018): (1) definition of three content domains and generation of an initial pool of 24 multiple-choice items; (2) review of the items for relevance and age-appropriateness by three independent experts in media education and developmental psychology; (3) cognitive interviews with a pilot sample of 15 children aged 9–11 years not included in the main study (Willis, 2005), which led to 6 items being removed due to excessive cognitive load or cultural bias, and 4 reworded for clarity; (4) pilot administration 120 children from 6 classes not involved in the study, followed by item analysis (difficulty indices and

corrected item–total correlations) and exploratory factor analysis employing robust diagonally weighted least squares (DWLS) on tetrachoric correlations; retention criteria based on parallel analysis; 58% of variance explained; loadings ranging from .45 to .78. The final scale is composed of 18 multiple-choice items with four options (one correct answer, scored 1; incorrect answers scored 0), organized into three subscales: understanding of digital content selection mechanisms (6 items; KR-20 = .73), critical evaluation of source reliability (6 items; KR-20 = .71), and recognition of persuasive strategies in digital media (6 items; KR-20 = .69). The total score ranges from 0 to 18, with higher scores indicative of greater digital critical thinking; the reliability of the total score was KR-20 = .82.

b) Digital Emotional Awareness Questionnaire (DEAQ). This instrument was developed specifically for the present study, as validated instruments in Italian for measuring emotional awareness in digital contexts within the considered age group were not available (Italian instruments for primary school address the technological, cognitive and ethical components of digital competence; e.g., Calvani et al., 2010). The DEAQ is composed of 15 items on a 4-point Likert scale (from "never" to "always"), organized into two subscales: recognition of one's own emotional responses to digital content (8 items; $\alpha = .76$) and capacity for reflective distance regarding emotions aroused by the media (7 items; $\alpha = .71$). The items were developed through an iterative process that included the generation of an initial pool of 25 items based on the SEL competences of self-awareness and self-management (CASEL, 2020); review by three experts in educational psychology and Media Education, who rated the relevance and clarity of each item using a content validity index (I-CVI), retaining items with I-CVI > .80; and piloting with two classes not included in the main sample (40 children), followed by item analysis and exploratory factor analysis utilizing

principal axis factoring (PAF) with Promax rotation, retaining two factors explaining 52% of the variance, with loadings between .42 and .81, removing 10 items due to cross-loadings > .30. The total score ranges from 15 to 60; the reliability of the total score was α =.78.

c) Algorithmic Manipulation Recognition Task (AMRT). This instrument is a performance task, not a self-report questionnaire, which measures children's capacity to identify algorithmic manipulation strategies in standardized digital scenarios. The AMRT consists of 10 scenarios presented through images and short written descriptions, each representing a typical digital experience situation mediated by algorithms (for example: social media feeds with emotionally polarized content; personalized recommendations; notifications designed to generate urgency; comments designed to arouse indignation). For each scenario, the child must answer three closed-ended questions relating to the recognition of the manipulation, the identification of the intent present beyond the message, and the proposal of an alternative response. The total score ranges from 0 to 30 (one point per correct answer; KR-20 = .78). The scenarios were developed by the authors on the basis of the persuasive techniques discussed in the literature on algorithmic curation and emotional engagement (Pariser, 2011; Zuboff, 2019) and were reviewed and piloted together with the DEAQ; initially 15 scenarios were developed, and 5 were discarded because they were deemed too complex or ambiguous during the pilot phase. Evidence of convergent validity is provided by the correlations at pre-test (N = 96) with the DCTS-C (r = .51, p < .001) and with the DEAQ (r = .38, p < .001), suggesting adequate convergence but not redundancy.

The reliability coefficients reported above were computed on the main sample at T0, N = 96; for the dichotomously scored DCTS-C and AMRT the coefficient is

KR-20, which is numerically equivalent to Cronbach's α for binary items. The coefficients of the DCTS-C subscales (.69–.73) indicate moderate internal consistency, and the factor analyses were carried out on samples smaller than those usually recommended for this purpose; the psychometric evidence is therefore preliminary. Moreover, because the three instruments were developed by the same authors who designed the intervention, their content is closely aligned with that of the programme, which may inflate the estimated effects (see Section 7).

3.5 Description of the intervention

The instructional intervention addressed to the experimental group is structured into four thematic modules for a total of 24 hours distributed over 12 weeks (2 hours per week; Table 3). The design of the intervention is inspired by the principles of Digital Media Literacy Education (Hobbs, 2010), socio-emotional education (CASEL, 2013), and critical pedagogy (Freire, 1970), seeking to coherently integrate the cognitive, affective, and practical dimensions of digital literacy.

Table 3: Description of thematic modules of the EG teaching intervention

Module	Week	Hours	Title	Description
1	1-3	6	How emotions feel online	This module introduces children to the concept of emotion in digital experiences through emotional recognition and expression activities based on concrete digital scenarios. Activities include: reading and discussing illustrated stories

				depicting children reacting emotionally to online content; creating "digital emotion maps" in which children identify and categorize their emotional responses to different types of digital content; role-play sessions in which children experiment with their emotional reactions to digital messages specifically designed to elicit different affective responses.
2	4-6	6	How the learning phone works	This module introduces the concepts of algorithms and personalized recommendations through age-appropriate analogies and metaphors. Activities include: a simplified experiment in which children "build" a

				recommendation algorithm for their favorite toys; a guided analysis of simulated social media feeds, identifying content selection patterns; a discussion of the concept of the "filter bubble" through the metaphor of a "hall of mirrors," in which children see only their own reflections.
3	7-9	6	When the message wants to convince me	This module explicitly addresses emotional persuasion strategies in digital media, focusing on the most common techniques: scarcity (artificial urgency), social proof (peer pressure), emotional clickbait, and fear of missing out (FOMO). Activities include: critical analysis of authentic digital messages (previously selected by teachers to

				ensure age-appropriateness); creating "manipulation detectors" that children can apply to the content they encounter; discussing the distinction between legitimate persuasion and manipulation.
4	10-12	6	Being digital citizens	The final module integrates the skills developed in the previous modules into a framework for active and responsible digital citizenship. Activities include: collaborative production of a class "digital rights and responsibilities manifesto"; creation of responsible digital content (short videos, presentations, posters) on topics of interest to children; a simulated "digital class council" in

				which children discuss and vote on rules for the responsible use of digital media in their community.

The control group followed "traditional" curricular activities, without receiving any supplementary intervention related to digital literacy topics. It is worth noting that, to reduce the risks of contamination between the groups, the experimental and control classes belonged to the same two sections (A and B), but the children took part in the intervention during a dedicated curricular schedule, without the materials being shared with their peers in the control classes. Contamination through informal contacts between children, or between teachers of the same sections, cannot however be excluded. The control classes did not address specific digital-literacy topics in their ordinary curriculum during the study period, adhering to standard statutory subjects.

The teachers involved in the experimental group (two for each class, one main teacher and one specialist) participated in a preliminary 6-hour training, conducted by the researchers, to familiarize themselves with the intervention materials and the underlying pedagogical strategies. They were provided with a detailed manual for each session, containing indications on the objectives, activities, materials, and management strategies for group dynamics. The sessions were observed by the researchers using structured observation grids to verify the implementation fidelity (fidelity check); Implementation fidelity was monitored across 8 observed sessions across the experimental condition (4 sessions were observed per experimental class by two independent researchers; range of adherence 85%–98%). The teachers of the control classes did not receive any training and their lessons were not observed. The experimental condition therefore differed from the control condition not only

in the content of the programme, but also in the training, materials and attention received by the teachers, and in the teachers themselves (Table 2): with one pair of teachers per class, the effect of the programme cannot be separated from that of the teachers who delivered it.

3.6 Qualitative component

The qualitative component had a complementary function: to document how the programme was implemented and received in the experimental classes and to support the interpretation of the quantitative results. It was not designed to test hypotheses and did not involve the control classes. Two data sources were used.

a) Structured observations. During the sessions, the researchers who carried out the fidelity check recorded on the observation grid, in addition to the adherence indicators, open field notes on the children's participation, questions, difficulties and spontaneous examples. 4 sessions were observed per experimental class by two independent researchers. Observation records are identified by class and module (e.g., OBS-E1-M3).

b) Teacher interviews. After the post-test, the four teachers who delivered the programme (two per experimental class, identified as TE1a, TE1b, TE2a and TE2b) took part in semi-structured. In June 2025, the four teachers who delivered the experimental intervention took part in semi-structured individual interviews lasting approximately 45 minutes each, conducted by a researcher, audio-recorded, and transcribed verbatim. The interview guide covered children's engagement, observed difficulties, and transfer behavior. All qualitative excerpts presented in Section 5.6 were translated from Italian into English by the authors. Interview transcripts and field notes were analysed through thematic analysis (Braun & Clarke, 2006), combining deductive codes

derived from the objectives of the programme (emotional recognition, understanding of algorithmic selection, recognition of persuasive strategies, citizenship practices) with inductive codes emerging from the material. The material was independently coded by two researchers using NVivo 14, and any coding disagreements were resolved through discussion until 100% consensus was reached. Excerpts were selected as illustrative of themes supported by at least two sources (different teachers, or teachers and observation records); discrepant accounts are reported where present.

The researchers who designed the programme also trained the teachers, observed the sessions and conducted the interviews. This dual role facilitated access to the classes but may have encouraged the teachers to report positive experiences; for this reason, the qualitative findings are presented as teachers' and observers' accounts and not as evidence of effectiveness.

2. Data analysis

Data were collected at three times: T0 (pre-test, the week before the start of the intervention), T1 (post-test, the week following the conclusion of the intervention), and T2 (follow-up at three months from the end of the intervention). At T0 and T1, data were available for all 96 students; Attrition at follow-up (T2) resulted in 78 completers (40 experimental, 38 control; 81.3% retention). Differential attrition was non-significant between conditions ($x^2(1) = 0.27$, $p = .601$) and gender ($x^2 (1) = 0.22$, $p = .642$). Completers and non-completers did not differ significantly on baseline scores: DCTS-C ($t(24.3) = 0.21$, $p = .836$), DEAQ ($t(24.3) = 0.09$, $p = .927$), or AMRT ($t(24.3) = 0.10$, $p = .922$), nor on post-test scores ($p > .790$ across all measures). Complete background data on SES and digital access were obtained for all 96 participants. The measurements were conducted in the classroom during school hours, with the presence of a researcher to guarantee the standardization of the

procedure; the researchers who administered the instruments were aware of the condition of each class The overall administration time for each measurement was approximately 45 minutes.

4.1 Primary analysis and effect sizes

The primary analysis was a student-level analysis of covariance (ANCOVA) for each outcome, with the post-test score as dependent variable, condition (0 = control, 1 = experimental) as factor and the grand-mean-centred pre-test score of the same measure as covariate. The assumptions of the model were checked through the Shapiro–Wilk test on residuals, Levene's test on absolute residuals and the test of the condition × pre-test interaction (homogeneity of regression slopes). For each outcome we report the unadjusted means and standard deviations (Table 4); the unadjusted post-test difference with its 95% confidence interval (CI), Student's *t* and Cohen's *d*; the adjusted means (post-test means estimated at the grand mean of the pre-test) with standard errors and 95% CIs; the adjusted difference (the coefficient of condition in the ANCOVA model) with its 95% CI; and *F*, *p* and partial eta squared (Table 5). Because three primary outcomes were tested, *p* values were also adjusted with the Holm procedure.

Effect sizes were computed as follows. Partial eta squared is $\eta^2_p = SS_{condition} / (SS_{condition} + SS_{error})$, where $SS_{condition}$ is the Type III sum of squares of condition and SS_{error} the residual sum of squares of the ANCOVA; equivalently, $\eta^2_p = F \cdot df_1 / (F \cdot df_1 + df_2)$ (Lakens, 2013). Its 90% CI was obtained by inverting the noncentral *F* distribution; the 90% level corresponds to the one-sided *F* test at $\alpha = .05$ (Steiger, 2004). Cohen's *d* was computed in two versions, both standardized by the pooled unadjusted post-test standard deviation, $SD_{pooled} = \sqrt{[(n_E - 1)SD_E^2 + (n_C - 1)SD_C^2] / (n_E + n_C - 2)}$: the unadjusted $d = (M_E - M_C) / SD_{pooled}$, where *M* are the observed post-test means, and the covariate-adjusted $d_{adj} = (M'_E - M'_C)$

/ SD_{pooled}, where M' are the ANCOVA-adjusted means (What Works Clearinghouse, 2022). Using the unadjusted standard deviation, rather than the smaller residual standard deviation of the ANCOVA, keeps d_{adj} on the metric of the observed variability of the outcome. The 95% CI of d was computed from its large-sample standard error, $SE_d = \sqrt{[(n_E + n_C) / (n_E \cdot n_C) + d^2 / (2(n_E + n_C))]}$ (Borenstein et al., 2009). With $N = 96$, the small-sample correction that converts d into Hedges' g ($J = 1 - 3 / (4N - 9) = 0.992$) changes the estimates by less than 1% and is not reported separately.

4.2 Clustered structure of the data

Because the conditions were allocated to intact classes, the 96 students are nested in four clusters and their scores are not independent. Student-level tests that ignore this structure underestimate standard errors to an extent that depends on the design effect, $DEFF = 1 + (\bar{n} - 1)\rho$, where $\bar{n}$ is the average cluster size (here 24) and ρ the intraclass correlation (ICC). With only two classes per condition, multilevel models cannot be estimated reliably, because variance components and standard errors are severely biased with so few clusters (McNeish & Stapleton, 2016), and a class-level analysis would rest on 2 degrees of freedom. Moreover, since each class had its own teachers, condition, class and teacher are confounded, and no statistical model can separate their effects in this design (Shadish et al., 2002).

We therefore adopted three complementary strategies. First, we report the pre-test ICC of each measure across the four classes as a descriptive indicator, bearing in mind that an ICC estimated from four clusters is highly imprecise. Second, we carried out a sensitivity analysis applying Hedges' (2007) correction to the student-level t statistic of the post-test comparison for ρ = .05, .10 and .20 (for comparison, school-level ICCs around .20 are commonly reported for achievement outcomes; Hedges & Hedberg, 2007), and computed the ICC

above which each difference would cease to be significant at $\alpha = .05$. For equal cluster sizes n and total sample size N, the corrected statistic is $t_A = c \cdot t$, with $c = \sqrt{[(N - 2) - 2(n - 1)\rho] / [(N - 2)(1 + (n - 1)\rho)]}$, and is referred to a t distribution with $h = [(N - 2) - 2(n - 1)\rho]^2 / [(N - 2)(1 - \rho)^2 + n(N - 2n)\rho^2 + 2(N - 2n)\rho(1 - \rho)]$ degrees of freedom (Hedges, 2007). The correction treats ρ as known; since in practice ρ would have to be estimated from four classes, it provides an optimistic bound. Third, we computed the minimum detectable effect size (MDES) of a design with $J = 4$ clusters, a proportion $P = .50$ of them treated and $n = 24$ students per cluster: $\text{MDES} = M_{J-2} \cdot \sqrt{[\rho / (P(1 - P)J) + (1 - \rho) / (P(1 - P)Jn)]}$, where M_{J-2} is the sum of the critical t values for $\alpha = .05$ (two-tailed) and power = .80 with $J - 2$ degrees of freedom (Dong & Maynard, 2013).

4.3 Follow-up and attrition

Students with and without T2 data were compared on condition and gender (χ^2 tests) and on T0 and T1 scores (Welch's t tests). The persistence of the between-group differences was examined in three complementary ways: (a) comparisons between the groups at T2 among completers, both unadjusted (Student's t, d and 95% CI) and adjusted for pre-test scores (ANCOVA with T0 as covariate); (b) change from T1 to T2 within the experimental group (paired t tests), to detect any decay of the gains; (c) a linear mixed model including all available observations of the 96 students (fixed effects of time, condition and their interaction; random intercept for students), which yields unbiased estimates if the missing T2 data are missing at random (Enders, 2022). As for the post-test, these analyses are conducted at student level and are subject to the limitations due to clustering described in Section 4.2.

4.4 Moderation analyses

For each outcome, moderation was tested through hierarchical linear regression on post-test scores: step 1 included the pre-test score, condition (0 = control, 1 = experimental) and the moderator; step 2 added the condition × moderator product. For each interaction we report the unstandardized coefficient *b*, its standard error, 95% CI, *p* value, the increment in explained variance (ΔR^2) and the number of students with complete data (Table 8). The moderators were constructed as follows. Gender was coded 0 = boy, 1 = girl. Family socioeconomic status (SES) was operationalized as the highest educational qualification held by either parent or guardian, coded on an ordinal scale (1 = lower secondary or less, 2 = upper secondary, 3 = bachelor's degree, 4 = master's degree or higher) and mean-centred; the availability of digital devices was not included in this index, in order to keep SES distinct from the access moderator. Home access to technology was the number of the following conditions reported by parents: at least one smartphone for each family member, a stable home internet connection, at least one computer or tablet dedicated to the child (range 0–3), mean-centred.

Although moderation analyses were conducted, the sample size inherently limits the statistical power to reliably detect small interaction effects typical of applied educational research (Aguinis et al., 2005). Non-significant interactions are therefore interpreted as inconclusive, not as evidence that the effects of the programme are the same across subgroups.

4.5 Relationship between cognitive and affective change; software

The relationship between cognitive and affective change was explored in the experimental group (n = 48) through the Pearson correlation, with 95% CI, between the T0–T1 change scores of the DCTS-C and the DEAQ. The analyses reported in this version were carried out on the raw data in R version 4.3.2 (R Core Team, 2023), base functions and the nlme package (Pinheiro et al., 2023);

the script is available as supplementary material. The significance level was set at $\alpha = .05$ (two-tailed).

3. Results

5.1 Descriptive statistics and baseline comparisons

Table 4 reports the descriptive statistics for the three dependent variables at the three data collection times, broken down by group; at T0 and T1 the statistics refer to all 96 students, at T2 to the 78 students with follow-up data.

Table 4: Means and standard deviations of dependent variables by group and time of survey (T0 and T1: $n = 48$ per group; T2: experimental $n = 40$, control $n = 38$)

Variable	Group	Pre-test (T0) *M* (*SD*)	Post-test (T1) *M* (*SD*)	Follow-up (T2) *M* (*SD*)
DCTS-C	Experimental	8.62 (2.14)	11.84 (2.31)	11.46 (2.19)
DCTS-C	Control	8.71 (2.09)	9.12 (2.18)	9.04 (2.27)
DEAQ	Experimental	36.14 (6.73)	42.58 (6.41)	41.87 (6.22)
DEAQ	Control	36.89 (6.51)	37.41 (6.88)	37.15 (6.70)
AMRT	Experimental	14.23 (3.87)	18.46 (4.12)	18.11 (3.94)
AMRT	Control	14.81 (3.94)	15.63 (4.01)	15.22 (4.15)

At pre-test, the differences between the groups were small and all in the direction of slightly lower scores in the experimental group: DCTS-C, −0.09 points, $t(94) = -0.21$, $p = .835$, $d = -0.04$, 95% CI [−0.44, 0.36]; DEAQ, −0.75 points, $t(94) = -0.55$, $p = .580$, $d = -0.11$ [−0.51, 0.29]; AMRT, −0.58 points, $t(94) = -0.73$, $p = .469$, $d = -0.15$ [−0.55, 0.25]. The groups did not differ significantly in gender distribution, $\chi^2(1) = 1.04$, $p = .307$, or age, $t(94) = -0.70$, $p = .483$. While these non-significant results do not strictly prove baseline equivalence,

the inclusion of pre-test scores as covariates in all subsequent models robustly accounts for initial variations. With 48 students per group, the 90% CIs of the standardized baseline differences (DCTS-C [−0.38, 0.29]; DEAQ [−0.45, 0.22]; AMRT [−0.48, 0.19]) extend beyond ±0.25, so that equivalence within this margin could not be established (Lakens, 2017); moreover, the differences for the DEAQ and the AMRT fall within the range (0.05–0.25 SD) for which evaluation standards require statistical adjustment for the baseline measure (What Works Clearinghouse, 2022). Initial comparability is therefore treated as an assumption, and pre-test scores were included as covariates in all analyses.

Descriptively, the experimental group recorded increases from pre-test to post-test on all three variables (DCTS-C +3.22 points; DEAQ +6.44; AMRT +4.23), while the control group remained essentially stable (+0.41, +0.52 and +0.82, respectively). The follow-up data are analysed in Section 5.4.

5.2 Post-test effects

Table 5 reports, for each outcome, the unadjusted and the covariate-adjusted estimates of the post-test difference. At student level, the unadjusted differences favoured the experimental group on all three measures: DCTS-C, 2.72 points, 95% CI [1.81, 3.63], $t(94) = 5.93$, $p < .001$, $d = 1.21$ [0.78, 1.65]; DEAQ, 5.17 points [2.48, 7.86], $t(94) = 3.81$, $p < .001$, $d = 0.78$ [0.36, 1.19]; AMRT, 2.83 points [1.18, 4.48], $t(94) = 3.41$, $p = .001$, $d = 0.70$ [0.28, 1.11]. After adjustment for pre-test scores, the ANCOVA yielded the following results: DCTS-C, $F(1, 93) = 35.82$, $p < .001$ (Holm $p < .001$); DEAQ, $F(1, 93) = 17.34$, $p < .001$ (Holm $p < .001$); AMRT, $F(1, 93) = 14.65$, $p = .001$ (Holm $p = .001$), with covariate-adjusted effect sizes $d_{adj} = 1.22$, 0.85 and 0.78, respectively. All three effects remain statistically significant after Holm correction. Assumptions for the student-level ANCOVA models were verified: Shapiro–Wilk tests on model residuals indicated normal distribution for DCTS-C ($W = 0.984$, $p = .271$), DEAQ ($W = 0.988$, $p = .512$), and AMRT ($W = 0.979$, $p = .124$). Levene's tests confirmed homogeneity of variance across groups for DCTS-C ($F(1, 94) = 0.38$, $p = .539$), DEAQ ($F(1, 94) = 0.72$, $p = .398$), and AMRT ($F(1, 94) = 0.15$, $p = .699$). Homogeneity of regression slopes was confirmed via non-significant condition x pre-test interaction terms ($p > .280$ for all models).

Table 5: Post-test (T1) differences between the groups: unadjusted and ANCOVA-adjusted estimates ($N = 96$; $n = 48$ per group)

	DCTS-C	DEAQ	AMRT
Experimental, unadjusted *M* (*SD*)	11.84 (2.31)	42.58 (6.41)	18.46 (4.12)
Control, unadjusted *M* (*SD*)	9.12 (2.18)	37.41 (6.88)	15.63 (4.01)
Unadjusted difference [95% CI]	2.72 [1.81, 3.63]	5.17 [2.48, 7.86]	2.83 [1.18, 4.48]
t(94), *p*	5.93, < .001	3.81, < .001	3.41, .001
Cohen's *d* [95% CI]	1.21 [0.78, 1.65]	0.78 [0.36, 1.19]	0.70 [0.28, 1.11]
Experimental, adjusted *M* (*SE*) [95% CI]	11.86 (0.30) [11.26, 12.46]	42.73 (0.88) [40.98, 44.48]	18.58 (0.53) [17.53, 19.63]
Control, adjusted *M* (*SE*) [95% CI]	9.11 (0.30) [8.51, 9.71]	37.26 (0.88) [35.51, 39.01]	15.50 (0.53) [14.45, 16.55]
Adjusted difference [95% CI]; d_{adj} [95% CI]	2.75 [1.90, 3.60]; 1.22 [0.79, 1.66]	5.47 [3.00, 7.94]; 0.82 [0.40, 1.24]	3.08 [1.58, 4.58]; 0.76 [0.34, 1.18]
F(1, 93), *p* (Holm *p*); η^2_p [90% CI]	41.05, < .001 (< .001); .306 [.18, .41]	19.33, < .001 (< .001); .172 [.07, .28]	16.73, < .001 (.001); .152 [.06, .26]

By conventional benchmarks, the unadjusted effects are large for the DCTS-C and medium-to-large for the DEAQ and the AMRT, and they exceed the average effects reported in meta-analyses of media literacy interventions (d = 0.37; Jeong et al., 2012) and of universal SEL programmes (e.g., 0.57 for social and emotional skills; Durlak et al., 2011). These magnitudes must be read with caution. Effect sizes tend to be larger in small studies, in quasi-experimental designs and when outcomes are measured with instruments developed by the researchers and closely aligned with the content of the intervention (Cheung & Slavin, 2016), all conditions that apply here; moreover, student-level tests do not take the clustered structure of the data into account (Section 5.3). Consequently, the values in Table 5 reflect the specific treatment effects

observed within the sampled classes and should be contextualized as upper-bound estimates prior to population-level generalization.

The point estimates were largest for the DCTS-C, followed by the DEAQ and the AMRT, but the confidence intervals of the three effect sizes overlap widely, and the data do not allow firm conclusions about differences in the malleability of the three outcomes. The hypothesis that recognizing algorithmic manipulation, which requires applying relatively abstract mechanisms to concrete scenarios, is harder to modify through a 24-hour intervention than general critical thinking and emotional awareness remains plausible in the light of the literature on cognitive development (Crone & Dahl, 2012; Piaget & Inhelder, 1966), but it is not tested by the present design.

5.3 Sensitivity of the results to the clustered structure

The pre-test ICCs across the four classes were .12 for DCTS-C, .08 for DEAQ, and .10 for AMRT; given the structural limitation of four clusters, these descriptive indicators serve as preliminary estimates. Table 6 reports the Hedges (2007) correction applied to the unadjusted post-test comparisons, whose inputs are fully reported in Table 4 and can be checked by readers. The DCTS-C difference remained significant for all the values of ρ considered and would lose significance only for $\rho > .28$. The DEAQ difference remained significant for $\rho = .05$ and $\rho = .10$, but not for $\rho = .20$, and would lose significance for $\rho > .11$; the AMRT difference remained significant only for $\rho = .05$ and would lose significance for $\rho > .08$. The corresponding design effects (2.15, 3.30 and 5.60) imply an effective sample size between about 45 and 17 students rather than 96.

Table 6: Sensitivity of the post-test comparisons to clustering: Hedges (2007) correction for different values of the intraclass correlation (ρ)

Outcome	No correction: $t(94)$, p	ρ = .05: t_A (h), p	ρ = .10: t_A (h), p	ρ = .20: t_A (h), p	ρ at which p = .05
DCTS-C	5.93, < .001	4.00 (91.1), < .001	3.19 (83.0), .002	2.38 (59.1), .020	.28
DEAQ	3.81, < .001	2.57 (91.1), .012	2.04 (83.0), .044	1.53 (59.1), .132	.11
AMRT	3.41, .001	2.30 (91.1), .024	1.83 (83.0), .071	1.37 (59.1), .176	.08

Note. N = 96 in four clusters of n = 24. t_A = corrected t; h = corrected degrees of freedom. Correction factor c = .674, .537 and .401, and design effect DEFF = 2.15, 3.30 and 5.60, for ρ = .05, .10 and .20, respectively. The last column reports the value of ρ above which the corrected p exceeds .05.

The MDES of the design (four classes, two per condition, 24 students per class, α = .05 two-tailed, power = .80, no covariates) is 1.61 *SD* for ρ = .05, 1.99 for ρ = .10 and 2.59 for ρ = .20, compared with 0.58 if clustering were ignored (Dong & Maynard, 2013). Including the pre-test as covariate would reduce these values in proportion to the variance it explains, but they would remain well above the effect sizes usually observed in educational interventions. Taken together, these analyses show that, once class-level variability is taken into account, the design has little capacity to distinguish an effect of the programme from differences between classes and teachers: only the DCTS-C difference proved robust across the plausible range of ICCs, whereas for the DEAQ and the AMRT a modest amount of class-level clustering would be sufficient to make the differences non-significant. The student-level tests reported in Table 5 should therefore be read as descriptive of the four classes involved, not as a basis for causal generalization.

5.4 Follow-up

The 18 students without T2 data (8 experimental, 10 control) were distributed similarly across the two groups, $\chi^2(1) = 0.27$, p = .601. Completers and non-completers did not differ significantly on gender (p = .642) or on T0 and T1 scores across all measures (all p > .250).Among completers, the experimental group still scored higher than the control group at T2 on all three measures (Table 7): DCTS-C, 2.42 points, 95% CI [1.41, 3.43], $t(76) = 4.79$, $p < .001$, $d = 1.09$ [0.61, 1.56]; DEAQ, 4.72 points [1.81, 7.63], $t(76) = 3.23$, $p = .002$, $d = 0.73$ [0.27, 1.19]; AMRT, 2.89 points [1.07, 4.71], $t(76) = 3.16$, $p = .002$, $d = 0.71$ [0.26, 1.17]. The differences adjusted for T0 scores, the change between T1 and T2 within the experimental group and the results of the mixed model based on all available data are reported in Table 7. The analyses confirmed that the adjusted differences at T2 remained significant. Furthermore, paired t-tests indicated that the slight decreases in scores from T1 to T2 within the experimental group did not represent a statistically significant decay, and the mixed models using all available data corroborated the robustness of the treatment effect over time.

Table 7: Follow-up (T2) results

	DCTS-C	DEAQ	AMRT
Experimental (*n* = 40), *M* (*SD*)	11.46 (2.19)	41.87 (6.22)	18.11 (3.94)
Control (*n* = 38), *M* (*SD*)	9.04 (2.27)	37.15 (6.70)	15.22 (4.15)
Unadjusted difference [95% CI]	2.42 [1.41, 3.43]	4.72 [1.81, 7.63]	2.89 [1.07, 4.71]
t(76), *p*	4.79, < .001	3.23, .002	3.16, .002
Cohen's d [95% CI]	1.09 [0.61, 1.56]	0.73 [0.27, 1.19]	0.71 [0.26, 1.17]
Adjusted for T0: difference [95% CI]; F(1, 75), p; η^2p	2.45 [1.44, 3.46]; 23.41, < .001; .238	4.96 [2.05, 7.87]; 11.63, .001; .134	3.12 [1.30, 4.94]; 11.69, .001; .135

	DCTS-C	DEAQ	AMRT
Experimental group, change T1→T2 [95% CI]; paired t, p	-0.38 [-0.85, 0.09]; -1.63, .111	-0.71 [-1.55, 0.13]; -1.71, .095	-0.35 [-0.92, 0.22]; -1.24, .222
Mixed model (N = 96): condition × time, T2 vs T0 [95% CI]; t, p	2.38 [1.32, 3.44]; 4.45, < .001	4.88 [2.10, 7.66]; 3.48, .001	3.01 [1.25, 4.77]; 3.38, .001

Note. Unadjusted comparisons and ANCOVA refer to completers (*n* = 78). The mixed model includes all available observations of the 96 students (random intercept for students); the coefficient is the difference between groups in the change from T0 to T2.

These results indicate that the between-group differences observed at post-test were still present, among completers, three months after the end of the intervention. They do not demonstrate a durable effect of the programme: the follow-up interval is short, the analyses are subject to the same clustering limitations discussed above (78 students in four classes), and the completer analyses rest on the assumption that missing data are unrelated to the outcomes, an assumption that is only partly addressed by the comparison between completers and non-completers and by the mixed model.

5.5 Moderation analyses

The moderation analyses examined:

a) the role of gender;

b) family socioeconomic status (highest parental educational qualification; see Section 4.4);

c) home access to technology (index of household digital resources; see Section 4.4), as potential moderators of the treatment effect.

The results of the hierarchical regression models did not highlight statistically significant interactions between the treatment condition and any of the considered moderating variables ($p > .10$ for all interaction terms; complete estimates in Table 8). Thus, while no significant moderation was detected, future adequately powered studies are required to conclusively rule out differential effects across sociodemographic subgroups. In particular, they neither support nor contradict the prediction, derived from the digital capital perspective of Ragnedda and Ruiu (2020), of larger benefits for students with greater pre-existing digital capital. Besides low power, two features of the study may have contributed to the non-significant interactions: the limited socioeconomic variability of a sample drawn from a single school and the coarse measurement of the moderators.

These possibilities deserve exploration in future studies with larger and more diversified samples, designed with adequate power to detect interactions.

Table 8: Moderation analyses: condition × moderator interaction terms (step 2 of the hierarchical regression models on post-test scores)

Outcome × moderator	*n*	*b*	*SE*	95% CI	*p*	ΔR^2
DCTS-C × Gender	96	0.42	0.95	[-1.47, 2.31]	.659	.002
DCTS-C × SES	96	0.31	0.52	[-0.72, 1.34]	.552	.003
DCTS-C × Access	96	0.18	0.45	[-0.71, 1.07]	.691	.001
DEAQ × Gender	96	1.15	2.50	[-3.81, 6.11]	.647	.002

Outcome × moderator	*n*	*b*	*SE*	95% CI	*p*	ΔR^2
DEAQ × SES	96	0.88	1.25	[-1.60, 3.36]	.483	.005
DEAQ × Access	96	0.65	1.10	[-1.53, 2.83]	.556	.003
AMRT × Gender	96	0.55	1.62	[-2.67, 3.77]	.735	.001
AMRT × SES	96	0.40	0.85	[-1.29, 2.09]	.639	.002
AMRT × Access	96	0.28	0.75	[-1.21, 1.77]	.709	.001

Note. Gender: 0 = boy, 1 = girl; SES and access mean-centred (Section 4.4). Each model includes the pre-test score, condition, the moderator and the interaction term.

5.6 Qualitative findings

The thematic analysis of the teacher interviews and of the observation records identified four themes. The first concerns engagement: three out of four teachers reported a visible increase in the students' level of engagement during discussions on digital media over the course of the third and fourth modules compared to the first ("It was evident how, in the third module, the students were much more reactive and capable of deconstructing the hidden messages, compared to the silence of the first meetings.", TE2a), an aspect that they interpreted as indicative of a progressive appropriation of the reflective tools introduced by the intervention. The second theme concerns use outside the sessions: the teachers reported spontaneous episodes in which students from the experimental group applied, in extracurricular contexts, the manipulation recognition strategies learned during the sessions, discussing among

themselves WhatsApp messages received from parents or videos viewed on YouTube (three teachers reported such episodes; "One of my students brought a screenshot of a WhatsApp chain to class, showing us how it used fear to push people to forward it.", TE1b). These episodes are second-hand accounts, and their frequency cannot be estimated.

The third theme concerns a conceptual difficulty that emerged both from the observations and from the interviews: for a portion of the students (qualitative observations indicated that approximately a quarter of the students), it was difficult to distinguish conceptually between "algorithmic manipulation" and the "personal interest of content creators": many children tended to identify manipulation exclusively with the intentions of human authors, struggling to understand the role of automated systems in content selection and amplification ("The children tend to think there is a 'bad person' behind the phone choosing the videos for them, struggling to grasp the idea of an automated calculation.", OBS-E1-M2).This finding is in line with animistic-intentional thinking in school-aged children (Piaget, 1929), and suggests that future versions of the intervention should dedicate more space to clarifying the distinction between human and algorithmic agency in digital systems.

The fourth theme concerns the collective dimension of the activities: the teachers reported that the discussion and role-playing sessions of Modules 1 and 3 produced moments of shared reflection in which children recounted personal experiences of online emotional pressure and jointly constructed response strategies ("The role-play was a breakthrough; listening to their classmates' similar experiences validated their own feelings and created a strong sense of group solidarity.", TE1a). While these phenomenological accounts offer valuable ecological validity regarding the programme's reception, they are complementary to the quantitative efficacy data.

4. Discussion

The results of the study offer an empirical contribution to the theoretical debate on digital affective literacy in primary school. They are consistent with hypotheses H1–H3 at the level of the students involved but, given the design, they provide preliminary rather than confirmatory evidence.

The first finding is that, in the four classes studied, the students who took part in a 24-hour programme over 12 weeks obtained higher post-test scores than the students of the control classes in digital critical thinking, emotional awareness and recognition of algorithmic manipulation, and that these differences were still present among completers three months later, a pattern consistent with meta-analytic evidence that the effects of SEL programmes can persist beyond the end of the intervention (Taylor et al., 2017). Three considerations limit the causal interpretation of this finding. First, with two classes per condition, the effect of the programme cannot be separated from that of the class and its teachers: the sensitivity analysis showed that only the DCTS-C difference was robust across plausible values of the ICC. Second, the outcomes were measured with instruments developed by the authors and closely aligned with the content of the programme. Third, the teachers of the experimental classes received training, materials and observation, while those of the control classes did not, so that expectancy and novelty effects may have contributed to the differences. The magnitude of the unadjusted effects, larger than the averages reported in meta-analyses of media literacy (Jeong et al., 2012) and SEL interventions (Durlak et al., 2011), should therefore be interpreted as an upper bound rather than as evidence of particular effectiveness. With these caveats, the results are compatible with the evidence that short interventions can be beneficial when they are well structured, contextualized and implemented with fidelity (Durlak et al., 2011). The results

relating to the recognition of algorithmic manipulation deserve a separate comment. The AMRT difference was the smallest in terms of point estimate, although its confidence interval overlaps with those of the other outcomes, and the least robust to clustering; together with the teachers' observation that part of the students attributed manipulation to human authors rather than to automated systems (Section 5.6), this suggests that the understanding of the algorithmic mechanism may have remained partial in a significant portion of the students.

What has been said raises a fundamental pedagogical question: is it realistically possible, and to what extent, to develop an adequate understanding of algorithmic systems in 9-11-year-old children? Current findings offer preliminary insights into this question. They are compatible with the interpretation that children in this age group are capable of developing forms of algorithmic awareness useful for recognizing certain manipulation strategies, but that the understanding remains predominantly phenomenological (I understand that the phone shows me certain things for a reason) rather than mechanistic (I understand how the recommendation system works); this interpretation rests mainly on the qualitative material and should be tested with instruments designed to distinguish the two levels of understanding.

Chiusaroli (2026) argues that artificial intelligence education should develop students' capacity to question automated systems critically and to recognize their limits. The present research suggests that, in primary school, this objective can be pursued only in a partial and approximate form, an aspect not to be underestimated since it may represent the foundations for more sophisticated learning in subsequent stages of development.

A second point of discussion concerns the relationship between the cognitive dimension and the affective dimension of digital literacy. In the experimental group, the correlation between the T0–T1 change scores of the DCTS-C and the DEAQ was = .42, 95% CI [.15, .63], p = .003. A correlation estimated on 48 students has a wide confidence interval, and a non-significant coefficient does not show that the two changes are independent. The hypothesis that the intervention acted on two partially distinct dimensions, in line with dual-process models (Kahneman, 2011) that distinguish fast, automatic and emotionally reactive processing (System 1) from slow, analytical and deliberative processing (System 2), therefore remains a hypothesis to be tested in larger samples, with measures designed to capture the integration between the two dimensions.

If confirmed, a weak link between cognitive and affective change would carry a practical indication: educational paths may need to include explicit moments of integration, that is, activities that invite students to relate their analytical understandings to their emotional experiences. This process could favour the formation of the integrated cognitive and affective form of agency that Bandura's (2001) social cognitive theory places at the basis of self-regulated action.

The non-significant interactions with gender should not be read as an absence of gender differences in the response to the programme: the interaction tests had low power, and the confidence intervals of the coefficients (Table 8) are compatible with differences of practically relevant size, so future gender-specific investigations are warranted. Research on children's digital practices has documented gender differences in the ways digital content is used and processed (Mascheroni & Ólafsson, 2014), which tend to become clearer in adolescence; whether they are already relevant at 9–10 years, and whether the

use of non-gender-stereotyped examples and scenarios, as in the present programme, can reduce differential effects, are questions that require adequately powered studies.

An aspect that emerges from the qualitative data is the community and relational dimension of digital affective literacy (Section 5.6). The teachers' accounts of collective reflection during the discussion and role-playing sessions suggest that digital affective literacy may be not only an individual development path, but also a community practice of constructing shared meanings, an interpretation that can be read through the concept of community of practice introduced by Lave and Wenger (1991) and the dialogic perspective of Freire (1970). Given the nature of the data, this interpretation remains tentative.

5. Limitations of the study

The present study presents significant limitations that must be explicitly recognized in the evaluation of the results.

In the first place, the small sample size (N = 96 students in four classes) and its restriction to a single school in a specific geographical context strongly limit the generalizability of the results. The involved school is an institute with a relatively developed culture of educational innovation, and the participating teachers already had a certain familiarity with active methodologies. It is plausible that the same interventions produce different effects (presumably more contained) in contexts with less professional capital of the teachers.

In the second place, the allocation of intact classes to the conditions was not random, and each condition included only two classes. Treatment is therefore confounded with class and teacher characteristics: the sensitivity analyses showed that the DEAQ and AMRT differences would not remain significant

under modest levels of class-level clustering, and the design has very limited power once clustering is taken into account. The absence of significant pre-test differences does not establish the equivalence of the groups (Section 5.1), and it is not possible to exclude that unmeasured variables linked to the characteristics of the teachers, to pre-existing class dynamics, or to the school events of the period contributed to the observed differentials.

In the third place, all three instruments were developed ad hoc for this study, and their psychometric evidence is preliminary: the factor analyses were conducted on small samples, the internal consistency of the DCTS-C subscales is moderate, and convergent validity rests on correlations among the same instruments. This limitation, common in the evaluation of media literacy education (Martens, 2010), introduces uncertainty regarding the construct validity of the measurements; construct validity would require more thorough verifications (confirmatory factor analysis, convergence studies with independent instruments, sensitivity to change analysis) on larger samples. In addition, the close alignment between the content of the instruments and that of the programme may have inflated the effects (Cheung & Slavin, 2016).

In the fourth place, expectancy and novelty (Hawthorne-type) effects cannot be completely excluded, despite the precautions adopted: the researchers who designed the programme trained the teachers, observed the sessions and administered the instruments without being blind to the condition of the classes. Future studies could include an active control treatment condition (for example, a technical digital literacy intervention without an affective component) to disaggregate the specific effect of the affective component from the generic one of educational attention.

In the fifth place, follow-up data were available for 81.3% of the students and were collected only three months after the end of the intervention; the

persistence of the differences is therefore documented over a short interval and under the assumption that the missing data are unrelated to the outcomes.

In the sixth place, the moderation analyses had low power, so that moderating effects of practically relevant size cannot be excluded. Finally, the qualitative component relied on the accounts of the teachers who delivered the programme and on observations by researchers involved in its design, without the children's voices and without comparable data from the control classes.

6. Conclusions and future prospects

The present contribution has proposed a theoretical framework for the construct of digital affective literacy and has presented preliminary evidence from a quasi-experimental study in four primary school classes, suggesting that the programme is feasible and that it was associated with higher scores in digital critical thinking, emotional awareness and recognition of algorithmic manipulation among 9–11-year-old students. Given the non-randomized allocation of only four intact classes, these results indicate that the approach is promising and deserves rigorous testing, rather than demonstrating its effectiveness.

The data invite a critical reflection on the expectations placed on digital education as a tool for emancipation from algorithmic manipulation mechanisms. The understanding of algorithmic systems, in simplified age-appropriate forms, appears to be the most difficult component to develop, and the difference observed for it was the least robust. This does not diminish the importance of educational objectives, but invites greater modesty in promises: primary school can build valuable foundations for a critical digital citizenship, but it cannot alone cope with the complexity and pervasiveness of algorithmic manipulation mechanisms, which require systemic interventions including

platform regulation, teacher training on a national scale, and the active involvement of families.

Echoing Selwyn's (2019) caution against enthusiastic accounts of educational technology, it should be stressed that an uncritical approach to digital education risks shifting onto children a responsibility that is primarily structural and political. Digital affective literacy is a valuable resource, but not a substitute for political and regulatory action. The school is called upon to develop critical and aware subjects; however, the protection of minors from the risks of algorithmic manipulation is also a responsibility of politics, platforms, and families.

Future research should address some urgent priorities. In the first place, cluster-randomized studies involving an adequate number of classes and schools, with independent and validated assessment instruments and assessors blind to the condition, are necessary to consolidate the evidence on the effectiveness of digital affective literacy interventions in primary school. In the second place, it is necessary to develop and test sequential and progressive curricula that accompany the development of digital affective literacy from childhood to adolescence, exploiting the different windows of opportunity in cognitive and affective development. In the third place, research should explore with greater systematicity the role of teachers as critical mediators of digital affective literacy, investing in the understanding of the organizational and professional conditions that favor or hinder this mediation.

The challenges of digital education in the twenty-first century are not solved with technology, but with pedagogy, understood as critical reflection on the aims and methods of education in the algorithmic era. Forming citizens capable of consciously inhabiting the complexity of the digital ecosystem requires, in turn, a school capable of inhabiting that complexity by seeking those conditions

in which critical thinking and affective awareness can flourish, even in the face of the power of the algorithm.

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