Analysis Plan: The health, human capital, and economic impacts of school-based mental health intervention among adolescents in Colombia

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1. Introduction

This project aims to estimate the short- and long-run impacts of a scalable, school-based mental health intervention targeting adolescents experiencing mild psychological distress. The intervention involves a low-intensity, transdiagnostic, problem-solving therapy delivered through five one-on-one sessions by trained non-specialist counselors. During sessions, students are guided to identify problems, explore solutions, and develop implementation strategies. Building on the success of the PRIDE program in India (Michelson et al., 2020; Malik et al., 2021), the research team adapted the model to the Colombian context, creating "*Clara-Mente*".

This analysis plan describes a large-scale RCT involving up to 275 public secondary schools across Medellín and Barranquilla, Colombia. The study will assess impacts on (i) socioemotional skills and mental health, (ii) school attendance and academic achievement, and (iii) long-term economic outcomes, including labor market earnings, using administrative data. The RCT incorporates a two-stage randomization design, with classroom- and student-level random assignment, to estimate the direct effects of the intervention on treated students, as well as spillover effects on untreated peers.

2. Research Questions

RQ1 (Primary): What is the causal impact of the *Clara-Mente* intervention on adolescents' socioemotional skills, their problem-solving skills, and mental health?

RQ2 (Primary): What is the causal impact of *Clara-Mente* on educational outcomes?

RQ3 (Primary): What is the impact of *Clara-Mente* on long-term economic outcomes?

RQ4 (Primary): Are there spillover effects of *Clara-Mente* on the primary outcomes of untreated students who share classrooms with treated peers?

RQ5 (Secondary): Are there differential impacts of *Clara-Mente* across subgroups?

3. Sample and Screening

The RCT is conducted in public schools in the urban areas of Medellín and Barranquilla, Colombia's second- and fourth-largest cities, targeting students in grades 9, 10, and 11. As of the posting of this preregistration in June 2026, we have sufficient funding to cover 64 schools where we will screen students who are present at school during data collection and have both parental consent and student assent to provide informed consent (approximately 11,142 students) using the Strengths and Difficulties Questionnaire (SDQ; Goodman, 1997). We have pending grant applications to extend the sample to 275 schools with approximately 67,000 students. We will update this pre-registration if and when additional funding sources are secured.

Eligibility criteria follow the definition used in the pilot and in PRIDE (Michelson et al., 2020; Malik et al., 2021), focusing on students experiencing mild to moderate psychological distress. Students are eligible if they meet the following criteria:

> (i) an SDQ Total Score ≥ 19 for male students, and ≥ 20 for female students, corresponding to the "abnormal" band in the traditional three-band categorization of the SDQ, or
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> (ii) an SDQ Impact Score ≥ 2 and a reported persistence of symptoms for more than one month on the SDQ chronicity index.

We exclude any students who report having received psychological therapy within the last year and students with a diagnosis of autism spectrum disorder or a cognitive disability. Based on lessons from a pilot RCT, approximately 30% of screened students are expected to be eligible, resulting in approximately 3,343 eligible students in about 455 classrooms.

4. Randomization

The RCT employs a two-stage randomization design, designed to identify both the direct treatment effect of *Clara-Mente* and potential spillover effects on untreated peers. This structure reflects a deliberate trade-off following Baird et al. (2018): our primary objective is to estimate the within-classroom treatment effect, which equals the ITT in the absence of spillovers, with maximum precision, while still being able to detect whether there are spillover effects. To balance these goals, we randomize at two levels. In the first stage, classrooms are assigned to one of two conditions: pure controls or treated; in the second stage, eligible students within treated classrooms are individually randomized to treatment or control. The treated share concentrates statistical power on the direct effect, while the pure-control classrooms provide the clean counterfactual needed to identify spillovers on non-treated students who share a

classroom with treated peers.

1. Classroom-Level Randomization: Classrooms are randomly assigned to either a treatment arm or a pure control arm, stratified at the school level. Approximately 85% of classrooms are assigned to the treatment arm and 15% to the pure control arm, yielding approximately 391 treatment classrooms and 64 pure control classrooms across 64 schools. Schools with four or fewer classrooms are assigned entirely to the treatment arm.

2. Student-Level Randomization within Treatment Classrooms: In the second stage, within each classroom assigned to the treatment arm, eligible students are individually randomized: 50% are assigned to receive the *Clara-Mente* intervention (treatment group) and 50% are assigned to the within-classroom control group. This randomization is stratified at the classroom level. No individual randomization occurs in pure control classrooms; all eligible students in those classrooms serve as the pure control group.

This two-stage design produces three groups of eligible students:

1. Treated students: eligible students in treatment classrooms randomized to receive *Clara-Mente* (~1,129 students)
2. Within-classroom control students: eligible students in treatment classrooms randomized to control (~1,129 students), who are exposed to treated peers
3. Pure control students: eligible students in pure control classrooms, where no peers receive the intervention (~365 students, implied by the classroom allocation)

Trained mentors are randomly assigned to schools and students using stratification criteria based on school and classroom. Each counselor treats up to 12 students per cycle (approximately three weeks).

5. Outcomes

The primary outcomes are assessed using validated psychometric instruments administered at follow-up (survey-based) and administrative records for medium- and long-term educational and economic outcomes. The core survey instrument is administered three times: (i) approximately one to two weeks before the beginning of the intervention (baseline survey), (ii) three to four weeks after the completion of the intervention (first follow-up survey), and (iii) approximately 9 to 12 months after the completion of the intervention (second follow-up survey). Enumerators use smartphones for data entry during school hours. The baseline and follow-up surveys include the following socioemotional and mental health instruments: (1) Strengths and Difficulties Questionnaire (SDQ); (2) Self-Reporting Questionnaire (SRQ); and (3) Social Problem-Solving Inventory (SPSI). The baseline survey also includes sociodemographic variables.

RQ1: To measure the impacts on adolescents' socioemotional skills, their problem-solving skills, and mental health, we use the following validated instruments:

(1) Strengths and Difficulties Questionnaire (SDQ) Total Difficulties Score (Goodman, 1997)
(2) Self-Reporting Questionnaire (SRQ) Total score (Beusenberg et al., 1994)
(3) Social Problem-Solving Inventory - Revised (SPSI-R) Total Score (D'Zurilla et al., 2002)

In addition to analyzing the impacts on each of these three scales, we will also create a summary index of those scales, following Anderson (2008).

In exploratory analyses, we will also study subscales of the SDQ, SRQ, and SPSI-R:
- SDQ Emotional difficulties scale
- SDQ Conduct difficulties scale
- SDQ Peer difficulties scale
- SRQ Anxiety symptoms
- SRQ Depression symptoms
- SPSI-R Positive Problem Orientation Scale
- SPSI-R Negative Problem Orientation Scale
- SPSI-R Rational Problem Solving Scale
- SPSI-R Impulsive/Carelessness Scale
- SPSI-R Avoidance Scale

RQ2: To measure impacts on academic achievement, we will use the following administrative data obtained from schools and/or the secretaries of education:
(1) School attendance (subject to data availability)
(2) Grade progression or graduation (versus grade repetition or school dropout) (administrative records)
(3) Saber 11 standardized test score (overall and by subject)
(4) GPA (administrative records)
(5) Tertiary education outcomes (longer-run study).

While we plan to present analyses of impacts on outcomes (1) through (4) in a research paper on short- and medium-run outcomes, outcome (5) will be part of a longer-run evaluation including administrative outcomes specified in RQ3. We will specify the details of these outcomes at a later date (before collecting these data).

RQ3: To measure long-term economic impacts, we will analyze administrative data on the following primary outcomes, including formal employment (PILA administrative records) and earnings (PILA administrative records). We will also analyze other administrative data sources, including crime-related outcomes (administrative records, under exploration) and mental health diagnoses (RIPS health claims records). We will specify the details of these outcomes at a later date (before collecting these data).

6. Empirical Analyses

6.1 Primary Analysis: Intent-To-Treat (ITT) Effect of *Clara-Mente* (RQ1 to RQ3)

The primary analysis (RQ1 to RQ3) will be conducted using an Intent-to-Treat (ITT) framework. The estimating sample consists only of **eligible students** in treatment classrooms. The primary empirical specification for estimating the causal effect of being assigned to the *Clara-Mente* intervention (T_i) on an outcome variable (Y_i) is as follows:

$$Y_{ic} = \beta_0 + \beta_1 T_i + \phi Y_{i,baseline} + X'_i \delta + \mu_c + \varepsilon_i$$

where:

- Y_{ic} is the outcome for student *i* in classroom *c*.
- β_1 is the primary parameter of interest, representing the causal effect of the intervention.
- T_i is the treatment indicator, equal to 1 if student *i* was randomly assigned to the treatment group and 0 if assigned to the control group within a treated classroom.
- $Y_{i,baseline}$ is the baseline value of the outcome variable (if available).
- X'_i is a vector of student-level baseline control variables (including age, gender, mother's education, and city).
- μ_c are classroom fixed effects, and
- ε_i is the error term.

Standard errors are clustered at the classroom level.

6.2 Classroom Spillover Analysis (RQ4)

The two-stage randomization design enables identification of spillover effects of *Clara-Mente* on students who share classrooms with treated peers, both among eligible untreated students and among non-eligible students. The key source of variation is that students in pure control classrooms are never exposed to any treated peers, while students in treatment classrooms share their classroom with approximately 50% of eligible peers who receive the intervention.

<u>Estimating spillover effects in the non-treated student sample:</u>

Our first two specifications estimate spillovers using only the sample of **untreated students** (including both eligible and non-eligible students):

$$Y_{ic} = \gamma_0 + \gamma_1 T_c + \gamma_2 E_i + \gamma_3 E_i T_c + \phi Y_{i,baseline} + X'_i \delta + \mu_s + \varepsilon_i$$

Where:

- Y_{ic} is the outcome for student *i* in classroom *c*.

- T_c is the treatment indicator, equal to 1 if the classroom c was randomly assigned to the treatment group and 0 if assigned to the control group.
- E_i is the eligibility indicator, equal to 1 if student i is eligible and to 0 if not.
- $Y_{i,baseline}$ is the baseline value of the outcome variable (if available).
- X'_i is a vector of student-level baseline control variables (including age, gender, socioeconomic stratum, and city).
- μ_s are school fixed effects, reflecting Stage 1 stratification.
- ε_i is the error term. Standard errors are clustered at the classroom level.

The key parameters of interest are γ_1 and γ_3:

- γ_1 is the spillover effect on *non-eligible* students, the difference between non-eligible students in treatment classrooms and non-eligible students in pure-control classrooms.
- γ_3 is the *differential* spillover for eligible relative to non-eligible untreated students, how much larger or smaller the classroom-exposure spillover is for eligible students than for non-eligible students. Spillover on eligible students = $\gamma_1 + \gamma_3$
- We can also construct a population-share weighted estimate of the average spillover effect on the untreated

Estimating spillover effects in the full sample

The second specification estimates spillovers using the full sample of all treated and untreated students (including both eligible and non-eligible students):

$$Y_{ic} = \alpha_0 + \alpha_1 T_i + \alpha_2 T_c + \alpha_3 E_i + \alpha_4 E_i T_c + \phi Y_{i,baseline} + X'_i \delta + \mu_s + \varepsilon_i$$

Where:

- Y_{ic} is the outcome for student *i* in classroom c.
- T_i is the treatment indicator, equal to 1 if student *i* was randomly assigned to the treatment group and 0 if assigned to the control group within a treated classroom.
- T_c is the treatment indicator, equal to 1 if classroom c was randomly assigned to the treatment group and 0 if assigned to the control group.
- E_i is the eligibility indicator, equal to 1 if student i is eligible and 0 otherwise.
- $Y_{i,baseline}$ is the baseline value of the outcome variable.
- X'_i is a vector of student-level baseline control variables (including age, gender, mother's education, and city).
- μ_s are school fixed effects

- ε_i is the error term.

Standard errors are clustered at the classroom level.

This equation estimates the following key coefficients of interest: α_1, α_2, and α_4:

- α_1 is the direct effect of receiving *Clara-Mente*: the difference between treated eligible students and control eligible students in treatment classrooms. This is the same estimand as β_1 in the first specification (estimated without classroom fixed effects).
- α_2 is the spillover effect on *non-eligible* students, the difference between non-eligible students in treatment classrooms and non-eligible students in pure-control classrooms.
- α_3 is the eligibility "level" difference in the absence of any classroom exposure, the gap in outcomes between eligible and non-eligible students within pure-control classrooms. This is a selection term, not a treatment effect; since eligible students are screened for higher distress, α_3 is expected to reflect worse baseline mental-health outcomes.
- α_4 is the *differential* spillover for eligible relative to non-eligible untreated students, how much larger or smaller the classroom-exposure spillover is for eligible untreated students than for non-eligible students. Spillover on eligible untreated students = $\alpha_2 + \alpha_4$: the comparison of treated classroom controls vs. eligible pure-control classroom students.

6.3 Social-Network "Friends" Spillover Analysis (RQ4):

Spillovers might be transmitted through friendships (including those outside the classroom). We consider a student to be more "exposed" the more of their friends receive the intervention, and we measure exposure by the *number of treated friends (TFi)*. At baseline, we ask each student to name their friends within the grade. We do not cap the number of names, so we observe each student's complete network and can count, for each student, how many of their friends were treated.

The number of treated friends, TF_i, is non-random: it reflects the random Stage-2 treatment assignment, but also the fixed structure of student *i*'s network — e.g., students with more friends might mechanically be exposed to more treated friends. We therefore follow Borusyak and Hull (2023) by recentering the exposure by its expected value. We compute the expected number of treated friends, Z_i, by re-running the Stage-2 within-classroom randomization many times — holding fixed the friendship network, eligibility, classroom membership, and the Stage-1 classroom arms — and averaging *i*'s treated-friend count across simulated assignments. The recentered exposure $TF_i - Z_i$ isolates the component of exposure generated purely by the realized random assignment. Using the full sample of all students, we estimate:

$$Y_i = \theta_0 + \theta_1 T_i + \theta_2 (TF_i - Z_i) + \phi Y_{i,baseline} + X'_i \delta + \mu_s + \varepsilon_i$$

where:

- θ_2 is the spillover effect of one additional treated friend
- Y_i is the outcome for student *i*.
- T_i is the treatment indicator, equal to 1 if student *i* was randomly assigned to the treatment group and 0 if assigned to the control group within a treated classroom.
- TF_i is the number of treated friends for student *i*.
- Z_i is the expected number of treated friends for student *i*, computed by simulating the within-classroom randomization.
- $Y_{i,baseline}$ is the baseline value of the outcome variable.
- X'_i is a vector of student-level baseline control variables (including age, gender, mother's education, and city).
- μ_s are school fixed effects.
- ε_i is the error term. Standard errors are clustered at the classroom level.

We will base inference on the known assignment process: we recompute β_2 under the same set of simulated counterfactual Stage-2 assignments used to construct Z_i and report randomization-inference p-values, supplemented by exposure-robust standard errors following Borusyak and Hull (2023).

As a secondary outcome of the spillover analysis, we will explore impacts on classroom conflict and school disciplinary measures, such as disciplinary referrals and recorded conduct incidents. The availability and coverage of these records vary across schools and cities, so this analysis is contingent on confirmation of data access from the Secretaries of Education of Barranquilla and Medellín.

6.4 Heterogeneous treatment effects (RQ5)

In a secondary analysis, we will also estimate whether *Clara-Mente* has heterogeneous treatment effects on different subgroups, including by gender, age, baseline severity of psychological distress, type of problem reported on the baseline survey, or socioeconomic classification.

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