A Week of Meditation Boosts Prosociality: A Lab Experiment

(working paper – May 2026)

Abstract

This study examines the causal impact of a five-day mindfulness program on prosocial behavior through a randomized lab experiment with students under 30. We assessed prosociality through incentivized tasks measuring altruism (using an incentivized Social Value Orientation measure), trustworthiness (money returned in a trust game), and responses to moral discrimination scenarios. We find that short-term meditation significantly increases prosocial behaviors: participants in the treatment group return more in trust games, make more altruistic choices, and show reduced racial discrimination — but unreduced discrimination on other topics. These results suggest that brief meditation practices can selectively enhance social and moral concern. As a low-cost, scalable intervention, meditation could be integrated into workplaces, universities, or family life to promote ethical behavior, reduce discrimination, and foster cooperation, especially in high-stress or polarized environments.

JEL codes: C91, D64, D91, I12, J71, Z13

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Declaration about generative AI
During the preparation of this document, we used ChatGPT for editorial support and language checking. After each use, we meticulously re-read and re-modified the material as required. The final content is our sole responsibility.

Ethical Committee

This non-interventional study was performed under the ethical standards as laid down in the 1964 Declaration of Helsinki and its later amendments. This research was validated by the ethical committee of the XXX (University of XXX).

Conflicts of Interest disclosure
The authors have no relevant financial or non-financial interests to disclose.

Pre-registration

This study was not pre-registered. The research design, hypotheses, and analysis plan were developed prior to data collection but were not formally registered in a public repository before the study was conducted. We acknowledge that the absence of pre-registration limits the extent to which confirmatory claims can be distinguished from exploratory findings.

Data availability statement
Data and code are available at:
https://osf.io/3n24t/files/osfstorage?view_only=d0fec7a10953417396832796bb4ea22a

CREDIT:
1. Conceptualization: TBK SD
2. Methodology: TBK SD PNV
3. Software: WB PNV
4. Validation: SD PNV
5. Formal analysis: SD WB PNV
6. Investigation: SD
7. Resources: SD
8. Data curation: WB
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11. Visualization: WB
12. Supervision: TBK SD
13. Project administration: TBK SD
14. Funding acquisition: TBK SD

1. Introduction

At the workplace and elsewhere, antisocial attitudes can have substantial negative consequences. For example, discrimination against colleagues, selfish behavior, or reduced trust within a team can impair the proper functioning of a firm — in addition to raising legal and moral concerns. Consequently, organizations have long sought to promote cohesion, cooperation, and prosocial attitudes among employees. Could meditation play a pivotal role here? It has been traditionally praised for its beneficial effects on physical and mental health – the well-known Mindfulness-Based Stress Reduction (MBSR) program, for instance, is concerned with stress reduction (see, e.g., Charness et al. (2024) for a recent study). Meditation has recently emerged as part of the wellness landscape in firms (Vonderlin et al, 2020), but it could have an impact on more than wellness (Anand et al, 2025, Valor et al 2025). Could there be some spillover effects of meditation in terms of prosocial behavior and decision-making, such as altruism, empathy, fairness, trust and non-discrimination? In other words, could meditation truly make us kinder and more compassionate, at the workplace and elsewhere? Experimental investigations have accumulated in the past twenty years or so, and a literature review (Condon, 2017) and two recent meta-analyses (Berry et al., 2020; Schindler and Friese, 2022) answer largely affirmatively, even when mindfulness training does not include explicit ethics-based instructions – the people become nicer to others, but not just because they were brainwashed to do so. For example, Iwamoto et al. (2020) also found that mindfulness meditation increased social cooperation, especially among subjects with low baseline willingness to contribute. Lueke and Gibson (2016) found that mindfulness meditation helped reduce implicit racial biases, leading to more prosocial behaviors toward members of different racial groups.

However, existing studies present several limitations, suggesting that the positive association between mindfulness and prosociality could be established more convincingly. Following Schindler and Friese (2022), several methodological criticisms can be formulated: about (i) the number of participants, (ii) their recruitment, (iii) how meditation is practiced, (iv) which tasks are considered, (v) how they are incentivized and (vi) what the environment is. First, (i) "many primary studies had remarkably small sample sizes" (Schindler and Friese, 2022, p.153), leading to some statistical weakness to show effects of realistic magnitudes. For instance, Kirk et al. (2016), Wallmark et al. (2013), Weng et al. (2013) have about 20 subjects in each treatment, and Innes et al. (2017), Engel et al. (2020), or Valor et al. (2024) only about 50. A second criticism is that (ii) several studies include important recruitment biases: Hunsinger et al. (2014) and Kang et al. (2014) recruited participants using flyers advertising for a study about meditation, Wallmark et al. (2013) marketed the study as "an opportunity to learn meditation, get greater balance and harmony in everyday life", while standards demand that participants know neither the theme nor what they may gain from the experiment. (iii) Regarding meditation itself, several criticisms can be made. The typical length of meditation programs (e.g., MBSR) are of eight weeks; if reaching this length can be hard on the practical side in experiments, many studies involve durations that are only of a few minutes (e.g., Lueke and Gibson, 2016; Galante et al., 2016; Hafenbrack and Vohs, 2018, 2020; Engel et al., 2020; Iwamoto et al., 2020; Valor et al., 2024) and are not really

compatible with meditation-induced effects – one may suspect that the observed effects actually come from psychological conditioning, social desirability, or experimental demand effect. Regarding the tasks (iv), Schindler and Friese (2022) regret that studies generally target "one specific expression of prosocial behavior only". For instance, they include only a trust game, or only one specific discrimination task, e.g., regarding race. (v) The tasks are not always incentivized (experiments rely only on declared preferences, like in Wallmark et al. (2013), Lueke and Gibson (2016) or Valor et al. (2024)) or the incentivization is hypothetical (Hafenbrack et al., 2020). (vi) Because several studies rely on online participation, the environment is not always controlled (Galante et al., 2016; Iwamoto et al., 2020) — even though this can be a crucial aspect of meditation practice. Finally, it happens that control groups without meditation are not present (Woerner et al., 2025).

This picture should not lead to pessimism. Many of the cited studies do very well on several methodological criteria – e.g., some studies make participants meditate several days in a week, or even several weeks, others have hundreds of participants. Our own experiment aims to fill in the identified gaps, by investigating the effects of meditation on prosocial behavior with a higher methodological rigor, in several of its dimensions. Let us consider the listed criteria in turn. (i) We included 188 participants, who were students below 30 years old. (ii) The recruitment was made through the database of the lab XXX in University XXX, without mentioning the theme or what could be gained from the experiment. Participants were randomly assigned either to a meditation treatment group (97 participants), or a control group (91 participants). (iii) In the meditation treatment, participants practiced 45-minute daily meditation sessions over five consecutive days. In the control group, they engaged in a passive intellectual engagement or mind-wandering activity, by listening to a narration. Both groups were guided by a certified MBSR instructor. The meditation sessions included breath meditation, group dialogues, and loving-kindness meditation, aimed at enhancing empathy and emotional well-being, while the control group focused on listening to a neutral narrative. (iv) Our experiment included several tasks, measuring altruism, trust, and discrimination attitudes, enabling us to get an overall picture. Regarding discrimination, we introduced a novel experimental task regarding declared discriminatory behavior based on race or physical appearance (both implicitly associated with criminality in the task), and regarding smoking and non-smoking habits. Thus, discrimination attitudes are studied in three aspects. We also measured participants' mindfulness, as well as socio-demographic status. (v) The tasks measuring altruism and trust are incentivized traditionally; it was not possible to do this for the discrimination task, precisely because of the topic, but participants were financially compensated for the time spent. (vi) The experiment was run in a lab, providing a controlled environment.

This experiment enables us to target two objectives. First, study the short-term effects of meditation on prosocial behaviors or attitudes (altruism, trust, non-discrimination) in a methodologically improved experimental design. A second aim is to specifically deepen the study of the effects of meditation on discrimination attitudes, by exploring their multiple dimensions. Not all discriminations have the same moral, social or economic dimensions. We are interested in questions like: do meditators discriminate differently with respect to physical appearance or to race more specifically? What about

forms of economic or health discrimination? As a consequence, our study should improve the understanding of meditation's societal effects and help to address contemporary social challenges. On the practical side, our findings could inform mediation programs that cultivate trust and altruism, leadership training that fosters ethical and inclusive decision-making, and community initiatives that directly address prejudice against marginalized groups.

We report three main results that underscore the multifaceted impact of meditation on social behaviors and decision-making. First, individuals who engage in meditation demonstrate a higher degree of altruism compared to non-meditators. This result is in line with several findings (Leiberg et al., 2011; Wallmark et al., 2013; Weng et al., 2013; Galante et al., 2016; Berry et al., 2020; Schindler and Friese, 2022). Second, in a trust game, meditating participants tend to act somewhat more generously as trustors, and markedly more generously as trustees when returning money. This finding is consistent with the recent results, notably, Di Bartolomeo and Papa (2016), showing that mindfulness enhances trustworthiness in an investment game, or Kirk et al. (2016), marking out the role of mindfulness in the increase of trust and cooperative economic decision making. Finally, in line with earlier research suggesting that meditation significantly reduces racial biases and injustice (Kang et al., 2014; Hunsinger et al., 2014; Lueke and Gibson, 2016), the participants demonstrate less discrimination against individuals of Black race. However, meditation had no significant effect on discrimination against smokers or individuals with facial scars. This finding nuances the existing literature and challenges intuitive expectations by suggesting that moral and social considerations play a central role in shaping non-discriminatory behavior.

The article is organised as follows. Section 2 presents our hypotheses. Section 3 presents the experimental design, describing experimental treatments and tasks. Section 4 reports the experimental results and their exploratory analysis. Section 5 discusses the limitations of the paper and further research possibilities while Section 6 concludes.

2. Hypotheses

Several studies indicate that various meditation practices, such as mindfulness meditation, compassion meditation, and loving-kindness meditation, are linked to an increase in prosocial behaviors, compassion, and altruism. An experimental study by Condon et al. (2013) found that meditation directly enhanced compassionate responding: meditating participants were more likely to give up their seat in a situation involving a person in distress. Similarly, Weng et al. (2013) showed that purely mental training in compassion can result in observable altruistic changes towards a victim. In particular, participants who received compassion training spent more money to redistribute funds to a victim compared with participants who received reappraisal training. Finally, Iwamoto et al. (2020) found that subjects who underwent a mindfulness meditation online session provided higher donations in a donation game than the control group.

Based on these findings, we measure altruism using the Social Value Orientation (SVO) questionnaire and expect that meditators will exhibit a stronger inclination to share resources equitably

and prioritize collective welfare over self-interest. As loving-kindness meditation enhances compassion, we formulate the following hypothesis:

Hypothesis 1: Individuals who engage in meditation exhibit higher levels of altruistic behavior compared to non-meditators.

Meditation has been shown to reduce implicit biases and prejudicial attitudes by fostering nonjudgmental awareness and emotional regulation. Thus, Lueke and Gibson (2016) found that a brief 10-minute mindfulness meditation session reduces implicit bias related to age and race in novice participants. Kang et al. (2014) showed that loving-kindness meditation practice reduced implicit bias against homeless people. Likewise, Hunsinger et al. (2014) found that a group of experienced compassion-based meditators expressed less racial prejudice and more empathy than non-meditators. In our study, discrimination is measured through explicit behavioral choices in hypothetical scenarios. We hypothesize that discriminatory attitudes will be reduced by the practice of meditation.

Hypothesis 2: Meditation reduces discriminatory behavior.

Trust plays a crucial role in social and economic relationships, and previous studies suggest that meditation fosters trust and cooperative behavior (Kirk et al., 2016). Bartolomeo and Papa (2016) show that subjects exposed to meditation demonstrate greater trust and trustworthiness in an investment game than the control groups. A qualitative study by Pruitt and McCollum (2010) reveals that the meditative traits developed by experienced meditators have significant relational effects on their intimate relationships, for which trust was the central element. This suggests that meditation cultivates qualities that enhance interpersonal interactions and facilitate trust.

Hypothesis 3: Meditation practice increases participants' trusting behavior and trustworthiness.

3. Experimental design

The study was conducted at the experimental economics laboratory of the University of XXX during September and October 2020. The average duration of each experimental session was 120 minutes. Participants were recruited through ORSEE (Greiner, 2015) and were then randomly assigned either to the meditation treatment or to the control treatment.

Recruitment procedure: The meditation treatment was administered first. Invitations were sent randomly to individuals in the laboratory's volunteer pool, informing them only of the required five-day availability — not of the experiment's content. Participation was therefore self-selected on availability alone. Once the meditation treatment was complete, a new round of random invitations was sent to the remaining pool, excluding anyone who had already participated. Importantly, participants had no way of knowing that the two studies were related. Those who had declined the first invitation for any reason could thus appear in the control group without any awareness of the connection. This design was further supported by the laboratory's broader research culture: participants routinely received at least one study

invitation per month across a wide range of disciplines, making it natural — rather than conspicuous — to receive a new, unrelated-seeming invitation. Finally, the risk of social contamination among meditation treatment participants was minimized in two ways. During the five meditation days, multiple session slots were offered daily and chosen by participants based on their own availability, reducing the likelihood of participants crossing paths or becoming aware of each other's involvement. On the fifth day — when participants additionally completed the computer-based session — the laboratory's capacity of 20 per session required the sample to be spread across several time slots, each chosen by participants according to their own availability, further limiting potential interactions.

3.1. Treatments

Within the **meditation treatment**, participants took part in 45-minute daily meditation sessions over five consecutive days. These sessions were led by a certified MBSR instructor[1] in a dedicated room. The meditation protocol consisted of three components: (i) breath meditation, focusing on the natural rhythm of inhalation and exhalation to cultivate mindfulness and presence; (ii) group exploratory dialogues to discuss emotions and sensations; and (iii) a secular form of loving-kindness meditation. The latter, rooted in the Buddhist tradition of Metta, involved cultivating feelings of goodwill and kindness toward oneself and others, with the aim of enhancing empathy and emotional well-being. The meditation treatment was structured into two sessions, each spanning two weeks. Participants engaged in five consecutive days of meditation during either the first week or the second week. A total of 127 participants were randomly assigned to one of the two meditation sessions. Among them, 16 individuals did not complete the prescribed five days of meditation and were therefore not included in the subsequent experiment. Of the remaining 111 participants, 5 were unable to take part in the experiment. Ultimately, 105 participants successfully completed the meditation treatment, 97 of whom were 30 years old or younger.

In the control treatment, 102 participants (91 of whom were 30 years old or younger) took part in a single-session activity during which they listened to a 45-minute narration. The narrative examined two documents exploring the emergence of early modern humans in Africa over several hundred thousand years. This treatment was administered by the same meditation instructor to maintain consistency in the auditory experience and in participants' visual perception of the instructor. Mind-wandering protocols are frequently employed in the scholarly literature to contrast the effects induced by meditation with those arising from a neutral storytelling technique, particularly in the context of decision-making (Arch & Craske, 2006; Hafenbrack & Vohs, 2018; Long & Christian, 2015; Mrazek et al., 2012). On the fifth day, immediately after the final meditation or mind-wandering session, participants completed the experimental tasks.

[1] It is noteworthy that the meditation instructor was an external practitioner and a certified MBSR instructor, a measure intended to mitigate any potential bias associated with the methodological quality of the study (Kreplin, Farias, and Brazil, 2018). All sessions were conducted by the same instructor to avoid potential bias arising from instructor heterogeneity.

One may worry that the requirement of attendance over five consecutive days for the meditation treatment, and not for the control treatment, may introduce a potential selection bias in the estimation of the treatment effect. We assess this potential bias by comparing various characteristics of the treatment and control groups. Table A2 (Appendix A) shows a significant difference only in laboratory experience (Mann-Whitney U test, $p < 0.005$), while no significant differences are observed for other characteristics (field of study, gender, age, knowledge of game theory). This confirms that the data were not affected by systematic differences stemming from this imperfect randomization.

The experiment formed part of a broader meditation-focused project comprising several tasks, with an average duration of approximately 120 minutes. This paper focuses exclusively on a subset of tasks and questionnaires.

3.2. Experimental tasks

The study included five tasks and questionnaires designed to collect explanatory data, in line with the mindfulness literature previously discussed. These tasks were: (i) completing the Mindful Attention Awareness Scale (MAAS) test, which assesses attentional states, mindfulness-related behaviors, and associated attitudes; (ii) an incentivized SVO measure to capture participants' social preferences; (iii) discrimination questionnaires comprising two distinct items—the first addressing discrimination toward individuals of Black race or with facial scars, and the second addressing discrimination toward smokers; (iv) a modified trust game measuring participants' prosocial behavior; and (v) a socio-demographic questionnaire.

The visual representations of the diverse tasks in English can be accessed in the Online Supplementary Material. Details of the tasks are available in the Appendix C. The sequence of tasks was identical across all treatments. To minimize potential feedback effects between decisions, participants were not informed of any results or payoffs until the end of the experiment. Furthermore, we ensured that there was no contamination across treatments by keeping participant groups strictly distinct throughout the experiment. The meditation treatment sessions were conducted first, followed by the control sessions one week later, which prevented any opportunity for participants from different conditions to interact or exchange information.

- Meditation measure and MAAS score

We collected data with the MAAS, which consists of a 15-item questionnaire designed to assess attentional states, mindfulness-related behaviors, and associated attitudes. Responses were recorded on a six-point rating scale ranging from 1 to 6 (Brown & Ryan, 2003).

- Social Value Orientation

We collected data on participants' social preferences using an SVO task, which measures an individual's preference for allocating resources between themselves and others, reflecting prosocial,

individualistic, or competitive tendencies (Murphy, Ackermann, & Handgraaf, 2011). Each allocation decision had real monetary consequences for both the participant and an anonymous partner, with payoffs determined using a fixed exchange rate. The instructions provided to participants can be found in the Appendix C.

- Discrimination

The questionnaire on discriminatory behaviors consisted of the following three scenarios. The first scenario explores racial issues by presenting a hypothetical encounter with individuals of Black race on a dimly lit street in Northampton, UK. Participants are informed of existing statistical data indicating a higher likelihood of criminal charges for people of Black race in this town. This setup assesses the influence of stereotype threat and racial prejudice on participants' behavioral responses. Participants are asked whether they would change sidewalks. Answering yes is coded as an indication of discrimination, whereas maintaining one's trajectory is coded as no discrimination.

The second scenario mirrors the first but focuses on discrimination based on facial scarring. Participants encounter individuals with visible facial scars, associated with a similarly higher likelihood of criminal charges, with the same path-changing decision used as an indicator of discrimination. Thus, the only difference between the first two scenarios lies in the proxy that may be used to infer criminality on the street: race or the presence of a facial scar. Deliberately, the two scenarios combine elements of taste-based and statistical discrimination (i.e., one may cross the street because one relies on group-level statistics and their consequences for oneself, or because one simply dislikes being on the same sidewalk as some category of people). Although this design does not isolate these two forms of discrimination, it arguably better approximates many real-life decisions, in which such motivations are often intertwined, and the scenarios enable to investigate whether meditation may influence discriminatory behavior across an ecologically plausible range of situations — if such an effect were observed, future studies could then investigate its impact on more specific and distinct forms of discrimination.

These two scenarios were randomised, with half of the participants receiving one of them. Specifically, 96 subjects (48 control, 48 treated) were questioned about racial discrimination, with six declining to respond (1 control, 5 treated), while 92 subjects (43 control, 49 treated) received the facial-scar scenario, with four abstaining (2 control, 2 treated).

The third scenario addresses discrimination against smokers by evaluating attitudes toward health-insurance price adjustments (or profiling) based on smoking status. Considering statistical associations between smoking and health risks, participants were asked to express their views on pricing. Their answers are measured on a 1-4 scale: 1 - Smokers should pay more than non-smokers, to compensate for all the extra expenses incurred as a result of smoking; 2 - Smokers should pay more than non-smokers, to compensate for some of the additional expenses incurred as a result of smoking; 3 - Smokers should pay more than non-smokers to compensate for some of the additional expenses incurred as a

result of smoking, unless they have recently consulted a tobaccologist in order to stop smoking; and 4 - Smokers should not pay more than non-smokers. A value of 1 represents discriminatory intent against smokers, and a value of 4 indicates no discrimination. Here, the possible discrimination is typically a statistical one (but may be morally problematic, cf. Boyer-Kassem and Duchêne, 2020).

- Modified trust game

Trust was assessed through a modified version of the trust game (Berg, Dickhaut, & McCabe, 1995). Participants were paired anonymously and randomly assigned to the role of either sender (trustor) or receiver (trustee). In this modified version, the trustee received no initial endowment — unlike the standard design in which both players typically start with equal amounts.

Each trustor was endowed with €10 and decided how much of this amount (S), ranging from €0 to €10 in integer increments, to transfer to the trustee. The transferred sum was then tripled upon receipt (i.e., the trustee received 3S), and the trustee then decided how much to return to the trustor, any amount between €0 and €3S. The trustor's final payoff was therefore (10 − S) plus whatever amount the trustee returned, while the trustee's final payoff was solely the amount kept (3S minus the amount returned).

Consistent with subgame perfection, a fully self-interested trustee would return nothing, in anticipation of which a purely rational trustor would also send nothing. Nonetheless, many trustors choose to send positive amounts, driven by altruism, expectations of reciprocity, or inequality aversion (Berg, Dickhaut, & McCabe, 1995; Johnson & Mislin, 2011; Cox, 2004).

Previous studies highlight the role of endowment asymmetry in shaping trust and reciprocity. Chai et al. (2018) and, more recently, Bejarano et al. (2025) show that trustees with lower endowments than trustors tend to return less, a tendency attributed to inequality aversion. Given this evidence, lower reciprocity could be expected in our modified setting. However, if meditation promotes empathy and reduces self-centred decision-making, its treatment effect on reciprocity should not be weakened by endowment asymmetry — if anything, it may be amplified (cf. Hypothesis 3).

3.3. Practical procedures

Participants were paid based on one randomly selected task (Pay-One-Random, POR), a standard procedure to ensure incentive compatibility and avoid wealth and portfolio effects (Andersen et al., 2006; Charness et al., 2016; Azrieli et al., 2018). Although POR may dilute per-task incentives in longer sessions, participants were not informed of which task would be selected until the session ended, preserving incentive salience throughout, and any residual dilution applies symmetrically across treatment and control groups. Payment was single-blind; all decisions were made on individual computer

terminals with no experimenter interaction and no feedback until the session ended, substantially limiting experimenter demand effects.

Average (maximum) earnings amounted to €19 (€49), excluding show-up fees. Given that the sessions lasted less than two hours, the incentives were relatively high compared with conventional laboratory standards. In addition, a fixed compensation of €6 was provided per attendance to cover travel expenses, amounting to €30 for the five sessions in the meditation treatment and €6 for the single session in the mind-wandering control.

The study forms part of a broader experimental project comprising eight incentivized tasks in total. In this paper, we focus on three of them—namely, the SVO, the Trust Game, and the discrimination questionnaires—because they capture key dimensions of prosocial behavior. Other tasks within the same project addressed distinct research questions, one of which has already been analysed in a separate publication on prosocial investment during financial crises (XXX for blind review, 2025). The present article should therefore be viewed as a complementary contribution, examining prosociality along different behavioral dimensions within the same overarching experimental framework.

The experimental protocol received ethical approval from the Ethics Committee of the XXX at the University of XXX.

4. Experimental results

This result section is divided in two parts: the first presents a descriptive and non-parametric analysis of the meditation effect, while the second explores the causal effect of meditation on dependent variables by means of regressions. Our results indicate that individuals who engage in meditation exhibit greater altruism compared to non-meditators. Furthermore, meditators show reduced racial discrimination against individuals of Black race, while no significant effects are observed regarding discrimination against individuals with scarred facial features or smokers. Ultimately, in a trust game, meditating participants exhibit behavior that is occasionally more generous as trustors, and particularly more generous as trustees when returning money.

4.1 Descriptive and non-parametric analysis of meditation impact

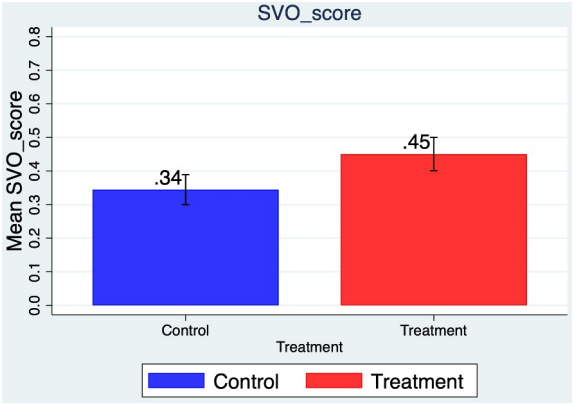
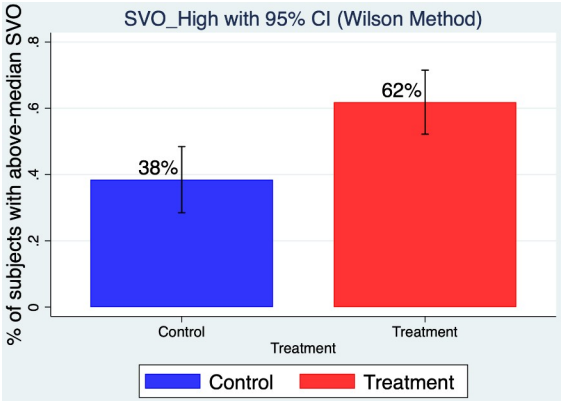
4.1.1 Meditation impact on social preferences (altruism)

As depicted in Figure 1, individuals engaging in meditation exhibit a higher propensity for altruistic behavior in comparison to those who do not meditate (Mann-Whitney-U tests, $p < 0.005$). Panel A compares the standard SVO score between the meditation and control groups, while Panel B

examines the SVO score of participants who can be considered as altruistic (the binary variable *SVO_High* equals 1 if the participant's score is higher than the median of the sample).[2]

[2] See Table A1 in Appendix A for more details on the differences in the SVO score levels.

Figure 1: Effect of meditation on SVO (altruistic measure)

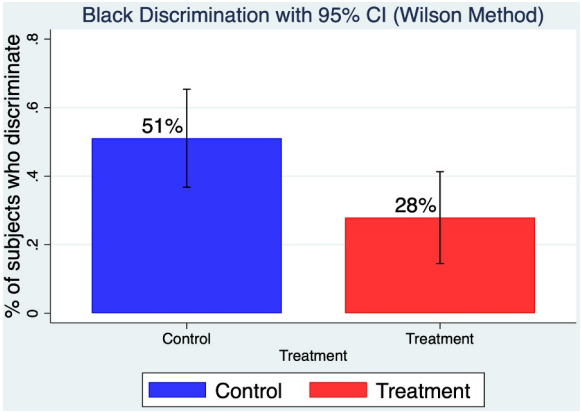
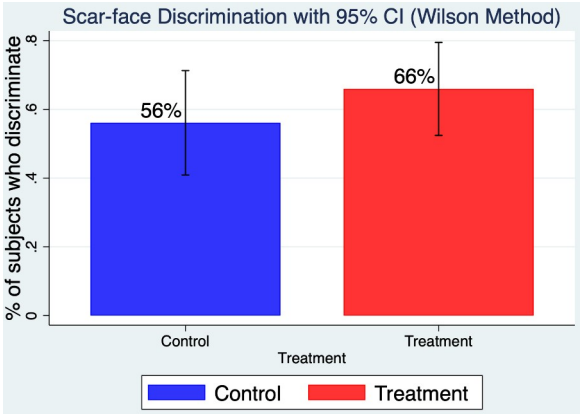
Panel A: *SVO_score*	Panel B: *SVO_High*
	
Difference in the level of *SVO_score* Mann-Whitney-U tests: $z = -3.241$, $p < 0.001$	Difference in the level of *SVO_High* Two-sample test of proportions $z = -3.206$, $p = 0.001$. Pearson chi2 = 10.280, $p < 0.001$

Result 1: Individuals who engage in meditation demonstrate a higher degree of altruism compared to those who do not.

4.1.2 Meditation impact on discrimination

Figure 2 presents the results of the discrimination questionnaires related to individuals of Black race and those with scarred faces. *Black_Discrimination_Yes* in Panel A and *Scar-face_Discrimination_Yes* in Panel B are binary indicators derived directly from experimental responses, where *Black_Discrimination_Yes* denotes a participant's choice to change sidewalks in the presence of a Black individual, and *Scar-face_Discrimination_Yes* reflects a similar reaction to individuals with facial scars.

Figure 2: Discrimination against people of Black race or with scarred faces

Panel A - *Black_Discrimination_Yes*	Panel B - *Scar-face_Discrimination_Yes*
	
Two-sample test of proportions z=2.239, p=0.025. Pearson chi2 = 5.017, p<0.025	Two-sample test of proportions z=-0.947, p=0.343. Pearson chi2 = 0.898, p < 0.343

We observe a significant difference between the control and treatment groups (two-sample test of proportions, z = 2.239, p = 0.025*) in Panel A related to discrimination against people of Black race and no significant results in Panel B related to people with scarred faces. This suggests that meditation reduces racial discrimination.[3] Moreover, in the control group, there is no statistically significant difference in the discrimination rate between Panels A and B. Specifically, 51% of participants chose to move to another sidewalk in response to the Black-discrimination scenario, compared to 56% in the Scarface-discrimination scenario. This difference is not statistically significant (two-sample test of proportions, z = -0.472, p = 0.637), suggesting that people in the control group do not discriminate differently depending on the proxy – race or scars.

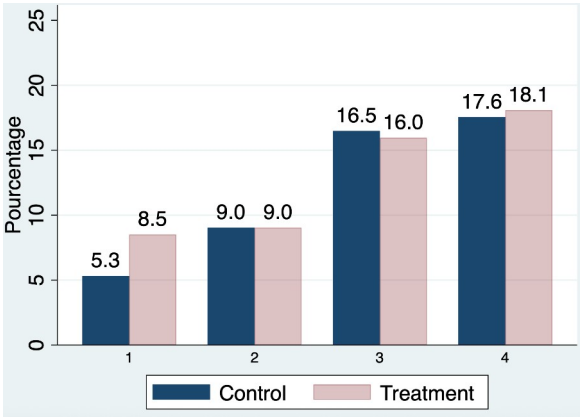
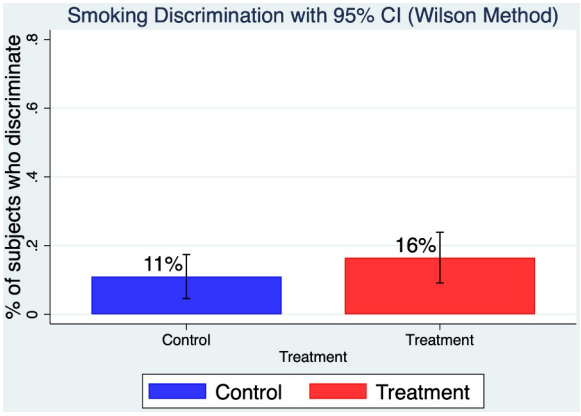
In contrast, in the treatment group, there is a statistically significant difference in the discrimination rate between Panels A and B. In the meditation group, 28% of participants chose to move to another sidewalk in response to the Black-discrimination scenario, compared to 66% in the Scarface-discrimination scenario. This difference is statistically significant (two-sample test of proportions, z = -3.610, p = 0.0003***), suggesting that meditators are more reluctant to use race as a proxy (perhaps because they are more sensitive to the fact that it is a socially and morally charged issue), whereas they are comfortable in using facial scars as a proxy, likely because it raises fewer ethical concerns.

It thus appears that the moral dimension underlying the decision to change sidewalks is particularly salient and that meditation heightens individuals' sensitivity to such moral considerations.

[3]See Table A1 in Appendix A for more details on test results. The results are corroborated by logit regressions. We report marginal effects estimated from logit models with cluster-robust standard errors at the participant level. Specifically, we find that subjects in the meditation treatment discriminate more against individuals of Black race compared to the control group (Table A3 in Appendix A, Model 1). The treatment effect is negative and statistically significant both with controls (coef. = –0.199, p < 0.05) and without controls (coef.=–0.225, p<0.05). By contrast, we do not find any significant treatment effect on discrimination against individuals with facial scars (Table A3 in Appendix A, Model 2): the treatment coefficient is positive but not significant, both with controls (coef. = 0.161, p > 0.05) and without controls (coef. = 0.098, p > 0.05). Importantly, we also re-estimated the same logit specifications including *risk_gp* as an additional control variable, and the results remain unchanged.

Figure 3 compares the meditators' discrimination attitudes against smokers with those of non-meditators. *Smoking_Discrimination* is measured on a 1-4 scale. For comparability, a binary variable, *Smoking_Discrimination_Yes*, was created to capture discriminatory behavior toward smokers (its value is 1 if and only if the answer on the 1-4 scale equals 1). We find no significant effect of meditation on discrimination against smokers, whether considering the full variable or the binary variable (Pearson chi2 = 0.030, p = 0.862).[4]

Figure 3: Discrimination against smokers

<table>
<tr><td>Panel A: Smoking_Discrimination</td><td>Panel B: Smoking_Discrimination_Yes_1</td></tr>
<tr><td> 
</td><td> 
</td></tr>
<tr><td>Difference in the level of smokers discrimination,
Mann-Whitney-U test statistic z=0.593, p<0.553</td><td>Difference in the level of smokers discrimination
Two-sample test of proportions z= -1.093, p=0.274
Pearson chi2 = 1.194, p < 0.274</td></tr>
</table>

Result 2: Meditators discriminate less against individuals of Black race than non-meditators. No effect of meditation on discrimination against individuals with facial scars and smokers is observed.

4.1.3 Meditation impact on trust and cooperative behavior

Consistent with findings from the existing literature, participants generally exhibit trust toward their counterparts. The distribution of transferred amounts by the trustor is illustrated in Figure 4. Only 24 participants (11 in the treatment group and 13 in the control group), representing 25% of the total

[4]See Table A1 in Appendix A for more details on test results. We also tested an alternative binary variable, *Smoking_Discrimination_Yes_12*, coded as 1 if the answer is 1 or 2. The results from the two-sample test of proportions ($z = -0.639$, $p = 0.522$) and Pearson's chi-squared test ($\chi^2 = 0.409$, $p = 0.523$) both confirm the absence of a treatment effect on smoking-related discrimination. See Figure 8 in the Appendix D. The results are also confirmed by logit regressions. Specifically, we find no significant treatment effect on discrimination against smokers (Table A3 in Appendix A, Model 3), with the dependent variable defined as the binary indicator *Smoking_Discrimination_Yes_1* or *Smoking_Discrimination_Yes_12*. The treatment coefficient is close to zero and not statistically significant both with controls (coef. = 0.046, $p > 0.05$) and without (coef. = 0.056, $p > 0.05$). Moreover, when we re-estimate the same logit specifications including risk_gp as an additional control variable, the results remain unchanged.

sample (22.92% in the treatment group and 27.08% in the control group), adopted the equilibrium strategy of sending €0. On average, trustors transferred €3.97 (€4.48 in the treatment group and €3.46 in the control group) with a median transfer of €3.5 (€4 in the treatment group and €3 in the control group). Although these numbers may seem to be higher in the meditation group, the non-parametric analysis indicates no significant effect of meditation on trusting behavior (Transfer to trustee: Mann-Whitney U tests, $p = 0.197$).[5] This result is further confirmed by the Kolmogorov-Smirnov test, which shows no significant difference in the distribution of transferred amounts between the control and treatment groups ($D = 0.167$, $p = 0.518$).

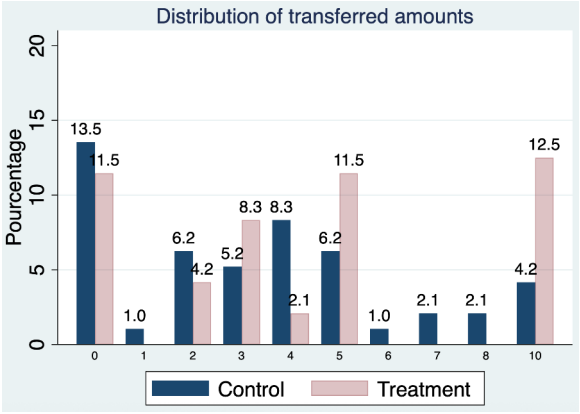
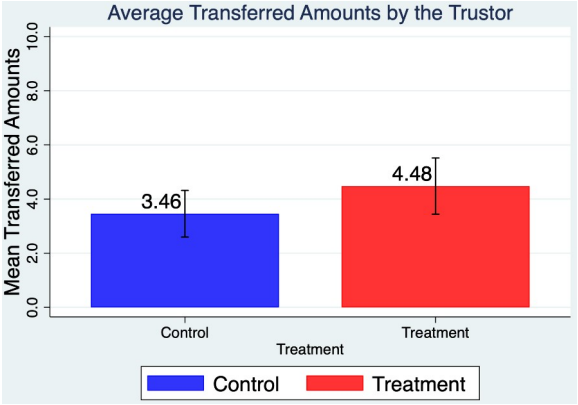
However, by visual inspection, we observe a potential difference in the distribution, particularly for the extreme amount of €10. A total of 16 participants (12 in the treatment group and 4 in the control group), representing 17% of the overall sample (25% of the treatment group and 8.3% of the control group), transferred the maximum amount of €10. A proportion test confirms the difference between meditators and non meditators as statistically significant (two-tailed test: $z = -2.196$, $p = 0.028$).

Expanding on this fine-grained analysis, a Fisher's exact test conducted on the full contingency table of transfer levels by treatment group also reveals a statistically significant difference in the distribution ($p = 0.043$)[6]. This suggests that, although the overall shape of the distributions may appear similar and standard non-parametric tests fail to detect significant differences, the frequency of specific transfer amounts — particularly at the distribution's extremes — differs meaningfully between groups. These results corroborate the visual impression of divergence in transfer behavior.

Overall, these findings suggest that while meditation does not significantly change average levels of trust, a closer look at the data shows meaningful differences in how people choose to trust. In particular, participants in the meditation group are more likely to make extreme trusting decisions, such as giving the full amount. These patterns are not visible through standard summary statistics, but they highlight a more subtle effect of meditation on trust-related choices.

[5] Please, see Table A1 Appendix A for more details on test results.
[6] For further details, see the Table D-1 Appendix D presenting proportion tests for each transfer level.

Figure 4: Distribution and average of transferred amounts (trustor to trustee)

Panel A: Distribution of transferred amounts	Panel B: Average of transferred amounts
	
Fisher's exact test: p<0.043. For further details on the empirical cumulative distribution functions (ECDF), see Figure 9 in Appendix D.	Difference in the average transferred amounts: Mann-Whitney-U test statistic z=-1.291, p<0.197.

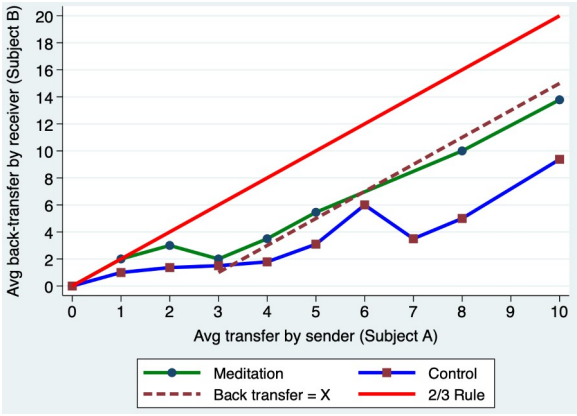
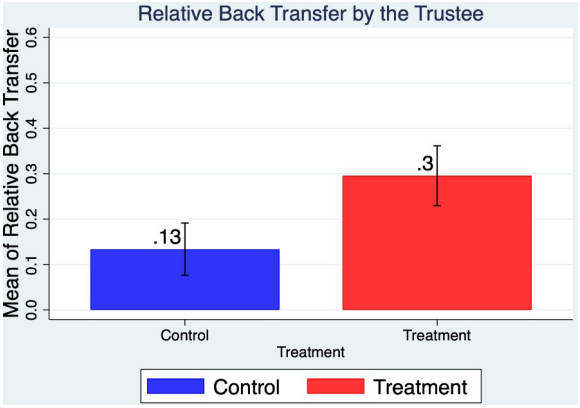
Turning to trustworthiness, we find a significant impact of meditation. The measure of trustworthiness among participants is determined by the amount returned by trustees in the trust game (back transfer). On average, the treatment group demonstrates significantly higher levels of back transfers compared to the control group (5.37 vs. 2.07; Back Transfer: Mann-Whitney U tests, $p < 0.005$).

The median reciprocity, measured as the ratio of the amount returned to the amount sent, is significantly higher (Trustworthiness Relative to Sent: Mann-Whitney U test, $p < 0.005$) in the treatment group (0.89) than in the control group (0.40), indicating a strong effect of the treatment on reciprocal behavior based on the trustor's initial investment. Additionally, when trustworthiness is measured as the ratio of the amount returned divided by the amount received (Trustworthiness Relative to Amount Received: Mann-Whitney U test, $p < 0.005$), the treatment group also shows a significantly higher ratio (0.29) compared to the control group (0.13). These findings collectively provide robust evidence that the treatment significantly enhances trustworthiness among participants.

Figure 5 corroborates these results.[7] Panel A illustrates the average back transfer in both groups. The dashed line represents the amount required for an equal payoff, while the red line indicates the scenario where the trustee returns two-thirds of the amount received. Panel B presents the average trustworthiness relative to the amount received (Back Transfer / Amount Received) for each group, where the amount received is calculated as three times the amount sent.

[7] OLS regression results presented in Table A4 Appendix A further support it (Columns 5-6: coefficients = 2.578 and 0.119, $p < 0.05$), demonstrating that trustworthiness levels are significantly higher in the treatment group.

Figure 5: Average back transfer and relative back transfer

Panel A: Average back transfer	Panel B: Average trustworthiness relative to the amount received
	
The dashed line represents the amount required for an equal payoff. The red line indicates the scenario where the trustee returns two-thirds of the amount received.	Difference in the trustworthiness relative to amount received = (Back Transfer / Amount Received). Mann-Whitney U statistic z=-3.455, p<0.000

Result 3: While meditation does not significantly affect average levels of trust, it increases the likelihood of making fully trusting decisions. Moreover, meditators demonstrate significantly higher levels of trustworthiness in the trust game compared to non-meditators.

4.2 Causal analysis with regression models

To estimate the causal effect of meditation on observed individual behaviors, we binarize the continuous variables (see below) and employ the following regression model:

$$Y_i = \alpha + \beta_1 T_i + X_i'\gamma + \varepsilon_i \qquad (1)$$

where Y is a binary variable representing individual i's behavior in the various tasks, T an indicator variable for the meditation treatment (=1 for the treatment group, =0 for control group), X a vector of individual characteristics (gender, age, field of study, an indicator whether the subject has already participated in a different experiment organized beforehand in an experimental laboratory, and an indicator whether they have knowledge of game theory), and finally ε_i the regression error term. To interpret the treatment coefficient (β_1) causally, random sampling was used to ensure that subjects were randomly assigned to the control and treatment groups.[8]

[8] Since participants with greater availability over five days may have been more likely to enter the meditation treatment, we controlled for this potential bias (cf. footnote 2). Our econometric balancing tests confirmed that the data were not affected by systematic differences related to this imperfect randomization. Our sampling procedure

We estimate the model in Eq. (1) using logit regression and OLS regression with cluster-robust standard errors at the participant level, as shown in Table 9.[9] The causal effect of meditation on various prosocial and discriminatory behaviors is captured by the average treatment effect (ATE) for different dependent variables, including *SVO_High*, *Black_Discrimination_Yes*, *Scar-face_Discrimination_Yes*, *Smoking_Discrimination_Yes, Back Transfer*, and *Trustworthiness Relative to the Amount Received*. To calculate the ATE, we apply several estimation techniques: inverse-probability weighting (IPW), augmented inverse-probability weighting (AIPW), and propensity score matching (PSMATCH)[10].

Our findings suggest that meditation has meaningful behavioral and attitudinal effects, particularly in fostering prosocial behavior and trustworthiness, while also reducing racial discrimination. These implications have potential relevance for policy, educational programs, and workplace interventions (cf. Section 6). The significant reduction in discrimination toward individuals of Black race ($p < 0.05$ in AIPW and PSMATCH methods) highlights meditation's potential role in addressing implicit biases — unconscious prejudices towards certain groups that can persist even among individuals who would consciously reject discrimination. Given the growing interest in mindfulness-based diversity training, our findings provide empirical support for meditation as a tool for fostering inclusivity and mitigating discriminatory attitudes.

Moreover, the estimates indicate that the meditation treatment induces a statistically significant ($p < 0.05$) positive effect on the SVO score, Back Transfer, and Trustworthiness Relative to the Amount Received across all methods. In line with prior research (Kristeller et al., 2005; Leiberg et al.,2011; Weng et al., 2013; Wallmark et al., 2013; Galante et al., 2016; etc.), this consistent and significant effect suggests that meditation fosters cooperative and altruistic behaviors. In practice, meditation interventions could be integrated into team-building programs, educational curricula, or community initiatives to promote social cohesion and trust.

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and econometric treatment allow us to eliminate selection bias due to observables (we admit that the possibility of bias due to unobservables cannot be ruled out).

[9] We additionally performed an ordered logit regression on the original measures of discrimination using the same set of control variables as the logit model. Full estimation results are reported in Tables A3, A4, and A5 in Appendix A.

[10] Inverse Probability Weighting (IPW) reweights observations by the inverse of their propensity score to correct for selection bias, creating a balanced pseudo-population. Augmented Inverse Probability Weighting (AIPW) extends IPW by incorporating an outcome regression model, providing double robustness (the estimator remains consistent if either the propensity score or the outcome model is correctly specified). Finally, Propensity Score Matching (PSMATCH) pairs treated and control units with similar propensity scores to construct a comparable control group. All these methods rely on the assumption that all relevant confounders are observed and accounted for in the estimation.

Table 9. Average Treatment Effects (ATE)

Panel A: Dummy Dependent Variables (Logit Models)

Method	SVO High	Black Discrimination	Scar-face Discrimination	Smoking Discrimination[11]
IPW	0.173* (0.075)	-0.190 (0.102)	0.151 (0.094)	0.043 (0.049)
AIPW	0.178* (0.074)	-0.195* (0.096)	0.127 (0.089)	0.041 (0.048)
PSMATCH	0.156* (0.074)	-0.261* (0.117)	0.167* (0.077)	0.080 (0.051)
#Obs	188	90	88	188

Panel B: Continuous Dependent Variables (OLS Models)

Method	MAAS	SVO	Transfer	Back Transfer	Trustworthiness Relative to Amount Received
IPW	1.183 (1.765)	0.084* (0.035)	0.684 (0.668)	2.940*** (0.839)	0.134*** (0.042)
AIPW	1.042 (1.723)	0.085* (0.034)	0.700 (0.667)	2.822*** (0.886)	0.131*** (0.042)
PSMATCH	2.106 (2.028)	0.077* (0.035)	1.299 (0.805)	2.842*** (0.834)	0.137*** (0.036)
#Obs	188	188	96	92	92

Notes. Robust standard errors in parentheses. Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$. ATE calculated using inverse-probability weighting (IPW); augmented inverse-probability-weighting (AIPW); and propensity score matching (PSMATCH).

[11] The dependent variable in this specification is the binary indicator *Smoking_Discrimination_Yes_1*. We obtain similar results when using the alternative definition *Smoking_Discrimination_Yes_12* as the outcome variable.

5. Methodological Considerations and Limitations

Several methodological aspects and contextual factors warrant careful interpretation of our results. First, the relatively modest sample size constitutes a limitation. Although our study included a larger number of participants than many existing meditation studies (cf. Schindler and Friese, 2022, p.153), the logistical complexity of organizing multi-day experimental sessions constrained recruitment. While statistically significant effects were observed, the relatively limited number of participants implies that findings remain preliminary.Note that our comparison graphs already report means and confidence intervals, allowing readers to directly assess the magnitude and precision of differences between groups. The wide confidence intervals reflect the limited sample size and should be interpreted as such. In particular, non-significant results such as those observed for trustor transfers and certain discrimination dimensions should be interpreted as inconclusive rather than as evidence of absence of effect, as limited statistical power increases the risk of Type II errors. As emphasized in other exploratory studies (e.g., Charness et al., 2024), replication with larger and more diverse samples is essential to strengthen the external validity and to improve statistical power of these findings.

Second, the composition of the sample also matters. The analysis was restricted to participants aged 30 or younger, which helped ensure demographic homogeneity and enhance internal validity. Over 90% of participants were university students in this age range, while those above 30 constituted a highly heterogeneous minority. Research in neuroscience and psychology suggests that meditation effects may vary with age due to neuroplasticity, attentional dynamics, or affective processing (Tang et al., 2015; Lehto et al., 2015), reinforcing the logic of this restriction. Therefore, the generalizability of our results to older populations (above 30 years old) remains an open question that should be explored in future studies.

Third, contextual effects related to the period of data collection may be present. The experiment was conducted in late 2020, during the second wave of the COVID-19 pandemic in France. Though strict lockdowns had ended, university life was still restricted by curfews and reduced social interaction. Students during this time experienced elevated psychological distress (Essadek & Rabeyron, 2020), and mindfulness interventions have shown particular efficacy under such conditions (Sanilevici et al., 2021). These external conditions could have increased receptiveness to meditation and potentially magnified the observed effects, thereby limiting the extrapolation of results to more typical social environments. In addition, changes in individual preferences during the pandemic — such as variations in risk-taking or patience (Tsutsui & Tsutsui-Kimura, 2022) — could have interacted with our treatment in ways not fully controlled for.

Some other possible objections may be readily addressed. Fourth, we have acknowledged the possibility of selection effects linked to how participants were recruited, since the meditation group committed to five consecutive days, while the control group participated only once (cf. footnote 2). Such asymmetry could attract individuals with different baseline traits. To mitigate this potential bias, we

applied several statistical approaches (bivariate analyses, IPW, AIPW, and PSM), all yielding convergent results, reinforcing the robustness of the observed treatment effects. This suggests that, for the variables measured here, the selection effect, if present, has no observable impact.

Fifth, one might worry that social desirability bias could partly influence participant responses, especially in tasks involving fairness or altruism. While prior work (e.g., Milfont, 2009) suggests minimal distortion in self-reported environmental behavior, other research questions the sensitivity of available scales in detecting "faking good" responses (Lanz et al., 2022). Unless meditation systematically increases concern for social image — something we do not observe — such bias is likely orthogonal to treatment assignment.

Sixth, the compensation structure may raise the question of a potential endowment or "house money" effect, since meditators received cumulative payments over five sessions, while controls participated in a single session. However, this risk is minimal. Participants in the meditation group received higher show-up fees because they attended multiple sessions. However, they may not have perceived this additional compensation as a windfall gain or a gift. Rather, it is plausible that they viewed it as legitimate compensation for the concrete costs and efforts associated with participation, including daily transportation expenses and the time spent commuting to the university. For instance, the French minimum hourly wage is 11.65 euros, while a streetcar ticket costs 1.60 euros. Given these costs, the compensation provided for the 45-minute meditation sessions and the associated commuting time appears relatively modest in relation to the effort and constraints involved, which may have limited the likelihood of an endowment effect. In fact, hourly compensation was lower for meditators (~11.20€/h) than for controls (~22.80€/h), reducing the plausibility of a generosity-inducing wealth effect. This interpretation is supported by behavioral economic theory, which suggests that the perceived origin of funds influences behavior. Moreover, the conflicting literature on the wealth-prosociality link (e.g., Andreoni & Vesterlund, 2001; List, 2011) further complicates any simple income-based behavioral prediction.

Seventh, note that our design does not hypothesize the mechanisms through which meditation exerts its effects. Is the impact driven by changes in emotional regulation, by shifts in attentional scope, or by altered perceptions of context? Future research should integrate mediation analyses or psychological assessments to illuminate the underlying cognitive or affective pathways.

Finally, another limitation concerns the interpretation of trust game behaviour. Our investment game design does not allow us to disentangle the distinct motivations underlying sender and receiver decisions — including trust, altruism, reciprocity, and inequality aversion — a limitation well documented in the literature (Chaudhuri & Gangadharan, 2007). Sender transfers may reflect expectations of reciprocity, inequality aversion, or altruism rather than trust per se, while receiver back-transfers may be driven by reciprocity or broader social preferences. Cox (2004) shows that a triadic design incorporating a dictator game alongside the trust game allows these motivations to be separated. While our primary objective was to measure overall trust and trustworthiness as behavioural outcomes

based on Berg et al. (1995) paradigm, future research should adopt a triadic design to identify more precisely whether meditation affects trust, altruism, reciprocity, or some combination thereof.

Future work could also consider several methodological improvements. Implementing a control treatment of equal duration would help isolate effects specific to meditation. Incorporating tasks that measure time preferences could clarify whether patience plays a mediating role. Equalizing financial incentives across conditions could further eliminate wealth-related confounds. Finally, increasing the sample size would allow for more precise inferences and subgroup comparisons.

6. Discussion and Conclusion

In this article, we investigated the impact of meditation on prosocial behavior – altruism, trust and different kinds of discrimination – by employing a controlled laboratory experiment on students aged under 30. We engaged participants in five days of guided meditation sessions and compared them to a mind-wandering control group. The meditation intervention, led by a certified MBSR instructor, incorporated techniques such as breath-focused meditation, group dialogues, and loving-kindness meditation. After the meditation or the mind-wandering, participants took part in a series of incentivized experiments to objectively measure their behavioral responses. We hypothesized that individuals who had completed meditation practice (i) exhibit higher levels of altruism, (ii) show diminished discrimination toward marginalized groups represented by individuals of black race, and (iii) demonstrate higher levels of trust and trustworthiness.

This study offers several important contributions to the literature on meditation and its effects on prosocial behavior. First, notwithstanding the limitations discussed in the preceding section, our experiment demonstrates substantive methodological improvements compared to the existing literature in terms of number of participants, their recruitment, the randomized assignment to treatment and control groups, the duration of meditation sessions (five days), the presence of a certified MBSR practitioner, the variety of the tasks, their incentivization, and the controlled environment. These requirements, which are relatively common in some fields, had not, to our knowledge, been jointly implemented in the study of prosocial behavior. By combining self-reported mindfulness assessments, experimental economic tasks, and behavioral measures, our study helps reduce subjective biases and provides a more comprehensive and reliable analysis of meditation's impact on prosociality.

Regarding the results themselves, we find that meditation enhances altruism, strengthens trustworthiness, and reduces racial discrimination, aligning with previous research on the prosocial benefits of mindfulness practices. However, our findings also introduce nuance, showing that some forms of discrimination (against smokers or individuals with scars) are not reduced. Our interpretation is that meditation has a significant effect on those forms of discrimination that are considered particularly sensitive from a moral and social viewpoint — namely, racial discrimination —, while it may have little or no effect on other forms of discrimination. Finally, the effects of meditation observed by our study are short-term, as they are measured immediately following the week-long meditation

intervention. Future research is needed to determine whether these effects persist over longer periods, for instance several weeks after the intervention.

From a practical perspective, our findings suggest potentially important implications for workplaces, education and public policy more broadly. Implementing meditation-based programs in organizations, universities, and community initiatives could represent a promising avenue for reducing discrimination, fostering social cohesion and team-building, and encouraging more responsible behavior. Within firms, such interventions may benefit both employees, through improved interpersonal relations and well-being, and organizations themselves, by contributing to more cooperative and inclusive work environments.

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Appendix A

Descriptive statistics about the previously defined variables is reported in Tables 1 and 2, which also include descriptions of participants' characteristics (gender, age, education level, knowledge on game theory, etc.). Note that we run Mann-Whitney U tests to compare the reported assessment between two groups regarding the discrimination behavior.

Table A1. Descriptive statistics and group comparison using the Mann-Whitney-U test.

	(1)	(2)	(3)	(4)	(5)
	Control group (Mind-wandering)	Treatment group (Mindfulness)			
Variable	Mean (SD)	Mean (SD)	z	p-value	One-tailed p-value
MAAS	54.48	53.77	0.519	0.604	
	(11.897)	(10.67)			
Discrimination measures					
Black_Discrimination_Yes (n, out of)	0.51 (24 out of 47)	0.28 (12 out of 43)	[2.239]	[0.025*]	0.012*
	(0 .505)	(0.454)			
Scar-face_Discrimination_Yes (n, out of)	0.56 (23 out of 41)	0.66 (31 out of 47)	[-0.947]	[0.343]	0.828
	(0.502)	(0.479)			
|Black_Discrimination_Yes - Scar-face_Discrimination_ Yes| (n, out of) // percent	(1 out of 88) 1.14%	(19 out of 90) 21.11%	4.219	0.000***	
Smoking_Discrimination	2.96	2.84	0.593	0.553	0.277
	(0.999)	(1.083)			
Smoking_Discrimination_Yes_1	0.11	0.16	[-1.093]	[0.274]	0.862
	(0.033)	(0.038)			
Smoking_Discrimination_Yes_12	0.30	0.34	[-0.639]	[0.522]	0.738
	(0.048)	(0.048)			
Altruistic measure					
SVO	0.34	0.45	-3.241	0.001***	0.001***
	(0.217)	(0.249)			
SVO_High	0.38	0.62	[-3.206]	[0.001***]	0.001***
	(0.489)	(0.488)			
Observations	91	97			

	(1)	(2)	(3)	(4)	(5)
	Control group (Mind-wandering)	Treatment group (Mindfulness)			
	Mean (SD)	Mean (SD)	z	p-value	One-tailed p-value
Modified trust game					
Panel A: Trusting behavior (level of trust)					
Sender (trustor)					
Transfer to trustee (out of 10)	3.46	4.48	-1.291	0.197	0.098
	(3.045)	(3.667)			
Observations	48	48			
Panel B: Trustworthy behavior (level of trustworthiness)					
Receiver (trustee)					
Back Transfer (out of 30)	2.07	5.37	-3.599	0.000***	0.000***
	(3.782)	(5.593)			
Trustworthiness Relative to Amount Sent = (Back Transfer/ amount sent)	0.40	0.89	-3.455	0.000***	0.000***
	(0.578)	(0.706)			
Trustworthiness Relative to Amount Received = (Returned amount / amount received)	0.13	0.29	-3.455	0.000***	0.000***
	(0.192)	(0.235)			
Observations	43	49			

Notes. Columns 3 and 4 report the z-statistic and p-value corresponding to the Mann-Whitney U test, respectively. For dummy variables and the row labeled |Black_Discrimination_Yes - Scar-face_Discrimination_Yes|, the z-statistic and p-value pertain to a two-sample test for proportions. Column 5 reports the one-tailed p-values. A one-tailed p-value is used when testing a hypothesis in a single direction. The two-tailed p-value is halved to test the effect in the expected direction, and if the effect is in the opposite direction, the one-tailed p-value is calculated as 1−(p-value/2). Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$.

Table A2. Descriptive statistics, individual characteristics.

	(1)	(2)	(3)	(4)
	Control (Mind-wandering)	Treatment (Mindfulness)	z	p-value
Field of study (economics or management/ scientific / literature or arts / other) (%)	32.97/38.46/5.49/ 23.08	51.55/26.8/4.12/ 17.53	1.915	0.056
Female (%)	60.44	49.48	-1.504	0.132
Age	22.87	22.45	1.461	0.144
Sd/min/max	(2.688)/18/29	(3.075)/18/30		
Laboratory experience	0.83	0.58	3.853	0.000***
	(0.373)	*(0.496)*		
Knowledge in game theory	0.26	0.30	-0.535	0.709
	(0.443)	(0.460)		
Observations	91	97		

Notes. The sample includes 188 student subjects (97 in the treatment group and 91 in the control group). Mean values are displayed, standard deviation in parentheses. The continuous variable Age is the age of the subject. Columns 3 and 4 present the z-statistic and p-value associated with the Mann-Whitney U test, respectively. Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$.

Table A3 : Logit regressions, marginal effects

Panel A	Discrimination measures			
With controls	(1)	(2)	(3)	(4)
	Black	Scar-face	Smoking	SVO_high
Treatment	-0.199*	0.161	0.046	0.172*
	(0.094)	(0.095)	(0.050)	(0.068)
Gender (Female)	0.303***	0.360***	-0.050	0.154*
	(0.092)	(0.081)	(0.047)	(0.069)
Literature Major	0.037	-0.408*	0.100	-0.154
	(0.336)	(0.202)	(0.099)	(0.170)
Economics Major	0.287*	0.023	0.080	-0.019
	(0.116)	(0.133)	(0.062)	(0.092)
Science Major	0.117	0.216	-0.186	-0.001
	(0.130)	(0.141)	(0.120)	(0.094)
lnAge	0.004	-0.741	-0.090	-0.172
	(0.455)	(0.412)	(0.263)	(0.334)
Lab experience	-0.010	0.156	0.121	-0.267***
	(0.130)	(0.113)	(0.066)	(0.078)
Knowledge in game theory	-0.251*	0.166	0.072	0.011
	(0.105)	(0.138)	(0.050)	(0.087)

Panel B	Discrimination measures			
Without controls	(1)	(2)	(3)	(4)
	Black	Scar-face	Smoking	SVO_high
Treatment	-0.225*	0.098	0.056	0.225***
	(0.091)	(0.102)	(0.051)	(0.063)
N	90	88	188	188

Notes. Marginal effects of logit regressions with cluster-robust standard errors at the participant level, accounting for intra-cluster correlation. Columns (1-3) include binary outcomes for discrimination (black, scare-face, smoking (Smoking_Discrimination_Yes_1)). Ten out of 188 participants did not respond to questions on black or scare-face discrimination. The *Treatment* variable indicates membership in the meditation group, with log-transformed *Age* as control. Fields of study are categorized into Literature and Arts, Economics and Finance, Science, and Other majors. Panel A includes controls; Panel B excludes them. Cluster-robust standard errors in parentheses. Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$.

Table A4: OLS regressions.

Panel A			Trust game		
With controls	(1)	(2)	(3)	(4)	(5)
	MAAS	SVO score	Transfer	Back transfer	*Trustworthiness Relative to the Amount Received*
Treatment	0.666	0.085*	0.757	2.578**	0.119*
	(1.765)	(0.036)	(0.716)	(0.913)	(0.046)
Gender (Female)	1.325	0.045	-0.367	-0.325	0.004
	(1.583)	(0.035)	(0.764)	(1.071)	(0.047)
Literature Major	-1.190	-0.038	-0.220	2.258	-0.025
	(3.875)	(0.071)	(2.200)	(2.282)	(0.077)
Economics Major	-2.228	-0.022	-0.595	0.410	-0.038
	(2.112)	(0.044)	(0.933)	(1.009)	(0.060)
Science Major	-4.202	-0.036	-0.293	2.217	0.001
	(2.226)	(0.046)	(0.930)	(1.454)	(0.068)
lnAge	6.044	-0.117	-2.233	-3.022	-0.122
	(7.261)	(0.190)	(3.752)	(4.140)	(0.209)
Lab experience	5.162*	-0.091	-1.409	-2.424*	-0.138*
	(2.012)	(0.047)	(1.091)	(1.196)	(0.057)
Knowledge in game theory	-0.187	-0.020	0.469	1.505	0.044
	(1.985)	(0.045)	(0.898)	(1.154)	(0.057)
Intercept	32.699	0.792	12.193	12.151	0.627
	(22.388)	(0.564)	(11.396)	(12.869)	(0.632)
N	188	188	96	92	92
R-sq	0.071	0.106	0.074	0.169	0.204

Panel B			Trust game		
With controls	(1)	(2)	(3)	(4)	(5)
	MAAS	SVO score	Transfer	Back transfer	*Trustworthiness Relative to the Amount Received*
Treatment	-0.834	0.106***	1.021	3.298***	0.162***
	(1.644)	(0.034)	(0.688)	(0.986)	(0.045)
Intercept	54.484***	0.344***	3.458***	2.070***	0.134***
	(1.247)	(0.023)	(0.440)	(0.576)	(0.029)
N	188	188	96	92	92
R-sq	0.001	0.049	0.023	0.106	0.124

Notes. OLS regressions with cluster-robust standard errors at the participant level. The dependent variables in columns (1) and (2) are *Score MAAS* and *Score SVO*, respectively. Columns (3-5) report outcomes related to the trust game: the amount sent by subject A (sender), the amount returned by subject B (receiver), and the *Trustworthiness Relative to the Amount Received* (calculated as *Back transfer / Amount transferred × 3*), respectively. The *Treatment* variable indicates membership in the meditation group, with log-transformed *Age* as control. Fields of study are categorized into Literature and Arts, Economics and Finance, Science, and Other majors. Panel A includes controls; Panel B excludes them. Cluster-robust standard errors in parentheses. Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$.

Table A5: Ordered logit regressions (marginal effects)

	Discrimination measures	
	(1)	(2)
	Smoking	
Treatment		
1._predict	0.005	0.019
	(0.036)	(0.032)
2._predict	0.004	0.015
	(0.027)	(0.026)
3._predict	0.000	0.002
	(0.003)	(0.004)
4._predict	-0.009	-0.036
	(0.066)	(0.060)
Gender (Female)		
1._predict	0.005	
	(0.032)	
2._predict	0.004	
	(0.025)	
3._predict	0.000	
	(0.003)	
4._predict	-0.010	
	(0.060)	

Literature Major		
1._predict	0.034	
	(0.104)	
2._predict	0.026	
	(0.079)	
3._predict	0.003	
	(0.009)	
4._predict	-0.062	
	(0.190)	
Economics Major		
1._predict	0.097*	
	(0.049)	
2._predict	0.075*	
	(0.036)	
3._predict	0.008	
	(0.015)	
4._predict	-0.180*	
	(0.085)	
Science Major		
1._predict	0.006	
	(0.047)	
2._predict	0.004	
	(0.036)	
3._predict	0.000	
	(0.004)	
4._predict	-0.010	
	(0.086)	
lnAge		
1._predict	-0.148	
	(0.172)	
2._predict	-0.113	
	(0.131)	
3._predict	-0.012	
	(0.028)	
4._predict	0.273	
	(0.319)	
Lab experience		
1._predict	0.029	
	(0.039)	
2._predict	0.022	
	(0.029)	
3._predict	0.002	

	(0.005)	
4._predict	-0.054	
	(0.070)	
Knowledge in game theory		
1._predict	0.048	
	(0.042)	
2._predict	0.037	
	(0.031)	
3._predict	0.004	
	(0.008)	
4._predict	-0.088	
	(0.076)	
N	188	188

Notes. Table A3 depicts the results of an Ordered Logit regression analysis (marginal effects) employing cluster-robust standard errors at the participant level, accounting for correlation between observations within clusters. The dependent variables in columns (1–2) correspond to the dummy variable *Discrimination_smokers*. *Discrimination_smokers* reflects opinions on smokers' financial contributions, from (1) higher costs for all smokers to (4) no additional charges, with exceptions for cessation efforts. The *Treatment* variable indicates membership in the meditation group, with log-transformed *Age* as control. Fields of study are categorized into Literature and Arts, Economics and Finance, Science, and Other majors. Panel A includes controls; Panel B excludes them. Cluster-robust standard errors in parentheses. Significance level: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$.

Appendix B

We check for consistency of our estimates by looking at the covariate balancing test for all of our regressions (Tables B1-4). These covariate balancing tests show that, for all dependent variables demonstrating a significant effect of meditation (*Black_Discrimination_Yes, Scar-face_Discrimination_Yes, SVO_High, Relative Back Transfer*), the treatment model successfully balanced the covariates (the p-value of the corresponding $\chi 2$ test is greater than 5% supporting the null hypothesis that the treatment model achieved covariate balance). In practical terms, this means that, based on the covariate balancing test, there is no significant difference in the distribution of covariates between the treated and control groups after implementing the treatment.

Table B1: Covariate balance change in discrimination towards black race (Black_Discrimination_Yes)

	IPW	AIPW	PSMATCH
	Standardized differences	Standardized differences	Standardized differences

	Raw	Weighted	Raw	Weighted	Raw	Weighted
Gender (Female)	-0,26	0,01	-0,26	0,01	-0,26	0,13
Literature Major	0,02	0,01	0,02	0,01	0,02	0,00
Economics Major	0,21	0,00	0,21	0,00	0,21	-0,04
Science Major	-0,34	0,01	-0,34	0,01	-0,34	0,00
lnAge	0,08	0,01	0,08	0,01	0,08	0,14
Lab experience	-0,43	0,00	-0,43	0,00	-0,43	0,03
Knowledge in game theory	0,10	0,02	0,10	0,02	0,10	0,05
$\chi 2$ test for balance	0,897		0,897			
Prob > chi2	0,999		0,999			
N	90		90		90 (Matched = 180)	

Table B2: Covariate balance change in scar face discrimination (Scar-face_Discrimination_Yes*)*

	IPW		AIPW		PSMATCH	
	Standardized differences		Standardized differences		Standardized differences	
	Raw	Weighted	Raw	Weighted	Raw	Weighted
Gender (Female)	-0.18	-0.04	-0.18	-0.04	-0.18	0.05
Literature Major	-0.13	0.02	-0.13	0.02	-0.13	0.11
Economics Major	0.54	0.00	0.54	0.00	0.54	-0.02
Science Major	-0.24	-0.02	-0.24	-0.02	-0.24	-0.08
lnAge	-0.47	-0.04	-0.47	-0.04	-0.47	0.00
Lab experience	-0.72	-0.02	-0.72	-0.02	-0.72	-0.12
Knowledge in game theory	-0.08	0.01	-0.08	0.01	-0.08	0.00
$\chi 2$ test for balance	2.180		2.180			
Prob > chi2	0.975		0.975			
N	88		88		88 (Matched = 176)	

Table B3: Covariate balance change in social values (SVO_high)

	IPW		AIPW		PSMATCH	
	Standardized differences		Standardized differences		Standardized differences	
	Raw	Weighted	Raw	Weighted	Raw	Weighted
Gender (Female)	-0.24	0.01	-0.24	0.01	-0.24	-0.01
Literature Major	-0.06	0.01	-0.06	0.01	-0.06	0.03
Economics Major	0.38	0.00	0.38	0.00	0.38	-0.01
Science Major	-0.27	0.01	-0.27	0.01	-0.27	0.02
lnAge	-0.20	0.02	-0.20	0.02	-0.20	0.01
Lab experience	-0.59	0.01	-0.59	0.01	-0.59	0.00
Knowledge in game theory	0.06	0.03	0.06	0.03	0.06	0.10

χ2 test for balance	2.391		2.391			
Prob > chi2	0.967		0.967			
N	188		188		188 (Matched = 367)	

Table B4: Covariate balance change in Trustworthiness Relative to the Amount Received (Relative Back Transfer)

	IPW		AIPW		PSMATCH	
	Standardized differences		Standardized differences		Standardized differences	
	Raw	Weighted	Raw	Weighted	Raw	Weighted
Gender (Female)	-0.26	-0.03	-0.26	-0.03	-0.26	0.02
Literature Major	-0.13	0.03	-0.13	0.03	-0.13	0.00
Economics Major	0.52	0.00	0.52	0.00	0.52	0.07
Science Major	-0.28	-0.04	-0.28	-0.04	-0.28	0.09
lnAge	-0.45	-0.07	-0.45	-0.07	-0.45	-0.04
Lab experience	-0.74	-0.02	-0.74	-0.02	-0.74	0.02
Knowledge in game theory	-0.08	0.03	-0.08	0.03	-0.08	-0.15
χ2 test for balance	2.256		2.256			
Prob > chi2	0.972		0.972			
N	92		92		92 (Matched = 184)	

Appendix C

Access to all public files (data, analyses, instructions, audios and written transcriptions):

Instructions are available at the following url:

Instructions (EN) (pdf)

Original instructions (FR) (pdf)

The audio narrative of the mind-wandering treatment is available at the following url:
https://osf.io/nr3gf?view_only=d0fec7a10953417396832796bb4ea22a

The audio of a typical meditation session in the meditation treatment group at the following url: https://osf.io/dcvqt?view_only=d0fec7a10953417396832796bb4ea22a

Written transcription of the reading in the mind-wandering treatment at the following url: https://osf.io/yfwnh?view_only=d0fec7a10953417396832796bb4ea22a

Written transcript of a typical meditation session in the meditation treatment group at the following url: https://osf.io/yfwnh?view_only=d0fec7a10953417396832796bb4ea22a

Appendix D:

Figure 8: Discrimination against smokers (dummy: *Smoking_Discrimination_Yes_12)*

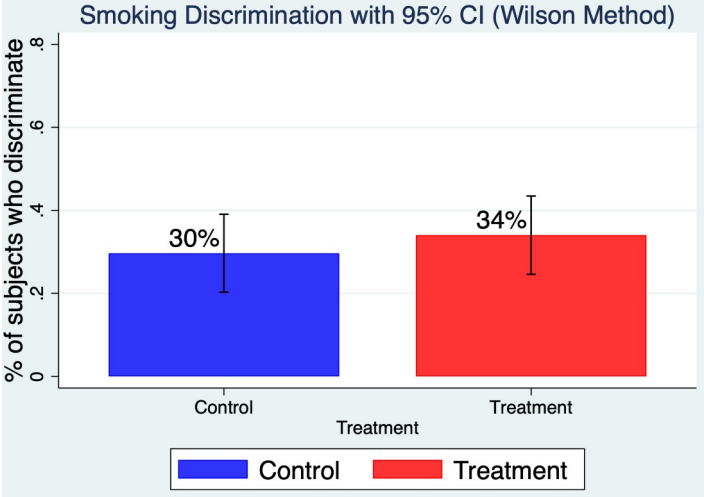
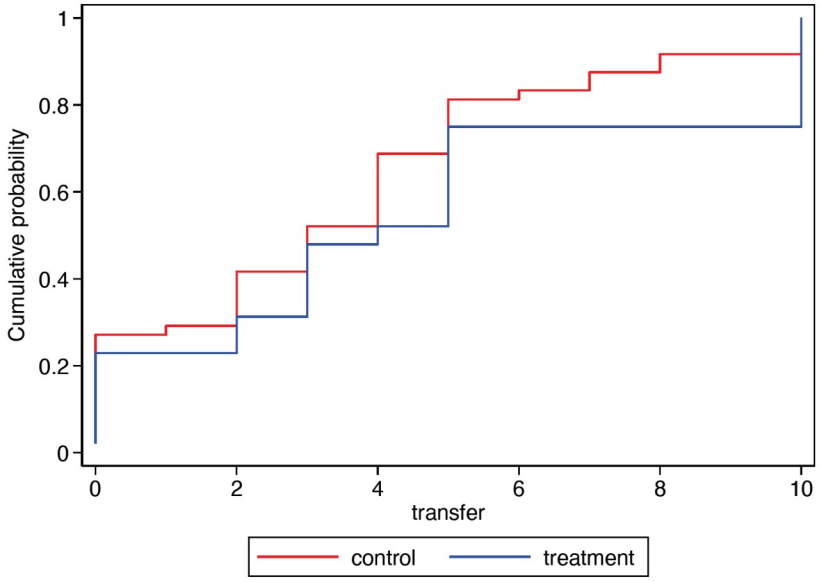


Figure 9: Empirical Cumulative Distribution of Transferred Amounts by Treatment Group



Note: This figure shows the empirical cumulative distribution functions (ECDF) of transfer amounts, in the modified trust game, for the control and treatment groups. The Kolmogorov–Smirnov test fails to reject the null hypothesis of equal distributions (D = 0.167, p = 0.518), indicating no statistically significant difference between the two groups

Table D-1: Proportion Tests for Each Transfer Amount by Treatment Group

	(1)	(2)	(3)	(4)	(5)	(6)
Amount Transfered	Control (n)	Treatment (n)	Control (%)	Treatment (%)	p-value	z
0	13	11	27.1%	22.9%	0.635	0.475
1	1	0	2.1%	0.0%	0.313	1.009
2	6	4	12.5%	8.3%	0.500	0.674
3	5	8	10.4%	16.7%	0.367	-0.902
4	8	2	16.7%	4.2%	0.045*	2.002
5	6	11	12.5%	22.9%	0.182	-1.335
6	1	0	2.1%	0.0%	0.313	1.009
7	2	0	4.2%	0.0%	0.151	1.435
8	2	0	4.2%	0.0%	0.151	1.435
9	0	0	0.0%	0.0%	-	-
10	4	12	8.3%	25.0%	0.028*	-2.196
Total	48	48				

Notes. This table reports two-sample tests of proportions for each transfer level between control and treatment groups. Columns 1 and 2 display the number of participants who transferred each specific amount in the control and treatment groups, respectively. Columns 3 and 4 present the corresponding percentages within each group. Columns 5 and 6 report the *p*-values

and *z*-statistics from two-sided two-sample tests of proportions conducted for each transfer level. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$. Differences are particularly pronounced at the extreme values of the distribution (e.g., €5 and €10), consistent with results discussed in the main text.