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From mental well-being to pro-environmental and distributive behavior? Evidence from a randomized mindfulness intervention

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Introduction: This paper investigates whether mindfulness training can generate spillovers to socially relevant economic behavior, particularly in the domains of environmental protection and income distribution.
Methods: We conduct a randomized controlled trial among university students, offering an eight-week online mindfulness course and measuring outcomes using incentivized behavioral tasks and policy preference surveys.
Results: While the intervention successfully increases mindfulness, we find no robust evidence of causal effects on pro-environmental behavior, distribution choices, or related policy preferences, and only selective effects on the proposed mediating channels.
Discussion: These findings suggest that mindfulness interventions, especially in scalable online formats and voluntary settings, may have limited potential to influence behavior and preferences in these domains.

KEYWORDS
distributive preferences, mindfulness training, pro-environmental behavior, randomized controlled trial (RCT), university students, mental health

1 Introduction

Mindfulness practices have become increasingly widespread in Western societies. Originally rooted in Buddhist contemplative traditions, mindfulness-based interventions (MBIs) have been adapted for secular contexts and are now offered in schools, workplaces, healthcare settings, and through digital platforms reaching millions of users worldwide. An extensive literature, including meta-analyses (Khoury et al., 2015; Vonderlin et al., 2020; Michaelsen et al., 2023), provides robust evidence that MBIs improve mental health by reducing stress, anxiety, and depression while enhancing subjective well-being.

Given these benefits for individual well-being, an important question is whether mindfulness practices also generate spillovers to other socially relevant outcomes and thereby contribute to collective well-being. Mindfulness-based interventions may represent an attractive means of fostering behavior change with relevance for public policy: they are relatively low-cost, scalable, and aim to shape preferences and behavior rather than imposing external constraints. In particular, mindfulness may influence how individuals form and act upon their preferences in ways that matter for how societies respond to major challenges of our time. Two such challenges—both posing

substantial threats to collective well-being—are climate change (IPCC, 2023) and rising economic inequality (Piketty, 2014).

This paper examines whether mindfulness-based interventions can generate spillovers to socially relevant behavior. We conduct a randomized controlled trial among university students to study the causal impact of an eight-week online mindfulness course on incentivized measures of pro-environmental behavior and distributive preferences.

A growing interdisciplinary literature suggests that mindfulness could plausibly influence environmentally relevant behavior. Several studies argue that mindfulness may shape values and preferences—for example by nurturing intrinsic rather than materialistic values and strengthening connectedness to nature—while also increasing present-moment awareness and self-regulation, which may reduce habitual behavior and help translate pro-environmental preferences into actual behavior (Ericson et al., 2014; Fischer et al., 2017; Thiermann and Sheate, 2021). Despite these theoretical considerations, the empirical evidence remains limited and inconclusive. Much of the existing literature relies on cross-sectional studies documenting positive correlations between trait mindfulness and various measures of pro-environmental attitudes and behavior, which do not allow causal inference (see Fischer et al., 2017; Thiermann and Sheate, 2021 for literature reviews; Dhandra, 2019; Wamsler and Brink, 2018 for concrete examples). The small number of intervention studies that do exist typically rely on non-incentivized self-report measures and find null effects (Geiger et al., 2020; Riordan et al., 2022), or, in incentivized tasks, even negative effects of MBIs on pro-environmental behavior (Le Houcq Corbi et al., 2024).

Turning to economic inequality, mindfulness training may affect distributive behavior by shaping individuals' normative views about fairness and social justice, potentially increasing the weight placed on equality when evaluating income distributions.[1] Existing empirical work in this area, however, is narrowly focused on perceptions of fairness in stylized economic games such as the Dictator Game or the Ultimatum Game, where roles are randomly assigned and merit plays no role (e.g. Reb et al., 2010; Weng et al., 2013; Kirk et al., 2016). Moreover, distributive decisions in these games directly affect participants' own payoffs, making it difficult to disentangle fairness views from economic self-interest.

Building on these arguments and the theoretical psychological framework developed by Stenfors et al. (2025), with adaptations inspired by Jansen et al. (2026),[2] we hypothesize that our mindfulness intervention may lead individuals to make choices that are more environmentally friendly and more supportive of more equal income distribution. The proposed mechanism is that increases in mindfulness translate into improvements in mental health (e.g., lower stress and greater flourishing) and in connectedness (to oneself, others, and nature), which in turn shape behavioral outcomes.

With financial support from a large German health insurance provider, we offered an eight-week online mindfulness course at the University of Regensburg with two certified teachers, primarily modeled on a Mindful Self-Compassion course and on the well-established Mindfulness-Based Stress Reduction (MBSR) program. Interested students could apply free of charge by completing an online questionnaire. Applicants were informed that participation was subject to a random allocation procedure because not all eligible applicants could be offered a place. The 304 eligible respondents were therefore randomly assigned to equally sized treatment and control groups, using re-randomization to ensure balance across the most relevant baseline characteristics. Students assigned to the treatment group were offered a place in the mindfulness course, while those assigned to the control group did not receive any intervention.

We measure pro-environmental behavior using three incentivized tasks: a CO_2 certificate game eliciting students' truthful willingness to pay for emissions reductions following the Becker–DeGroot–Marschak method (Becker et al., 1964); a novel travel lottery task capturing real trade-offs between flying and train travel; and a product choice task measuring students' willingness to exert costly effort to select the more environmentally friendly option and thereby reduce emissions. Distributive preferences are measured using a standard fairness allocation task following Almås et al. (2020), in which participants act as impartial spectators and distribute income between two unequally productive Prolific workers. These incentivized measures are complemented by survey questions eliciting self-reported policy preferences related to climate change and income inequality.

In addition, we collected self-reported measures of mindfulness and five potential mediators: self-compassion, perceived stress, flourishing, connectedness to others and connectedness to nature. All games and survey questions were administered both at the application (baseline) stage and one week after the end of the intervention, thereby increasing statistical power through within-subject comparisons. We also collected standard demographic information at baseline, including age, sex, field of study, and semester. The study was pre-registered on the Open Science Framework prior to the start of the intervention.

Our findings are clear. While the intervention achieved its primary goal of improving students' mindfulness, we find no robust evidence of positive causal effects on either pro-environmental or distributive behavior. Policy preferences related to climate change and income inequality likewise remain unaffected. Moreover, the intervention had only selective effects on the hypothesized mediators. Regarding mental health, it reduced perceived stress but did not increase flourishing. As for connectedness, it improved self-compassion and connectedness to nature, but did not significantly affect connectedness to others.

One possible explanation for these null effects is the composition of our sample: the course attracted applicants who

[1]While direct evidence linking mindfulness training to distributive justice views is scarce, related studies provide suggestive support. McCall et al. (2014) show that experienced meditators—primarily trained in loving-kindness and compassion practices—exhibit systematically different responses to unfairness in Ultimatum Game settings. In addition, a meta-analysis by Berry et al. (2020) finds that secular mindfulness training increases prosocial behavior and reduces retaliation and prejudice. Together, these findings suggest that mindfulness-related practices may shape normative evaluations of fairness.

[2]See in particular Supplementary Figure 1, p. 4 of the cited paper, which we report again in Supplementary Appendix B.

already exhibited strong pro-environmental and egalitarian preferences at baseline. This likely reflects the fact that our participants are self-selected university students and that the sample is predominantly female—a group that prior research shows to hold, on average, stronger pro-environmental (Zelezny et al., 2000) and redistributive preferences (Alesina and Giuliano, 2011) than the general population. Women account for around 81 percent of our participants, compared to roughly 61 percent in the overall student population at the University of Regensburg. Taken together, these patterns suggest important limitations of mindfulness interventions in addressing major societal challenges, particularly in contexts characterized by voluntary participation and self-selection. In such settings, mindfulness programs may disproportionately attract individuals who already hold strong pro-environmental and egalitarian preferences, potentially limiting their effectiveness in influencing the very groups whose behavior and attitudes policymakers may wish to change.

More broadly, our results call for a cautious interpretation of the existing literature on mindfulness and socially relevant outcomes. Much of this literature relies on cross-sectional or other non-experimental designs, which provide valuable insights into associations but offer more limited evidence on causal effects. In addition, many studies focus on self-reported attitudes and intentions rather than incentivized behavioral measures. By contrast, our findings from a randomized intervention using incentivized behavioral outcomes suggest that the causal effects of mindfulness interventions in these domains may be more limited or context-dependent than previously suggested. Overall, the relationship between mindfulness, environmental action, and preferences for social equality appears more complex than previously thought and merits further investigation.

The paper proceeds as follows. Section 2 reviews the relevant literature and outlines our contribution. Section 3 describes the experimental design, Section 4 the mindfulness intervention. Section 5 presents our empirical strategy before Section 6 reports the main findings and Section 7 discusses these findings and their implications. Section 8 concludes.

2 Literature review

2.1 Literature in economics

First, we contribute to the emerging economics literature studying the effects of mindfulness-based interventions on mental health and economic behavior. Existing studies show that mindfulness training can improve psychological well-being, including reductions in perceived stress (e.g., Alem et al., 2021; Shreekumar and Vautrey, 2024; Cassar et al., 2024). Evidence on economic and cognitive outcomes is more mixed. Alem et al. (2021), for example, find significant reductions in perceived stress in a randomized trial of an online mindfulness program but mostly insignificant effects on economic preferences such as risk-taking and patience. Charness et al. (2024) document improvements in mindfulness and reductions in stress in a workplace intervention but find limited effects on incentivized cognitive tasks. Similarly, Shreekumar and Vautrey (2024) report improvements in mental health outcomes but limited effects on cognitive performance. In contrast, Cassar et al. (2024) conduct a randomized controlled trial with university students and find positive effects of mindfulness meditation on academic performance, with improvements in students' average grades six months after the intervention. Our study contributes to this literature by examining whether mindfulness-based interventions can influence incentivized economic behavior in domains directly related to collective well-being.

Second, our study relates to a broader literature examining the relationship between well-being and economic behavior (for the widest overview to date, see Lane, 2017). While the bulk of this literature studies the economic determinants of well-being, a growing body of literature examines the reverse direction: experimentally induced positive affect has for example been found to reduce impatience (Ifcher and Zarghamee, 2011), influence saving and consumption decisions (Guven, 2012) and increase cooperation (Drouvelis and Grosskopf, 2016). More recently, Kessler et al. (2022) showed that exogenous increases in happiness led to greater charitable giving. However, this evidence largely relies on short-term, laboratory-induced mood changes. In contrast, we study whether sustained improvements in well-being—through an eight-week mindfulness training—affect incentivized economic behavior. Only a few exceptions consider longer-term mental health, showing that chronic distress can impair decision-making in poverty contexts (Haushofer and Fehr, 2014; Ridley et al., 2020).

Third, our paper connects to a broader literature studying the impact of psychological interventions on economic outcomes. For a recent overview, see Haushofer and Salicath (2023). Psychotherapy—predominantly cognitive behavioral therapy—has, for example, been shown to improve labor market outcomes for individuals with common and severe mental disorders in lower- and middle-income countries (Lund et al., 2024), raise parental investment and financial empowerment for perinatally depressed women in Pakistan (Baranov et al., 2020), and to reduce criminal activity and increase educational attainment in disadvantaged youth in Chicago (Heller et al., 2017). Large parts of this evidence, however, come from psychological interventions on clinical and/or disadvantaged populations, with non-clinical settings receiving less attention so far. Moreover, existing studies focus on economic outcomes related primarily to individual welfare. Our study contributes to this literature by exploring whether a well-being intervention can shape incentivized, collective-focused economic behavior in a non-clinical, non-disadvantaged population.

Finally, our study relates to the literature investigating the determinants of pro-environmental behavior and preferences for redistribution. Prior research shows that these outcomes are shaped by a range of socio-demographic, economic, and psychological factors (e.g., Blankenberg and Alhusen, 2019). For example, education has been shown to causally increase several forms of pro-environmental behavior (Meyer, 2015), while preferences for redistribution depend on factors such as income, beliefs about fairness, and early-life experiences (Alesina and Giuliano, 2011; Alesina et al., 2018). Experimental studies further document systematic heterogeneity in distributive preferences and fairness perceptions (Cappelen et al., 2013; Müller and Renes, 2021). We contribute to this literature by examining whether a learnable and practicable trait such as

mindfulness can influence behavior and preferences in these domains relevant to collective well-being.

2.2 Literature in other fields

The literature most closely related to our paper has primarily emerged outside economics, predominantly in psychology. As discussed above, cross-sectional studies generally report a positive association between mindfulness and pro-environmental attitudes and behavior, whereas causal evidence paints a more mixed picture. Riordan et al. (2022), for example, find that participants in an eight-week Mindfulness-Based Stress Reduction (MBSR) course do not outperform an active control group in terms of gains in self-reported sustainable attitudes, pro-environmental behavior (measured using the Ecological Footprint Calculator), or sustainable well-being (N = 125). Similarly, a sustainability-adapted mindfulness intervention implemented over eight weeks increased mindful experiences in samples of university students (N = 60) and employees (N = 71) and reduced materialistic value orientations, while improving well-being among students. However, the authors find no evidence of direct effects on sustainable consumption behavior, related attitudes, or the gap between them (Geiger et al., 2020).

The study most closely related to ours is Le Houcq Corbi et al. (2024), who conduct a randomized controlled trial with 82 participants, 42 of whom took part in a 31-day non-interactive mindfulness training via an online platform. In contrast to much of the existing literature, the authors employ incentivized behavioral measures. Their experimental tasks include environmental decisions involving trade-offs between monetary payoffs and real CO_2 reductions, environmental donations, interpersonal allocation decisions across social distances, and intertemporal choices measuring future orientation. The study finds no effect of the intervention on future orientation and, if anything, a reduction rather than an increase in pro-environmental and pro-social preferences. These findings suggest that the relationship between mindfulness and socially relevant behavior may be more complex than previously assumed.

Our study builds on this literature by combining several features that have typically appeared only separately in previous work. Like Le Houcq Corbi et al. (2024), we employ incentivized behavioral tasks. At the same time, our intervention resembles the earlier studies in providing live sessions in which participants interact with the instructor and with other participants. In addition, our study features a substantially larger sample and examines not only behavior but also policy preferences related to environmental protection and income distribution.

Evaluating the causal impact of mindfulness training on social-equality and distributive behavior and preferences is, to the best of our knowledge, novel outside the economics literature. Related work instead focuses on compassion training and monetary distribution decisions (Weng et al., 2013) or on non-monetary measures of prosocial behavior (see Donald et al., 2019, for an overview). The few studies linking mindfulness to monetary distribution outcomes are either correlational (e.g., Schindler and Pfattheicher, 2023) or involve decisions that directly affect participants' own payoffs, such as Ultimatum Game settings (Kirk et al., 2016). Our study contributes to this literature by examining the causal impact of mindfulness training on an incentivized measure of distributive fairness that does not involve participants' own payoffs. This design allows us to focus on equality preferences in a performance-based allocation setting while also eliciting policy preferences regarding redistributive measures.

3 The experiment

A free eight-week mindfulness course was offered to students at the University of Regensburg through a collaboration with a major German health insurance provider. Interested students submitted applications by registering and completing an online survey. Applicants who were found to be eligible were randomly assigned to either the control or the treatment group, with the latter receiving course placements. The experiment was preregistered on the Open Science Framework and obtained ethics approval from the university's institutional review board. A detailed description of the experiment follows in this section.

3.1 Recruitment and timeline

The mindfulness meditation course was offered to students across all faculties at the University of Regensburg during the 2024 summer term. Students from other universities could generally participate as well, although the advertising was restricted to the University of Regensburg. Advertisement commenced on April 22, 2024, and was communicated through a university-wide mailing list (see Supplementary Appendix Figure C.1), social media posts, and about two hundred posters that were distributed across campus (see Supplementary Appendix Figure C.2). These advertisements directed students to the course website (see Supplementary Appendix Figure C.3), which contained information on mindfulness, compassion and empathy, as well as the positive well-being effects of practicing them. The website also provided interested students with general information on the course and its expected learning outcomes.[3] It was furthermore indicated that the study would be evaluated by a team of researchers from the University, and that all applicants would have to fill out an online questionnaire at the time of application and after the course. The website included a link to a further page implemented with the survey software Qualtrics, where interested students could submit applications, by registering and completing the baseline survey, until May 5, 2024. Supplementary Appendix Table C.1 provides an overview of the relevant dates and events of the study.

The survey reiterated the same information shared on the website and requested that students specify which time slots suited their schedules during the course period. Students were notified that participation in the course could not be guaranteed to all applicants due to the project's nature and limited resources, and that selecting additional time slots would increase

[3]Obviously our hypothesis that the course might have spillovers on environmental and distributive behavior was never mentioned.

their chances of receiving a spot. In order to advance to the main questions, students were required to accept the conditions of participation: participation was free, though not everyone could receive a spot; allocation to the course was random; survey responses would not influence admission, though only those completing the entire form could participate; agreement to also complete the post-test survey, where they would receive €10 for participation and have the chance to win vouchers worth up to €300; participation in the course was voluntary and students could exit anytime via email with no questions being asked. Applicants also had to accept the privacy policy in order to move on to the questionnaire. Those who were unwilling to accept the conditions of participation and the privacy policy, as well as those who were unable to attend any of the suggested time slots, were informed that they could not participate and were instructed to close the browser window (see Supplementary Appendix Figures C.4, C.5).

Beyond our main outcome measures, the baseline questionnaire included items on sociodemographic characteristics (such as major, semester, sex, age, and political affiliation), as well as instruments to assess the effectiveness of the intervention via a mindfulness questionnaire and to capture potential mediating channels. In particular, we examine mental health (perceived stress and flourishing) and different dimensions of connectedness, namely to oneself (self-compassion), others, and nature.[4] All items from the questionnaires are listed in Supplementary Appendix Table C.2. The baseline survey was pre-tested on student assistants, who completed it in 30–40 minutes. As our participants were permitted to pause and resume the survey, completion times ranged from nearly 17 minutes to nearly eight days.

3.2 Outcome measures

3.2.1 Mindfulness

We assess the effectiveness of the intervention in achieving its primary objective—enhancing students' mindfulness—using the Freiburg Mindfulness Inventory (Walach et al., 2006). Participants reported how frequently, over the past 14 days, their behavior corresponded to 13 scenarios reflecting mindful conduct, using a four-point Likert scale. After reverse-coding one item, we computed the mean score, with higher values indicating greater mindfulness.

3.2.2 Environmental behavior

We assessed participants' willingness to pay for and willingness to exert effort towards pro-environmental outcomes through three games and tasks.

Firstly, in a CO_2 certificate game, participants were informed that from a hypothetical €100 endowment, they could submit an offer between €0 and €100 for a genuine CO_2 certificate from the EU Emissions Trading System. The certificate had a market value of €100 and allowed the emission of 1.2 tons of CO_2. Real-life examples of equivalent quantities were provided. Once the bid was made, a random number between 0 and 100 was generated. If the participant's bid exceeded or equaled the random number, the bid was won. The participants were notified that after the survey, two among all participants would be randomly selected. For each selected participant who won their bid, a CO_2 certificate would be purchased by the researchers and retired, preventing the emission of that amount of CO_2. The two selected participants would also receive any portion of their endowment that they did not bid; if a selected participant had not won their bid, they would instead receive the full €100 endowment as payoff. This created a trade-off between environmental protection and personal monetary gain. The bid amount thus served as a measure of willingness to pay for environmental protection.

Secondly, a travel lottery game employed a nine-row multiple price list design. In each row, participants were instructed to choose between a flight voucher and a train voucher of varying values, ranging from €25 to €200. As the train represents the more environmentally friendly mode of transport, the switching point up to which participants chose the train voucher over a higher-value flight voucher serves as a measure of their willingness to pay for environmental protection. Again, participants were informed that two participants would be randomly selected. For these individuals, one row was again selected at random, and they received a voucher for either a flight or train ticket based on their choice in that row of the multiple price list.

Thirdly, in a product choice game, participants faced a choice between two hypothetical products, one being environmentally neutral and the other being environmentally damaging. Each of the two products displayed three fictional labels. A link provided access to a separate page, where participants could find explanations of the labels, which they had to interpret to determine which of the two products was environmentally friendly, and which was damaging. Participants were notified that for each participant who selected the environmentally friendly product, researchers would purchase a genuine CO_2 certificate from the EU Emissions Trading System valued at €1. The combination of clicking the link and choosing the environmentally friendly product was used as a measure of willingness to exert effort to protect the environment.

3.2.3 Fairness allocation game

We also investigated whether the intervention influenced the participants' fairness views. To assess this, we designed a game in which the participants were informed that the researchers would hire two so-called crowdworkers via the platform Prolific to enter data from other projects into a digital databank. We asked the subjects to split the total wage of €30 based on several scenarios. The sole restriction was that no worker could receive less than €5 and therefore also not more than €25. Scenario one involved worker A performing 1%–10% better than worker B, i.e., entering 1%–10% more data into the database. In scenarios two through six, worker A performed 11%–25%, 26%–50%, 51%–75%, 76%–100%, and more than 100% better than worker

[4]We also elicited preferences over a range of monetary and non-monetary job attributes. For reasons of focus, we do not report these results here. Notably, the treatment did not affect job preferences either.

B, respectively. The participants were told that one participant would be randomly selected and the actual wages for the two crowdworkers would depend on that individual's choice for the scenario that materialized when the crowdworkers performed the task. The deviation of worker A's wage from an equal split of €15 was used to measure allocation preferences. Values closer to zero indicate a more egalitarian distribution, while larger deviations reflect a more inegalitarian one. The sign was reversed so that higher values correspond to stronger egalitarian preferences.

3.2.4 Policy preferences

Finally, we assessed whether treatment altered policy preferences concerning environmental protection, economic growth, and the trade-off between them. We employed 7-point Likert scales to assess several dimensions: first, whether the German government should prioritize environmental protection or economic growth in the future; second, whether the German government should increase or decrease action to protect the environment; and third, how willing the participants would be to reduce conservation areas in order to open them up for economic development. We further elicited preferences for redistribution, asking whether the German government should prioritize reducing economic inequalities or maximizing economic growth in the future. Additionally, using a 6-point Likert scale, we asked whether higher-income citizens should pay a larger, similar, or smaller share of their income in taxes than lower-income citizens. All exact questions and experiments are listed in the Supplementary Appendix.

3.2.5 Secondary measures

In addition to the primary outcome measures, we assessed potential mediating channels highlighted in the psychological literature, in particular in the theoretical framework by Stenfors et al. (2025) and adapted by Jansen et al. (2026), namely mental health and connectedness.

Mental health was captured through perceived stress and flourishing. Perceived stress was measured using the 10-item German Perceived Stress Scale (Schneider et al., 2020), which asks participants how often they experienced certain thoughts and feelings over the past month on a five-point Likert scale. After reverse-coding six items, we calculated the mean, with higher values indicating higher perceived stress. Flourishing was assessed using the Flourishing Scale (Diener et al., 2010; German version: Esch et al., 2013), which comprises eight statements describing aspects of psychological well-being. Participants rated their agreement on a seven-point Likert scale, and we used the mean score, with higher values indicating greater flourishing.

Connectedness was captured along several dimensions: to oneself, others, and nature. Self-connectedness was measured using the German version of the short form of the Self-Compassion Scale (see for the original Neff, 2003; and for the short form Raes et al., 2011), where participants indicated how often they would respond to themselves in a self-compassionate manner in given situations on a five-point Likert scale. After reverse-coding six items, we computed the mean, with higher values indicating greater self-compassion. Connectedness to nature was assessed using the Connectedness to Nature Scale (Pasca et al., 2017), which includes 13 statements rated on a five-point Likert scale. After reverse-coding two items, we calculated the mean, with higher values indicating stronger connectedness to nature. Connectedness to others was measured using the Prosocialness Scale for Adults (Caprara et al., 2005), which consists of 16 items on a five-point Likert scale. The mean of these items was used, with higher values indicating stronger connectedness to others.

3.3 The data

3.3.1 Sample size and randomization

The final sample size was determined through the following screening process: when the application period closed on May 5, 2024, we had received 337 completed surveys, from which we excluded 33 participants for various reasons: four were not students, 23 had missing email addresses, five did not consent to the data protection agreement, and one withdrew from the study. This screening process yielded 304 eligible participants. We randomly allocated participants to the treatment and the control group (152 each), using the rerandomization approach based on Morgan and Rubin (2012) to ensure that both groups were balanced along most baseline characteristics. Participants assigned to the treatment group were offered a place in the eight-week mindfulness course, whereas participants assigned to the control group did not receive any intervention. In the post-test survey, we received 221 eligible responses, including only participants who completed the survey in full and submitted it no more than once. When matching post-test responses to pre-test surveys, we lost an additional 17 observations due to inconsistent personal identifiers entered by participants, resulting in a final sample size of 204.

3.4 Balance checks, representativeness and attrition

Supplementary Appendix Table A.1 presents mean values for the eight outcome variables, as well as demographics and the six secondary variables. Mean values are presented separately for the control and treatment subsamples, and the overall sample. The p-values from t-tests of mean differences across the two subgroups show that almost all variables were well balanced, with few exceptions. Two preference outcomes ("Government Climate Action" and "Progressive Taxation") were less well balanced, likely due to strong ceiling effects. Additionally, the dummy variable for members of the subject group "Philosophy and Arts-History-Social Sciences" ($p = 0.013$) was not well balanced. With few participants from several subject groups, balance for this variable could not be achieved. Finally, connectedness to nature was not balanced, with the treatment group scoring slightly lower than the control group. Of the 304 subjects, 152 were each allocated to the control and treatment groups.

The final sample of 204 participants who completed the questionnaire and could be matched with baseline data was comprised of 111 participants (54.4%) from the treatment group and 93 (45.6%) from the control group. Attrition in the questionnaire is, therefore, not balanced between the treatments ($\chi^2 = 4.83$, $p = 0.028$). Despite this difference, we do not find it to affect balance between the treatments on any relevant variable at baseline (see Supplementary Appendix Table A.2).[5]

All students enrolled at a university were eligible to apply for a place in the meditation course. As the course was only advertised on the campus of the University of Regensburg, 202 of the 204 individuals in the final sample were students of the university. One more studied at the neighboring university of applied sciences and the affiliation of the remaining individual could not be identified. Supplementary Appendix Table A.3 compares our sample with the overall student body of the University of Regensburg, allowing us to assess whether specific subgroups were more likely to select into the study. We observe that students from the natural sciences, mathematics, law, business, and economics were under-represented in our experiment, whereas students of the humanities were markedly over-represented. This imbalance is likely connected to a second representativeness issue: strong female self-selection into our study. The humanities, followed by Language-Literature-Culture, contributed the largest number of participants, and both faculties had a predominantly female student body. Although women constitute roughly 61% of the university's total student population, they account for about 81% of our sample. Female students from all faculties except mathematics and physics were more likely than their male counterparts to apply to our course. The average age of students of the University of Regensburg during the summer term of 2024 was 23.61 years (N = 18,877). Our average sample age of 24.8 years thus seems uncharacteristically high, indicating that the advertised mindfulness course attracted more advanced and older students.

With an average PSS score of 31.8 (range: 10 to 50), compared to an average of 22.75[6] in a representative sample of 2,463 Germans in 2016 (Klein et al., 2016), our final sample reports relatively high levels of perceived stress. Our sample was also slightly less mindful than a representative sample of the German population ($N = 1,012$; Walach et al., 2025), with a mean total score of 34.3 compared to 37.99. With regard to self-compassion, our sample (mean = 32.6) compares relatively well to that of a US student population ($N = 415$, mean = 36.0; Raes et al., 2011). To the best of our knowledge, no representative norm data exists for this scale at this time.

4 The intervention

The intervention consisted of an 8-week mindfulness course that drew from several mindfulness traditions. The first four of the eight weekly sessions were derived from the renowned MBSR course. The course was developed in the 1970s in the United States by Jon Kabat-Zinn (Kabat-Zinn, 1982). The program's main goal is to reduce stress and improve overall mental health through practices including formal sitting mindfulness meditation, body awareness, and mindful movement exercises. Today, the program is used in a plethora of settings worldwide. Sessions five and six were based on the Mindful Self-Compassion (MSC) course, which teaches self-compassion and emotional resilience.[7] Session seven was derived from Rick Hanson's neuroplasticity training (Hanson, 2013), while session eight integrated topics from MBSR and MSC with elements from the Thich Nhat Hanh tradition. These final four sessions were selected because they target and train internal transformative qualities, with a particular emphasis on connectedness. Two experienced MBSR and MSC teachers led the courses.

Each session lasted 90 minutes and followed a consistent structure. Sessions began with a guided mindfulness practice, followed by paired or small-group discussions, which were subsequently reflected upon and shared in the larger group. Instructors then provided a brief theoretical introduction to the week's topic, followed by a second brief practice section. Participants received weekly assignments to support their progress, which often included recording and uploading 15- to 20 minutes videos documenting their meditation.

The course was held live via Zoom with no pre-recorded components, allowing direct interactions between instructors and participants and among participants themselves, and enabling instructors to adapt sessions as needed. To maintain manageable group sizes, participants were divided into five groups, ranging from 29 to 31 participants each. One group met on Mondays, with two groups meeting on Tuesdays and two on Thursdays. The course comprised eight weekly sessions.

Of the 111 participants allocated to the treatment group, 79 (71.2%) attended at least four of the sessions. Supplementary Appendix Table A.4 reports weekly attendance for the final sample of 204 students, averaging 63.6%, and showing a gradual decline over time, yet remaining above 50% even in the final three weeks.

5 Empirical strategy

We start by assessing whether our intervention achieved its primary objective, namely increasing students' mindfulness. Second, we examine whether the intervention had any spillover effects on our primary measures: environmental behavior, distributive behavior, and policy preferences. Third, we analyze potential channels through which the intervention may affect our primary outcomes, namely, mental health and connectedness. For each outcome, we report a variety of OLS regression results, employing three specifications that differ in structure and controls, following the approach used in Cassar et al. (2024)[8]. All outcome variables were standardized to a

[5]An exception is the product choice task in which the control group is marginally more likely to make a pro-environmental choice ($p = 0.07$) making it harder to identify a treatment effect for this variable

[6]Fitted to our scale. The authors calculated a mean of 12.75 on a scale from 0 to 40.

[7]https://self-compassion.org/the-program/

[8]For the product choice task, logistic regression was employed instead of OLS given the binary nature of the dependent variable, with otherwise identical model specifications.

mean of zero and a standard deviation of one to facilitate comparison across measures.

Endline (E) specifications regress the post-intervention measure on a dummy variable for treatment that equals one if the student was assigned to the treatment group, and zero if the student was assigned to the control group. This specification includes no control variables and simply compares mean outcomes between the treatment and control groups at endline.

Value added (VA) specifications additionally control for the baseline value of the dependent variable, as well as age, gender, and fixed effects for study program (twelve categories).[9] In this setting, the treatment coefficient represents the average treatment effect conditional on these variables.

Finally, the first difference (FD) specification presented in Supplementary Appendix A regresses the change in each outcome between baseline and endline on the treatment dummy, thereby isolating variation on the individual level and eliminating bias from any time-invariant confounders, whether observed or unobserved.[10]

These specifications account for baseline imbalances with increasing stringency. The endline model assumes no baseline differences, VA adjusts for observed covariates, and FD eliminates all time-invariant differences through differencing.

TABLE 1 Mindfulness regression results.

Variable	E (1)	VA (2)	FD (3)
Treatment	0.384***	0.340***	0.241*
	(0.137)	(0.123)	(0.136)
Pre-Test	–	0.546***	–
		(0.059)	
Age	–	0.007	–
		(0.009)	
Gender	–	0.103	–
		(0.151)	
Intercept	0.255**	0.113	0.334***
	(0.112)	(0.371)	(0.105)
Study Program FE	No	Yes	No
Observations	204	199	203
R-squared	0.039	0.345	0.016

This table shows OLS regressions of mindfulness on the treatment dummy. Model (1) does not include any control variables. Model (2) controls for baseline mindfulness, age, gender, and study program fixed-effects (12 dummies). Model (3) uses the change in mindfulness (endline–baseline) as dependent variable and does not include any control variables. All models include a constant. Heteroskedasticity robust standard errors are in parentheses. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

6 Results

6.1 Effects on mindfulness

Prior to assessing the impact of our intervention on the primary variables, we examine whether the course was successful in increasing students' mindfulness. Table 1 shows highly significant treatment effects ($p < 0.01$ for endline and value added, $p < 0.1$ for the first-difference specification), with increases of approximately 0.24 to 0.38 standard deviations compared to the control group, depending on the specification. Hence, the results validate the intervention's success in achieving its main goal.

6.2 Effects on pro-environmental outcomes

Table 2 presents results from OLS regressions of participants' pro-environmental behavior along the three measures described above. The endline models [(1), (3), (5)] have endline behavior as the dependent variable and include the binary treatment variable. The value-added specifications [(2), (4), (6)] repeat this analysis but additionally control for baseline behavior, age, and gender, as well as study-program fixed effects. Note that the sample for the travel lottery in models (3) and (4) is further restricted to individuals exhibiting monotonic choice behavior, defined as switching at most once in the multiple price list. This restriction yields a sample of 177 individuals (86.7%), of whom 97 are in the treatment group (87.4%) and 80 in the control group (86.0%).

As can be seen, we find positive point estimates (0.03 to 0.26 SD) for all three domains and for all six regressions, though none reached statistical significance ($p > 0.41$ for all specifications except for the value-added specification of the travel lottery, for which $p = 0.11$). In the first-difference specifications reported in the Supplementary Appendix Table A.5, the results look more favorable with treatment effects significant at the 5% level for the travel lottery and at the 10% level for the product choice task.

For the two measures of pro-environmental policy preferences[11], the OLS regressions are presented in Table 3 and Supplementary Appendix Table A.5: we find positive point estimates (0.02 to 0.16 SD), yet none are statistically significant ($p > 0.29$).

As we observe ceiling effects in the distributions of responses to the two questions, we also perform ordered logistic regressions for the two outcomes [cf. columns (1)–(4) in Supplementary Appendix Table A.8]. Results are qualitatively similar to the OLS results, which adds robustness to our findings.

Taken together, these findings indicate that the intervention did not robustly increase pro-environmental behavior or

[9]Although semester fixed effects were considered, they were excluded because the survey did not differentiate between bachelor's and master's enrollment, meaning the same semester number is not comparable across students. Results are robust to this exclusion.

[10]The first-difference specification produces exactly the same coefficient and standard error for the treatment dummy as a difference-in-differences model with standard errors clustered on the individual level [using the "cluster" option in Stata that is based on Liang and Zeger (1986)] of the form $y_i = \alpha + \beta\ \text{TreatGroup}_i + \gamma\ \text{Time}_i + \delta(\text{TreatGroup}_i * \text{Time}_i) + \varepsilon$.

[11]Regression analysis was not conducted for the item assessing participants' willingness to reduce conservation areas for economic development, as the responses exhibited pronounced ceiling effects, resulting in insufficient variance to support meaningful model estimation.

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TABLE 2 Environmental behavior variables regression results.

	CO_2 bid		Travel lottery		Product choice	
Variable	E (1)	VA (2)	E (3)	VA (4)	E (5)	VA (6)
Treatment	0.105	0.113	0.025	0.140	0.041	0.256
	(0.149)	(0.138)	(0.142)	(0.087)	(0.299)	(0.361)
Pre-Test	–	0.485***	–	0.668***	–	2.225***
		(0.085)		(0.048)		(0.399)
Age	–	0.015	–	0.011	–	0.048
		(0.014)		(0.007)		(0.038)
Gender	–	0.121	–	−0.095	–	0.259
		(0.153)		(0.092)		(0.483)
Intercept	−0.163	−0.771*	−0.047	−0.846***	0.693***	−3.107**
	(0.115)	(0.449)	(0.109)	(0.278)	(0.220)	(1.261)
Study Program FE	No	Yes	No	Yes	No	Yes
Observations	204	199	177	172	204	199
R-squared	0.002	0.256	0.000	0.665	–	–
Pseudo-R^2	–	–	–	–	0.000	0.194
Log-Likelihood	–	–	–	–	−129.1	−100.7

This table shows OLS regressions of the CO_2 bid game (Models 1 and 2) and the travel lottery (3 and 4) on the treatment dummy, as well as logit regression of the product choice task (5 and 6) on the treatment dummy. Models (1), (3) and (5) do not include any control variables. Models (2), (4) and (6) control for the baseline values of the dependent variable, age and gender, and study program fixed effects (12 dummies). All models include a constant. Heteroskedasticity robust standard errors for OLS and standard errors for logit are in parentheses. Pseudo-R^2 is McFadden's $(1 - \mathcal{L}/\mathcal{L}_0)$.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

preferences among treated participants. While some effects emerge in first-difference specifications, these findings do not consistently carry through to the endline or value-added models. These null results will be discussed further in Section 7.

6.3 Effects on preferences for social equality

Table 4 presents results from OLS regressions of the incentivized distribution task and the two measures of social equality-related policy preferences. For the distribution game, represented by columns (1) and (2), the results from the six scenarios, in which participants were asked to distribute a set budget of wages to two workers, were aggregated over the scenarios, i.e., the mean absolute deviation from an equal distribution was taken over the six stages. We do not find a significant treatment effect, with the treatment coefficient being close to zero for both the endline and the value-added specifications.[12] First-difference estimates (Supplementary Appendix Table A.6) likewise show no significant treatment effect. Results for the individual stages are presented in Supplementary Appendix Table A.9. The point estimates do not show a clear trend over the stages and we do not observe a significant treatment effect for any of the individual stages.

For the two measures of social equality-related policy preferences, we again observe consistently positive but not significant treatment coefficients. As we once again observe ceiling effects, particularly for the tax preferences, we again perform ordered logistic regressions (cf. Supplementary Appendix Table A.8). Results are qualitatively similar to the OLS results, which adds robustness to the finding of no significant overall treatment effect. In first-difference specifications (Supplementary Appendix Table A.6), treatment is significant at the 10% level for the inequality-vs-growth measure but remains insignificant for tax progressivity. Possible reasons for the observed null effects of our intervention on behavior and preferences regarding social equality are discussed in Section 7.

6.4 Effects on secondary outcomes

Despite the absence of effects on the primary outcomes, we report results for the secondary outcomes for completeness. As shown in Table 5, the findings for both mental health and connectedness are mixed. The intervention reduced perceived stress by approximately 0.28 standard deviations ($p < 0.05$). In contrast, coefficients close to zero indicate no discernible effect on flourishing ($p > 0.82$). Turning to connectedness, the intervention had a statistically significant positive effect on self-compassion, increasing it by about 0.37 standard deviations ($p < 0.01$). The intervention further significantly increased

[12]As a robustness check, we re-estimated both specifications using the signed rather than the absolute deviation from an equal distribution. Qualitative results are unchanged and available upon request.

TABLE 3 Environmental preferences variables regression results.

Variable	Climate prot. v. growth		Government climate action	
	E (1)	VA (2)	E (3)	VA (4)
Treatment	0.032	0.015	0.154	0.095
	(0.138)	(0.102)	(0.145)	(0.112)
Pre-Test	–	0.689***	–	0.664***
		(0.049)		(0.073)
Age	–	0.008	–	0.002
		(0.009)		(0.006)
Gender	–	−0.004	–	−0.027
		(0.123)		(0.099)
Intercept	−0.055	−0.641	−0.099	−0.207
	(0.101)	(0.399)	(0.122)	(0.246)
Study Program FE	No	Yes	No	Yes
Observations	203	198	203	198
R-squared	0.000	0.550	0.006	0.448

This table shows OLS regressions of two environmental policy preference measures on the treatment dummy: (1)–(2) for prioritizing environmental protection vs. economic growth, (3)–(4) for increasing vs. decreasing government climate action. Models (1) and (3) do not include any control variables. Models (2) and (4) control for the baseline values of the dependent variable, age and gender, and study program fixed effects (12 dummies). All models include a constant. Heteroskedasticity robust standard errors are in parentheses. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

connectedness to nature by about 0.23 standard deviations ($p = 0.041$), though only for the value-added specification. We find no evidence of treatment effects on connectedness to others, however.[13] First-difference specifications reported in Supplementary Appendix Table A.7 add robustness: treatment effects for self-compassion and connectedness to nature are significant at the 1%, level for perceived stress at the 5% level. There are once again no significant treatment effects on flourishing and connectedness to others.

Overall, these results suggest that the intervention had only selective effects on the proposed mediating channels, reducing stress, and increasing self-compassion and connectedness to nature but leaving flourishing and connectedness to others unaffected.[14]

7 Discussion

There are several possible explanations for why the intervention did not produce the hypothesized effects on our primary outcomes.

A first explanation concerns the link between the psychological channels targeted by the intervention and the behavioral outcomes we measure. The course produced significant but selective effects on the proposed mediating mechanisms: it increased mindfulness, self-compassion, and connectedness to nature, and reduced perceived stress, but had no discernible impact on flourishing or connectedness to others. This raises the question of why significant changes in psychological states did not translate into changes in incentivized behavior or policy preferences, which arguably constitute a more demanding test than questionnaire-based outcomes. Either the link between these psychological mechanisms and consequential economic behavior is weaker than the literature suggests, or the magnitude of the changes was not sufficient to generate downstream behavioral effects across the full sample. Supplementary evidence on these two interpretations is mixed. Moderation analyses interacting the treatment with individual changes in the four secondary dimensions most clearly affected by the intervention yield interaction terms that are almost uniformly insignificant, suggesting that even participants with comparatively large psychological gains did not exhibit stronger behavioral effects.[15] More consistent with the second interpretation, however, Supplementary Appendix Tables A.10–A.12 report regressions on primary and secondary outcomes replacing the treatment indicator with dummies for high and low attendance, split at the median level.[16] While these estimates are clearly subject to endogeneity concerns, they reveal marginally significant positive treatment effects on several environmental outcomes among participants with higher attendance, whereas low attenders show no such effects. Moreover, higher attendance is associated with a substantially larger reduction in perceived stress and stronger increases in mindfulness, self-compassion, and connectedness to nature. Taken together, this pattern may suggest that a stronger intervention—one that fosters more consistent engagement, for instance through an in-person format—might have been more effective in generating behavioral change.

A second explanation concerns the selective nature of our sample. Participation in the course was voluntary, and applicants were predominantly female. This matters because women tend, on average, to hold stronger pro-environmental and more egalitarian preferences than men. However, re-estimating our main specifications using post-stratification weights that align the sample's gender composition with that of the University of Regensburg student population yields results that are very similar to the unweighted estimates (see Supplementary Appendix Tables A.13–A.17). This suggests that the overrepresentation of women is unlikely to be the sole or primary driver of our findings. Nevertheless, participants may differ from the broader student population along other dimensions associated with environmental concerns and

[13]A more detailed empirical analysis and discussion of the effects of the intervention on these psychological outcomes can be found in Jansen et al. (2026).

[14]This pattern may not be surprising given that the mindfulness training was based on MBSR and MSC, suggesting that spillovers to related constructs beyond those directly targeted by the course may be limited.

[15]Specifically, we interact the treatment with individual changes in mindfulness, self-compassion, perceived stress, and connectedness to nature. Across all four moderators and all primary outcomes, only isolated interaction terms reach statistical significance. The full results are available upon request.

[16]The split closest to the median assigns participants with 7-8 sessions to high attendance (44%) and those with 6 or fewer to low attendance (56%).

TABLE 4 Social equality variables regression results.

Variable	Distribution game (Agg)		Reduce ineq. v. growth		Tax progressivity	
	E (1)	VA (2)	E (3)	VA (4)	E (5)	VA (6)
Treatment	−0.126	−0.013	0.081	0.189	0.113	0.055
	(0.134)	(0.096)	(0.150)	(0.115)	(0.152)	(0.122)
Pre-Test	–	0.669***	–	0.665***	–	0.595***
		(0.054)		(0.069)		(0.071)
Age	–	0.018	–	−0.000	–	−0.006
		(0.013)		(0.009)		(0.012)
Gender	–	0.317***	–	−0.081	–	−0.213
		(0.114)		(0.152)		(0.178)
Intercept	0.126	−1.040***	−0.030	−0.365	−0.131	0.349
	(0.107)	(0.340)	(0.119)	(0.324)	(0.111)	(0.344)
Study Program FE	No	Yes	No	Yes	No	Yes
Observations	204	199	203	198	201	195
R-squared	0.005	0.550	0.002	0.470	0.003	0.357

This table shows OLS regressions of the distribution game (aggregated over the six stages) (Models 1 and 2) and two redistribution policy preference measures on the treatment dummy: (3)–(4) for prioritizing reducing economic inequalities vs. maximizing economic growth, (5)–(6) for whether higher income citizens should pay more or less taxes than lower income citizens. Models (1), (3) and (5) do not include any control variables. Models (2), (4) and (6) control for the baseline values of the dependent variable, age and gender, and study program fixed effects (12 dummies). All models include a constant. Heteroskedasticity robust standard errors are in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE 5 Secondary variables regression results.

Variable	Perc. stress		Flourishing		Self-compassion		Conn. to nature		Conn. to others	
	E (1)	VA (2)	E (3)	VA (4)	E (5)	VA (6)	E (7)	VA (8)	E (9)	VA (10)
Treatment	−0.277**	−0.286**	−0.035	0.016	0.397***	0.367***	0.073	0.234**	0.044	−0.049
	(0.132)	(0.127)	(0.148)	(0.118)	(0.133)	(0.107)	(0.154)	(0.114)	(0.142)	(0.109)
Pre-Test	–	0.419***	–	0.631***	–	0.587***	–	0.717***	–	0.669***
		(0.074)		(0.064)		(0.061)		(0.062)		(0.061)
Age	–	−0.005	–	0.010	–	0.009	–	0.007	–	0.010
		(0.009)		(0.010)		(0.010)		(0.013)		(0.012)
Gender	–	0.102	–	−0.020	–	−0.001	–	0.063	–	0.237*
		(0.161)		(0.153)		(0.151)		(0.147)		(0.137)
Intercept	−0.086	−0.175	0.227**	0.241	0.190*	−0.158	0.232*	0.504	0.071	−0.081
	(0.108)	(0.333)	(0.113)	(0.388)	(0.100)	(0.322)	(0.120)	(0.391)	(0.104)	(0.381)
Study Program FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	203	198	203	198	204	199	204	199	204	199
R-squared	0.022	0.247	0.000	0.421	0.042	0.449	0.001	0.541	0.000	0.502

This table shows OLS regressions of several secondary measures on the treatment dummy: perceived stress (Models 1 and 2), flourishing (3 and 4), self-compassion (5 and 6), connectedness to nature (7 and 8) and connectedness to others (9 and 10). Models (1), (3), (5), (7) and (9) do not include any control variables. Models (2), (4), (6), (8) and (10) control for the baseline values of the dependent variable, age and gender, and study program fixed effects (12 dummies). All models include a constant. Heteroskedasticity robust standard errors are in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

distributive preferences. Consistent with this possibility, our participants already displayed high baseline levels of pro-environmental behavior and support for more equal income distributions, leaving limited scope for further improvement (see Supplementary Appendix Figure B.2). Participants showed a high willingness to pay for environmental protection at baseline: 80.9% bid at least €50 in the CO_2 game, and 61% were willing to forgo at least €50 in order to receive the more

environmentally friendly travel voucher. Similarly, about 70% already prioritized focusing on climate change (70.6%) and reducing inequality (69.6%) over maximizing economic growth. Moreover, 86.3% of subjects stated that higher-income individuals should pay a larger or significantly larger share of their income in taxes than individuals with lower incomes. In other words, the intervention may have attracted precisely those individuals whose preferences and behaviors were already aligned with the outcomes the course might otherwise have been expected to influence. This pattern raises broader questions about the extent to which voluntary mindfulness programs can reach and affect the groups whose attitudes and behavior policymakers may be most interested in changing.

A final consideration concerns the incentive structure of our behavioral measures. While all primary outcomes involved real monetary consequences, payments were determined through a random-payment mechanism and the resulting expected incentives were therefore modest.[17] Consequently, social desirability concerns may not have been fully eliminated and may still have influenced participants' decisions. However, such concerns would likely bias behavior toward more pro-environmental and egalitarian choices. To the extent that mindfulness participants felt a stronger desire to behave in socially desirable ways, this would tend to inflate rather than attenuate positive treatment effects. The fact that we nevertheless find largely null effects therefore suggests that social desirability bias is unlikely to account for our results. At the same time, participants were aware that their choices could affect actual payments, implying that the tasks provided incentives for truthful preference revelation that are absent in purely hypothetical survey measures. Moreover, experimental evidence suggests that once real incentives are introduced, behavior is often relatively robust to variation in stake size (Camerer and Hogarth, 1999) and to whether incentives are implemented for all participants or only for a randomly selected subset (Charness et al., 2016; Clot et al., 2018). Nevertheless, future research could examine whether mindfulness interventions affect pro-environmental and distributive behavior under higher-stakes conditions.

8 Conclusion

In this paper, we examined whether an eight-week online mindfulness intervention can generate spillovers to socially relevant economic behavior. Using a randomized controlled trial among university students and a set of incentivized measures, we tested whether participation in the course affected pro-environmental behavior, distributive choices, and related policy preferences. While the intervention successfully increased mindfulness, we find no robust evidence that it changed behavior or preferences in these domains. The intervention did produce selective significant effects on several proposed mediating channels—perceived stress, self-compassion, and connectedness to nature all improved significantly—but these psychological shifts did not translate into downstream behavioral change. The evidence on why this is the case is mixed: moderation analyses suggest that individual psychological gains are not linked to behavioral effects, while attendance patterns point toward a possible relationship between treatment intensity and outcomes. In addition, the scope for treatment effects may have been constrained by the selective composition of our sample, which consisted of self-selected (and mostly female) students who already held comparatively strong pro-environmental and egalitarian preferences at baseline.

Taken together, our findings point to limits in the capacity of mindfulness-based interventions to generate spillovers from improved mindfulness to consequential social and economic behavior. Even when such interventions succeed in their primary aim, this does not necessarily imply downstream effects on incentivized behavior related to climate protection or income distribution. The relationship between mindfulness, mental health, connectedness, and socially relevant behavior therefore appears more contingent and complex than some of the existing literature might suggest.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving humans were approved by the Ethics Committee of the University of Regensburg. The studies were conducted in accordance with the local legislation and institutional requirements. The participants provided their informed consent to participate in this study.

Author contributions

LC: Conceptualization, Formal Analysis, Methodology, Writing – original draft, Writing – review & editing. TN: Conceptualization, Data Curation, Formal Analysis, Methodology, Writing – original draft, Writing – review & editing. PC: Conceptualization, Methodology, Writing – original draft, Writing – review & editing.

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[17]Average response-based payouts per participant amounted to €2.65 at baseline and €3.84 at endline, comprising direct monetary payoffs, travel vouchers, and purchases of CO_2 certificates on participants' behalf.

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Conflict of interest

The author(s) declared that this work was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Correction Note

This article has been corrected with minor changes. These changes do not impact the scientific content of the article.

Generative AI statement

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Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/frbhe.2026.1875793/full#supplementary-material

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