

Bohmann, Sandra; Fiedler, Susann; Kasy, Maximilian; Schupp, Jürgen; Schwerter, Frederik

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*Sandra Bohmann, Susann Fiedler, Maximilian Kasy, Jürgen Schupp,
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Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

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Cash Transfers, Mental Health and Agency: Evidence from an RCT in Germany*

Sandra Bohmann, Susann Fiedler, Maximilian Kasy,
Jürgen Schupp, Frederik Schwerter†

July 11, 2025

Abstract

Mental health and wellbeing are unequally distributed in high-income countries, disadvantaging low-income individuals. Unconditional, regular, and guaranteed cash transfers may help address this inequality by promoting financial security and agency. We conducted a preregistered RCT in Germany, where treated participants received monthly payments of EUR 1,200 for three years. Cash transfers improve mental health and wellbeing. These effects are substantively large and robust. Cash transfers also improve perceived autonomy, savings, prosocial giving, time with friends, and sleep. Our findings suggest that cash transfers improve mental health and wellbeing if they empower agency and meaningful life changes.

1 Introduction

Good mental health and wellbeing enable individuals to be resilient, realize their abilities, learn well, work productively, contribute to their communities and live healthier lives (Prince et al., 2007; WHO, 2021). Even in high-income countries, however, mental health and wellbeing outcomes are unequally distributed: economically disadvantaged individuals tend to have poorer mental health and wellbeing (Fryers et al., 2003; Marmot, 2013). Unconditional, regular, and guaranteed cash transfers are a promising policy to address this inequality. Such cash transfers reduce uncertainty about future income, and empower agency of recipients. Greater financial security and agency may allow individuals to better fulfill personal needs and pursue their own values. In addition, greater financial security and autonomy directly benefit mental health and wellbeing (Ryan and Deci, 2017; Frey et al., 2004; Bartling et al., 2014; Guan et al., 2022).

Randomized controlled trials (RCTs) in low- and middle-income countries indeed find that cash transfers can lead to improvements in mental health and wellbeing (Baird et al., 2013;

*The RCT in this paper received ethics approval by the University of Cologne (Reference Number 20008FS) and was pre-registered in the AEA RCT Registry at <https://www.socialscisceregistry.org/trials/7734>. Bohmann: DIW; sbohmann@diw.de. Fiedler: WU Wien; susann.fiedler@wu.ac.at. Kasy: Oxford University; maxkasy@gmail.com. Schupp: DIW and FU Berlin; jschupp@diw.de. Schwerter: Frankfurt School of Finance and Management and University of Cologne; f.schwerter@fs.de. †Corresponding author.

Haushofer and Shapiro, 2016; Christian et al., 2019; McGuire et al., 2022). However, contemporaneous cash-transfer RCTs suggest that such improvements do not extend to high-income countries (West and Castro, 2023; Jaroszewicz et al., 2024; Miller et al., 2024; Balakrishnan et al., 2024). One explanation, consistent with classical insights from psychology (Ryan and Deci, 2001; Diener and Biswas-Diener, 2002), is that cash transfers may be less impactful in high-income countries because basic needs are already met and material aspirations grow as income rises. Alternatively, previous cash transfer programs in high-income countries may not have been generous enough to fully realize the potential mental health and wellbeing effects of regular, guaranteed, and unconditional transfers.

In this paper, we present evidence from a generous cash transfer program that we implemented in Germany in the form of a preregistered RCT. The treatment group received regular, guaranteed, and unconditional cash transfers of EUR 1,200 on a monthly basis for a total of three years. Cash transfers improve mental health and wellbeing during the study period. These improvements remained roughly constant over the duration of the cash transfer program and persisted up to six months after the final transfer. In additional analyses, we find that cash transfers empower recipients to make meaningful changes in their lives: They save more, give more to others, they spend more time with friends, and sleep longer and better. In addition, cash transfers improved perceived autonomy of recipients. Overall, our findings suggest that cash transfer programs can lead to lasting improvements of mental health and wellbeing if they are regular, guaranteed, unconditional, and generous enough to empower agency.

In Section 2, we describe the design of our RCT. We selected a study sample of German participants with demographic characteristics similar to those in contemporaneous studies (Miller et al., 2024). In particular, we focused on individuals with monthly net incomes between EUR 1,100 (around the poverty line) and EUR 2,600 (approximately 2.4 times the poverty line), making our sample economically less well-off than the average German. We estimate causal effects using a stratified random assignment design. We used participants' responses to a baseline survey for the stratification. Members of the treatment group received monthly payments of EUR 1,200 for a total of three years. Cash transfers increased their baseline monthly household income by 46% to 110%. After the baseline survey, we measured self-reported mental health and wellbeing of the treatment and control group in seven survey waves spread over six-months intervals. The first wave was six months after the beginning of the cash transfer program and the last wave was six months after the program finished.

We measure mental health using the WHO-5 Well-Being Index (Staeher, 1998) and five items from the Perceived Stress Scale (PSS-10, Cohen et al.; Cohen, 1983; 1988). The WHO-5 WBI has high clinimetric validity and is a sensitive and specific screener for depression (Topp et al., 2015). The PSS questions are widely used to measure perceived stress (Yılmaz Koğar and Koğar, 2024). Both questionnaires correlate with physical health outcomes and health-related behaviors (Topp et al., 2015; Richardson et al., 2012; Tenk et al., 2018). In addition, we measure participants' eudaimonic and evaluative wellbeing. To this end, we adapted questions from the

German Socio Economic Panel (Richter et al., 2017) on participants’ perceived purpose in life and participants’ general life satisfaction, and their satisfaction in several life domains.

In Section 3, we present the main results of our RCT. We first present treatment effects averaged across all survey waves during the three-year program. Unconditional, regular, and guaranteed cash transfers improve mental health by 0.306 SD, purpose in life by 0.250 SD, and life satisfaction by 0.426 SD during the study period. These treatment effects are statistically significant based on robust standard errors, and on Neyman and Fisher’s exact p -values that account for our stratified random treatment assignment (Athey and Imbens, 2017). The effect on mental health is present in both questionnaires. The effect on life satisfaction is present for general life satisfaction and for several life domains: income, health, sleep, and leisure. We also find directionally positive but statistically insignificant improvements of social satisfaction and work satisfaction.

Second, we consider the dynamics of treatment effects during the cash transfer program. The effects are mostly constant in time. There are two important exceptions. (i) The improvement in income satisfaction decreases over time. (ii) While we find no average effect on work satisfaction over the full study period, we find a delayed positive effect during the final 18 months of the program.

Third, we study the temporal persistence of treatment effects six months after the final cash transfer. To quantify this, we aggregate our mental health, purpose in life and life satisfaction into a single mental health and wellbeing (MHW) index. The MHW index improved by 0.379 SD during the program and by 0.331 SD six months after the cash transfer program, implying persistence of 87% of the size of the average treatment effect during the program.

In Section 4, we present evidence that unconditional, regular, and guaranteed cash transfers empower participants to implement and experience life changes. In particular, we document five patterns: (i) Recipients of cash transfers state that they feel greater financial security, and their self-reported household finance data suggests that they saved one third of their monthly cash transfers for the future. (ii) Treated participants shared seven to eight percent of their cash transfers with others, in the form of financial support to family and friends and charitable giving. (iii) Cash transfers allowed recipients to spend 1.3 hours per week more time with their friends. (iv) Treated participants tended to sleep longer (between 1.2 and 2.3 hours per week) and invested more in recreational activities. (v) Cash transfers improved the perceived autonomy of recipients. Importantly, there is ample evidence that mental health and wellbeing benefit from greater financial security (Jachimowicz et al., 2022; Guan et al., 2022), prosocial behavior (Dunn et al., 2008; Hui et al., 2020), social connectedness (Harandi et al., 2017; Park et al., 2020; Waldinger and Schulz, 2023; Annan and Archibong, 2023), sleep and recreational activities (Scott et al., 2021; Kuykendall et al., 2015), and autonomy (Inglehart et al., 2008; Steckermeier, 2021). These five patterns provide suggestive evidence for the mechanisms driving the main treatment effects. Cash transfers improve mental health and wellbeing directly by

allowing recipients to experience greater financial security and agency and indirectly by allowing them to implement meaningful life changes.

In Section 5, we compare our results to the related literature. Miller et al. (2024) study monthly cash transfers of USD 1.000 for a total of three years in the US and *rule out* mental health improvements beyond 0.028 SD. In addition, cash transfers in their US setting do not lead to changes in recipients’ time use and household finances (Vivalt et al., 2024; Bartik et al., 2024). We argue that cash transfer programs must empower recipients to make life changes in order to achieve enduring improvements in mental health and wellbeing.

We also contribute to the literature that estimates the causal effect of money on mental health and wellbeing more broadly. Such estimates are mainly based on lottery winnings and provide a mixed picture: mental health is found to be affected positively (Gardner and Oswald, 2007; Apouey and Clark, 2015), not at all (Lindqvist et al., 2020) or, potentially even, negatively (Raschke, 2019); life satisfaction is reported to be affected positively (Apouey and Clark, 2015; Lindqvist et al., 2020; Dwyer and Dunn, 2022), or not at all (Kuhn et al., 2011). The unconditional, regular, and guaranteed cash transfers that we study seem to be particularly suited to improve mental health and wellbeing. They enhance the predictability of financial security, help sustain reasonable spending habits, limit potential stress and anxiety related to the management of large lump sum payments.

In Section 6, we discuss our findings in light of internal and external validity concerns. Regarding internal validity, we show that our results are robust to multiple-hypothesis-testing adjustments and argue that alternative explanations do not provide reasonable accounts for our findings. In particular, experimenter demand effects, social desirability concerns, Hawthorne effects, a disappointed control, and differential attrition are unlikely to be the drivers of our results (as supported by additional analyses). Regarding external validity, we discuss limitations of the generalizability of our results.

We conclude in Section 7 and discuss policy implications of our findings.

2 Design and Analyses

We estimate the causal effect of a generous, regular, guaranteed, and unconditional cash-transfer program on recipients’ mental health and wellbeing in a preregistered, randomized controlled trial in Germany.^{1,2} We display an overview of the timeline of the RCT in Figure 1. In the following, we describe the setup of the RCT in more detail and discuss the analyses that we use to evaluate the RCT.

¹We preregistered the RCT at the AEA registry: <https://www.socialsciscenceregistry.org/trials/7734>

²We study the labor supply effects of the cash transfers of the RCT in a companion paper (Bernhard et al., 2025). The description of the RCT in the companion paper hence overlaps in parts with the description of the RCT below.

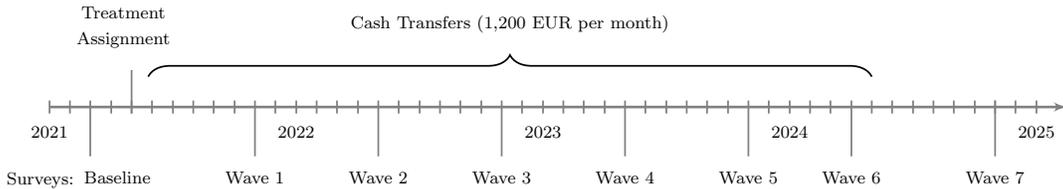


Figure 1: Timeline of the RCT

2.1 Study sample and treatment assignment

The RCT was implemented with our implementation partner, the German NGO Mein Grundeinkommen e.V. MG, which is funded through private donations, financed the cash transfers. Prior to the RCT, MG made regular cash transfers of EUR 1,000 per month for a single year—which we have no data on and are hence not evaluated in this paper—to 818 randomly assigned applicants, making MG a credible partner to finance the cash transfers in our RCT.

We used a multi-step sampling and treatment assignment procedure to construct our study sample. The steps in this procedure are (i) a public call and voluntary registration of potential participants, (ii) selection of a subsample based on demographic and economic eligibility criteria, (iii) stratified sampling of eligible registrants to construct a representative baseline sample, members of which were then invited to fill out a baseline survey, (iv) blocking of participants in the baseline sample who have a completed baseline survey, based on a rich set of baseline covariates, and random assignment to treatment within blocks, and (v) selection of a representative subsample of blocks based on the budget constraints of the study.

Public call In August 18, 2020, MG and the German Institute for Economic Research (DIW Berlin) publicly announced the launch of the RCT during Spring/Summer 2021 and made a public call to apply for participation in the RCT. The announcement included a description of the main features of the study: Selected participants of the study would be randomly assigned to a treatment group or a control group; treatment and control groups would participate in biannual online surveys; members of the treatment group would receive monthly payments of 1,200.00 EUR for three years; members of the control group would receive monetary incentives to complete the surveys; additional research activities may be offered. During signup, we collected the following screening information: Age, gender, education, monthly net income, number of people living in their household, number of kids, zip code, and their general attitude towards universal basic income. Between August 18 and December 10 in 2020, 2,048,370 potential participants registered in response to this public signup call.

Eligibility criteria We then invited a subsample of registered individuals (called “baseline sample”) to complete the baseline survey. Selection into the baseline sample is based on the following eligibility criteria with respect to participants’ demographic and socioeconomic char-

acteristics.

- (i) Participants have to be between 21 and 40 years old.
 - (ii) Participants are required to be German residents and to have a monthly net income between 1,100.00 and 2,600.00 EUR
 - (iii) Individuals who, at the time of the baseline survey, were receiving social benefits for long term unemployment are excluded from participation.³
 - (iv) Households of size greater than one, and individuals with dependent children, are excluded from participation. Participants of our study whose household size changes, or who have a child, will, however, not lose their participation status.
- * Participants whose status on these criteria changed after treatment assignment stayed in the study sample.

Eligibility criteria (i)-(iii) ensured comparability to contemporaneous cash-transfer studies (Miller et al., 2024; Vivalt et al., 2024; Bartik et al., 2024), (iii) also implied that our cash transfers would not reduce potential longterm unemployment benefits of recipients, and (iv) ensured that our cash transfers were relatively more generous than in other cash-transfer studies (Miller et al., 2024; Vivalt et al., 2024; Bartik et al., 2024).

Baseline survey and stratification Among the potential participants who satisfied these criteria, our implementation partner invited 20,000 individuals to our baseline survey (see Online Appendix OA1.1). We considered 8,971 of these participants, who completed the baseline, in the randomized block assignment. Amongst the 11,029 individuals we did not consider, 49% were no longer eligible and/or had missing responses in the baseline variables used for stratification, 33% did not complete the baseline survey, and 17% did not sign the data sharing consent form.

We use the answers to the baseline survey to sort participants into homogenous blocks. In particular, we use questions on mental health, wellbeing, gender, socio-economic background, financial household, and political attitudes. We display the full list of stratification variables used for blocking in Online Appendix OA1.2.

We construct blocks that minimize the total sum of Mahalanobis distances in the stratification variables between pairs of observations within blocks.⁴ We chose the number of participants per block to be 32 because of the following constraints and considerations: (i) Our implementation partner was willing to finance 107 treated participants and 1589 control participants (see Online Appendix OA1.3). (ii) Within each block, we assign two participants to the treatment

³Given current benefit eligibility rules, such social benefits would have been cut by up to the full amount of the cash transfer, if these individuals were to participate in our study. The net transfer to such individuals would thus have been significantly below the expenditure for MG.

⁴The Mahalanobis distance of two covariate vectors x_1 and x_2 that are realizations of a random vector X is given by $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot \text{Var}(X)^{-1} \cdot (x_1 - x_2)}$. We use the R package *blockTools* (Moore and Schnakenberg, 2021) for blocking.

group to be able to calculate standard errors for the sample average treatment effect (see Athey and Imbens, 2017, and our discussion of inference below), but not more than two participants to keep the number of blocks as large as possible and each block as homogenous as possible.

This procedure results in 281 blocks, while our project budget allows for 53 blocks. We select the final set of blocks in two steps. First, we discard all blocks with a maximum within-block distance greater than 14 (to avoid poorly matched observations), as well as one block with less than 32 observations. Second, amongst the remaining 273 blocks, we selected a representative sample of blocks that matched the distribution of gender, education groups, and income groups of eligible participants in the screening survey.

Outcome	Treated	Control	Difference	SE	t-statistic	p-value
Age 29-32	0.355	0.331	0.024	0.048	0.498	0.619
Age 33-40	0.336	0.373	-0.036	0.048	-0.757	0.449
Female	0.477	0.412	0.065	0.050	1.290	0.197
German citizen	0.981	0.981	0.000	0.014	0.014	0.989
UBI proponent	0.505	0.547	-0.042	0.050	-0.837	0.403
Tenure	0.766	0.766	0.000	0.043	0.005	0.996
Education: Hauptschule	0.037	0.038	0.000	0.019	-0.020	0.984
Education: Realschule	0.215	0.214	0.001	0.041	0.035	0.972
Education: Fachabitur	0.243	0.241	0.002	0.043	0.044	0.965
Education: Abitur	0.037	0.054	-0.016	0.019	-0.843	0.399
Net monthly income	1944.888	1925.767	19.121	40.181	0.476	0.634
Monthly saving	271.607	296.407	-24.800	24.742	-1.002	0.316
Wealth	25327.103	25392.157	-65.054	4450.093	-0.015	0.988
Debt	10170.374	9077.122	1093.252	2655.173	0.412	0.681
High financial security	0.327	0.312	0.016	0.047	0.329	0.742
Working for money	0.935	0.944	-0.010	0.025	-0.383	0.702
In training or education	0.178	0.151	0.027	0.038	0.691	0.489
In vocational training	0.411	0.432	-0.021	0.050	-0.421	0.674
Searching work	0.037	0.038	0.000	0.019	-0.020	0.984
Sick days	7.776	10.850	-3.075	1.152	-2.669	0.008
Weekly hours worked	37.826	37.346	0.480	1.458	0.329	0.742
Political preferences (PC1)	0.015	0.142	-0.127	0.142	-0.893	0.372
Political preferences (PC2)	0.164	0.053	0.112	0.125	0.893	0.372
Subjective wellbeing (PC1)	-0.360	-0.129	-0.231	0.183	-1.263	0.207
Body mass index	24.656	25.452	-0.797	0.490	-1.627	0.104
Transfers to others	363.551	330.733	32.819	103.753	0.316	0.752
Donations in 2020	100.664	96.562	4.101	21.002	0.195	0.845
Binary gender	1.000	1.000	0.000	0.000	-	-

Table 1: Balance in average baseline covariates between the treated and control group in our study sample. Standard errors, t-statistic, and p-value are “naive” (ignore blocked assignment).

Final sample Within each of the remaining 53 blocks, we randomly assigned two participants to the treatment group. Because the funding allowed for an odd number of treated participants, one additional individual from one block was chosen at random to participate in the treatment. After the selected participants in the treatment group and control group were informed about their treatment status, seven individuals in the control group wanted to be excluded from the study sample, one individual in the treatment group resigned his/her spot in the treatment group because of a job opportunity outside of Germany, and one individual in the treatment group could not be reached. For each of these two missing individuals, we sampled one individual from the control group within the same block, to receive the corresponding treatment status.

Our final study sample hence includes 107 participants in the treatment group and 1,580 participants in the control group. We find that the treatment and control group are well-balanced in stratification variables, see Table 1.

2.2 Treatment conditions and outcomes

Cash transfers Members of the treatment group received cash transfers of EUR 1,200, paid monthly, over the course of three years. Importantly, the cash transfers were tax-free, see Appendix OA1.4. Cash transfers increased the baseline monthly household income of the treatment group by 46% to 110%. There were no conditions attached to receiving the cash transfers, apart from completing six semi-annual surveys. All surveys refer to online surveys that lasted between 15 and 20 minutes and were distributed to participants by a professional German online survey company. Members of the control group did not receive cash transfers, and were asked to complete the same six semi-annual surveys. For every completed survey, control participants received an incentive of EUR 10, plus an additional payment of EUR 30 if they completed all six surveys. We incentivized survey participation of our control group to limit attrition, which we discuss in detail below. To study the persistence of treatment effects six months after the end of the cash transfer program, we invited all participants to a seventh survey and paid both treated and control participant who completed the survey EUR 20.

Mental health and wellbeing outcomes To construct our mental health and wellbeing outcomes, we use all questions on participants' mental health, purpose in life, and life satisfaction that we measured in the baseline survey ($t = 0$), the survey waves during the cash transfer program ($1 \leq t \leq 6$), and the final survey wave six months after the program ($t = 7$). The wording of these questions is stated in Online Appendix OA2.1.

Our mental health outcomes are based on the WHO-5 questionnaire (Staeher, 1998) and a subset of the PSS-10 questionnaire (Cohen et al., 1983). The WHO-5 questionnaire has high clinimetric validity and is a sensitive and specific screener for depression (Topp et al., 2015). The PSS questions are widely used to measure perceived stress (Yılmaz Koğar and Koğar, 2024). Both indicators correlate with physical health outcomes and health-related behaviors (Topp et al., 2015; Richardson et al., 2012; Tenk et al., 2018). The endpoints of the Likert response

scales are “0” and “5” for the WHO-5 questions and “1” and “5” for the PSS questions. After inverting negatively framed PSS questions, larger response values imply better mental health for each question of both questionnaires. We then obtain scores for both sets of questions (dep_{it} and str_{it}) by summing the respective response values for each participant i and survey wave t .

Our wellbeing outcomes are based on a single-item question that elicits participants’ perceived meaningfulness of their life (eudaimonic wellbeing) and on seven single-item questions that elicit separate aspects of participants’ evaluative wellbeing: their general life satisfaction and their health, sleep, work, income, leisure, and social satisfaction. The endpoints of the response scales for these questions are “1” and “11.” Larger response values imply greater purpose in life or satisfaction, respectively. We obtain scores for purpose in life (p_{it}), general life satisfaction (g_{it}), health satisfaction (h_{it}), sleep satisfaction (s_{it}), income satisfaction (i_{it}), work satisfaction (h_{it}), social satisfaction (o_{it}), and leisure satisfaction (l_{it}) for each participant i and survey wave t based on the corresponding response values.

We construct our outcomes based on these mental health, purpose in life and life satisfaction scores. In doing so, we consider the mental health, purpose in life and life satisfaction scores both individually for each wave $t > 0$ and averaged on the participant-level across all waves during the cash transfers program, $0 < t < 7$. We then subtract from each of these scores its corresponding baseline score and divide by the study sample’s standard deviation of the baseline score, see Table A.1 in Appendix A. Our outcomes are standardized changes relative to the baseline. This allows us to estimate treatment effects that are adjusted for treatment-control imbalances at baseline, which remained despite stratification, and to directly compare our findings to the related literature that primarily presents treatment effects in standard deviations. We also present the results based on outcomes in levels, as we discuss below.

2.3 Analyses

Our analyses follow three preregistered steps. First, we estimate treatment effects by considering the strata-level difference in mean outcomes between the treatment group and control group, averaged across strata. Second, we determine statistical significance of treatment effects based on robust standard errors, and Neyman and Fisher’s exact p -values (Athey and Imbens, 2017) that account for stratified assignment. Third, we apply the Benjamini-Hochberg procedure (Benjamini and Hochberg, 1995) to adjust for multiple hypothesis testing.

Object of interest We denote individual treatment assignment by D and outcomes by Y . Our primary object of interest is the sample average treatment effect

$$\delta = \sum_i (Y_i^1 - Y_i^0), \tag{1}$$

for the individual-level outcomes Y_i for individuals i and corresponding potential outcomes Y_i^0, Y_i^1 . Our primary estimator is based on block-level differences in mean outcomes, averaged

across blocks b :

$$\begin{aligned}\bar{Y}_b^1 &= \frac{1}{n_b^1} \sum_{i: b_i=b} D_i Y_i & \bar{Y}_b^0 &= \frac{1}{n_b^0} \sum_{i: b_i=b} (1 - D_i) Y_i \\ \hat{\delta}_b &= \bar{Y}_b^1 - \bar{Y}_b^0 & \hat{\delta} &= \frac{1}{N} \sum \hat{\delta}_b,\end{aligned}\tag{2}$$

where n_b^1 and n_b^0 are the number of treated and untreated individuals in block b , and N is the number of blocks.

Inference Inference is based on two alternative methods, both of which yield valid inference for the sample average treatment effect: (i) Standard errors and confidence intervals based on a normal approximation, and (ii) randomization inference.

To calculate a standard error for $\hat{\delta}$ as an estimator of δ , we calculate block-level standard-errors (allowing for arbitrary heteroskedasticity), and aggregate:

$$\begin{aligned}\hat{\sigma}_b^{2,1} &= \frac{1}{n_b^1 - 1} \sum_{i: b_i=b} D_i \cdot (Y_i - \bar{Y}_b^d)^2 & \hat{\sigma}_b^{2,0} &= \frac{1}{n_b^0 - 1} \sum_{i: b_i=b} (1 - D_i) \cdot (Y_i - \bar{Y}_b^d)^2 \\ \hat{\sigma}_b^2 &= \frac{1}{n_b^1} \hat{\sigma}_b^{2,1} + \frac{1}{n_b^0} \hat{\sigma}_b^{2,0} & \hat{\sigma}^2 &= \frac{1}{N} \sum_b \hat{\sigma}_b^2.\end{aligned}\tag{3}$$

95% confidence intervals for δ are then calculated as

$$CI = [\hat{\delta} - 1.96 \cdot \hat{\sigma}^2, \hat{\delta} + 1.96 \cdot \hat{\sigma}^2].\tag{4}$$

Neyman p -values (denoted p -N in our tables) are similarly based on these standard errors and the normal approximation for the distribution of $\hat{\delta}$.

Our second approach toward inference is based on permutations of treatments, that is, based on randomization inference. This approach allows us to test the null hypothesis that the intervention had no effect of any kind, that is, $Y_i^1 = Y_i^0$ for all individuals i and potential outcomes Y_i^1, Y_i^0 .

We re-assign treatment at random *within* each of the blocks b . For this counterfactual treatment assignment, we re-calculate any given test-statistic. Repeating this process many times, we calculate the share of re-assignments for which the test-statistic is bigger than the realized value of the test-statistic. This share is the Fisher p -value (denoted p -F in our tables) for the null hypothesis of no effects.

Compound hypotheses In order to deal with the issue of multiple testing in a principled manner, we preregistered to use the Benjamini–Hochberg procedure, which allows us to control the false discovery rate, that is, the share of rejected hypotheses which in fact hold true. This procedure works as follows. We sort the p -values for each of the m hypotheses tested by size,

Outcome	Treated	Control	ATE	SE	t-stat	<i>p</i> -N	<i>p</i> -F	n treated	n control
Aggregates									
MHW Index	0.305	-0.074	0.379	0.077	4.912	0.000	0.000	107	1477
Mental Health	0.322	0.016	0.306	0.074	4.122	0.000	0.002	107	1477
Purpose in Life	0.116	-0.134	0.250	0.087	2.888	0.004	0.004	107	1476
Life Satisfaction	0.312	-0.114	0.426	0.075	5.657	0.000	0.000	107	1477
Aggregate components									
WHO-5 Depression	0.355	0.036	0.319	0.079	4.066	0.000	0.000	107	1445
PSS Stress	0.261	-0.034	0.295	0.080	3.706	0.000	0.002	107	1470
General Life Satisfaction	0.275	-0.076	0.351	0.080	4.377	0.000	0.000	107	1445
Domain Satisfaction Index	0.311	-0.113	0.424	0.087	4.894	0.000	0.000	107	1477
Domain satisfactions									
Health Satisfaction	-0.017	-0.308	0.291	0.088	3.315	0.001	0.002	107	1477
Sleep Satisfaction	0.191	-0.099	0.290	0.088	3.290	0.001	0.000	107	1477
Work Satisfaction	-0.046	-0.189	0.143	0.096	1.484	0.138	0.142	107	1471
Income Satisfaction	0.540	-0.011	0.551	0.108	5.099	0.000	0.000	107	1477
Leisure Satisfaction	0.408	0.163	0.245	0.092	2.663	0.008	0.006	107	1476
Social Satisfaction	0.116	-0.009	0.125	0.072	1.739	0.082	0.104	107	1476

Table 2: We report average treatment effects (ATE) in standard deviations on our outcomes as changes relative to the baseline, see outcome definitions in Table A.1. Inference is based on robust standard errors (SE), and Neyman (N) and Fisher’s exact (F) *p*-values. We reject the null of no effect for the aggregated mental health, purpose in life, and life satisfaction outcomes, the WHO-5 depression scale, the PSS stress scale, the domain satisfaction index, general life satisfaction, and health, sleep, income, and leisure satisfaction. We cannot reject the null for work and social satisfaction. All results are robust to multiple-hypothesis-testing adjustments, as we show in detail in the supplementary materials.

resulting in ordered values $P_{(j)}$. For a critical value α , we find the largest value k such that

$$P_{(k)} \leq \frac{k}{m} \alpha. \tag{5}$$

We reject the null hypothesis for all $i = 1, \dots, k$.

We preregistered to apply this procedure separately for different groups of outcomes and stated that one group of outcomes refers to mental health and wellbeing.

3 Main results

To obtain estimates for treatment effects during the cash transfer program, we estimate average treatment effects on outcomes across all waves during the cash transfer program (see the right-most column in Table A.1). We show these results in Table 2. Our mental health and wellbeing (MHW) index improves by 0.379 standard deviations, mental health by 0.306 SD, purpose in life by 0.250 SD, and life satisfaction by 0.426 SD. Improvements in mental health are separately present for the WHO-5 depression screener (0.319 SD) and the PSS questions (0.295 SD). Improvements are also separately present for general life satisfaction (0.351 SD) and the domain satisfaction index (0.424 SD). Income satisfaction improves by 0.551 SD, health satisfaction by 0.291 SD, sleep satisfaction by 0.290 SD, and leisure satisfaction by 0.245 SD.

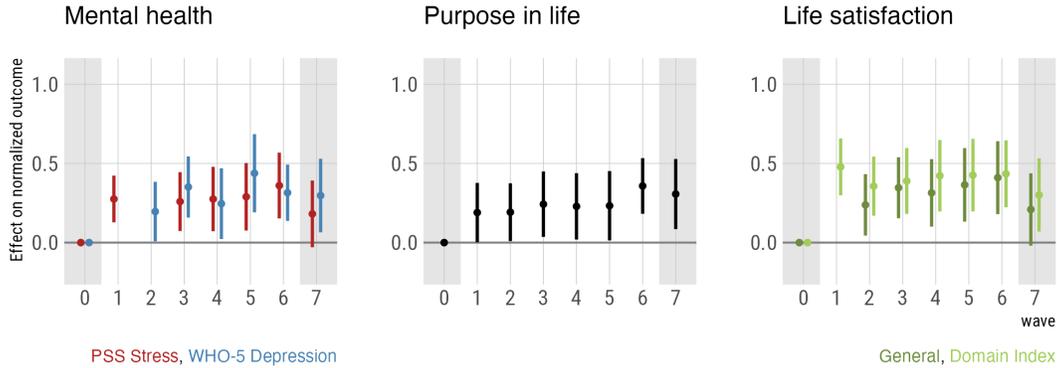


Figure 2: Average treatment effects (ATE) in standard deviations and 95% confidence intervals for our main by-wave outcomes (WHO-5 Depression, PSS Stress, Purpose in Life, the Domain Satisfaction Index, and General Life Satisfaction) as changes relative to the baseline, see outcome definitions in Table A.1. Since our outcomes are changes relative to baseline, there are no treatment differences at baseline.

All of these improvements correspond to statistically significant treatment effects based on Newman and Fisher’s exact p -values, see Table 2. Social and work satisfaction also improve, respectively by 0.125 SD and 0.143 SD, but these treatment effects are not statistically significant. However, for work satisfaction, the average of treatment effects over time masks a delayed effect during the final three waves, as we discuss below.

We also report average outcomes separately for treated participants and control participants in Table 2. We find that treatment effects for the mental health outcomes, income satisfaction, leisure satisfaction, and social satisfaction are mainly the result of greater positive changes relative to the baseline for treated participants than for control participants. Treatment effects for general life satisfaction, sleep satisfaction, and purpose in life are the result of both positive changes for treated participants and negative changes for control participants. Health and work satisfaction depreciated on average relative to the baseline both for treated and control participants, but more extensively for control participants than for treated participants.

Robustness: outcomes in levels Our results are robust to considering outcomes in levels, that is, without adjustment for treatment-control imbalances at baseline, which remained despite stratification, see Table B.2 in Appendix B.1. There are two differences, however: The treatment effect on purpose in life in levels is only weakly statistically significant and the treatment effect on social satisfaction is statistically significant. These different results reflect small and statistically insignificant treatment-control imbalances at baseline for purpose in life (-0.132 SD) and social satisfaction (0.055 SD).

Dynamics during the cash transfer program We display treatment effects separately for all waves and outcomes in Figures 2 and 3. As treatment effects are calculated by adjusting for baseline imbalances in outcomes, there are, by construction, no outcome differences at baseline. Figures 2 and 3 show that the improvements remain fairly constant over time during the cash transfer program. There are two notable exceptions: (i) The improvement in income satisfaction decreases over time. While the treatment effect on income satisfaction was 0.680 SD six months into the cash transfer program, it was nearly half in size (0.350 SD) at the end of the cash transfer program. (ii) While we find at most small treatment effects on work satisfaction during the first one and half years (0.046 SD in wave 1, -0.012 SD in wave 2, and 0.12 SD in wave 3), cash transfers improve work satisfaction to a greater extent during the final one and half years of the cash transfer program (0.24 SD in wave 4, 0.24 SD in wave 5, and 0.23 SD in wave 6).

Persistence after the cash transfer program Treatment effects remain mostly present six months after the final cash transfers, but they reduce slightly in size, see Table B.3 in Appendix B.2. To quantify this, we compare the treatment effects on our MHW index during the cash transfer program and after the cash transfer program. The MHW index improved by 0.379 SD during the program and by 0.331 SD six months after the cash transfer program. Thus, the treatment effect on mental health and wellbeing remained at 87% of the average effect size during the cash transfer program. While the treatment effects on mental health (0.258 SD) and life satisfaction (0.276 SD) are smaller—yet still statistically significant—than during the cash transfer program, and the treatment effect on purpose in life increased to 0.307 SD six months after the program.

4 Agency and life changes

In exploratory analysis, we present evidence that cash transfers empower recipients to implement and experience meaningful life changes. These analyses are based on recipients’ retrospective accounts of their experiences with the cash transfers, treatment effects on participants’ self-reported household finance data, treatment effects on participants’ time use data, and treatment effects on participants’ perceived autonomy. Five patterns emerge that directly relate to drivers of mental health and wellbeing discussed in the literature. We hence view these patterns to provide suggestive evidence on the mechanisms behind the mental health and wellbeing improvements that we documented in the previous section. The five patterns are:

(i) According to recipients’ own accounts of their experience with the cash transfers, and according to positive treatment effects on participants’ saving behavior, the cash transfers improved the financial security of recipients. The corresponding literature identifies financial security to be a foundation for mental health and wellbeing (Jachimowicz et al., 2022; Guan et al., 2022).

(ii) Recipient statements as well as positive treatment effects on participants’ charitable giving and financial support of family and friends show that the cash transfers increased recipients’

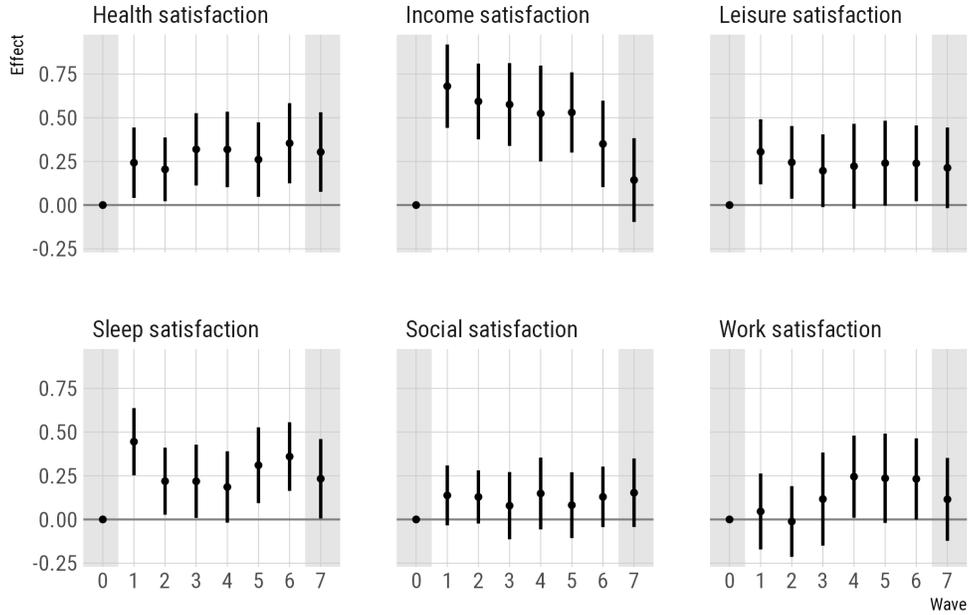


Figure 3: Average treatment effects (ATE) in standard deviations and 95% confidence intervals for the by-wave domain satisfaction outcomes as changes relative to the baseline, see outcome definitions in Table A.1. Since our outcomes are changes relative to baseline, there are no treatment differences at baseline.

prosocial behavior. There is ample evidence that spending money on others improves wellbeing (Dunn et al., 2008; Hui et al., 2020).

(iii) Cash transfers increase the time participants spend with their friends. The related literature finds that a richer social life and less loneliness are cornerstones of good mental health and wellbeing (Harandi et al., 2017; Park et al., 2020; Waldinger and Schulz, 2023).

(iv) Cash transfers tend to increase participants' sleeping time and their spending on leisure activities and traveling. Evidence from a meta-analyses show that better sleep (Scott et al., 2021) and better recreational activities (Kuykendall et al., 2015) improve mental health and wellbeing.

(v) Recipient statements as well as treatment effects on perceived autonomy show that cash transfers improved participants' perceived agency and control over their lives. Correspondingly, ample evidence suggests that autonomy and freedom of choice are important prerequisites for wellbeing (Inglehart et al., 2008; Steckermeier, 2021).

In the following, we present the results behind patterns (i)-(v) more thoroughly.

Retrospective accounts of treated participants At the end of the survey in wave 6, that is, at the end of the cash transfer program, we asked cash-transfer recipients in a free form question to retrospectively reflect on how the cash transfers impacted them. By making use of a free form question, we avoid anchoring participants’ responses on specific aspect chosen by researcher (Haaland et al., 2024). Instead, participants freely chose to write about what they deemed to be of relevance.

Three research assistants independently analysed participants’ text responses and indicated for each response whether it mentions one or multiple of the following categories: Financial security, freedom/autonomy, wellbeing, cash transfers had no effect, changes in education, changes in work, changes in leisure activities, changes in health, changes in sleep, changes in social relationships, changes in romantic partnerships, and changes in donations and financial support of others.

We find that 72% of treated participants mentioned that their financial security improved, and 67% mentioned that the cash transfers changed their lives. Regarding life changes, 37% treated participants mentioned their work and education, 38% leisure activities, and 63% donations and financial support of others. Relatedly, 25% of treated participants explicitly stated greater freedom of choice. Only 8% of treated participants stated that the cash transfers had no impact on their lives.

These findings provide some support to the patterns (i) and (v) by showing, respectively, that a substantial share of treated participants directly mentioned to experience/have experienced financial security and that a large share of treated participants mention specific life changes and some participants even mentioned increased freedom of choice abstractly.

Household finance All surveys included questions on monthly saving, biannual donations, biannual financial transfers given to family and friends in the past six months, current debt, and current assets, and the surveys of wave 3-6 also included questions on monthly spending on housing, energy costs, appliances, daily uses, mobility, leisure activities, apparel (clothes and shoes), and travel. The wording of these questions, and all other questions discussed below, is stated in Online Appendix OA2.2.

We report treatment effects on these outcomes, averaged across waves, in levels in Table 3 (and changes, when possible, in Table B.4 in Appendix B.3). We find positive and statistically significant treatment effects on participants’ monthly saving, donations, transfers to family and friends, total assets, spending on apparel, travel and leisure activities. We do not find significant treatment differences on participants’ debt, spending on housing, energy costs, mobility, appliances, and daily uses.

We first turn in more detail to the treatment effect on monthly saving. Participants in the control saved on average EUR 332 on a monthly basis during the cash-transfer program. Treated participants’ monthly saving was EUR 779, that is EUR 447 greater than in the control. Accounting for baseline difference, the treatment effect amounts to EUR 455. Overall, treated participants saved more than one third of their monthly cash transfer during the cash transfer

program. At the same time, we find no treatment effect on participants' debt, but the share of participants with assets less than EUR 10.000 at the end of the cash transfer program decreases significantly by 14pp due to the cash transfers (with Neyman and Fisher's exact p -value <0.01). Our results hence suggest that the cash transfers allowed recipients to improve their financial security. This supports pattern (i) reported above.

We next turn to the treatment effects on donations and financial support of family and friends. Control participants donated to charitable causes and financially supported their family and friends with, on average, EUR 62 per month. The cash transfers impacted prosocial giving of treated participants substantially during the program, increasing the sum of donations and financial support to family and friends by EUR 92.5 to overall EUR 154.5 per month. Accounting for baseline difference, the treatment effects amount to EUR 89 greater prosocial giving per month. Consequently, treated participants shared 7-8% of their monthly cash transfer with others. These results support pattern (ii) reported above.

Because of limited survey length, we did not cover participants' consumption rigorously. Based on questions intended to provide a broad overview of consumption, however, we find that treated participants spend on average EUR 33 per month more on clothes and shoes, EUR 37 per month more on leisure activities, and EUR 188 per month more on travelling. The latter two treatment effects are in sum EUR 225, roughly 19% of the monthly cash transfers, and suggest that the cash transfers allowed treated participants to have a more active leisure time, supporting pattern (iv) reported above.

Time use In waves 0-3 and 5, we surveyed participants' time use (in hours per week) on chores, education, entertainment, family, friends, partner, sleep, sport, and volunteering. We report treatment effects on these outcomes, averaged across waves, in levels in Table 3 (and changes in Table B.4 in Appendix B.3).

We find positive treatment effects on time spent with friends. The control group spent, on average, roughly 8.6 hours with friends per week. Treated participants spent 1.3 hours more time with friends per week than the control, an increase of 15%. Accounting for baseline differences, the treatment effect increases to slightly more than 2 hours. These results support pattern (iii) stated above.

We also find a positive treatment effects on participants' sleeping time when considering changes relative to the baseline: According this estimated treatment effect, treated participants sleep longer for 2.3 hours per week relative to control participants. On a nightly basis, this amounts to roughly 20 minutes longer sleep. Consistent with this finding, treated participants report greater sleep satisfaction, an important mediator for the positive effects on longer sleep on greater mental health and wellbeing (Scott et al., 2021). While these treatment effects support pattern (iv) reported above, we do not find a statistically significant treatment effect on sleeping time in levels. According to this insignificant estimate, treated participants slept "only" 1.2 hours longer per week than control participants.

Outcome	Treated	Control	ATE	SE	t-stat	<i>p</i> -N	<i>p</i> -F	n treated	n control
Financial household									
Donations (6mo), levels	170.310	73.105	97.205	27.822	3.494	0.000	0.018	107	1475
Transfers (6mo), levels	757.156	299.882	457.274	117.067	3.906	0.000	0.006	107	1474
Savings (monthly), levels	779.251	332.175	447.076	44.996	9.936	0.000	0.000	107	1473
Debt (stock), levels	14137.688	13703.791	433.897	3812.833	0.114	0.909	0.898	107	1474
Monthly spending									
Appliances, levels	421.294	496.737	-75.443	91.439	-0.825	0.409	0.410	107	1419
Daily needs, levels	340.421	327.582	12.838	15.498	0.828	0.407	0.414	107	1419
Apparel, levels	145.082	112.143	32.939	11.425	2.883	0.004	0.006	107	1419
Leisure, levels	184.168	147.135	37.033	13.971	2.651	0.008	0.010	107	1419
Mobility, levels	170.999	151.970	19.029	12.627	1.507	0.132	0.136	107	1419
Travel, levels	436.628	248.402	188.227	81.013	2.323	0.020	0.032	107	1419
Housing, levels	642.378	647.918	-5.540	32.441	-0.171	0.864	0.866	107	1419
Energy, levels	127.259	125.587	1.672	15.697	0.107	0.915	0.910	107	1419
Time use, h/w									
Chores, levels	7.241	7.305	-0.064	0.357	-0.179	0.858	0.868	107	1465
Education, levels	4.497	4.193	0.305	0.582	0.524	0.601	0.616	107	1465
Entertainment, levels	17.805	18.089	-0.284	0.958	-0.296	0.767	0.760	107	1465
Family, levels	6.806	6.056	0.750	0.718	1.045	0.296	0.314	107	1465
Friends, levels	9.834	8.512	1.322	0.538	2.457	0.014	0.016	107	1465
Partner, levels	13.491	11.766	1.724	1.449	1.190	0.234	0.274	107	1465
Sleep, levels	49.102	47.867	1.235	0.884	1.396	0.163	0.176	107	1465
Sport, levels	3.922	3.782	0.140	0.330	0.425	0.671	0.674	107	1465
Volunteering, levels	1.327	0.949	0.378	0.261	1.451	0.147	0.166	107	1465
Autonomy									
Autonomy, levels	4.024	3.706	0.320	0.065	3.824	0.000	0.000	107	1477

Table 3: We report average treatment effects (ATE) for the household finance (in EUR), time use (in hours per week), and autonomy (in standard deviations) outcomes defined in terms of levels. Inference is based on robust standard errors (SE), and Neyman (N) and Fisher’s exact (F) *p*-values.

Perceived autonomy Starting in wave $t = 1$, we elicited participants perceived autonomy in each wave. We asked participants how autonomous they feel in their own life. We report the treatment effect on this outcome in levels in the final row of Tables 3. We find that cash transfers improve perceived autonomy by 0.320 SD on average across waves. Consistent with participants’ retrospective accounts on live changes and greater freedom of choice, and consistent with the positive treatment effects on participants’ spending and time use, this finding suggests that participants experienced greater agency, allowing them to shape their lives in greater extends according to their values and needs. This treatment effect hence supports pattern (v) documented above.

5 Related literature

Our results contribute to several literatures. In particular, our results differ from contemporaneous RCTs on cash transfers in the US and Canada, which do not find that cash transfers improve mental health. We argue that the difference in results may be caused by the fact that cash transfers, in our case, allow recipients to experience greater agency and implement mean-

ingful life changes. In addition, our paper contributes to the broader literature on the effects of money on mental health and wellbeing. We argue that cash transfers that are unconditional, regular, and guaranteed, are particularly suited to have positive effects on recipients. We close our discussion with more specific contributions on the link between autonomy and wellbeing and on the stability of wellbeing improvements over time.

The effect of cash transfers on mental health and wellbeing The literature on cash transfers in low and mid-income countries finds that cash transfers allow to improve recipients' mental health and wellbeing (Haushofer and Shapiro, 2016; Christian et al., 2019; Romero et al., 2021). A recent meta study reports modest improvements in mental health of 0.07 SD and in life satisfaction of 0.13 SD (McGuire et al., 2022). Prominent authors have argued that there is a smaller scope of mental health and wellbeing improvements in richer countries (Ryan and Deci, 2001; Diener et al., 2009; Easterlin and Sawangfa, 2010; Kahneman and Deaton, 2010), where basic needs tend to be satisfied, comparatively generous social benefit programs are in place, and adaption and satiation effects limit potential improvements, as material goals rise with income gains (Easterlin and Sawangfa, 2010; Kahneman and Deaton, 2010). Indeed the contemporaneous literature on cash transfers in high-income countries finds no evidence for durable improvements in mental health (West and Castro, 2023; Dwyer et al., 2023; Jaroszewicz et al., 2024; Miller et al., 2024; Balakrishnan et al., 2024).

We contribute to this literature by showing that cash transfers can lead to large, durable improvements in mental health and wellbeing. Our findings suggest that cash transfers need to be large enough to cause life changes and improvements in agency. Among evaluations by other research teams, the most generous is a contemporaneous cash-transfer RCT in the US that paid USD 1,000 on a monthly basis for three years (Miller et al., 2024; Vivalt et al., 2024; Bartik et al., 2024). Recipients were households with, on average, three members. This cash transfer increased average household income by 40% during the treatment period of three years. In our setup, by contrast, recipients received EUR 1,200 on a monthly basis for three years to *single member* households. Our cash transfer increased household income by 46-110% (on average 61%). We conjecture that it is critical that cash transfers empower recipients to experience greater agency and to make meaningful changes in their lives, in order to enable them to sustain mental health and wellbeing improvements.

The following observations support this interpretation. Our findings on mental health and wellbeing improvements are accompanied by the following additional patterns: (i) Cash transfers recipients state to feel greater financial security and their self-reported household finance data suggests that they saved one third of their monthly cash transfers for the future. (ii) Treated participants shared seven to eight percent of their cash transfers with others, in the form of financial support to family and friends and charitable giving. (iii) Cash transfers allowed recipients to spend 1.3 hours per week more time with their friends. (iv) Treated participants tended to sleep longer (between 1.2 and 2.3 hours per week) and invested more in recreational activities. (v) Cash transfers improved the perceived autonomy of participants, allowing them

experience greater control to live according to their values and needs. Importantly, the vast literature present ample evidence that mental health and wellbeing benefit from greater financial security (Jachimowicz et al., 2022; Guan et al., 2022), prosocial behavior (Dunn et al., 2008; Hui et al., 2020), social connectedness (Harandi et al., 2017; Park et al., 2020; Waldinger and Schulz, 2023), sleep and recreational activities (Scott et al., 2021; Kuykendall et al., 2015), and autonomy (Inglehart et al., 2008; Steckermeier, 2021). We hence view the five patterns mentioned above to provide evidence on the mechanisms behind the mental health and wellbeing improvements. Importantly, Miller et al. (2024); Vivalt et al. (2024); Bartik et al. (2024) do not find that cash transfers have equivalent effects on their cash-transfer recipients. Instead, they report much smaller effects on monthly savings, donations, and financial support to others, and they do not find positive effects on time spent with friends and sleeping time. We hence conclude that cash transfers need to be generous enough to empower recipients to implement and experience the changes in their lives that lead to enduring mental health and wellbeing improvements.

Lottery winnings versus cash transfers We also contribute to the literature on whether money more generally improves wellbeing in high-income countries. Much of this literature relies on lottery winnings to estimate the causal effects (Gardner and Oswald, 2007; Kuhn et al., 2011; Apouey and Clark, 2015; Raschke, 2019; Lindqvist et al., 2020; Dwyer and Dunn, 2022). The reported findings are mixed: mental health is found to be affected positively (Gardner and Oswald, 2007; Apouey and Clark, 2015), not at all (Lindqvist et al., 2020) or, potentially even, negatively (Raschke, 2019); life satisfaction is reported to be affected positively (Apouey and Clark, 2015; Lindqvist et al., 2020; Dwyer and Dunn, 2022), or not at all (Kuhn et al., 2011). For instance, large lottery winnings of USD 100,000 or more in Sweden do not improve mental health and improve long-term life satisfaction by merely 0.034 SD for every USD 100,000 of winning (Lindqvist et al., 2020).

Our results suggest that generous, regular, unconditional, and guaranteed cash transfers may be particularly well-suited to improve well-being. Guaranteed and regular cash transfers that continue into the future enhance the predictability of financial security, help to sustain reasonable spending habits, and limit potential stress and anxiety related to the management of large lump sum payments. They appear to limit adaptation and satiation effects, potentially by providing continual reminders and positive reinforcement of financial security.

Autonomy and wellbeing improvements Regular, unconditional, and guaranteed cash transfers ease financial constraints. As a result, recipients may find themselves with more freedom to shape their lives according to their tastes, values, and psychological needs (Maslow, 1943). In turn, they may attribute this increased sense of control to their own autonomous disposition, rather than to changes in circumstances (Kraus et al., 2009). While previous work shows that the correlation between perceived autonomy and life satisfaction is often greater in richer than in poorer countries (Inglehart et al., 2008), we add more direct evidence on the link

between autonomy and wellbeing improvements.

Stability of wellbeing improvements Our findings imply relatively stable wellbeing improvements. Previous research, mostly based on correlational evidence, suggests otherwise: Models of adaptation effects posit that, as people get used to greater income, wellbeing improvements attenuate (Vendrik, 2013). Models of satiation effects, relatedly, suggest that wellbeing improvements due to income gains are less pronounced, the more income is already available (Easterlin and Sawangfa, 2010; Kahneman and Deaton, 2010). While we do find some evidence for adaptation effects on income satisfaction, we find no evidence for adaptation effects in mental health, purpose in life, and in the other domains of life satisfaction, consistent with more recent correlational evidence against adaptation effects (Kaiser, 2020; Stevenson and Wolfers, 2013). This suggests that wellbeing improvements caused by financial improvements may be more stable than *previously* thought.

6 Discussion

In this section, we discuss the internal and external validity of our findings. We show that our findings are robust to multiple-hypothesis-testing adjustments and are unlikely the results of alternative explanations. We discuss limitations of the generalizability of our findings.

Multiple hypothesis testing Following our preregistration, we apply the BH procedure to control for multiple hypothesis testing within the group of mental health and wellbeing outcomes. The BH procedure uses an adjustment of the cutoff for p -values that is used to determine statistical significance of an estimated treatment effect, as described in Section 2.3.

Our main results are based on the $m = 42$ estimated treatment effects stated in Table 2, Table B.2, and Table B.3. 32 of the 42 estimated treatment effects in these tables are significantly different from zero. We continue to determine 31 of these 32 treatment effects to be statistically significant when applying BH critical values. Only the treatment effect on sleep satisfaction six months after the cash transfer program is no longer statistically significant, its p -value is not below its BH critical value.

These results are robust to extending the BH procedure by including also the treatment effects regarding the explorative analyses on financial household, consumption, time use, and perceived autonomy (Tables 3 and B.4). Among the treatment effects on mental health and wellbeing, we continue to determine all treatment effects to be statistically significant when applying the extended BH critical values, except the treatment effect on sleep satisfaction six months after the cash transfer program, which is no longer statistically significant. All statistically significant treatment effects on financial household, consumption, time use, and perceived autonomy, as reported in Tables 3 and B.4, continue to be statistically significant when applying the extended BH critical values.

Experimenter demand, social desirability, and Hawthorne effects The internal validity of experiments with human participants may, in principle, suffer from experimenter demand, social desirability and Hawthorne effects (Levitt and List, 2011; De Quidt et al., 2018; Bursztyjn et al., 2025). Since our implementation partner, who financed the cash transfers, actively supports basic income policies, a potential experimenter demand or social desirability effect would be that treated participants might self-report greater mental health and wellbeing to lend support to basic income policies more generally. If our findings are due to Hawthorne effects, participants change their behavior only because they are experimentally observed. The following observations suggest that experimenter demand, social desirability and Hawthorne effects do not account for our findings.

First, our experimental design limits the potential roles of experimenter demand, social desirability and Hawthorne effects. To limit demand and social desirability effects, we explicitly asked participants to respond accurately to factual questions, stated that there are no right or wrong answers to subjective questions, and used a third-party survey company to implement our surveys. Regarding Hawthorne effects, treated and control participants are observed to the same extent. This makes explanations of our findings based on Hawthorne effects, where outcomes are moved by observation rather than treatment, implausible.

Second, if experimenter demand and social desirability effects are relevant in the context of our RCT, they should similarly affect participants’ self-reported labor supply. This follows from the public debate about cash transfers in Germany at the start of our RCT, which focused primarily on whether the cash transfers will severely reduce recipients’ labor supply, that is, make recipients “lazy.” We analyse the effect of the cash transfers on participants’ labor supply in a companion paper (Bernhard et al., 2025). Importantly, our analyses on labor supply rely not only on self reports, but also on administrative records (based on the Integrated Labor Market Biographies dataset constructed by the German Institute for Employment Research, see Schmucker and Vom Berge, 2025).⁵ We find that the negative, yet small effects on labor supply are consistent between self reports and administrative records (Bernhard et al., 2025). That we find no inflated self reports on labor supply relative to participants administrative records suggests that experimenter demand and social desirability concern are not relevant in the context of our RCT.

Third, we find no treatment effect on participants’ political support for basic income policies. In the end of the final wave, we asked participants the following question: “Do you support the idea of a universal basic income for all citizens?”⁶ Participants could respond on a 1-4 scale, with 1 = “yes, absolutely,” 2 = “rather yes,” 3 = “rather no,” 4 = “no, absolutely not.” The average response of treated participants is 1.928 and of control participants is 1.964. The treatment

⁵We asked participants after the first year of the cash transfer program whether they allow us to link their treatment status with their administrative records. Participants did not know we would ask this during the first year. The administrative records, however, allow us to observe labor supply of participants also retrospectively during the first year of the cash transfer program.

⁶In German, we asked: “Befürworten Sie die Idee eines bedingungslosen Grundeinkommens für alle Bürger/innen?”

difference is -0.035 , which is quite small and not statistically significantly different from zero, with a one-sided Newman p -value of 0.33 and one-sided Fisher’s exact p -value of 0.3.⁷ This finding contradicts explanations based on experimenter demand and social desirability, where recipients try to give answers that lend support to basic income policies.

Fourth, the dynamics over time of treatment effects on wellbeing are not consistent with experimenter demand, social desirability and Hawthorne effects, either. These effects cannot explain the decreasing treatment effect on income satisfaction over time, nor the increasing treatment effects on work satisfaction.

Finally, experimenter demand, social desirability, and Hawthorne effects are also not consistent with the overall results of cash-transfer RCTs in rich-income countries. If experimenter demand, social desirability, and Hawthorne effects would provide reasonable explanations for our results, these effects should produce similar results in comparable cash-transfer RCT. Miller et al. (2024), however, do not find positive treatment effects on mental health in the US, arguably because the cash transfers in their setting did not empower agency and life changes, see our discussion in Section 5 above. Importantly, the US RCT was also publicly linked to the debate on basic income policies.⁸ Experimenter demand and social desirability effects should hence equally be of concern in the US setting. That the results of the US RCT differ from our results therefore suggests that experimenter demand, social desirability, and Hawthorne effects may be less relevant in these long-term RCT settings.

Based on these analyses and observations, we conclude that our results are not likely to be driven by experimenter demand, social desirability, and Hawthorne effects.

Attrition We encountered no attrition during waves 1 to 6 in our treatment group and some attrition in our control group, where survey response rates declined slightly. Nonetheless, 71% of our control group completed all 6 surveys, 79% completed at least 5 out of 6 waves, 88% at least 3 out of 6 waves, and 97% at least 1 out of 6 waves. Based on the following two analyses, we however conclude that attrition does not appear to be selective in a way that would impact our findings.

If attrition were selective, this might cause bias in our treatment effect estimates. We might find significant effects, even if in truth (without selection) there is no effect. As a placebo test for this possibility, we estimate treatment effects on our baseline covariates (where true treatment effects are by construction equal to 0), while artificially restricting our sample to only those individuals without missing data for any subsequent wave. The P -values corresponding to these estimates are shown in Figure B.1 in Appendix B.4. We find no significant effects for the restricted sample. This increases our confidence that our findings are not driven by selective

⁷Since demand effects predict greater support for treated participants, we conducted one-sided tests, in favor of finding significant demand and social desirability effects.

⁸The US RCT was financed by Y Combinator. Sam Altman, president of Y Combinator at the time the RCT was conceived, publicly stated that the RCT should speak to the debate on basic income and argued that he is “fairly confident that at some point in the future, [...] we’re going to see some version of [basic income] at a national scale” (Altman, 2016).

attrition.

As a second test for selective attrition, we restrict attention to the control group. Within this group, we compare the average of our main outcome variables (mental health, purpose in life, life satisfaction) at baseline (wave 0), across observations with different numbers of missing waves. If attrition were selective, we would expect that these means vary across the number of waves missing, see Table B.5 in Appendix B.4. We find no differences across number of waves missing.

Based on these two analyses, we conclude that our results are not likely to be affected by selective attrition.

Disappointed control An alternative explanation for our findings is that the control group’s mental health and wellbeing declined due to disappointment at not receiving the cash transfers, rather than the treatment group truly improving. Based on several observations, we find this “disappointed control” account unlikely, and instead attribute the results to genuine mental health and wellbeing improvements of the treatment group.

First, the pattern of mental health and wellbeing improvements does not match what one would expect if control-group disappointment were driving the effects. Many of the observed mental health and wellbeing improvements are due to positive improvements in the treatment group rather than declines in the control group, see Table 2. For instance, the treatment group’s mental health increased by about 0.322 SD from baseline, whereas the control group’s remained essentially unchanged (+0.02 SD), yielding a sizable treatment effect (0.305 SD). Similar patterns hold for outcomes like income, leisure, and social satisfaction. In contrast, for outcomes such as general life satisfaction, sleep satisfaction, and purpose in life, the control group reported small declines over time while the treated group improved, contributing jointly to the treatment effects. Only work and health satisfaction declined over time in both treatment and control groups, with more pronounced declines for the control. This selective pattern of improvement is not consistent with a uniform “disappointed control” effect that would broadly depress the control group’s mental health wellbeing.

Second, the analyses stated in the previous paragraph cannot rule out an extended version of control-group disappointment that combines control-group disappointment with population-wide shocks to mental health and wellbeing that differ between the different outcomes. Assume that the entire German population experienced shocks (unrelated to the cash transfers) that caused large improvements of mental health, income satisfaction, and leisure satisfaction, medium-sized improvements of purpose in life and general life satisfaction improvements, and no improvements of work and health satisfaction during the program. In combination with control-group disappointment, such shocks could in principle account for our results. To address also this extended version of control-group disappointment, we turn to the SOEP and find *no* evidence for such shocks in the German population or in the study population (the part of the German population that would have been eligible in 2021 to participate in our study), see Appendix B.5. We hence conclude that also the extended version of control-group disappointment

is not consistent with our findings.

Finally, our analyses show that recipients used their cash transfers in ways that meaningfully changed their lives, see Section 4. These tangible changes align with improved mental health and wellbeing of treated participants. Importantly, changes in each one of the domains that we discussed—improved financial security, prosocial giving, social connectedness, better sleep, and empowered agency—are independently linked in the literature to better mental health and wellbeing outcomes. Our results are hence not only inconsistent with control disappointment, see the two paragraphs above, but our explorative analyses provide evidence for why the cash transfers allowed the treatment group to improve their mental health and wellbeing, namely empowered agency and life changes.

Based on these analyses and observations, we conclude that our results are not likely to be driven by a disappointed control.

External validity The cash transfer program covered in our RCT is resource intensive, costing in total EUR 4.6 million. The size of the treatment group is hence limited, so that we cannot study general equilibrium effects. Our results are furthermore based on a non-representative sample, see Table B.7 in Appendix B.6, which potentially limits generalizability. Our effect sizes may also not generalize to settings where cash transfers are either less or more generous (both in terms of the monthly amounts and the time they are paid out). Cash transfers might in practice be combined with other policy changes, such as increases of taxation to finance the cash transfers, that are not covered in our RCT.

7 Conclusion

We show that generous, regular, unconditional, and guaranteed cash transfers improve recipients' mental health, purpose in life, and life satisfaction. Exploratory analyses suggest that these improvements are mediated by greater personal agency and subsequent life changes. These findings contribute to policy debates on how high-income countries can directly improve their citizens' well-being. They also suggest that such transfers can enhance individuals' agency and better equip them to navigate the profound transformations of the 21st century. Optimal tax and transfer theory in economics (Saez, 2002; Chetty, 2009; Kleven, 2021) also incorporates the costs of transfer programs to assess its overall appeal. As cash transfers can be financed by raising progressive income taxes, or by some other form of taxation, the costs of cash transfer programs depends in particular on the extent to which the tax base is affected via labor supply responses to the policy. We explore these questions of labor market effects and optimal policy design in greater detail in Bernhard et al. (2025).

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A Outcomes

Outcomes	by wave $t > 0$	across waves $1 \leq t \leq 6$
<i>Mental health</i>		
WHO-5 Depression	$\text{Dep}_{it} = \frac{(dep_{it} - dep_{i0})}{SD(dep_{i0})}$	$\frac{1}{5} \sum_{t=2}^6 \text{Dep}_{it}, \dagger$
PSS Stress	$\text{Str}_{it} = \frac{(str_{it} - str_{i0})}{SD(str_{i0})}$	$\frac{1}{5} \sum_{t=1, t \neq 2}^6 \text{Str}_{it}, \dagger$
Mental Health	$\text{MH}_{it} = \frac{(\text{PSS}_{it} + \text{WHO5}_{it})}{2}$	$\frac{1}{4} \sum_{t=3}^6 \text{MH}_{it}, \dagger\dagger$
<i>Purpose in Life</i>		
Purpose in life	$\text{P}_{it} = \frac{(p_{it} - p_{i0})}{SD(p_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{P}_{it}$
<i>Life satisfaction</i>		
General Life Satisfaction	$\text{G}_{it} = \frac{(g_{it} - g_{i0})}{SD(g_{i0})}$	$\frac{1}{5} \sum_{t=2}^6 \text{G}_{it}, \dagger$
Health Satisfaction	$\text{H}_{it} = \frac{(h_{it} - h_{i0})}{SD(h_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{H}_{it}$
Sleep Satisfaction	$\text{S}_{it} = \frac{(s_{it} - s_{i0})}{SD(s_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{S}_{it}$
Income Satisfaction	$\text{I}_{it} = \frac{(i_{it} - i_{i0})}{SD(i_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{I}_{it}$
Work Satisfaction	$\text{W}_{it} = \frac{(w_{it} - w_{i0})}{SD(w_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{W}_{it}$
Social Satisfaction	$\text{O}_{it} = \frac{(o_{it} - o_{i0})}{SD(o_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{O}_{it}$
Leisure Satisfaction	$\text{L}_{it} = \frac{(l_{it} - l_{i0})}{SD(l_{i0})}$	$\frac{1}{6} \sum_{t=1}^6 \text{L}_{it}$
Domain Satisfaction Index	$\text{D}_{it} = \frac{\text{H}_{it} + \text{S}_{it} + \text{I}_{it} + \text{W}_{it} + \text{O}_{it} + \text{L}_{it}}{6}$	$\frac{1}{6} \sum_{t=1}^6 \text{D}_{it}$
Life Satisfaction	$\text{Z}_{it} = \frac{\text{D}_{it} + \text{G}_{it}}{2}$	$\frac{1}{5} \sum_{t=2}^6 \text{Z}_{it}, \dagger\dagger$
<i>Overall index</i>		
MHW Index	$\text{X}_{it} = \frac{\text{MH}_{it} + \text{Z}_{it} + \text{P}_{it}}{3}$	$\frac{1}{4} \sum_{t=3}^6 \text{X}_{it}, \dagger\dagger$

Table A.1: Outcomes are changes relative to the baseline and are normalized by the standard deviation (SD) at baseline. \dagger The WHO-5 questionnaire and general life satisfaction question were missing in wave $t = 1$, and the PSS questions were missing in wave $t = 2$. $\dagger\dagger$ We compute aggregated outcomes only for waves where all components were elicited.

B Additional Analyses

B.1 Treatment Effects on Mental Health and Wellbeing in Levels

Outcome	Treated	Control	ATE	SE	t-stat	<i>p</i> -N	<i>p</i> -F	n treated	n control
Aggregates									
MHW Index	4.568	4.300	0.268	0.072	3.721	0.000	0.000	107	1477
Mental Health	4.074	3.837	0.237	0.072	3.287	0.001	0.000	107	1477
Purpose in Life	3.003	2.881	0.122	0.067	1.814	0.070	0.100	107	1476
Life Satisfaction	4.471	4.135	0.335	0.063	5.285	0.000	0.000	107	1477
Aggregate components									
WHO-5 Depression	2.916	2.669	0.2472	0.078	3.160	0.002	0.000	107	1445
PSS Stress	4.547	4.302	0.246	0.066	3.713	0.000	0.000	107	1470
General life satisfaction	3.783	3.506	0.277	0.065	4.246	0.000	0.000	107	1445
Domain Satisfaction Index	4.446	4.113	0.333	0.064	5.224	0.000	0.000	107	1477
Domain satisfactions									
Health Satisfaction	3.106	2.935	0.171	0.065	2.632	0.001	0.01	107	1477
Sleep Satisfaction	2.637	2.499	0.138	0.056	2.441	0.015	0.016	107	1477
Work Satisfaction	2.638	2.572	0.066	0.056	1.182	0.237	0.262	107	1471
Income Satisfaction	3.222	2.700	0.522	0.066	7.944	0.000	0.000	107	1477
Leisure Satisfaction	2.777	2.574	0.203	0.053	3.859	0.000	0.000	107	1476
Social satisfaction	2.798	2.608	0.190	0.056	3.384	0.001	0.002	107	1476

Table B.2: We report average treatment effects (ATE) in standard deviations on our outcomes in levels (that is without adjustment for baseline differences). Inference is based on robust standard errors (SE), and Neyman (N) and Fisher’s exact (F) *p*-values.

B.2 Treatment Effects on Mental Health and Wellbeing Six Months after Program

Outcome	Treated	Control	ATE	SE	t-stat	<i>p</i> -N	<i>p</i> -F	n treated	n control
Aggregates									
MHW Index	0.257	-0.074	0.331	0.112	2.950	0.003	0.002	104	1107
Mental Health	0.286	0.028	0.258	0.113	2.284	0.022	0.022	104	1107
Purpose in Life	0.132	-0.175	0.307	0.113	2.717	0.007	0.006	104	1105
Life Satisfaction	0.187	-0.089	0.276	0.117	2.364	0.018	0.016	104	1107
Aggregate components									
WHO-5 Depression	0.348	0.051	0.297	0.119	2.506	0.012	0.012	103	1107
PSS Stress	0.184	0.003	0.181	0.108	1.686	0.092	0.092	104	1104
General Life Satisfaction	0.168	-0.041	0.209	0.116	1.795	0.073	0.070	104	1106
Domain Satisfaction Index	0.173	-0.128	0.301	0.118	2.551	0.011	0.006	104	1107
Domain satisfactions									
Health Satisfaction	-0.069	-0.372	0.303	0.116	2.614	0.009	0.010	104	1107
Sleep Satisfaction	0.097	-0.136	0.233	0.116	2.009	0.045	0.050	104	1107
Work Satisfaction	-0.078	-0.193	0.115	0.121	0.950	0.342	0.338	104	1087
Income Satisfaction	0.207	0.064	0.143	0.122	1.168	0.243	0.244	104	1104
Leisure Satisfaction	0.373	0.160	0.213	0.118	1.810	0.070	0.056	104	1106
Social Satisfaction	0.132	-0.021	0.153	0.100	1.528	0.127	0.102	104	1106

Table B.3: We report average treatment effects (ATE) in standard deviations on our outcomes at wave 7 as defined in Table A.1. Inference is based on robust standard errors (SE), and Neyman (N) and Fisher’s exact (F) *p*-values.

B.3 Treatment Effects on Household Finance variables and Time Use in Changes

Outcome	Treated	Control	ATE	SE	t-stat	<i>p</i> -N	<i>p</i> -F	n treated	n control
Financial household									
Donations (6mo)	71.162	-24.719	95.881	20.014	4.791	0.000	0.000	107	1475
Transfers (6mo)	388.759	-50.737	439.497	128.023	3.433	0.001	0.004	107	1474
Savings (monthly)	494.698	39.217	455.481	41.966	10.854	0.000	0.000	107	1473
Debt (stock)	4065.681	4257.961	-192.279	3500.340	-0.055	0.956	0.958	107	1474
Time use, h/w									
Chores	0.397	-0.691	1.087	0.365	2.983	0.003	0.002	107	1465
Education	-0.451	-1.606	1.155	0.691	1.673	0.094	0.104	107	1465
Entertainment	-2.318	-2.423	0.105	1.052	0.100	0.920	0.942	107	1465
Family	1.050	0.050	1.000	0.634	1.576	0.115	0.112	107	1465
Friends	1.322	-0.710	2.031	0.539	3.768	0.000	0.000	107	1465
Partner	2.668	0.802	1.866	1.444	1.292	0.196	0.192	107	1465
Sleep	1.018	-1.520	2.539	0.793	3.202	0.001	0.002	107	1465
Sport	-0.690	-0.167	-0.523	0.296	-1.766	0.077	0.106	107	1465
Volunteering	0.114	-0.153	0.267	0.222	1.202	0.229	0.204	107	1465

Table B.4: We report average treatment effects (ATE) in standard deviations for household finance and time use outcomes in changes. Inference is based on robust standard errors (SE), and Neyman (N) and Fisher’s exact (F) *p*-values.

B.4 Attrition

No. of missing waves	Mental Health	Purpose in life	Life Satisfaction	n control
0	2.049	3.010	4.298	1,124
1	2.032	2.970	4.183	127
2	2.100	3.153	4.436	80
3	2.070	3.020	4.286	70
4	2.134	3.138	4.359	55
5	2.001	2.974	4.286	73
6	2.225	3.075	4.449	51

Table B.5: Average (non-normalized) responses to (i) all mental health questions, (ii) the purpose in life question, and (iii) all questions regarding life satisfaction at baseline by control participants’ number of missing waves later on in the RCT.

B.5 Disappointed control

In Table B.6, we present the average main outcomes (non-standardized) in levels that are available also in the SOEP and compare these average outcomes between our control group, our study population, and the German population. Please note that the SOEP data is only available until 2022. We do not find evidence for systemically different directional changes over time between

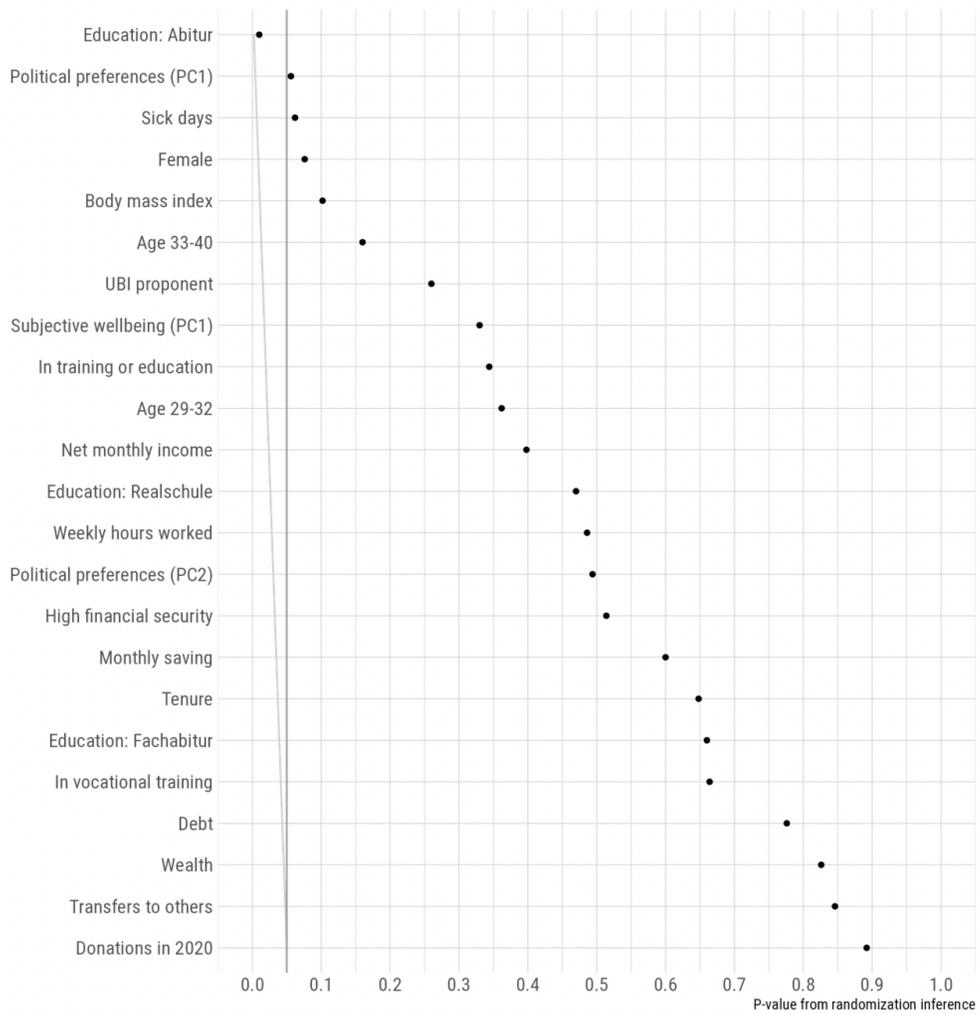


Figure B.1: P-values for differences between treatment and control of baseline covariates, after restricting the sample to observations without any missing waves.

the control and the study population or the control and the German population. Income, sleep, health, work, and general life satisfaction are declining over time in all three samples. Leisure satisfaction is increasing in time in our control, constant in time in our study population, and decreases in time in the German population. None of these patterns suggest that the wellbeing of our control group changed over time relative to the study population and the German population in ways attributable to control-group disappointment.

	Control Group				Study pop		German pop	
	02/2021 (Baseline)	11/2021 Wave 1	05/2022 Wave 2	11/2022 Wave 3	2021	2022	2021	2022
Income Satisfaction	6.62	6.41	6.57	6.52	7.24	6.80	7.01	6.79
Leisure Satisfaction	6.42	6.61	6.96	6.90	7.08	7.08	7.16	7.05
General Life Satisfaction	7.23	-	7.05	6.99	7.40	7.38	7.41	7.29
Sleep Satisfaction	7.17	6.83	6.96	6.94	6.89	6.66	6.67	6.48
Health Satisfaction	8.04	7.33	7.40	7.20	7.68	7.49	6.90	6.59
Work Satisfaction	7.35	6.82	6.84	6.82	6.94	6.85	7.00	6.85

Table B.6: Average, non-standardized values of outcomes in levels of our study sample at baseline in comparison to study population and German population.

B.6 Sample descriptives

We summarize how our study sample relates to the German population and our study population (the part of the German population that would have been eligible in 2021 to participate in our study) in key demographic variables and wellbeing outcomes at baseline in Table ??.⁹

⁹We used the SOEP to construct Table ?. This forces us to restrict attention to the subset of the wellbeing outcomes that were elicited in 2021.

	Study sample	Study population	German population
Female	41%	43%	51%
Age	31	31	51
Age between 20-40	100%	100%	34%
Net monthly income (NMI) in EUR	1,934	1,819	2,353
NMI between EUR 1100-2600	100%	100%	55%
Working hours	37.4	38.72	36.35
Non-working	0	6%	33%
Education	11%	4%	2%
Unemployed	4%	2%	4%
Employed	91%	91%	62%
General Life satisfaction	7.23	7.40	7.41
Health satisfaction	8.02	7.68	6.90
Sleep satisfaction	7.14	6.89	6.67
Income satisfaction	6.62	7.24	7.01
Work satisfaction	7.34	6.94	7.00
Leisure satisfaction	6.41	7.08	7.16

Table B.7: Descriptive statistics of the study sample (based on our survey data) relative to study population and German population (based on the SOEP).