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

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Updated Version

Abstract

Mental health and wellbeing are unequally distributed in high-income countries, disadvantaging low-income individuals. Unconditional, regular, guaranteed, and individual 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 substantially, with effects persisting 18 months after the program ended. 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 life changes.

JEL Codes: C93, I31, D10
Keywords: Basic Income; RCT; Purpose in Life; Life Satisfaction

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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, guaranteed, and individual cash transfers are a promising policy tool to address this inequality (Pateman, 2004; Van Parijs and Vanderborght, 2017; Marinescu, 2018; Bidadanure, 2019). Such cash transfers reduce uncertainty about future income, and empower the 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; 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 promote financial security and agency.

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 unconditional, regular, guaranteed, and individual 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 18 months after the final transfer. In additional analyses, we find that cash transfers empower recipients to make meaningful changes in their lives: Treated participants save more, give more to others, 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, individual, and generous enough to empower agency.

In Section 2, we describe the design of our RCT. Our goal is to estimate the causal effect of unconditional, regular, guaranteed, and individual cash transfers on mental health and wellbeing. We restricted eligibility to single-person households with below-average income, which

has two important implications. First, it ensures treatment representativeness: each recipient has full individual control over the transfer, without having to negotiate its use within a shared household budget, as envisioned in leading proposals for cash transfers (Pateman, 2004; Van Parijs and Vanderborght, 2017). From an implementation perspective, this restriction also yields independent survey responses across participants, avoiding the possibility that members of the same household may influence each other's reports. Second, our study sample is also policy-relevant. Single-person households constitute roughly one third of all households in the United States and Europe, and the literature identifies living alone as a structural risk factor for loneliness and, in turn, for the onset of depression (Wu et al., 2022; Ma et al., 2022; Holt-Lunstad, 2024). Combined with our participants' below-average baseline income, this makes our sample a population at elevated risk for poor mental health and wellbeing.

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 groups in eight follow-up surveys. Waves 1 through 6 were administered during the cash transfer program at six-month intervals; waves 7 and 8 were administered 6 and 18 months after the final transfer.

We measure mental health using the WHO-5 Wellbeing Index (Staehr, 1998) and five items from the Perceived Stress Scale (PSS-10, Cohen et al., 1983; Cohen, 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 instruments 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 (SOEP) on participants' perceived purpose in life and participants' general life satisfaction, and their satisfaction in several life domains (Richter et al., 2017).

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 survey instruments. The effect on life satisfaction is present for general life satisfaction and for the domains of income, health, sleep, and leisure. We also find directionally positive but statistically insignificant improvements of work satisfaction and satisfaction with social life.

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 6 and 18 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. This MHW index improved by 0.379 SD during the program compared to the control, by 0.331 SD 6 months after the program, and by 0.362 SD 18 months after the program, implying that on average across the two post-program waves 91% of the during-program treatment effect persisted.

In Section 4, we show in exploratory analyses that cash transfers empower participants to implement and experience life changes that are meaningful for mental health and wellbeing. 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 more time per week with their friends. (iv) Treated participants tended to sleep longer (between 1.2 and 2.5 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 addition, we study the labor supply effects of the cash transfers of our RCT in a companion paper (Bernhard et al., 2025). There, we find no reduction in recipients' labor force participation. This likely supports the positive treatment effects on mental health and wellbeing in this paper. Employment provides time structure, social contact, collective purpose, status, and activity that are essential for mental health and wellbeing independently of income (Jahoda, 1981, 1982; Paul et al., 2023).

In Section 5, we compare our results to the related literature. The most comparable evidence comes from a contemporaneous US-based RCT (Miller et al., 2024; Vivalt et al., 2024; Bartik et al., 2024), which evaluates 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 (Miller et al., 2024). A key difference

is that their transfers accrued to households with, on average, three members, likely diluting both the effective generosity and the individual discretionary control over the transfer. We argue that individual-level transfers are more likely to empower the kind of personally meaningful life changes that drive lasting improvements in mental health and wellbeing, and that differences in the institutional context, including Germany's largely universal health insurance and more comprehensive social safety net, may also contribute to the divergent findings. Notably, the study sample in Miller et al. (2024) was more economically disadvantaged than ours, yet we find large effects where they find none—the opposite of what standard diminishing-returns reasoning would predict, but consistent with individual control over the transfer being the operative margin.

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). We argue that unconditional, regular, guaranteed, and individual cash transfers are particularly suited to improve mental health and wellbeing. Their recurring nature limits hedonic adaptation, while their predictable monthly structure integrates into recipients' normal budgeting routines rather than requiring active financial management of a lump sum.

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, unconditional, and individual cash-transfer program on recipients' mental health and wellbeing in a preregistered, randomized controlled trial in Germany.[1] 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.

[1] We study the labor supply effects of our 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.

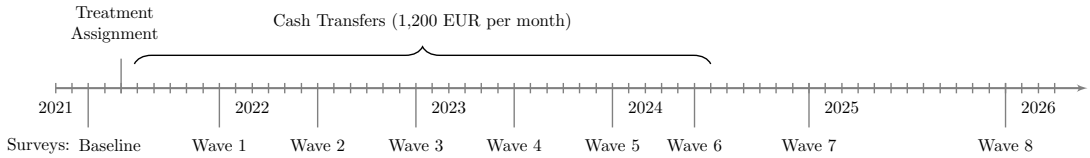

Figure 1: Timeline of the randomized controlled trial. The treatment group received monthly cash transfers of EUR 1,200 from Spring 2021 through Spring 2024, for a total of three years. Both treated participants (N=107) and control participants (N=1,580) completed a baseline survey prior to treatment assignment and eight follow-up surveys. Waves 1 through 6 were administered during the cash transfer program at six-month intervals; waves 7 and 8 were administered 6 and 18 months after the final transfer. The cash transfer program, survey schedule, and treatment assignment are indicated along the timeline.

2.1 Study sample and treatment assignment

The German NGO Mein Grundeinkommen e.V. (MG) financed the RCT, including the cash transfers. MG is funded through private donations. Prior to the RCT, MG made regular cash transfers of EUR 1,000 per month for a single year to 818 randomly assigned applicants, making MG a credible partner to finance the cash transfers in our RCT.[2]

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 completed the 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 On 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 study participants would be randomly assigned to a treatment group or a control group; treatment and control groups would participate in semi-annual 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 children, zip code, and their general attitude towards universal basic income. Between August 18 and December 10, 2020, 2,048,370 potential participants registered in response to this public signup call.

[2]We have no data on these single-year cash transfers and hence cannot evaluate them in this paper.

Study sample goals and eligibility criteria Achieving a representative study sample while providing sufficiently generous and genuinely individual cash transfers was financially infeasible. We hence prioritized treatment representativeness and restricted our sample to single-person households. This ensured that each recipient retains full discretionary control over the transfer, as envisioned in leading proposals for unconditional cash transfers (Pateman, 2004; Van Parijs and Vanderborght, 2017), and allows us to obtain independent survey responses across participants. Within these constraints, we selected a subsample of registered individuals (called "baseline sample") based on the following additional eligibility criteria with respect to participants' demographic and socioeconomic characteristics.

(i) Participants must 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 EUR (slightly below the poverty line in 2021) 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]

- Participants whose status on any of the criteria (including household size) changed after treatment assignment stayed in the study sample.[4]

Eligibility criteria (i)-(iii) ensured comparability to contemporaneous cash-transfer studies (Miller et al., 2024; Vivalt et al., 2024; Bartik et al., 2024) and (iii) also implied that our cash transfers would not reduce potential long-term unemployment benefits of recipients. By focusing on individual households, 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 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 during signup and to the baseline survey to sort participants into homogeneous blocks. In particular, we use questions on mental health, wellbeing, gender, socioeconomic background, financial household, and political attitudes. We display the full list of stratification variables used for blocking in Appendix OA1.2.

[3]Given 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.

[4]By wave 6, approximately one third of participants in both the treatment and control groups lived in households with more than one member, and roughly 6% had children. These rates are balanced across treatment and control, suggesting that household status changes are not differentially driven by the cash transfers.

We construct blocks that minimize the total sum of Mahalanobis distances in the stratification variables between pairs of observations within blocks. 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. (ii) Within each block, we assign two participants to the treatment 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 homogeneous 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.

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 their 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. Since only two participants were replaced, we do not adjust our inference for this reassignment.

Our final study sample hence includes 107 participants in the treatment group and 1,580 participants in the control group. Treatment and control groups 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 OA1.3. 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

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: Lower	0.037	0.038	0.000	0.019	-0.020	0.984
Education: Intermediate	0.215	0.214	0.001	0.041	0.035	0.972
Education: Vocational	0.243	0.241	0.002	0.043	0.044	0.965
Education: Academic	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 of baseline covariates between the treatment group (N=107) and the control group (N=1,580): We report means for the treatment and control groups, the difference in means, heteroskedasticity-robust standard errors, t-statistics, and p-values. All statistics are computed without adjustment for the blocked treatment assignment ("naïve" inference). The covariates listed correspond to the stratification variables used in the blocked random assignment described in Section 2.1. Income and financial variables are denominated in EUR. Covariates are well balanced between the two groups; the only p-value below 0.05 is for sick days, and this difference is not statistically significant after adjusting for multiple comparisons. Education categories refer to the highest school-leaving qualification obtained within the German secondary school system: Lower secondary (Hauptschulabschluss); Intermediate secondary (Realschulabschluss); Upper secondary vocational (Fachhochschulreife) denotes a qualification granting access to universities of applied sciences; Upper secondary academic (Abitur) denotes the general university entrance qualification after 12–13 years.

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 after the end of the cash transfer program, we invited all participants to a seventh and eighth survey six and 18 months after the final payment, and paid treated and control participants who completed the survey EUR 20.

Mental health and wellbeing outcomes To construct our outcomes, we use all questions on participants' mental health and wellbeing from the baseline survey. These questions were also included in the follow-up waves.[5] Specifically, we use participants' responses to these questions in the baseline survey ($t = 0$), the survey waves during the cash transfer program ($1 \leq t \leq 6$), and the final survey waves six ($t = 7$) and 18 ($t = 8$) months after the program. The wording of these questions is stated in Appendix OA1.4.

Our mental health outcomes are based on the WHO-5 questionnaire (Staehr, 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 item values imply better mental health across both instruments. We then obtain wave-specific scores for both instruments (dep_{it} and str_{it}) by summing the respective item values for each participant i and non-missing 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 (w_{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 OA1 in Appendix OA1.6. Our outcomes are standardized changes relative to the baseline. This allows us to estimate treatment effects that are adjusted for treatment-

[5]The WHO-5 questions and the general life satisfaction question were missing in wave $t = 1$, and the PSS questions were missing in wave $t = 2$.

control imbalances at baseline, which remained despite stratification. Moreover, this allows us 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

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

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:

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

95% confidence intervals for δ are then calculated as

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

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, 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 $i = 1, \ldots, k$.

We preregistered to apply this procedure separately for different groups of outcomes and specified mental health and wellbeing as one group of outcomes.

2.4 Preregistration

We preregistered the RCT at the AEA registry (https://www.socialscienceregistry.org/trials/7734). We originally preregistered that each block consists of two treated participants, 26 control participants, and 4 "spare participants," because our implementation partner was initially only willing to finance survey participation for 26 control participants per block. Our implementation partner thereafter changed their willingness and we were able to add the spare participants to the control group, resulting in 1,580 rather than 1,377 control participants; see Section 2.1 for details.

In this paper, we analyze all measures included in the baseline survey that capture mental health and wellbeing constructs. The baseline survey was finalized before treatment assignment. We provide a list of all items from the baseline questionnaire (excluding labor supply items, which we analyze in Bernhard et al., 2025) in Table OA2 in Appendix OA1.7.1 and we show screenshots of these questions in Appendix OA1.7.2. We did not include these survey-based measures in the

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-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: Average treatment effects on mental health and wellbeing outcomes during the three-year cash transfer program (wave 1-6): We report average treatment effects (ATE) in standard deviations on our outcomes as changes relative to the baseline, see outcome definitions in Table OA1. 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 treatment effects that are statistically significant based on unadjusted p-values remain significant after applying the Benjamini-Hochberg procedure to control the false discovery rate across the m = 56 mental health and wellbeing hypotheses tested in Table 2, Table A.1, Table A.2, and Table A.3, with the exception of sleep satisfaction 6 months after the program (Table A.2).

preregistration. The preregistration, however, included the collection of hair samples to measure cortisol levels as a biomarker for stress. We were unable to collect a sufficient number of valid samples to permit any meaningful analyses due to low participation in hair sample collection.

3 Main results

To obtain estimates for treatment effects during the cash transfer program, we estimate average treatment effects on outcomes across all waves one through six (see the right-most column in Table OA1). 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.

All of these improvements correspond to statistically significant treatment effects based on Neyman and Fisher's exact p-values, see Table 2. Social and work satisfaction also improve, by 0.125 SD and 0.143 SD respectively, but these treatment effects are not statistically significant. 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. 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 declined on average relative to the baseline both for treated and control participants, but depreciation was less strong for treated participants.

Robustness: outcomes in levels We show in Table A.1 in Appendix A.1 that our results are robust to considering outcomes in levels, that is, without adjustment for treatment-control imbalances at baseline, which remained despite stratification. 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, with 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 as large (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 a 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 more substantially during the final one and a 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 present and sizable 6 and 18 months after the final cash transfers, see Figures 2 and 3, and Tables A.2 and A.3 in the Appendix. The MHW index improved by 0.379 SD during the program, by 0.331 SD six months after the program, and by 0.362 SD 18 months after the program, implying that, on average, 91% of the treatment effect on mental health and wellbeing persisted after monthly payments were discontinued.

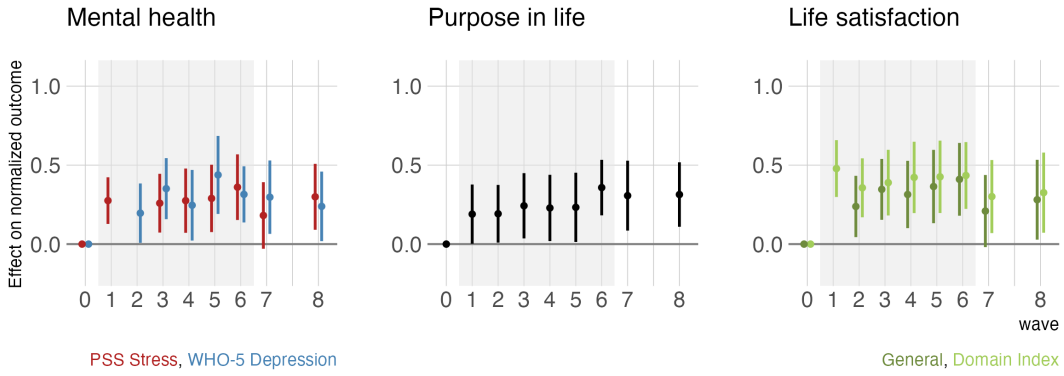

Figure 2: Dynamics of treatment effects on main mental health and wellbeing outcomes over the study period. Each panel displays estimated average treatment effects (ATE) in standard deviations, with 95% confidence intervals, for each survey wave. Outcomes are changes relative to baseline, so treatment effects are zero by construction at wave 0. Left panel: Mental health, decomposed into the WHO-5 Depression screener (blue) and perceived stress from the PSS (red). Center panel: Purpose in life (eudaimonic wellbeing). Right panel: Life satisfaction, decomposed into general life satisfaction (dark green) and the domain satisfaction index (light green). Treatment effects on mental health, purpose in life, and life satisfaction remain broadly stable over the three-year program, with no evidence of attenuation over time. Wave 7, conducted six months after the end of the program, and wave 8, conducted 18 months after the program, show persistent effects (see Tables A.2 and A.3). The WHO-5 questions and the general life satisfaction question were missing in wave $t = 1$, and the PSS questions were missing in wave $t = 2$.

The dynamics of the individual outcome components during and after the program are informative about the nature of this persistence. Not all domains of life satisfaction evolved in the same way. Income satisfaction — the outcome most directly tied to the cash transfer itself — shows a declining treatment effect already during the program and is no longer statistically significant 6 or 18 months after the program ends (0.140 SD and 0.130 SD, respectively). This trajectory is consistent with hedonic adaptation to the income gain, and with the mechanical loss of the income stream. By contrast, mental health, general life satisfaction, health satisfaction, sleep satisfaction, and purpose in life maintain treatment effects of 0.277–0.314 SD even 18 months after the transfers ceased, closely tracking their magnitudes during the program. Work satisfaction, which showed a delayed positive effect emerging only in the second half of the program, persists at 0.217 SD 18 months post-program.

The fading of income satisfaction alongside the stability of broader wellbeing and mental health improvements suggests that the lasting effects of the cash transfers are not simply a reflection of the additional income itself. Instead, the observed patterns suggest that the cash-transfers enabled treated participants to establish more beneficial sleeping habits and leisure activities, to improve their social relations, to build financial reserves and to improve their work situation during the study period, and that these changes may have become self-sustaining even

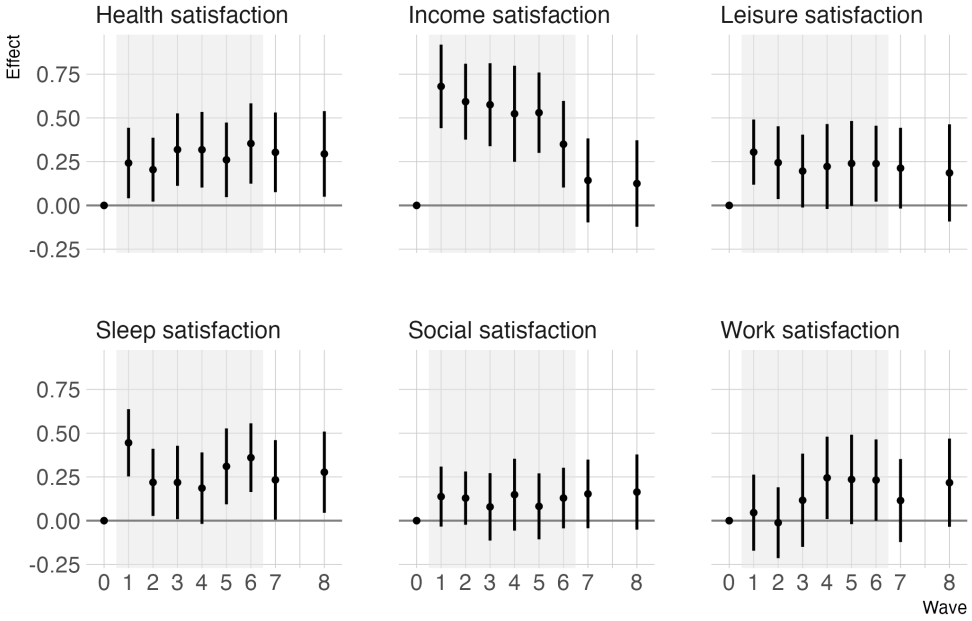

Figure 3: Dynamics of treatment effects on domain-specific satisfaction outcomes over the study period. Each panel displays estimated average treatment effects (ATE) in standard deviations, with 95% confidence intervals, for each survey wave. Outcomes are changes relative to baseline. Top row, left to right: health satisfaction, income satisfaction, leisure satisfaction. Bottom row: sleep satisfaction, social satisfaction, work satisfaction. Two notable dynamic patterns emerge. First, income satisfaction shows a declining treatment effect over time, falling from approximately 0.68 SD in wave 1 to 0.35 SD by wave 6, consistent with hedonic adaptation to the income gain. Second, work satisfaction exhibits a delayed positive effect: treatment effects are small during the first 18 months but increase substantially during waves 4–6 (approximately 0.23–0.24 SD). Health, sleep, and leisure satisfaction effects remain broadly stable throughout the program. Social satisfaction effects are directionally positive but not statistically significant at conventional levels in most waves.

after the income stream ended. In line with this interpretation, we present further exploratory analyses on treatment effects on life changes and perceived agency in Section 4.

4 Agency and life changes

We present exploratory analyses of 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 as providing suggestive evidence on the mechanisms behind the mental health and wellbeing improvements that we documented in the previous section – namely that cash transfers empower recipients to implement and experience meaningful life changes. The five patterns are:

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

(iii) Improved social life: 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) Improved recreation: Cash transfers tend to increase participants' sleeping time and their spending on leisure activities and traveling. Evidence from meta-analyses shows that better sleep (Scott et al., 2021) and new recreational activities and experiences (Kuykendall et al., 2015) improve mental health and wellbeing.

(v) Higher perceived autonomy: 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 had impacted them. By making use of a free-form question, we avoid anchoring participants' responses on specific aspects chosen by the researcher (Haaland et al., 2024). Instead, participants freely chose to write about what they deemed to be of relevance.

Three research assistants independently analyzed participants' text responses and indicated for each response whether it mentions one or more 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, and changes in donations and financial support of others – i.e., reflecting the domain satisfactions.

We find that 72% of treated participants mentioned improvements in their financial security,

and 67% mentioned that the cash transfers changed their lives. Regarding life changes, 37% of 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 suggestive support for patterns (i) through (v) by showing that a substantial share of treated participants directly mentioned experiencing greater 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, semiannual donations, semiannual 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 provided in Appendix OA1.5.

We report treatment effects on these outcomes, averaged across waves, in levels in Table 3 (and changes, when possible, in Table A.4 in Appendix A.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 differences, 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, supporting pattern (i).

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 differences, 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).

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-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: Average treatment effects on household finance, monthly spending, time use, and perceived autonomy outcomes during the cash transfer program. Household finance outcomes are reported in EUR and averaged across waves during the program; donation and transfer variables refer to six-month periods, savings to monthly amounts, and debt to stocks. Monthly spending variables (available in waves 3–6) are reported in EUR per month. Time use outcomes (available in waves 0–3 and 5) are reported in hours per week. Perceived autonomy is measured on a 1–11 Likert scale and reported in standard deviations. All outcomes are in levels (without baseline adjustment). Inference is based on heteroskedasticity-robust standard errors (SE) and both Neyman (p-N) and Fisher's exact (p-F) p-values. Treated participants saved approximately one-third of their monthly cash transfer, shared 7–8% with others through donations and financial support, spent significantly more on apparel, leisure, and travel, spent 1.3 more hours per week with friends, and reported significantly higher perceived autonomy (0.32 SD).

To prevent excessive survey burden, we did not cover participants' consumption rigorously. Based on questions intended to provide a broad overview of consumption, however, we find that treated participants spent 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 traveling. The latter two treatment effects amount to EUR 225 or roughly 19% of the monthly cash transfers, suggesting that the cash transfers allowed treated participants to have a more active leisure time, supporting pattern (iv).

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 A.4 in Appendix A.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 effect on participants' sleeping time when considering changes relative to the baseline: According to this estimated treatment effect, treated participants slept 2.5 hours more per week relative to control participants. On a nightly basis, this amounts to roughly 20 minutes longer sleep. Consistent with this finding, treated participants also report greater sleep satisfaction, which is consistent with longer sleep being one mediator of improved 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 1.2 hours longer per week than control participants.

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 Table 3. We find that cash transfers improve perceived autonomy by 0.320 SD on average across waves. Consistent with participants' retrospective accounts on life 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 to a greater extent 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 meaningful 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 middle-income countries finds that cash transfers allow recipients to improve 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 adaptation 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 and durable improvements in mental health and wellbeing. Our findings suggest that cash transfers need to be effectively generous at the individual level and preserve recipients' discretionary control over the transfer. The most comparable study 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). The single monthly transfer of USD 1,000 accrued to households with, on average, three members, making it likely that the effective transfer available to any individual recipient was well below the nominal amount. In our setting, by contrast, each participant received EUR 1,200 individually, as we restricted the study to single-person households.

We argue that individual-level transfers ensure that each recipient retains full discretionary control over the transfer, without having to negotiate its use within a shared household budget. This individual agency is likely a precondition for the kind of personally meaningful life changes that our evidence suggests drive lasting improvements in mental health and wellbeing. Several patterns in the comparison with Miller et al. (2024); Vivalt et al. (2024); Bartik et al. (2024) support this interpretation. First, cash transfers in the US setting do not lead to the type of life changes that we observe for our sample. Bartik et al. (2024) document that recipients' spending patterns are consistent with the transfer being shared within the household rather than being used to reshape individual life domains. By contrast, our recipients — who retained full individual control — invested in social relationships, recreational activities, and personal financial security. Evidence from a different context reinforces this point: Baird et al. (2025) find that cash transfers to adolescent girls in Uganda failed to improve mental health when recipients had to divert funds towards essential family needs rather than pursuing their own goals. Second, cash transfer recipients in the US setting reduce labor force participation (Vivalt et al., 2024), whereas we find no such reduction in our companion paper (Bernhard et al., 2025). This difference likely matters for mental health and wellbeing. Employment provides time structure, social contact, collective purpose, status, and activity that benefit mental health independently of income (Jahoda, 1981, 1982; Paul et al., 2023). A reduction in labor force

participation may therefore offset potential wellbeing gains from the transfer, while the absence of such a reduction in our setting allows the positive effects of greater financial security and agency to operate without this countervailing force.

Beyond the individual versus household distinction, the two settings differ along several baseline dimensions. First, the US sample was more economically disadvantaged: only 58% of participants were employed at baseline, and household net worth was close to zero or negative when excluding real estate (Bartik et al., 2024). Moreover, the study included a substantial share of single parents — 22% of the sample were single mothers (Vivalt et al., 2024) — who face particularly severe financial constraints and competing demands on any additional income. By contrast, 94% of our sample were working for pay and the average net wealth of our sample was positive. Second, institutional differences likely put the US sample at another disadvantage relative to our study sample: Germany's largely universal health insurance, comprehensive social safety net, and stronger worker protections mean that many of the basic needs that cash transfers might otherwise absorb — medical expenses, income volatility, housing insecurity — are already partially addressed through existing institutions.

Overall, this suggests that what matters for lasting mental health and wellbeing improvements is not the severity of baseline deprivation per se, but whether the cash transfer is effectively generous enough to empower individual agency and meaningful life changes. The German institutional environment may indeed facilitate this mechanism because basic needs are largely covered and additional income can be directed toward discretionary life improvements. In this sense, the more comprehensive safety net does not reduce the scope for cash transfers to make a difference. It channels their impact toward the domains — social relationships, recreation, autonomy — that our evidence suggests matter for sustained 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). Lottery winnings, even when large, represent one-off wealth shocks that recipients must actively manage and allocate. As one-off events, they are particularly susceptible to hedonic adaptation (Brickman et al., 1978; Frederick and Loewenstein, 1999), and the burden of managing a large windfall can itself be a source of psychological distress (Raschke, 2019).

Our results suggest that regular, unconditional, guaranteed, and individual cash transfers

may be particularly well-suited to improve wellbeing. 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 continuous reminders and positive reinforcement of financial security. In addition, when such transfers accrue to individuals rather than to households, recipients retain full discretionary control, allowing them to direct resources toward personally meaningful life changes rather than negotiating their use within a shared household budget.

Autonomy and wellbeing improvements Regular, unconditional, guaranteed, and individual 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 – in particular if they lead to lasting adjustments in lifestyle choices.

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 and the methods discussed in Section 2.3, we apply the BH procedure to control for multiple hypothesis testing within the group of mental health and wellbeing outcomes. Our main results are based on the $m = 56$

estimated treatment effects stated in Table 2, Table A.1, Table A.2, and Table A.3. 42 of the 56 estimated treatment effects in these tables are significantly different from zero. We continue to determine 41 of these 42 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.

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; Bursztyn 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).[6] We find no effects on labor force participation and small negative effects on working hours. Importantly, these effects 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 concerns play a limited role in the context of our RCT.

Third, we find no treatment effect on participants' political support for basic income policies.

[6] We 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.

At 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?"[7] 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 difference is -0.035, which is quite small and not statistically significantly different from zero, with a one-sided Neyman p-value of 0.33 and one-sided Fisher's exact p-value of 0.3.[8] 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 high-income countries. If experimenter demand, social desirability, and Hawthorne effects would provide reasonable explanations for our results, these effects should produce similar results in a 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.[9] 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

[7] In German, we asked: "Befürworten Sie die Idee eines bedingungslosen Grundeinkommens für alle Bürger/innen?"

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

[9] The 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).

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 A.1 in Appendix A.4. We find no significant effects for the restricted sample. This increases our confidence that our findings are not driven by selective 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 A.5 in Appendix A.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 in 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 and 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 in 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 A.6. 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, but our exploratory 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 A.6 in Appendix A.5, 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 in taxation to finance the cash transfers, that are not covered in our RCT.

A further limitation concerns the duration of the cash transfer program. Our participants received monthly transfers for three years, a period that seems to have been long enough to enable meaningful life changes that lead to durable improvements in mental health and wellbeing. It is unclear whether shorter programs would generate the same type of agency-driven improvements, and whether longer programs would generate different behavioral responses, including stronger labor supply effects (than we report in Bernhard et al., 2025) or different patterns of hedonic adaptation.

By restricting our sample to single-person households, we ensured that each recipient had full individual control over the transfer. This makes our treatment more representative of genuinely individual cash transfer programs — the form most commonly discussed in policy proposals (Pateman, 2004; Van Parijs and Vanderborght, 2017) — than studies where a single transfer is shared among multiple household members. To the extent that the policy debate centers on individual-level transfers, our design provides a more informative test of their potential effects on mental health and wellbeing, even if our sample does not generalize straightforwardly to multi-person households or the broader population.

7 Conclusion

We show that unconditional, regular, guaranteed, and individual 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. Our findings contribute to policy debates on how high-income countries can directly improve their citizens' wellbeing.

First, they 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 their 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 depend 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).

Second, our finding that treatment effects persist at roughly 91% of their during-program magnitude 18 months after the final transfer suggests that cash transfer programs need *not* be permanent to generate lasting benefits. A time-limited program of sufficient duration may allow recipients to implement life changes that sustain improvements in mental health and wellbeing beyond the program itself, substantially reducing the fiscal cost relative to a permanent transfer.

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A Additional Tables, Figures, and Analyses

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

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-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	0.247	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 A.1: Average treatment effects on mental health and wellbeing outcomes during the three-year cash transfer program: 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. We reject the null of no effect for the aggregated mental health and life satisfaction outcomes, the WHO-5 depression scale, the PSS stress scale, the domain satisfaction index, general life satisfaction, and social, health, sleep, income, and leisure satisfaction. We cannot reject the null for work satisfaction and purpose in life. All treatment effects that are statistically significant based on unadjusted p-values remain significant after applying the Benjamini-Hochberg procedure to control the false discovery rate across the m = 56 mental health and wellbeing hypotheses tested in Table 2, Table A.1, Table A.2, and Table A.3, with the exception of sleep satisfaction 6 months after the program (Table A.2).

A.2 Treatment Effects on Mental Health and Wellbeing after Program

6 Months after Program:

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-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 A.2: Average treatment effects on mental health and wellbeing outcomes 6 months after the three-year cash transfer program: We report average treatment effects (ATE) in standard deviations on our outcomes as changes relative to the baseline, see outcome definitions in Table OA1. 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 domain satisfaction index, and health and sleep satisfaction. We cannot reject the null for work, social, leisure, and income satisfaction, and for general life satisfaction, and the PSS stress scale. All treatment effects that are statistically significant based on unadjusted p-values remain significant after applying the Benjamini-Hochberg procedure to control the false discovery rate across the m = 56 mental health and wellbeing hypotheses tested in Table 2, Table A.1, Table A.2, and Table A.3, with the exception of sleep satisfaction 6 months after the program (Table A.2).† Not significant after Benjamini-Hochberg adjustment.

18 Months after Program:

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-F	n treated	n control
Aggregates									
MHW Index	0.272	-0.091	0.362	0.106	3.416	0.000	0.004	100	1105
Mental Health	0.290	0.012	0.278	0.109	2.558	0.011	0.012	100	1104
Purpose in Life	0.149	-0.165	0.314	0.104	3.014	0.003	0.002	100	1103
Life Satisfaction	0.213	-0.123	0.336	0.128	2.627	0.009	0.014	100	1105
Aggregate components									
WHO-5 Depression	0.280	0.041	0.239	0.112	2.125	0.034	0.03	99	1102
PSS Stress	0.272	-0.027	0.299	0.106	2.812	0.005	0.01	99	1103
General Life Satisfaction	0.201	-0.080	0.281	0.129	2.178	0.029	0.036	99	1105
Domain Satisfaction Index	0.175	-0.151	0.326	0.129	2.517	0.012	0.008	100	1105
Domain satisfactions									
Health Satisfaction	-0.046	-0.340	0.294	0.125	2.359	0.018	0.018	100	1105
Sleep Satisfaction	0.130	-0.147	0.277	0.118	2.339	0.019	0.032	100	1105
Work Satisfaction	0.014	-0.203	0.217	0.129	1.688	0.091	0.088	99	1080
Income Satisfaction	0.162	0.037	0.125	0.126	0.993	0.321	0.302	100	1102
Leisure Satisfaction	0.329	0.143	0.186	0.142	1.311	0.190	0.218	100	1104
Social Satisfaction	0.093	-0.071	0.163	0.110	1.492	0.136	0.120	100	1102

Table A.3: Average treatment effects on mental health and wellbeing outcomes 18 months after the three-year cash transfer program: We report average treatment effects (ATE) in standard deviations on our outcomes as changes relative to the baseline, see outcome definitions in Table OA1. 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, general life satisfaction, the domain satisfaction index, and health and sleep satisfaction. We cannot reject the null for work, social, leisure, and income satisfaction. All treatment effects that are statistically significant based on unadjusted p-values remain significant after applying the Benjamini-Hochberg procedure to control the false discovery rate across the m = 56 mental health and wellbeing hypotheses tested in Table 2, Table A.1, Table A.2, and Table A.3, with the exception of sleep satisfaction 6 months after the program (Table A.2).

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

Outcome	Treated	Control	ATE	SE	t-stat	p-N	p-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 A.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.

A.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 A.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.

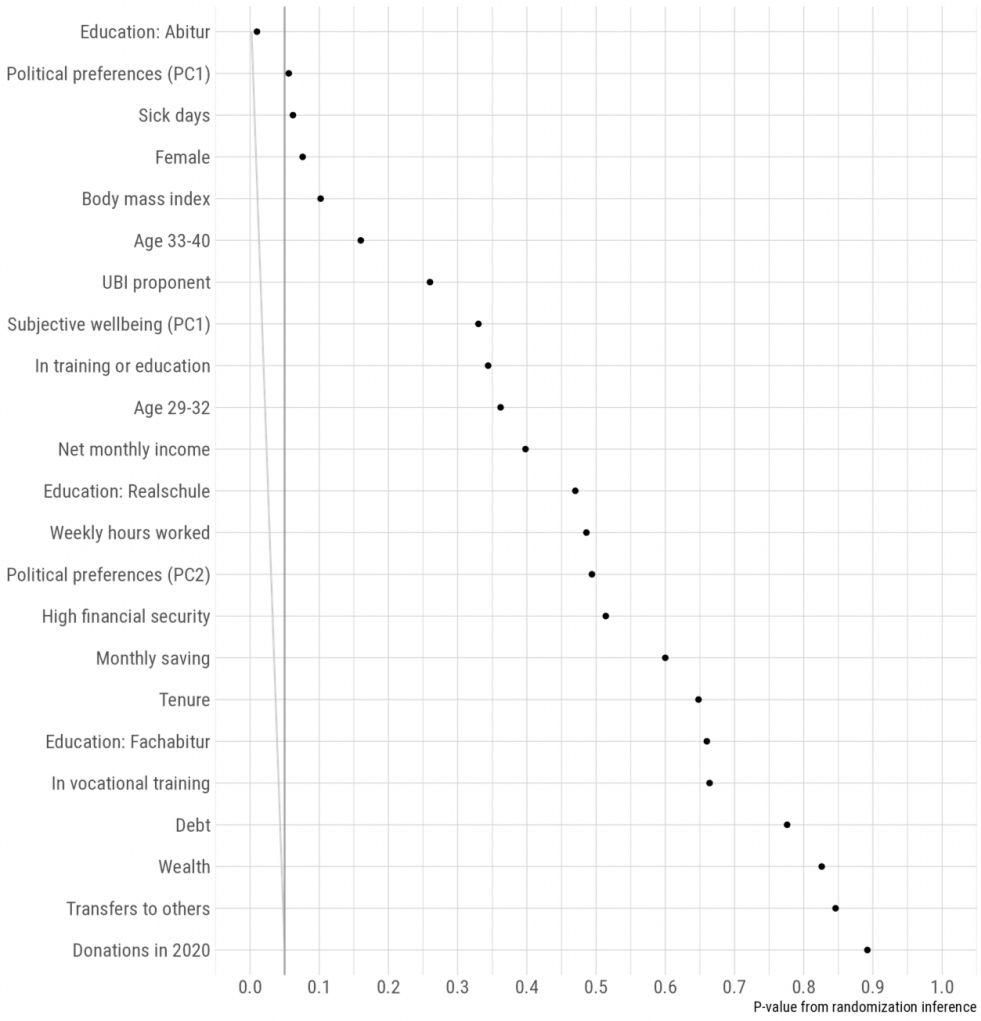

Figure A.1: P-values for differences between treatment and control of baseline covariates, after restricting the sample to observations without any missing waves.

A.5 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 A.6.[10]

	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 A.6: Descriptive statistics of the study sample (based on our survey data) relative to study population and German population (based on the SOEP).

A.6 Disappointed control

In Figure A.2, we plot the average main outcomes (non-standardized) in levels that are available also in the SOEP (income satisfaction, leisure satisfaction, general life satisfaction, sleep satisfaction, health satisfaction, and work satisfaction) and compare these average outcomes between our control group (solid black), our treatment group (solid green), our study population (dashed red), and the German population (dashed blue). Please note that the SOEP data is only available until 2023.

The alternative explanation for our treatment effects based on the disappointed-control conjecture would predict that the cash transfers depreciate outcomes of the control rather than improving outcomes in the treatment group. To identify this, we compare how outcomes evolve over time between our control condition and the study population and the German population. We do not find evidence for systematically different directional changes over time between the

[10] We used the SOEP to construct Table A.6. This forces us to restrict attention to the subset of the wellbeing outcomes that were elicited in 2021.

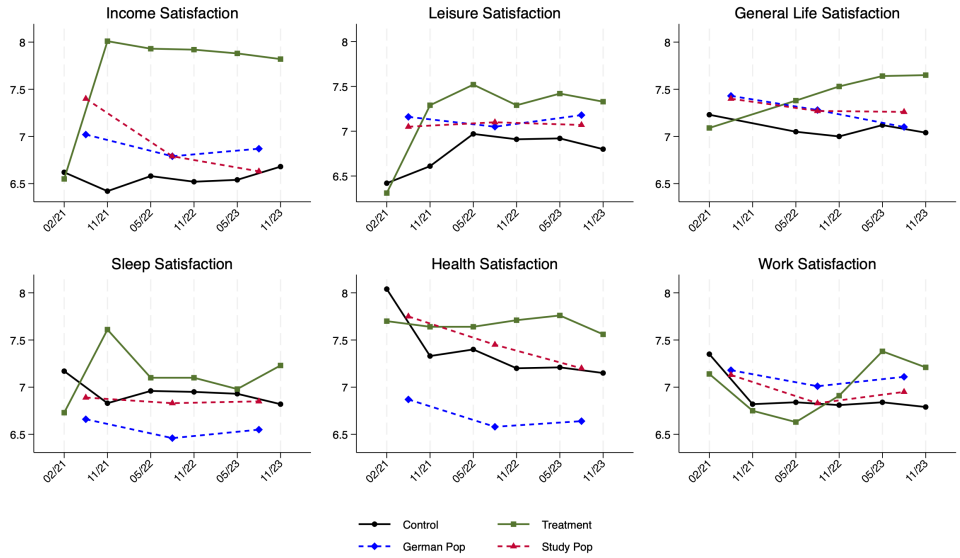

Figure A.2: Average domain-specific satisfaction outcomes over time for the treatment group (solid green), the control group (solid black), the study population (dashed red), and the German population (dashed blue). Each panel displays non-standardized mean satisfaction levels for income satisfaction, leisure satisfaction, general life satisfaction, sleep satisfaction, health satisfaction, and work satisfaction. The study population refers to the subset of the German population that would have been eligible to participate in our study in 2021. Study population and German population data are based on the German Socio-Economic Panel (SOEP), which is available through 2023. The trajectories of the control group, the study population and the German population across all domains do not provide evidence that control-group disappointment systematically depressed outcomes relative to the broader population.

control and the study population or the control and the German population. General life satisfaction, health satisfaction, and work satisfaction, for instance, decrease over time in the control and, similarly so, in the study population and German population; they however increase over time in the treatment group. Overall, 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.