The Effect of Economic Transfers on Mental Well-Being *

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Abstract

Transfers of cash or other economic interventions in randomized-controlled trials have received renewed attention from policymakers, philanthropists, academics, and the general public in recent years. However, there is little evidence on the systematic impact of economic interventions on mental health well-being. We reviewed 1,639 abstracts and 120 full-text papers for a final sample of 59 studies containing 240 treatment effects. We identified different economic interventions (conditional and unconditional cash transfers, poverty graduation programs, asset transfers, housing vouchers, health insurance provision, and lottery wins) with mental health outcomes (depression, stress or anxiety, and happiness). We find that economic interventions have a considerable positive effect on mental well-being: on average, an intervention had a positive impact of 0.125 standard deviations on the mental health outcome of the control group at endline. This impact is larger for unconditional cash transfers (0.172 SD). Effects do not differ substantially when transfers are directed to men vs. women, or are done via lump sum vs. not lump sum. Effects were larger in low-/middle-income countries partly due to the different composition of interventions taken in place.

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1. Introduction

According to WHO estimates, mental illnesses are amongst the most important sources of disease burden in the world. In Sub-Saharan Africa, they are thought to be the most prevalent non-communicable disease, ahead of malnutrition. This is perhaps not surprising because mental illness is strongly linked to poverty in cross-sectional analyses: low-income individuals have significantly higher rates of mental illness than wealthier individuals (Bromet et al. 2011; Lund et al. 2010). This correlational result gives rise to the possibility that there may be a causal relationship, potentially bi-directional, between poverty and mental health. In the public health literature, these twin hypotheses have been called "social causation" (poverty causes mental illness) and "social drift" (mental illness causes poverty). However, causal evidence on these relationships, and their magnitude, has been scant.

In recent years, policy makers, philanthropists, academics, and the general public have re-discovered direct transfers to low-income individuals as a powerful tool of poverty alleviation. These transfers take various forms, ranging from unconditional and conditional cash transfers to in-kind transfers, e.g. of food, to transfers of services such as insurance or training. A growing number of studies has investigated the impact of such transfers on welfare outcomes. Often, the outcomes of interest are economic in nature, including e.g. consumption, asset holdings, and labor supply. However, more recently, researchers have increasingly turned their attention to the impact of these interventions on subjective well-being, including measures of mental health.

The purpose of the present systematic review is to synthesize the evidence on the impact of economic transfers on mental health and subjective well-being. We focus on studies that meet the following criteria: first, the study has to use randomized assignment of transfers to identify causal effects. There are two study categories in our sample which both meet this criterion: randomized controlled trials (RCTs), and studies of lottery wins. Second, the intervention has to consist of an economic transfer, which we define as a transfer of money, goods, or services to individuals without a requirement for repayment. Note that this definition is relatively broad in that it includes not just cash transfers, but also, for example, asset transfers, provision of free insurance, and housing vouchers. Third, the study has to measure the impact of this intervention on an aspect of mental health or subjective well-being, including, for example, depression, happiness, or life satisfaction. We do not restrict the geographic location of the studies, although many are conducted in low-income countries and with low-income individuals as participants. Due to the relative paucity of studies targeting specific age groups, we also do not restrict the age of the recipients, although we study heterogeneity of treatment effects by age.

Using a systematic, pre-registered search strategy, we screened over 1,600 abstracts from published and unpublished research papers in economics, psychology, medical science, and other disciplines. We then reviewed the full-text of 120 papers and extracted information about the intervention and its effect on measures of mental health; most importantly, the monetary value of the intervention, and its treatment effect (in standard deviation units, SD) on measures of mental health. The final meta-analysis consists of 59 papers, in which 240 treatment effects were analyzed to determine the overall impact of economic transfers on mental health and subjective well-being.

We report two main findings. First, we observe considerable statistically significant positive effects of economic transfers on measures of mental health. The median intervention in our sample of studies makes a transfer worth USD 680 and thereby generates an improvement in mental health outcomes of 0.105 standard deviations (SD) two years after the intervention. There is no clear relation of effect size to transfer magnitude, possibly due to heterogeneity in samples. We did not find a differential effect on men vs. women, and on lumpsum interventions.

Second, unconditional cash transfers have shown the largest impact on mental health outcomes (0.172 SD). This increased difference is present through the distinct mental health outcomes.

Third, transfers in low-/ median-income countries (LMIC) have on average a larger effect (0.125 SD) than high-income countries (0.069 SD). However, this mark-up is partly due to the different composition of interventions, in which unconditional cash transfers and asset transfers, interventions with larger impacts, are more prevalent in LMIC.

2. Methods

2.1 Search strategy

Our aim was to examine the impact of economic transfers on mental health and subjective well-being. In June 2020, a systematic review protocol was registered with the international prospective register of systematic reviews, PROSPERO , with registration ID number CRD42020189558. We identified published studies, working papers, and technical reports in the electronic databases of PubMed and RePEc. We also reviewed the websites of known researchers in the field and reference lists to identify additional studies. Randomized controlled trials (RCTs) examining the impact of economic transfers on mental health were selected if they reported treatment effects on any aspect of mental health or subjective well-being, including, for example, depression, happiness, or life satisfaction. We did not restrict the publication date and geographic location of the studies, although many are conducted in low-income countries and with low-income individuals as participants. No further exclusion criteria were applied.

Two sets of search terms were used to search the PubMed database, and three sets to search RePEc for relevant articles through July 2020. The first set referred to economic transfers with terms including: “cash transfer”, “cash”, “income”, “lottery”, “graduation”, “debt relief”, “asset transfer”, and “housing voucher”. The second set referred to mental health and subjective well-being with terms including: “mental health”, “depression”, “psychological”, “wellbeing”, “happiness”, “well-being”, and “life satisfaction”. Due to an inability to restrict the sample to RCTs only in RePEc, a third set was added to the RePEc search strategy related to RCT design and included the following terms: “randomized”, “RCT”, and “trial”. Given that lottery wins are not typically examined through RCTs, we conducted an additional identical search in PubMed and RePEc to ensure the inclusion of these types of transfers without the restriction to an RCT design.

2.2 Selection criteria

For the initial search, two reviewers (KE, JR) used a software called abstrackr to independently screen abstracts and subsequently accept or reject each study for full text review. Following our Population Intervention Comparison Outcome (PICO) search strategy, abstracts were rejected if the studies did not include (1) an economic transfer intervention, defined as a transfer of money, goods, or services to individuals without a requirement for repayment, (2) an RCT study design or a lottery design, and (3) at least one quantitative mental health or subjective well-being outcome measure.

Furthermore, the mental health outcome was included if it complied with the following criteria: a) the outcome is a self report, that is an individual reports on an aspect of their own life using a Likert scale, b) the self report elicits feelings or thoughts about how the individual's life is going. [1], c) the self reports elicit assessments of life broadly: We don't accept feelings about a discrete domain of life, but we do accept an item that's an index of many domains. Any disagreements regarding the eligibility of particular studies were resolved through discussion with a third independent reviewer (JH).

The same two reviewers (KE, JR) independently reviewed the full text of the studies identified in the abstract screening phase and used a standardized, pre-piloted digital spreadsheet to extract data from all included studies. The following data were extracted: publication title and authors; study year; country; description of study population; population age range and/or average; share of female beneficiaries; type of economic transfer; transfer value in USD; details of the intervention and control conditions, including number of participants assigned to each group; delay between intervention and measurement of outcomes; description of mental health outcomes measured and times of measurement; and treatment effects. The extracted data was later reviewed by a third reviewer (JM), and then used to determine study eligibility for inclusion in the review. Discrepancies between the data extracted and the final determination to include or exclude a particular study were reconciled by the third independent reviewer (JH).

We classified the interventions into seven categories: unconditional cash transfers (UCTs); conditional cash transfers (CCTs); neighborhood programs (housing vouchers); graduation programs; lotteries; asset transfers; and insurance provision. Similarly, we classified the outcomes into four outcome groups: depression, stress or anxiety, happiness or life satisfaction, and self-esteem. Depression outcomes included the Center for Epidemiologic Studies Depression Scale (CES-D), Geriatric Depression Scale, John Hopkins Depression Checklist, Symptom-Driven Diagnostic System for Primary Care, among others, CES-D being the most common. Stress or Anxiety included the Kessler Psychological Distress Scale (K6), cortisol levels, the General Health Questionnaire (GHQ), Cohen's Perceived Stress Scale, etc., with the K6 being the most common. Happiness or Satisfaction included measures of self-reported happiness, life satisfaction, subjective well-being, and unhappiness (Q-12), the most common being self-reported happiness. Self-Esteem was mostly measured with the Rosenberg Self-Esteem Scale and the Tennessee Self-Concept Scale.

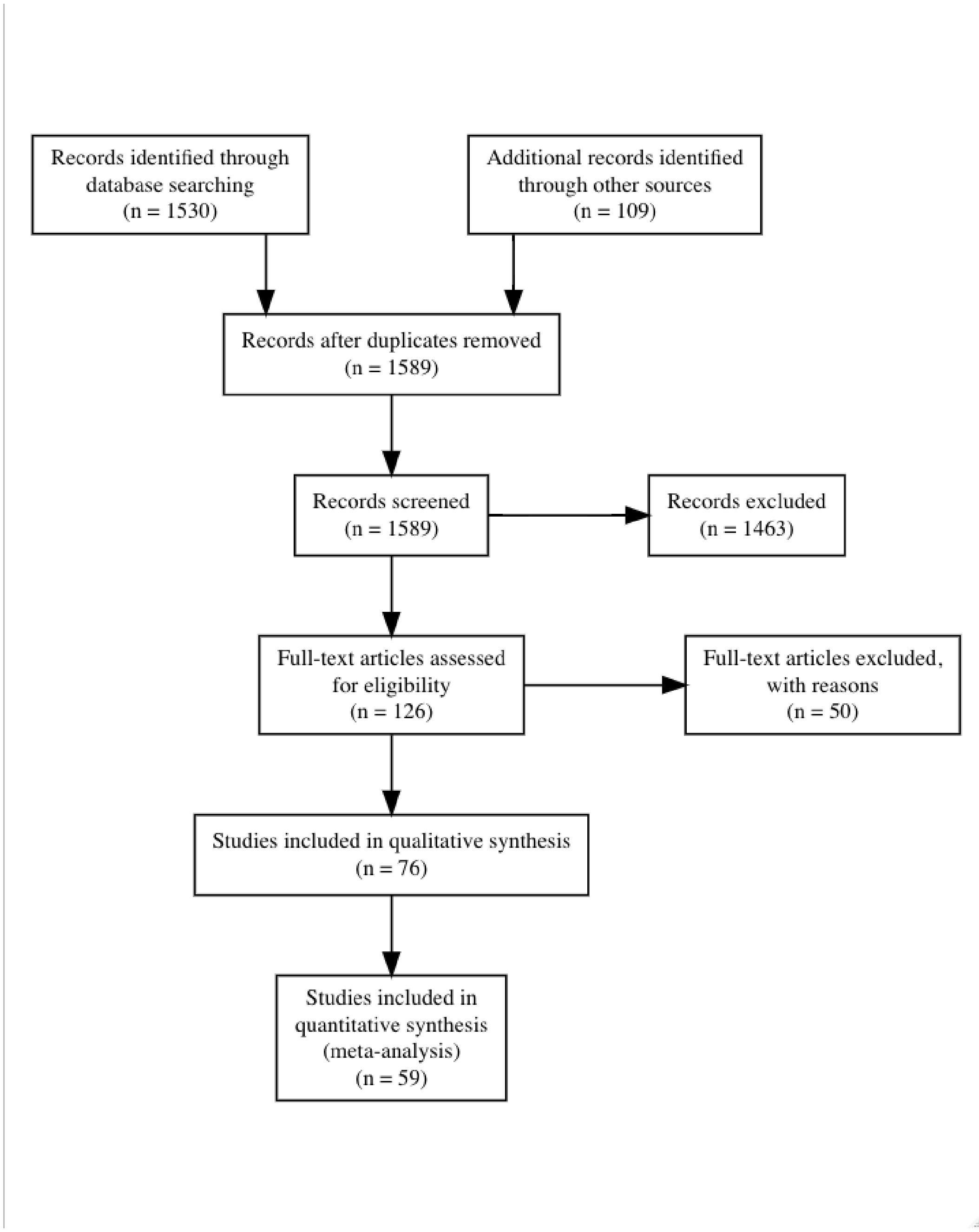
1. We are interested in questions that elicit assessments of how life is going.These can either pick up experiences / emotions or evaluations / moods. Many mental health questionnaires seem to ask about behaviors. These questions may individually determine but not necessarily reflect how someone feels.

We excluded outcomes that did not correspond to any of these groups, such as cognitive outcomes or anti-social behaviors. We also extracted treatment effects on index variables that summarize several mental health measures; however, to avoid double-counting outcomes, we do not include these variables in our meta-analysis.

The search followed PRISMA guidelines, and an overview of the process is shown in Figure 1. From 1,639 abstracts returned from the search strategy, 126 were chosen for full-text review in the screening process. Fifty of these papers were excluded after full-text review because they did not meet the inclusion criteria. From the remaining 76 papers, we extracted treatment effects for every mental health index and component reported. Thus, if a study reported treatment effects for more than one outcome variable, or more than one treatment, they were extracted separately[2]. This yielded 59 papers with 240 treatment effects to be analyzed.

2. To avoid double counting, we did not include general indexes of mental health well-being if more specific outcomes were extracted

Figure 1: PRISMA diagram for study selection and inclusion



Notes: Flow diagram using PRISMA guidelines.

2.3 Data analysis

2.3.1 Standardization

To make treatment effects comparable across studies, we began by standardizing them, i.e. converting them into standard deviation (z-score) units. This is accomplished by dividing the treatment effect with the standard deviation of the control group at endline. This is the correct standard deviation to use for the same reasons that the average outcomes of the control group at endline are the correct counterfactual for the treatment effect itself. Where the standard deviation of the control group at endline was not available, we estimated it as follows:

i) We check if the treatment is already standardized with the standard deviation of the control group. For example, we check if the treatment effect of an index is presented as a z-score. ii) If it is not standardized, we check if we have the standard deviation of the control group at the endline to do the standardization. iii) If this value is not available, then we check if it is a continuous or binary outcome. iv) If the outcome is a binary question, that is a yes or no question such as "Have you felt depressed in the last month?", we use the definition of a standard deviation of a proportion. If p is the percentage of respondents affirmative to a question, the standard deviation of the outcome is $SD = \sqrt{p(1-p)}$. We input this formula with the proportion of the control group at endline. If this is not available, we utilize the proportion of the control group at baseline. v) If the outcome is a continuous outcome, such as an index from 1-12 points, we approximate the standard deviation of the control group using the t test formula and assuming equal variances for the control and treatment group. Parting from t-test formula of difference in means $t = \frac{\bar{x}_1 - \bar{x}_2}{SE} = \frac{\bar{x}_1 - \bar{x}_2}{\sqrt{\frac{SD_1^2}{n_1} + \frac{SD_2^2}{n_2}}}$, and assuming equal variances $SD^2 = SD_1^2 = SD_2^2$ in both groups, we can clear the SD with respect the standard error of the treatment effect SE as: $SD = \sqrt{\frac{SE^2 n_1 n_2}{n_1 + n_2}}$, where n_1 and n_2 are the sample size of the treatment and control group.

Previous to the standardization, all “negative” outcomes, such as stress and depression, were recoded so that high values correspond to “positive” outcomes (i.e. the absence of stress or depression). All z-scores were adjusted for small sample sizes using Hedges’ formula.[3] The standard error of the treatment effect was standardized in the same fashion, thus holding the t-value constant.

2.3.2 Meta Analysis Methods

We analyzed the resulting 240 effect sizes using a random-effects (RE) meta-analysis model[4], with the Sidik-Jonkman estimator.[5] Each effect size gets weighted with the inverse of its standard error, thereby giving studies with greater precision more weight.

Because the same study often contributes more than one treatment effect, we use cluster-robust standard errors at the study level. We run this analysis both for all interventions and outcomes, and

3. Hedges’ adjustment for small sample sizes is $z^* = z \times (1 - \frac{3}{(4n_1 + 4n_2 - 9)})$.

4. Random effect models assume that true effects of each study are drawn from a distribution of true effects (Borenstein et al. 2010), while fixed effects (FE) models assume that all included studies share a common true effect

5. This estimator does not significantly differ from the rest of random effects methods. See Figure A1

separately for each intervention type and outcome. In addition, we run the analysis once for the sample as a whole, and for the subsets of low-/middle-income countries and high-income countries.

To assess the effect of characteristics of interventions and beneficiaries we implemented a meta regression by mental health outcome and type of economic intervention. We included variables for average age of the participants (or, where average age is not available, midpoint of the age range); intervention value in 1,000s of USD; average delay between intervention and outcome measurement; female share of the participants (ranging from 0 for all-male samples to 1 for all-female samples); and whether the intervention is lump sum or not.

We then examined evidence of publication bias with three adjustment methods: Vevea and Hedges 1995, Aert and Assen 2018a, and Andrews and Kasy 2019. See Appendix A2 for further information on these methods.

3. Results

3.1 Study Overview

Our final sample of 59 studies consists of 26 studies of unconditional cash transfers (Alzua et al. 2020; Angeles et al. 2019; Baird, Hoop, and Özler 2013; Bando, Galiani, and Gertler 2020; Ahbijit Banerjee et al. 2020; Blattman, Fiala, and Martinez 2011, 2014; Blattman, Jamison, and Sheridan 2017; Blattman, Fiala, and Martinez 2019; Egger et al. 2019; Fernald and Hidrobo 2011; Green et al. 2016; Haushofer and Shapiro 2016; Haushofer, Mudida, and Shapiro 2019, 2020; Heath, Hidrobo, and Roy 2020; Hjelm et al. 2017; Kilburn et al. 2018; McIntosh and Zeitlin 2020; Molotsky and Handa 2021; Muller, Pape, and Ralston 2019; Natali et al. 2018; Paxson and Schady 2010; Powell-Jackson et al. 2016; Roy et al. 2019; Stein et al. 2020); 8 studies with conditional cash transfers (Baird, Hoop, and Özler 2013; Buller et al. 2016; Kilburn et al. 2019; Macours, Schady, and Vakis 2012; Morris et al. 2017; Ohrnberger et al. 2020; E. J. Ozer et al. 2009; Emily J. Ozer et al. 2011); 2 of graduation programs and other multifaceted interventions such as enterprise development programs (Abhijit Banerjee et al. 2015; Ismayilova et al. 2018); 4 of asset transfers and savings programs (Edmonds and Theoharides 2020; Glass et al. 2017; Quattrochi et al. 2020; Richardson et al. 2018); 3 of insurance provision (Baicker et al. 2013; Finkelstein et al. 2012; Haushofer et al. 2020); 10 of housing vouchers (Fauth, Leventhal, and Brooks-Gunn 2004; Katz, Kling, and Liebman 2001; Kessler et al. 2014; Kling, Liebman, and Katz 2007; Leventhal and Brooks-Gunn 2003; Leventhal and Dupéré 2011; Ludwig et al. 2013; Nguyen et al. 2013; Osypuk et al. 2012; Sanbonmatsu et al. 2011); and 6 of lotteries (Gardner and A. Oswald 2001; Gardner and A. J. Oswald 2007; Kim and Oswald 2021; Kuhn et al. 2011; Lindqvist, Östling, and Cesarini 2020)[6].

An overview of studies and their characteristics is given in Table 1. Figure 2 shows the forest plot for the studies included in the paper. If we have more than one outcome per study, the forest plot presents the pooled estimate.

6. Studies can be repeated because they may have more than one type of economic intervention.

Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Age range	Country	Sample size
Alzua et al., 2021	UCT	384	Happiness or Satisfaction, Depression	1 year, 6 months	Poor households with beneficiaries 65+	65+	Nigeria	6059
Angeles et al., 2019	UCT	192	Depression	2 years	Ultra-poor, labor constrainted household	15 to 22	Malawi	1366
Baicker et al., 2013	Healthcare	6000	Depression, Happiness or Satisfaction	2 years	Adults	19 to 64	United States	12229
Baird et al., 2013	CCT, UCT	430	Stress	12 months, 24 months	Adolescent girls	13 to 24	Malawi	1820
Bando, 2021	UCT	1104	Depression	1 year	People 65 year old or older which live under the poverty line	65 or older	Paraguay	1939
Banerjee et al., 2015	Grad	6475	Anxiety, Stress, Happiness or Satisfaction	24 months, 36 months	Ultra poor households	18 to 60	Ethiopia, Ghana, Honduras, India, Pakistan, Peru	14595
Banerjee et al., 2020	UCT	675	Depression	30 months, 24 months	Households in poor countries	Adults, average 49 years	Kenya	4909
Blattman et al., 2011	UCT	374	Depression	24-30 months	Poor and underemployed “youth”—roughly ages 16 to 35	16 to 35	Uganda	1881
Blattman et al., 2014	UCT	374	Happiness or Satisfaction	4 years, 24-30 months	Poor and unemployed adults	16 to 35	Uganda	1996
Blattman et al., 2017	UCT	200	Depression	2-5 weeks, 12-13 months	High risk men from 18-35	18 to 35	Liberia	470
Blattman et al., 2019	UCT	374	Stress, Depression	9 years	Poor and unemployed adults - male	Young adults, average 25	Uganda	1868
Buller et al., 2016	CCT	240	Happiness or Satisfaction	8 months	Poor households in carchi	15 to 69	Ecuador	1226
Edmonds & Theoharides, 2020	Asset	518	Happiness or Satisfaction, Depression	3 years	Children of beneficiary households	5 to 17	Philippines	3620
Egger et al., 2019	UCT	1000	Depression	11 months	Poor households	NULL	Kenya	4121

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Age range	Country	Sample size
Fauth et al., 2004	Neighborhood	17227	Anxiety, Depression	almost 2 years	Adults	Adults on average 36 years	United States	315
Fernald et. al, 2011	UCT	360	Depression	2 years	Mothers of vulnerable households	Mothers on average 23 yeras	Ecuador	1196
Finkelstein et al., 2012	Healthcare	3500	Happiness or Satisfaction, Depression	14 months	Adults	20-60	United States	23741
Gardner & Oswald, 2001	Lottery	200	Happiness or Satisfaction, Stress	12 months	Lottery winners	Adults	United Kingdom	9493
Gardner & Oswald, 2007	Lottery	4303	Stress	4 years	Lottery winners	Adults	United Kingdom	12620
Glass et al., 2017	Asset	70	Anxiety, Depression	18 months	Households in conflict-affected villages	16 to 61	Congo	833
Green et al., 2016	UCT	150	Depression	16 months	Vulnerable people	14 to 30	Uganda	1726
Haushofer & Shapiro, 2016	UCT	354	Stress, Happiness or Satisfaction, Anxiety	9 months	Heads of poor households	Around 35	Kenya	1474
Haushofer et al., 2019	UCT	485	Stress, Happiness or Satisfaction, Depression	1 year	Rural population	Adults under 75	Kenya	2140
Haushofer et al., 2020	UCT, Healthcare	338	Stress, Happiness or Satisfaction, Depression	1 year	Metalworkers of the Kamukunji Jua Kali Association	18+	Kenya	693
Heath et al., 2020	UCT	324	Stress	18 months	Houseohld hat had a child aged 6–23 months - monogamous-male spouse	Adults, average age 32	Mali	2446
Hjelm et. al, 2017	UCT	396	Stress	36 months	Poor households with a child under the age of five.	Mothers on average 49 years old	Zambia	2490
Ismayilova et al., 2018	Grad	100	Depression	12 months, 24 months	Children	10 to 15	Burkina Faso	240

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Age range	Country	Sample size
Katz et al., 2001	Neighborhood	20307	Anxiety, Happiness or Satisfaction, Depression	On average 2.2 years, with a range from 1 to 3.5 years	Adults	19 to 50	United States	412
Kessler et al., 2014	Neighborhood	102627	Depression	10-15 years	Adolescents	13 to 19	United States	182
Kilburn et al., 2018	UCT	85	Happiness or Satisfaction	17 months	Caregivers in ultra-poor, labor-constrainted households	Caregivers (around 57 years old)	Malawi	6896
Kilburn et al., 2019	CCT	480	Depression	36 months	Households with never-married or pregant females aged 13-20 attending grades 8-11	16 to 23	South Africa	2533
Kim, 2020	Lottery	254	Happiness or Satisfaction	1 year	Lottery players	Adults	Singapore	5626
Kling et al., 2007	Neighborhood	43827	Stress, Depression	5 years after on average	All	25 to 54	United States	2533
Kuhn et al., 2011	Lottery	30000	Happiness or Satisfaction	6 montths	Lottery winners	Adults on average 50.	Netherlands	1458
Leventhal & Brooks-Gunn, 2003	Neighborhood	27027	Stress, Depression	3 years	Adults	Parents (around 35), 8 to 18, parents (around 35)	United States	369
Leventhal & Dupere, 2011	Neighborhood	52227	Stress, Anxiety	5-7 years	Adolescents (girls)	12 to 19	United States	1780
Lindqvist et al., 2020	Lottery	1e+05	Stress, Happiness or Satisfaction	5 to 22 years	Lottery players	Adults, average age 59	Sweden	3331
Ludwig et al., 2013	Neighborhood	85827	Depression, Stress, Happiness or Satisfaction	10-15 years	Female youth from low-income families	15 to 20, Adults around 43 years	United States	2595
Macours, 2012	CCT	252	Happiness or Satisfaction, Depression	9 months, 9 moths, 2 years	Mothers of vulnerable households	Mothers on average 42 years	Nicaragua	1857

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Age range	Country	Sample size
McIntosh & Zeitlin, 2020	UCT	750	Depression	15 months	Youth 16-30 from poor households with less than secondary education,	16 to 30	Rwanda	666
Molotskya, 2020	UCT	120	Happiness or Satisfaction, Stress	Combined Follow up waves	Vulnerable households	Caregivers (around 57 years old)	Malawi	7551
Morris et al., 2017	CCT	6000	Depression, Anxiety	2 years	Adolescents	9th grade (around 14)	United States	511
Muller et al., 2019	UCT	1000	Happiness or Satisfaction		Youth	18 to 34	South Sudan	1495
Natali e al., 2018	UCT	816	Happiness or Satisfaction	36 months, 48 months	Women in poor households	Mothers on average 29 years	Zambia	2203
Nguyen et al., 2013	Neighborhood	48027	Stress	4-7 years	Adolescents (girls)	12 to 19	United States	1426
Ohrnberger et al., 2020	CCT	25	Happiness or Satisfaction	1 year	Adults (age 16+) living in three rural districts	Adults older than 16	Malawi	790
Osypuk et al., 2012	Neighborhood	48027	Stress	4-7 years	Youth (girls)	1589587200	United States	2829
Ozer, 2009	CCT	2193	Anxiety	51 months	Children of vulnerable households	Children on average 5 years	Mexico	945
Ozer, 2011	CCT	2193	Depression	51 months	Mothers of vulnerable households	Mothers on average 37 years	Mexico	6343
Paxson & Schady, 2010	UCT	255	Depression, Stress	17 months	Mothers 1st quartile	Around 24	Ecuador	1046
Powell-Jackson et al., 2016	UCT	15	Depression, Happiness or Satisfaction, Anxiety	11 months	Women giving birth	15 to 49	India	1695
Quattrochi, 2020	Asset	14	Depression, Happiness or Satisfaction	6 weeks, 1 year	Vulnerable households	Adults on average 35 years	Congo	769
Richardson et al., 2018	Asset		Stress	12 months	Poor households	Adults, average age 30	India	3041

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Age range	Country	Sample size
Roy et al., 2019	UCT	456	Happiness or Satisfaction	4 years after	Poor households	Adults, average age 27	Bangladesh	1989
Sanbonmatsu et al., 2011	Neighborhood	85827	Stress	10-15 years	Adults	21 to 30, 10 to 20	United States	4644
Stein et al., 2020	UCT	1000	Happiness or Satisfaction	7 months aprox	Refugee households	NULL	Uganda	1264

3.2 Pooled effect of transfers on mental health and well-being

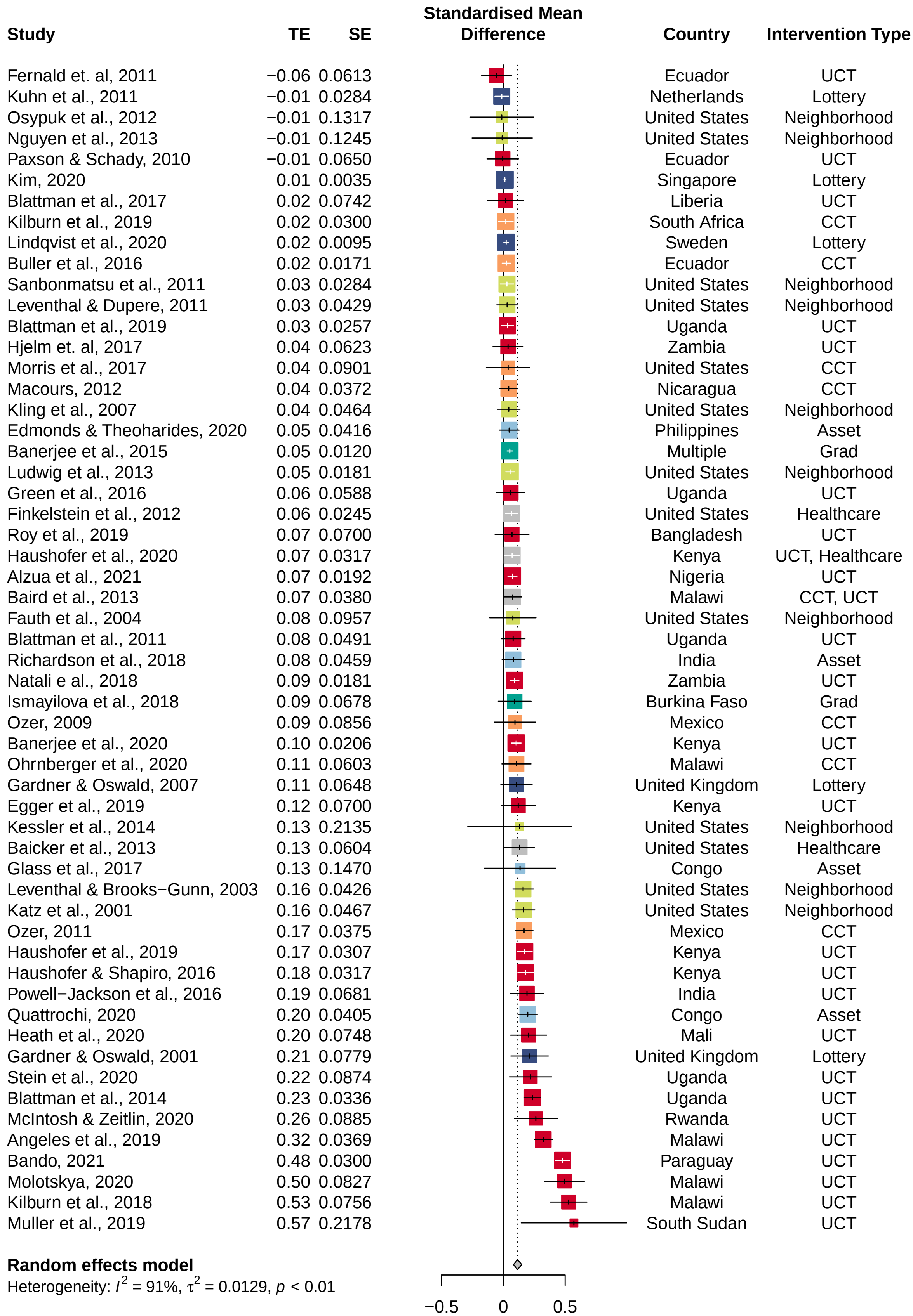
We extracted 240 treatment effects from these studies. Multiple treatment effects in one study occur when different interventions are delivered (e.g. large vs. small cash transfers), or when separate treatment effects are reported for different subgroups (e.g. cash transfers to men and women).

Table 2 shows the pooled treatment effects for different kinds of economic interventions and various mental health outcome generated using the meta-analysis approach described above. Each cell corresponds to a single meta-analysis regression, and shows the meta-analytic effect size and its standard error.

We find an overall effect of 0.105 SD of any transfer on mental health outcomes, statistically significant at the 1 percent level. There is some heterogeneity across types of intervention: the largest statistically significant treatment effect is observed for unconditional cash transfers, which increase measures of mental health by 0.172 SD, significant at the 1 percent level. Poverty graduation programs had a statistically significant effect on mental health of 0.052 SD, significant at the 5 percent level. Health insurance provision, studied in the Oregon Health Insurance Experiment and an RCT in Kenya, improved mental health by 0.089 SD, significant at the 10 percent level (note that the concept of significance at the 10 percent level is used in economics but not psychology and medical science). In the Kenya study, this was driven by a reduction in stress and the stress hormone cortisol, while in the US study, it was driven by an improvement in a short depression module. Lotteries had an effect of 0.069 SD, significant at the 10 percent level. Asset transfers generated a 0.149 SD increase in mental health, statistically significant at the 5 percent level. Housing vouchers, such as the neighborhood program "Moving to Opportunity", and a similar program in New York, in which low-income families in the USA received vouchers to move to wealthier neighborhoods, had a effect on mental health of 0.067, significant at the 5 percent level. Conditional cash transfer (CCT) programs had a positive effect of 0.052 SD, significant as well at the 5 percent level.

Turning to different outcome variables, we observe the largest effect of transfers on happiness (0.138 SD) and on depression (0.132 SD), both significant at the 1 percent level. Stress and anxiety show a 0.058 SD improvement on average, significant at the 1 percent level. The largest overall effects are generated by UCTs on happiness (0.239 SD), significant at the 1 percent level. Figure 3 presents a comparison of these effects by intervention type and mental health outcome.

Figure 2: Metanalysis Studies



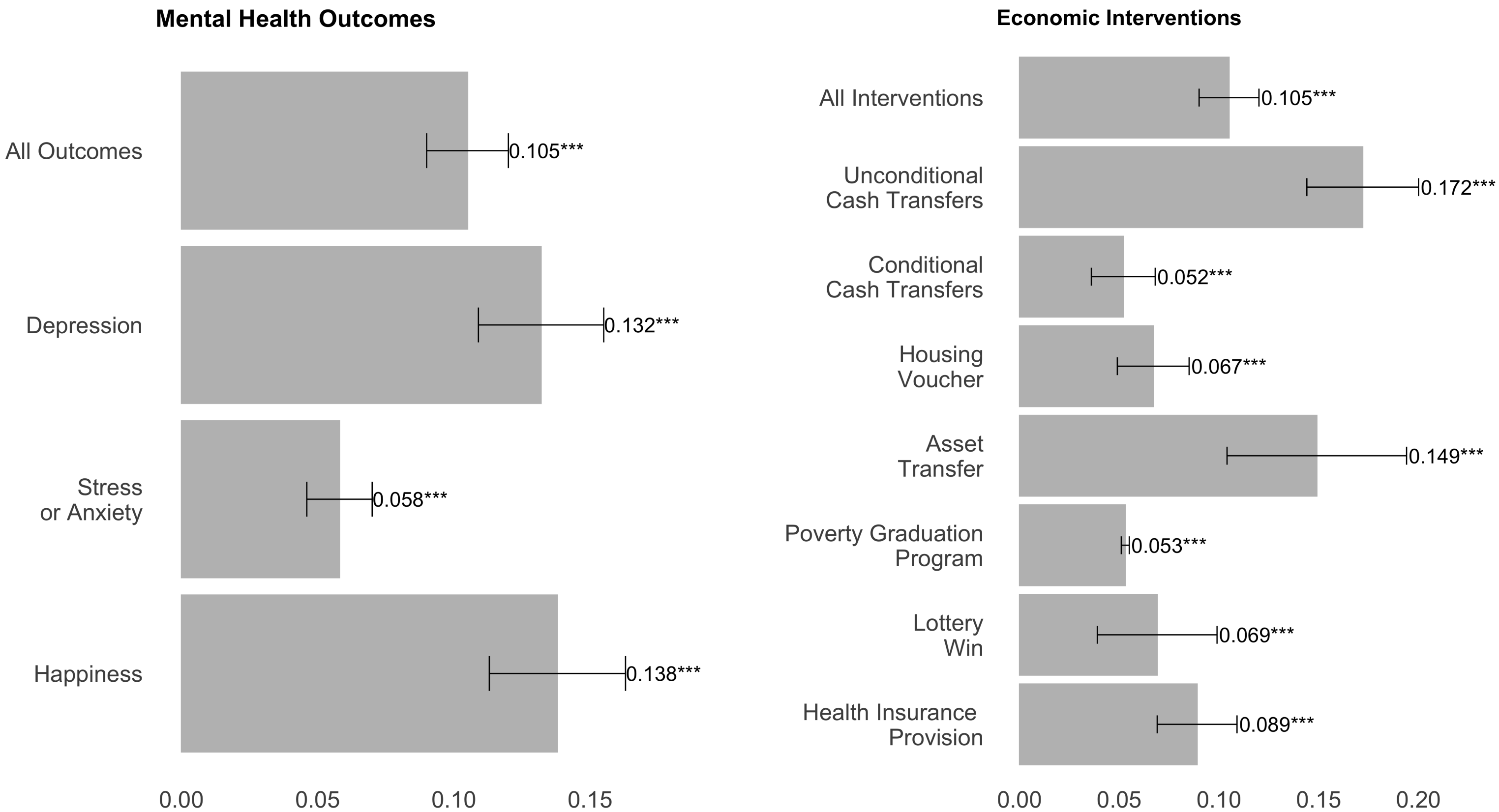
Notes: Point estimates of the pooled effects of the different mental health outcomes per study. Study lines show 95% confidence interval. Size according to sample size. Outcomes are colored by type of economic intervention.

Table 2: Pooled Random Effects

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.105	0.132	0.058	0.138
	(0.015)***	(0.023)***	(0.012)***	(0.025)***
	[240 / 59]	[82 / 33]	[92 / 28]	[66 / 27]
Unconditional Cash Transfer	0.172	0.165	0.095	0.239
	(0.028)***	(0.038)***	(0.030)**	(0.052)***
	[84 / 26]	[38 / 15]	[19 / 10]	[27 / 12]
Conditional Cash Transfer	0.052	0.052	0.058	0.036
	(0.016)**	(0.041)	(0.020)	(0.016)
	[16 / 8]	[5 / 4]	[6 / 3]	[5 / 3]
Housing Voucher	0.067	0.110	0.044	0.057
	(0.018)**	(0.031)**	(0.013)**	(0.007)*
	[63 / 10]	[24 / 6]	[33 / 9]	[6 / 2]
Poverty Graduation Program	0.053	0.092	0.026	0.098
	(0.002)**	(0.068)	(0.014)*	(0.016)***
	[35 / 2]	[2 / 1]	[20 / 1]	[13 / 1]
Lottery Win	0.069	—	0.117	0.020
	(0.030)*		(0.036)*	(0.009)
	[14 / 6]	[0 / 0]	[8 / 3]	[6 / 5]
Asset Transfer	0.149	0.137	0.083	0.181
	(0.045)**	(0.057)	(0.006)**	(0.044)
	[13 / 4]	[6 / 3]	[2 / 2]	[5 / 2]
Health Insurance Provision	0.089	0.075	0.179	0.092
	(0.020)**	(0.041)	(0.064)**	(0.025)*
	[11 / 3]	[5 / 3]	[2 / 1]	[4 / 3]

Notes: Meta-analytic effect sizes for specific combinations of interventions (rows) and outcomes (columns). The first row shows the impact of any intervention on various mental health outcomes, the remaining rows correspond to specific interventions. Similarly, the first column reports the effect of interventions on any mental health outcome, while the remaining columns focus on specific outcomes. Each cell shows the meta-analytic effect size estimate using random effects with the Sidik-Jonkman estimator. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

Figure 3: Pooled Effect of Economic Interventions on Mental Health Outcomes (SD)



Notes: Meta-analytic effect sizes using random effects using the Sidik-Jonkman estimator. The figure on the left shows the impact on mental health outcomes. The figure on the right shows the impact of different economic interventions.

In summary, economic interventions have a consistent and robust positive effect on mental health. We observe no significantly negative impacts.

3.3 Moderator Analysis

To understand how specific intervention characteristics may influence the impact of economic transfers on mental health outcomes, we examined five variables: i) average age of the beneficiaries, ii) a binary variable indicating if the intervention took place in a low-/middle-income country, iii) share of female beneficiaries, iv) value of the intervention as % of the GDP per capita, and v) years since the transfer began until the survey is taken. We estimate this specification by type of intervention (Table 3) and mental health outcomes (Table 4) using the following equation:

$$y_i = \beta_0 + \beta_1 age_i + \beta_2 LMIC_i + \beta_3 Female_i + \beta_4 Lumpsum_i + \beta_5 value_i + \beta_6 years_i + \epsilon_i$$

We could not detect any moderator effect that is uniform throughout all the intervention types or mental health outcomes. However, the most prominent finding is the statistically significant negative effect of the delay between intervention and survey, which is present in unconditional cash transfers, lottery wins, and asset transfers and is consistently negative throughout all mental health outcomes (though only statistically significant for depression outcomes).

To mitigate the possible loss of power caused by breaking the studies into seven intervention types, we explore this analysis with a division of "in-cash" and "in-kind" transfers on Table A6. We confirm the finding that the delay between intervention and survey has a negative effect on the impact on mental health outcomes, this time statistically significant for both in-kind and in-cash transfers.

Table 3: Determinants of Treatment Effects by Intervention Type

	(1) All Interventions	(2) Unconditional Cash Transfer	(3) Conditional Cash Transfer	(4) Housing Voucher	(5) Poverty Graduation Program	(6) Lottery Win	(7) Asset Transfer	(8) Health Insurance Provision
Constant	0.111	0.147	0.019	0.015	0.126	-1.987	0.572	0.575
	(0.061)*	(0.162)	(0.135)	(0.077)	(0.112)	(0.551)***	(0.598)	(0.560)
Age	0.000	0.001	0.002	-0.001	0.000	0.041	-0.007	-0.011
	(0.001)	(0.003)	(0.004)	(0.002)	(0.003)	(0.010)***	(0.021)	(0.013)
Low-/Middle-Income Country	0.023	—	0.042	—	—	—	—	—
	(0.033)		(0.062)					
Female Share	2.652	-1.785	-13.005	17.773	-0.289	-5.885	-12.731	—
	(4.048)	(7.179)	(15.155)	(5.894)**	(10.021)	(7.372)	(102.785)	
Lump Sum	-0.013	-0.011	-0.011	—	—	—	0.072	—
	(0.037)	(0.057)	(0.054)				(0.233)	
Intervention Value (as % of GDP per capita in thousands)	-5.881	389.825	103.769	206.633	-42.492	0.067	—	0.053
	(16.727)	(428.187)	(308.264)	(220.787)	(23.130)*	(0.019)***		(0.125)
Delay Intervention-Survey (Years)	-0.008	-0.019	0.023	-0.069	-0.021	-0.044	-0.147	0.044
	(0.005)	(0.006)**	(0.029)	(0.074)	(0.023)	(0.014)**	(0.080)*	(0.119)
Observations/Studies	[240 / 59]	[84 / 26]	[16 / 8]	[63 / 10]	[35 / 2]	[14 / 6]	[13 / 4]	[11 / 3]

Notes: Meta regression of possible determinants of effect sizes. Each row is a independent variable. Each column is a regression for distinct economic interventions using random effects with the Sidik-Jonkman estimator. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

Table 4: Determinants of Treatment Effects by Mental Health Outcome

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
Constant	0.111	0.137	0.075	0.135
	(0.061)*	(0.084)	(0.071)	(0.121)
Age	0.000	-0.001	0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.002)
Low-/Middle-Income Country	0.023	0.099	-0.057	0.093
	(0.033)	(0.061)	(0.036)	(0.066)
Female Share	2.652	2.786	6.663	-6.485
	(4.048)	(6.605)	(5.761)	(8.937)
Lump sum	-0.013	-0.041	-0.003	-0.034
	(0.037)	(0.060)	(0.066)	(0.062)
Intervention Value (as % of GDP per capita in thousands)	-5.881	58.401	-16.483	4.608
	(16.727)	(38.145)	(13.787)	(27.431)
Delay Intervention-Survey (Years)	-0.008	-0.020	-0.007	-0.005
	(0.005)	(0.010)**	(0.007)	(0.008)
Observations/Studies	[240 / 59]	[82 / 33]	[92 / 28]	[66 / 27]

Notes: Meta regression of possible determinants of effect sizes. Each row is a independent variable. Each column is a regression for distinct mental health outcomes using random effects with the Sidik-Jonkman estimator. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

3.3.1 Heterogeneity Low-/Middle-Income Countries vs. High-Income Countries

To further explore the heterogeneity of the economic intervention effects on mental health outcomes regarding the income of the country in which the intervention took place, we repeat the analysis described in the section 3.2 for samples of studies in low-/middle income countries (LMIC) and in high-income countries (HIC), in tables 5 and 6 respectively. We find larger overall effects on LMIC (0.125 SD) than HIC (0.069 SD). The impacts are higher for depression and happiness, and similar in stress or anxiety. However, this larger effect may be due to the different composition of intervention types: unconditional cash transfers and asset transfers, interventions with the highest estimated effects, are present in LMIC and not HIC.

Table 5: Pooled Random Effects: Studies in Low-/Middle-Income Countries

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.125	0.148	0.057	0.165
	(0.024)***	(0.030)***	(0.019)**	(0.034)***
	[154 / 39]	[52 / 23]	[50 / 15]	[52 / 18]
Unconditional Cash Transfer	0.172	0.165	0.095	0.239
	(0.028)***	(0.038)***	(0.030)**	(0.052)***
	[84 / 26]	[38 / 15]	[19 / 10]	[27 / 12]
Conditional Cash Transfer	0.052	0.067	0.048	0.036
	(0.018)**	(0.046)	(0.013)	(0.016)
	[14 / 7]	[4 / 3]	[5 / 2]	[5 / 3]
Housing Voucher	—	—	—	—
	[0 / 0]	[0 / 0]	[0 / 0]	[0 / 0]
Poverty Graduation Program	0.053	0.092	0.026	0.098
	(0.002)**	(0.068)	(0.014)*	(0.016)***
	[35 / 2]	[2 / 1]	[20 / 1]	[13 / 1]
Lottery Win	—	—	—	—
	[0 / 0]	[0 / 0]	[0 / 0]	[0 / 0]
Asset Transfer	0.149	0.137	0.083	0.181
	(0.045)**	(0.057)	(0.006)**	(0.044)
	[13 / 4]	[6 / 3]	[2 / 2]	[5 / 2]
Health Insurance Provision	0.110	0.080	0.179	0.028
	(0.049)**	(0.100)	(0.064)**	(0.067)
	[5 / 1]	[1 / 1]	[2 / 1]	[2 / 1]

Notes: Meta-analytic effect sizes of transfers on mental health and related outcomes. This table reproduces Table 2, except that it only includes studies in Low and Middle Income Countries. All other characteristics are as in Table 2.

Table 6: Pooled Random Effects: Studies in High Income Countries

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.069	0.100	0.059	0.043
	(0.013)***	(0.024)**	(0.015)**	(0.014)**
	[86 / 20]	[30 / 10]	[42 / 13]	[14 / 9]
Unconditional Cash Transfer	—	—	—	—
	[0 / 0]	[0 / 0]	[0 / 0]	[0 / 0]
Conditional Cash Transfer	0.038	-0.052	0.129	—
	(0.090)	(0.088)	(0.088)	
	[2 / 1]	[1 / 1]	[1 / 1]	[0 / 0]
Housing Voucher	0.067	0.110	0.044	0.057
	(0.018)**	(0.031)**	(0.013)**	(0.007)*
	[63 / 10]	[24 / 6]	[33 / 9]	[6 / 2]
Poverty Graduation Program	—	—	—	—
	[0 / 0]	[0 / 0]	[0 / 0]	[0 / 0]
Lottery Win	0.069	—	0.117	0.020
	(0.030)*		(0.036)*	(0.009)
	[14 / 6]	[0 / 0]	[8 / 3]	[6 / 5]
Asset Transfer	—	—	—	—
	[0 / 0]	[0 / 0]	[0 / 0]	[0 / 0]
Health Insurance Provision	0.080	0.081	—	0.100
	(0.024)	(0.058)		(0.023)
	[6 / 2]	[4 / 2]	[0 / 0]	[2 / 2]

Notes: Meta-analytic effect sizes of transfers on mental health and related outcomes. This table reproduces Table 2, except that it only includes studies in High Income Countries. All other characteristics are as in Table 2.

3.4 Publication Bias

To address for the possibility of publication bias (the higher likelihood of publishing studies that are statistically significant, that confirm some prior belief or, conversely, that are surprising (AK 2019)), we adjust the estimates using three methods: Vevea and Hedges (1995), P-uniform*, and Andrew & Kasy. These methods were chosen because they allow us to have between-study variance of outcomes and results that are not statistically significant (see Appendix A2 for more information on the specification of these methods).

Tables 7 and 8 show the adjusted effects of each publication bias correction method by mental health outcome and intervention type respectively. Results are consistent and significant overall, showing no signs of publication bias. The lack of evidence of publication bias may be due to the fact that most of these papers come from field studies, which tend to publish their results regardless of the outcome. In addition, a considerable section results were extracted from studies in which they were not the primary outcomes but were part of the moderator analysis (which are less prone to be selectively published.

Table 7: Publication Bias Adjustment by Mental Health Outcome

	(1) Naïve Estimation	(2) P-uniform*	(3) Andrews & Kasy	(4) Vevea & Hedges
All Interventions	0.105	0.114	0.120	0.141
	(0.015)***	(0.032)***	(0.012)***	(0.023)***
Depression	0.132	0.154	0.145	0.179
	(0.023)***	(0.063)**	(0.022)***	(0.059)**
Stress or Anxiety	0.058	0.071	0.052	0.094
	(0.012)***	(0.050)*	(0.013)***	—
Happiness	0.138	0.167	0.151	0.282
	(0.025)***	(0.066)**	(0.023)***	(0.080)***

Notes: Meta-analytic effect sizes adjusted for publication bias with different methods for each mental health outcome. Each row is a distinct health outcome. Each column is a different method to adjust for publication bias: the naïve estimation, p-uniform*, Andrews&Kasy and Vevea &Hedges. See Appendix for more information. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

Table 8: Publication Bias Adjustment by Intervention Type

	(1) Naïve Estimation	(2) P-uniform*	(3) Andrews & Kasy	(4) Veva & Hedges
All Interventions	0.105	0.114	0.120	0.141
	(0.015)***	(0.032)***	(0.012)***	(0.023)***
Unconditional Cash Transfer	0.172	0.218	0.197	0.267
	(0.028)***	(0.067)***	(0.026)***	(0.049)***
Conditional Cash Transfer	0.052	0.075	0.077	0.099
	(0.016)**	(0.122)	(0.030)**	(0.053)*
Housing Voucher	0.067	0.093	0.034	0.121
	(0.018)**	(0.071)*	(0.008)***	(0.028)***
Poverty Graduation Program	0.053	0.063	0.047	0.082
	(0.002)**	(0.065)	(0.014)***	—
Lottery Win	0.069	0.044	0.076	0.058
	(0.030)*	(0.074)	(0.039)**	—
Asset Transfer	0.149	0.201	0.203	0.255
	(0.045)**	(0.167)	(0.051)***	(0.315)
Health Insurance Provision	0.089	0.111	0.057	0.142
	(0.020)**	(0.125)	(0.019)**	—

Notes: Meta-analytic effect sizes adjusted for publication bias with different methods for each economic intervention. Each row is a distinct economic intervention. Each column is a different method to adjust for publication bias: the naïve estimation, p-uniform*, Andrews&Kasy and Vevea &Hedges. See Appendix for more information. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

4. Discussion

In this systematic review, we have summarized the evidence on the effect of economic transfers on measures of mental health and subjective well-being. We generally find positive effects of transfers, with an improvement especially in depression and happiness and life satisfaction. These improvements are most robustly documented for unconditional cash transfers (UCTs). The effects of UCTs, but not other interventions, are somewhat larger for younger recipients. Effects do not differ much depending on the gender of the recipient and the transfer amount. There is a small negative effect of the delay between the intervention and the outcome measurement on the observed effect size.

Together, these results suggest that economic transfers are effective in improving mental health and related outcomes, lending support to the "social causation" hypothesis familiar to public health researchers. Our findings were confirmed by qualitative interviews with people with lived experience of unconditional cash transfers. These interviews additionally shed some light on the mechanisms through which transfers improve mental health and related outcomes: it appears that improved relations with others, and an increased ability to support one's family, are prominent reasons for the improvements. It is worth noting that existing research has also suggested domestic violence as an important mechanism by which transfers may affect mental health (Bryant et al., 2017; Haushofer et al., 2019); this mechanism may have been difficult to observe in the somewhat impersonal phone interviews we conducted here.

While the present study is a helpful step, it has a number of limitations. First, the number of studies is small overall, and especially for individual intervention types and outcome variables, many cells only contain a single or a small number of studies. This fact raises concerns about external validity, because the inferences drawn for the particular combination of intervention type and outcome are confounded with the characteristics of the specific studies, including location, sample, intervention value, and delay between intervention and outcome measurement. Second and relatedly, there is substantial heterogeneity in the effect sizes we observe, deepening the concerns about individual combinations of interventions and outcomes being driven by study-specific characteristics, and also weakening the case for summarizing these findings using meta-analysis. Table A7 shows that the overall heterogeneity is of 93.1%, a substantial measure. However, this measure fluctuates between 0 and 97 for distinct estimations of intervention types and mental health outcomes. At the same time, however, we note that the almost uniformly positive treatment effects of the interventions in question, despite their heterogeneity, suggests that negative or zero impacts are unlikely.

Together, our results suggest that the renewed interest of policy-makers and others in economic transfer as welfare interventions is supported not only by the economic impact of such interventions, but also by their effects on mental health.

TO-DO

- two rows to mental health outcomes done
- decimals aligned pending
- long dashes done
- multiply value/gdppc * 1MM to see numbers done
- change label: Intervention Value (as % of GPPC x 1,000) done
- graphs: flip the bars on the side. make them all black. increase the size of the text. one panel for different interventions, one panel for different outcomes. Add significance stars to the graphs (look at Johannes example)done
- Change variable name to Delay intervention - survey (years) over two rows.done - change column Grad to Graduation done - change All to All Interventionsdone
- make a table with the publication bias 3psm (intercept) puniform(p*), ak. one that groups across interventions but shows the different health outcomes, and one that shows all interventions for one oucome. row 1: all interventions, row 2: uct, etc. each column 1 is the naive estimate, column 2-4 is the different publication methods. done
- appendix on publication bias done
- improve standardization description done
- start cleaning and tossing out things to archive.tex done
- which can of transformation from the treatment effect to the standard deviation. Derive formula on footnote (t-test -> assume equal variances). Narrate this in a whole paragraph. "we proceed as follows: the standard error of the treatment effect is .... assuming equal variances... we can solve for the standard deviation" show step of equalizing S1 = S2 -> $2S^2$. change sigma to S.done
- narrate footnote 2 to define what is p. for dummy outcomes we procede as follows. we use the definition of the standard deviation of a proportion which is ..., where p is the proportion of individuals of the sample for whom the outcome is one. for example, the share of people classified that "ever had depression symptoms". show order of p chosen (endline control, etc) done
- increase font size of bar graph done
- update study overview type and outcomes done
- notation for standard deviation is SD done
- table notesdone

- the clustered SEs should be in the table note done
- table 6 determinants of treatment effects by intervention type, table 7 determinants of treatment effects by outcome varianle done
- table 8 misspelled Naïve and Andrews : publication bias adjustment by intervention type. table 9 publicatiom bias adjustment by outcome variabledone
- get rid of p-curve done
- update reference list to include all the papers done
- update results on the main text and order section done
- explore reducing number of categories according to other criteria (for example cash vs in-kind). done
- verify we are keeping interventions that are cash + extra in the matrix. pending
- intervention value: make it as large to see non-zero coefficients one digit after the decimal point)done
- table 6-8 notes are incomplete. done
- write GDPPC as GDP per capita done
- write down observations instead of Obs done
- write LMIC (Low-/Middle-Income Country) done
- better column titles:Unconditional cash transfer / Conditional cash transfer / Housing voucher / Poverty graduation program / Lottery win / Asset transfer / Health insurance provision done
- text aligned to new results done
- re-structure the paper: abstract - introduction-methods (data/standardization/etc)-results . drop section of lived experience done
- review references are equal to the overview table (haushofer, mudida shapiro 2019) done
- appendix: keep consistency. either author names or change vevea hedges to 3PSM. done
- change phrasing to "the adjusted result for "all intervention and all mental health outcomes" statistically significant at the 1 percent level. The following table: should be table A4. and summarize sentence of the effect done
- update figure of extraction (prisma flow diagram)done
- list of different outcomes per categories pending

- foresplot: Make it more complete, include average transfer value amount, intervention type, country done
- heterogeneity in discussion I^2. Include table in appendix done
- equation for table 3 and 4 done
- table 1 first - study overview and refer to it in 3.1. In 3.2 refer to the figure . repeat title in every page done
- update forestplot (haushofer 2019 2020). in colors by intervention type. done
- include table notes for appendix done
- figure legend of forest plot (why is black and white). instead of SeT put SE done
- I^2 table cite source of thresholds (cochrane) done
- citar con van?: shoud be changed to van aert van assen pending
- add legend to forestplot colors. change to treatment effect - standard error. align country and intervention type to the left. pending
- check line on prisma graphs. studies included in qualitative synthesis -> studies that meet eligibility criteria -> studies excluded due to insuficient infomration in the quantitative analysis.

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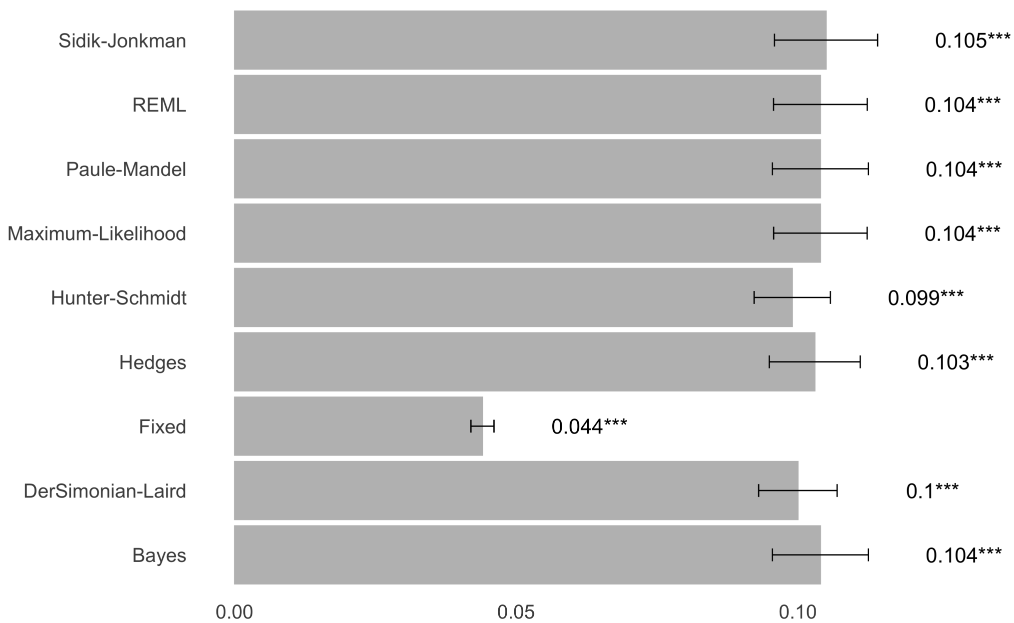
Appendix

A1. Pooled Estimates by Different Random Effects Methods

Figure A1: Pooled Effects on Different Fixed or Random Methods



Notes: Meta-analytic effect sizes using different methods for random effects and fixed effects.

A2. Publication Bias

In the meta analysis literature, it is broadly studied that results may be biased because there is a higher likelihood of publishing studies that are statistically significant, that confirm some prior belief or, conversely, that are surprising (Andrews and Kasy 2019). To adjust the results for publication bias, we consider three methods: Vevea and Hedges 1995, Aert and Assen 2018a, and Andrews and Kasy 2019. These methods were chosen because they allow us to have between-study variance of outcomes and results that are not statistically significant.

A2.1 Vevea & Hedges (1995)

Vevea and Hedges 1995 is a weight-function model that determines the likelihood of getting published. It uses a step function that creates intervals of p-values, where p-values in the same interval get the same weight, and that estimates probability of publication for each interval (Aert and Assen 2018a). First, the model estimates an unadjusted fixed, random, or mixed-effects model, where the observed effect sizes are assumed to be normally distributed as a function of predictors. Then, an adjusted model with weights for pre-specified p-value intervals is estimated (using a step-wise function), generating weights that reflect the likelihood of observing effect sizes in each interval. This model is specified in the weightr package in R, and uses the p-value cut-off of 5%.

Table A1: Publication Bias (Vevea and Hedges Weight Function) Adjusted Estimates

Interval	Adjusted Estimate
Intercept	0.141
	(0.023)***
$0.05 < p < 1$	11.519
	(6.452)*

Notes: Adjusted estimated pooled effect using Vevea and Hedges 1995 method. The first row shows the adjusted pooled effect, controlling for the likelihood of publication in the p-value interval 0.05 <p <1. Second rows shows the estimate for the interval 0.05<p<1.

The adjusted result for all interventions and all mental health outcomes using this selection model is 0.141, statistically significant at 1%.

A2.2 Aert & Assen (2018)

Aert and Assen 2018a developed P-uniform* as an extension of the p-uniform model. The p-uniform approach is based on the fact that if the null hypothesis is true the p-values of hypothesis tests follow a standard uniform distribution (Assen, Aert, and Wicherts 2015). In other words, all studies with statistically significant findings are equally likely to be published and included in the meta-analysis (Assen, Aert, and Wicherts 2015). This method has three major drawbacks: i) it uses only statistically significant effect sizes, ii) effect sizes are positively biased when there is between-study variance in the true effect size, and iii) they do not estimate and test for presence of this between-study variance[7]

The p-uniform* solves these drawbacks. It assumes that the probability of publishing a statistically significant effect size and a non-statistically significant one are constant, but these may be different around a cut-off value (for example around at a 5% significance).[8] It uses a maximum likelihood estimation with truncated densities in between the cut-offs, which allows to implicitly estimate the weights. This model is specified in the puniform package in R.

Table A2 and A3 compare the p-uniform and p-uniform* approaches. Each column is a different method used for the estimation. In table A2, different methods to test the uniformity of the p-values: P (Irwin-Hall), LNP (Fisher), LN1MINP (transforming p values to 1-P before applying Fisher's method), and Kolmogorov-Smirnov. Table A3 computes the p-uniform* for P (Irwin-Hall), LNP (Fisher), and ML (maximum likelihood estimation of the effect size and the between-study variance).

The adjusted result for all interventions and all mental health outcomes using the preferred method (maximum likelihood) is 0.114.

Table A2: Publication Bias (P-Uniform) Adjusted Estimates

Estimation	P	LNP	LN1MINP	KS
All Interventions	0.571	0.543	0.601	0.571
	(0.264)**	(0.295)***	(0.233)**	-

Notes: Pooled effects using Assen, Aert, and Wicherts 2015 publication bias adjustment. Each column corresponds to a different method to test uniformity of p-values: P (Irwin-Hall), LNP (Fisher), LN1MINP (transforming p values to 1-P before applying Fisher's method), and Kolmogorov-Smirnov.

7. Aert and Assen 2018a
8. Aert and Assen 2018a

Table A3: Publication Bias (P-Uniform*) Adjusted Estimates

Estimation	P	LNP	ML
All Interventions	0.120	0.064	0.114
	(0.028)***	(0.035)*	(0.032)***

Notes: Pooled effects using Aert and Assen 2018b publication bias adjustment. Each column corresponds to a different method to test uniformity of p-values: P (Irwin-Hall), LNP (Fisher), and ML (maximum likelihood estimator)

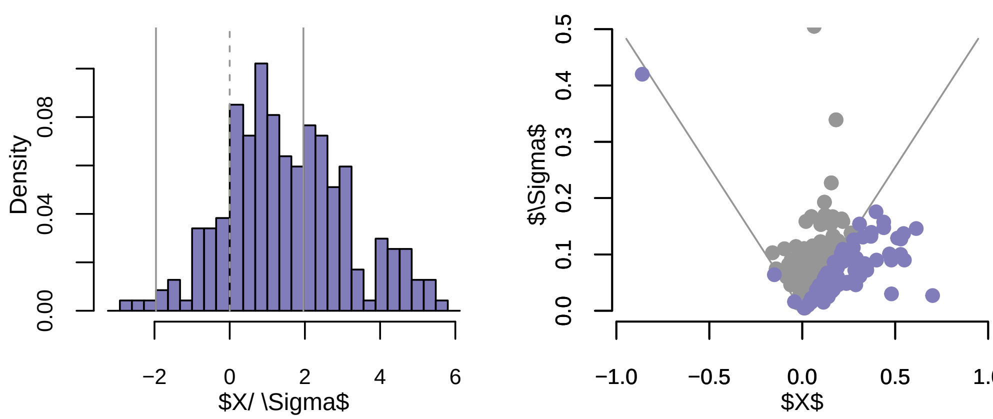
A2.3 Andrews & Kasy

Andrews and Kasy 2019 model is a selection model that corrects the estimates once the probability of publication is known, and this can be identified around specific cut-offs of p-values. It makes the following distributional and functional assumptions: the publication bias is assumed be to a step function and the distribution of effect sizes to be normal, and effect sizes and sample sizes are independent. While this latter assumption is common, it is criticized as unrealistic, given that researchers in practice place great emphasis on power calculations for determining sample size (Lau et al. 2006).

This model is characterized by three parameters: 1) the mean true effect size, μ; 2) the ratio of the probability of publication of non-significant studies to that of statistically significant studies, $\beta \in [0, 1]$ (P(study is published | z < cutoff) / P(study is published | z >= cutoff)); and 3) a heterogeneity parameter, τ , equal to the standard deviation of the sampling distribution of underlying true effect sizes.

The following figures plot are used to identify if there is a cut-off in which p-values appear more likely to be published. The graph on the left plots the density of the estimate X against its standard error ($\sum$). The lines in the figure mark where |X|/$\sum$ =1.96. The graph on the right plot the estimate X against its standard error ($\sum$). The grey lines in the figure mark where |X|/$\sum$ =1.96. The grey dots mark estimates that are not statistically significant at the 5% level. In contrast, the purple dots mark the statistically significant estimates.

Figure A2: A&K Identification Plots



Notes: Left graph plots the density of the estimate X as a ratio of its standard error σ. Right graph plots the studies' estimate against its standard error (grey dots are not significant results). Taken from A&K R package.

The following table presents the results assuming the probability of publication is symmetric and there is a cut-off around 1.96: the true effect (μ) is 0.104 with a large standard deviation (τ) of 0.107. β is the publication probability, where the publication bias probability above the highest step is normalized to one. These results where estimated manually, but it can be estimated using this online application and through the R package uploaded in this site.

The adjusted result for all interventions and all mental health outcomes assuming a symmetric distribution with a cut-off around 1.96 is 0.104.

Table A4: Publication Bias (A-K) Adjusted Estimates

(1)	(2)	(3)
μ	τ	β
0.104	0.107	1.000
(0.010)***	(0.007)***	(0.160)***

Notes: Pooled effects using Andrews and Kasy 2019 publication bias adjustment. Column 1 μ refers to the estimation of the true effect, column 2 τ to the variance of the true effect, and column 3 β to the publication bias probability.

A3. In cash vs In kind

Table A5: Pooled Random Effects for In Cash and In Kind Transfers

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.105	0.132	0.058	0.138
	(0.015)***	(0.023)***	(0.012)***	(0.025)***
	[240 / 59]	[82 / 33]	[92 / 28]	[66 / 27]
In Cash	0.151	0.149	0.085	0.206
	(0.025)***	(0.036)***	(0.022)**	(0.045)***
	[100 / 33]	[43 / 19]	[25 / 12]	[32 / 15]
In Kind	0.073	0.109	0.051	0.081
	(0.011)***	(0.021)***	(0.012)***	(0.016)***
	[140 / 27]	[39 / 15]	[67 / 17]	[34 / 13]

Notes: Meta-analytic effect sizes for specific combinations of interventions (rows) and outcomes (columns). The first row shows the impact of any intervention on various mental health outcomes, the remaining rows correspond to interventions done in-cash or in-kind. Similarly, the first column reports the effect of interventions on any mental health outcome, while the remaining columns focus on specific outcomes. Each cell shows the meta-analytic effect size estimate using random effects with the Sidik-Jonkman estimator. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

Table A6: Determinants of Treatment Effects for In Cash and In Kind Transfers

	(1) All Interventions	(2) In Cash	(3) In Kind
Constant	0.234	0.265	0.099
	(0.088)**	(0.121)**	(0.084)
Age	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)
Female Share	-7.989	-5.708	-3.958
	(5.275)	(6.647)	(11.096)
Lumpsum	-0.017	-0.002	0.030
	(0.040)	(0.049)	(0.045)
Intervention Value	0.001	0.032	-0.021
(as % of GDP per Capita in logs)	(0.017)	(0.035)	(0.013)
Delay Intervention-Survey	-0.017	-0.017	-0.044
(Years)	(0.006)**	(0.006)**	(0.016)**
Obs/Studies	[154 / 39]	[98 / 32]	[56 / 8]

Notes: Meta regression of possible determinants of effect sizes. Each row is a independent variable. Each column is a regression for economic interventions with transfers done in-cash or in-kind, using random effects with the Sidik-Jonkman estimator. Standard errors, clustered at the study level, are shown in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level (note that this convention of denoting significance levels is used in economics but not in psychology and medicine).

A4. Studies' Heterogeneity

Table A7: Heterogeneity of Studies: I^2 Statistic

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	93.1	88.3	83.7	96.6
	[240 / 58]	[82 / 33]	[92 / 28]	[66 / 26]
Unconditional Cash Transfer	88.5	88.9	61.4	91.4
	[84 / 26]	[38 / 15]	[19 / 10]	[27 / 12]
Conditional Cash Transfer	45.1	62.5	29.2	21.3
	[16 / 8]	[5 / 4]	[6 / 3]	[5 / 3]
Housing Voucher	77.6	79.5	69.1	23.7
	[63 / 10]	[24 / 6]	[33 / 9]	[6 / 2]
Poverty Graduation Program	80.3	0.2	82.8	51.9
	[35 / 2]	[2 / 1]	[20 / 1]	[13 / 1]
Lottery Win	98.5	-	94.7	92.1
	[14 / 5]	-	[8 / 3]	[6 / 4]
Asset Transfer	62.3	60.9	0.6	70.7
	[13 / 4]	[6 / 3]	[2 / 2]	[5 / 2]
Health Insurance Provision	80.3	83.3	26.7	15.9
	[11 / 3]	[5 / 3]	[2 / 1]	[4 / 3]

Notes: Heterogeneity statistic I^2 for each pooled effects estimation presented in Table 2. The I^2 estimates the proportion of the variance in study estimates that is due to heterogeneity, as calculated in Higgins (2003). The standard thresholds according to Higgins et al. 2019 are: 0% to 40%: might not be important; 30% to 60%: may represent moderate heterogeneity; 50% to 90%: may represent substantial heterogeneity; 75% to 100%: considerable heterogeneity.