The Effect of Economic Transfers on Psychological Well-Being and Mental Health *

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Abstract

Transfers of cash or other economic interventions have received renewed attention from policymakers, philanthropists, academics, and the general public in recent years. We conducted a systematic review and meta-analysis on the causal impact of economic interventions on psychological well-being and mental health. We reviewed 1,640 abstracts and 127 full-text papers to obtain a final sample of 57 studies containing 253 treatment effects. We distinguish between different economic interventions (conditional and unconditional cash transfers, poverty graduation programs, asset transfers, housing vouchers, health insurance provision, and lottery wins) and different well-being outcomes (depression, stress or anxiety, and happiness or life satisfaction). The average intervention is worth USD 540 PPP, and impacts on well-being are measured two years after the intervention on average. We find that economic interventions have a positive effect on well-being: on average, an intervention increased well-being by 0.100 standard deviations (SD). We observe the largest impacts for asset transfers (0.158 SD) and unconditional cash transfers (0.150 SD). Effects decay over time, and do not differ substantially when transfers are directed to men vs. women. We conclude that economic interventions have significant potential to improve the psychological well-being and mental health of recipients.

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1. Introduction

According to WHO estimates, mental illnesses are amongst the most important sources of disease burden in the world. 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 raises the question whether there is a causal relationship, potentially bi-directional, between poverty and mental health. In the public health literature, these twin hypotheses are referred to as "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 psychological well-being and 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 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, free provision of 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. (In the following, we use "well-being" as a summary term for brevity.) We do not restrict the geographic location of the studies, although many are conducted in low-income countries, or have 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.

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 127 papers and extracted information about the intervention – such as its monetary value – and its effect on measures of well-being. We conduct a meta-analysis on the resulting dataset, which contains 57 papers and 253 treatment effects.

We observe moderately sized and statistically significant positive effects of economic transfers on measures of well-being. The median intervention in our sample of studies makes a transfer worth USD 540, and thereby generates an improvement in well-being of 0.100 standard deviations (SD) two years after the intervention. Asset transfers and unconditional cash transfers have the largest impact on well-being (0.158 SD and 0.150 SD, respectively). There is no clear relationship between effect size and transfer magnitude, possibly due to heterogeneity in samples. We do not find differential effects when transfers are made to men vs. women, and when they are made as lump-sums or in installments. Transfers in low- and middle-income countries (LMICs) have a larger effect on average (0.115 SD) than in high-income countries (0.067 SD). However, this difference 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.

Our paper contributes to a set of reviews and meta-analyses on the relationship between economic and psychological outcomes (Lund et al. 2010; Lund et al. 2011; Ridley et al. 2020). Most closely related is a recent systematic review and meta-analysis about the effect of cash transfers on psychological well-being in low- and middle-income countries (McGuire, Kaiser, and Bach-Mortensen 2020). This meta-analysis reports an average treatment effect of cash transfers on psychological well-being of 0.100 SD. Our study extends this work beyond cash transfers to include other types of economic transfers, and beyond low- and middle-income countries.

2. Methods

2.1 Search strategy

In June 2020, a systematic review protocol was registered with the international prospective register of systematic reviews, PROSPERO, with registration ID number CRD42020189558. We used three approaches to identify both published and unpublished studies (working papers, technical reports) that met our selection criteria. First, we conducted systematic searches of the electronic databases PubMed and RePEc. For each database, we used one set of search terms to identify studies on economic transfers, which included "cash transfer", "cash", "income", "lottery", "graduation", "debt relief", "asset transfer", and "housing voucher"; and another set of search terms to identify studies on mental health and psychological well-being, which included "mental health", "depression", "psychological", "well(-)being", "happiness", and "life satisfaction". To restrict results to randomized controlled trials, we used the option to restrict results to randomized studies in PubMed. RePEc does not provide an option to restrict results to RCTs, and we therefore included an additional set of search terms to identify randomized studies, including "randomized", "RCT", and "trial". Because 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. Our second approach was to screen the reference lists of studies identified in these searches for relevant papers. Finally, we identified a small number of high-profile researchers who have published on this topic and searched their websites for relevant papers.

2.2 Selection criteria

We aimed to include randomized controlled trials (RCTs) and lottery studies which report treatment effects on any aspect of mental health or subjective well-being, including, for example, depression, happiness, or life satisfaction.

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. Abstracts were selected for full-text review if they described (1) an economic transfer intervention, defined as a transfer of money, goods, or services to individuals without a requirement for repayment; (2) at least one quantitatively measured mental health or subjective well-being outcome; and (3) an RCT study design or a lottery design.

Subjective well-being outcomes had to fulfill the following criteria: (1) the outcome is measured with a self-report, i.e. an individual reports their well-being using a Likert or similar scale; in addition, we include measures of stress hormones, in particular cortisol levels. (2) The self-report elicits feelings or thoughts about how the individual's life is going or how they are feeling (emotions, evaluations, moods). (3) The self-reports elicit broad assessments, rather than assessments restricted to a single domain of life, such as financial well-being. However, we do accept indices that combine items that cover many domains. Any disagreements regarding the eligibility of particular studies were resolved through discussion with a third 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 frequency of measurement; and magnitude of the treatment effect. 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 an additional reviewer (JH).

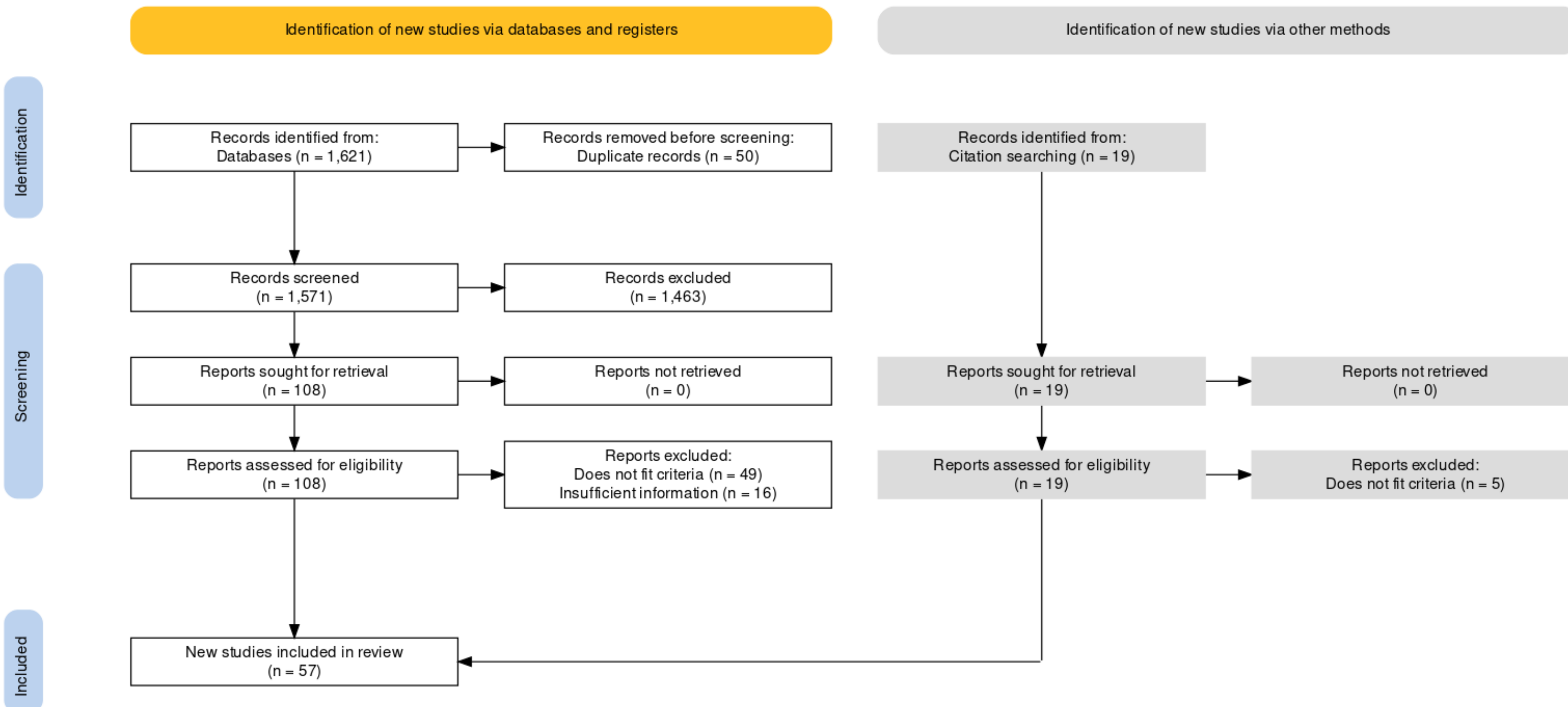
We classified the interventions into seven categories: unconditional cash transfers (UCTs); conditional cash transfers (CCTs); neighborhood programs (housing vouchers); poverty graduation programs; lotteries; asset transfers; and insurance provision. Poverty graduation programs are compound interventiosn that typically consist of an asset transfer, training, and a cash transfer. Similarly, we classified the outcomes into three outcome groups: depression; stress or anxiety; and happiness or life satisfaction. Depression outcomes included the Center for Epidemiological Studies Depression Scale (CES-D), the Geriatric Depression Scale, the John Hopkins Depression Checklist, and the Symptom-Driven Diagnostic System for Primary Care. The CES-D was the most common. Measures of stress and anxiety included the Kessler Psychological Distress Scale (K6), cortisol levels, the General Health Questionnaire (GHQ), and Cohen's Perceived Stress Scale, with the K6 being the most common. Happiness and life satisfaction included measures of self-reported happiness, life satisfaction, subjective well-being, and unhappiness, such as the Gallup Q-12 index, the most common being self-reported happiness.

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 1,640 abstracts returned from the search strategy, 127 were chosen for full-text review in the screening process. Fifty-four of these papers were excluded after full-text review because they did not meet the inclusion criteria, and 16 because they did not contain sufficient information to compute standardize treatment effects. From the remaining 57 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[1]. This yielded 57 papers with 253 treatment effects to be analyzed.

1. 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



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:

(1) We check if the treatment effect is already standardized. In this case, we use this standardized treatment effect (thereby adopting the standardization method of the paper, even if it differs from our preferred approach). (2) If the treatment effect is not standardized, we check if the paper reports the standard deviation of the control group at the endline, in which case we use it to perform the standardization. (3) If this value is not available, we check if the outcome is continuous or binary. (4) If the outcome is binary, we check whether the share p of individuals responding affirmatively in the control group is reported. In this case, we compute the control group standard deviation using the formulate for the standard deviation of a proportion, $SD = \sqrt{p(1-p)}$. If the share of affirmative responses in the control group at endline is not reported, we use baseline information if available. (5) If the outcome is continuous and the study reports sample sizes and the standard error of the treatment effect, we approximate the standard deviation of the control group using the t-test formula, assuming equal variances for the control and treatment groups. Specifically, the t-test formula for a difference in means is $t = \frac{\bar{x}_1 - \bar{x}_2}{SE} = \frac{\bar{x}_1 - \bar{x}_2}{\sqrt{\frac{SD_1^2}{n_1} + \frac{SD_2^2}{n_2}}}$. Assuming equal variances $SD^2 = SD_1^2 = SD_2^2$ in both groups, we can calculate the SD as a function of the standard error and sample sizes: $SD = \sqrt{\frac{SE^2 n_1 n_2}{n_1 + n_2}}$, where n_1 and n_2 are the sample sizes of the treatment and control groups, respectively. The standard error of the treatment effect was standardized in the same fashion as the treatment effects.

Prior to the standardization, all "negative" outcomes, such as stress and depression, were re-coded 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.[2]

2.3.2 Meta-analysis Methods

We analyzed the 253 effect sizes that resulted from our systematic review using a random-effects (RE) meta-analysis model[3], using the Sidik-Jonkman estimator.[4] 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

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

3. 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.

4. Results are similar when we use alternative random effects estimators; 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 relationship between effect sizes and various characteristics of the interventions and beneficiaries, we additionally estimate a meta-regression in which we include the following variables on the right-hand side: average age of the participants (or, where average age is not available, midpoint of the age range); intervention value as percentage of per-capita GDP in the study country, measured in 1,000s of USD; average delay between intervention and outcome measurement in years; female share of the participants (ranging from 0 for all-male samples to 1 for all-female samples); and whether the intervention was delivered as a lump-sum transfer or in repeated installments. Specifically, we estimate:

$$\theta_i = \beta_0 + \beta_1 Age_i + \beta_2 LMIC_i + \beta_3 Female_i + \beta_4 Lumpsum_i + \beta_5 Value_i + \beta_6 Years_i + \epsilon_i$$

We again run this analysis both for all interventions and outcome measures, and separately for each type of intervention and outcome measure.

Finally, we examine evidence of publication bias with three adjustment methods: Vevea and Hedges 1995; Aert and Assen 2018; and Andrews and Kasy 2019. See Appendix A2 for further information on these methods.

3. Results

3.1 Study Overview

Our final sample of 57 studies consists of 27 studies of unconditional cash transfers,[5] 6 of conditional cash transfers,[6] 11 of housing vouchers,[7] 6 of lotteries,[8] 3 of graduation programs and other multifaceted interventions such as enterprise development programs,[9] 3 of asset transfers,[10] and 3 of insurance provision.[11][12]

An overview of studies and their characteristics is shown in Table 1, which shows for each study: (1) the intervention type (unconditional cash transfer, conditional cash transfer, insurance provision, housing voucher, asset transfer, lottery, or poverty graduation program); (2) the average transfer value; (3) the classification of the outcomes measured; (4) the delay between the time of the start of the intervention

5. 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; Ferrah and Mvukiyehe, n.d.; Green et al. 2016; Haushofer and Shapiro 2016, 2018; 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; Roy et al. 2019; Stein et al. 2020

6. Baird, Hoop, and Özler 2013; Blattman, Fiala, and Martinez 2019; Kilburn et al. 2019; Macours, Schady, and Vakis 2012; Morris et al. 2017; Ohrnberger et al. 2020

7. Andersen, Kotsadam, and Somville 2021; 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

8. Burger et al. 2020; Gardner and A. Oswald 2001; Gardner and A. J. Oswald 2007; Kim and Oswald 2021; Kuhn et al. 2011; Lindqvist, Östling, and Cesarini 2020

9. Abhijit Banerjee et al. 2015; Bossuroy et al. 2021; Ismayilova et al. 2018

10. Edmonds and Theoharides 2020; Glass et al. 2017; Quattrochi et al. 2020

11. Baicker et al. 2013; Finkelstein et al. 2012; Haushofer et al. 2020

12. Some studies are counted doubly because they contain more than one type of economic intervention.

and outcome measurement; (5) the target population; (6) the country in which the intervention took place; and (7) the sample size.

Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Country	Sample size
Edmonds & Theoharides, 2020	Asset Transfer	518	Happiness or Satisfaction, Depression	20 months	Children and adolescentes aged 5 to 17 of beneficiary households	Philippines	3620
Glass et al., 2017	Asset Transfer	70	Depression, Anxiety	18 months	Adults in post-conflict settings (over 16)	Congo	833
Quattrochi, 2020	Asset Transfer	71	Happiness or Satisfaction, Depression	1 year, 6 weeks	Adults of vulnerable households	Congo	769
Blattman et al, 2019	CCT	300	Anxiety, Depression	a year after, 5 years after	Underemployed youth	Ethiopia	1020
Kilburn et al., 2019	CCT	360	Depression	24 months	Young women in vulnerable households (from 16 to 23)	South Africa	2533
Macours, 2012	CCT	452	Depression	2 years, 9 months	Mothers of vulnerable households	Nicaragua	1151
Morris et al., 2017	CCT	6200	Depression, Anxiety	2 years	Adolescents in 9th grade from vulnerable households	United States	511
Ohrnberger et al., 2020	CCT	25	Happiness or Satisfaction	1 year	Adults (over 16) in rural households	Malawi	790
Andersen, 2021	Housing Voucher	19091	Happiness or Satisfaction, Stress	2 years	Lottery winners	Ethiopia	3049
Fauth et al., 2004	Housing Voucher	15400	Anxiety, Depression	almost 2 years	Adults beneficiaries of the Yonkers Project for relocation	United States	315
Katz et al., 2001	Housing Voucher	20307	Happiness or Satisfaction, Anxiety, Depression	On average 2.2 years, with a range from 1 to 3.5 years	Adults beneficiaries from Moving to Oportunity	United States	412
Kessler et al., 2014	Housing Voucher	106827	Depression	10-15 years	Adolescents beneficiaries from Moving to Oportunity	United States	182
Kling et al., 2007	Housing Voucher	43827	Stress, Depression	5 years after on average	Adults and adolescents beneficiaries from Moving to Oportunity	United States	2533

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Country	Sample size
Leventhal & Brooks-Gunn, 2003	Housing Voucher	27027	Stress, Depression	3 years	Adults and children beneficiaries from Moving to Oportunity	United States	369
Leventhal & Dupere, 2011	Housing Voucher	52227	Stress, Anxiety	5-7 years	Adolescents beneficiaries from Moving to Oportunity	United States	1780
Ludwig et al., 2013	Housing Voucher	106827	Depression, Stress, Happiness or Satisfaction	10-15 years	Adults and adolescents beneficiaries from Moving to Oportunity	United States	2595
Nguyen et al., 2013	Housing Voucher	48027	Stress	4-7 years	Adolescents beneficiaries from Moving to Oportunity	United States	1426
Osypuk et al., 2012	Housing Voucher	48027	Stress	4-7 years	Adolescents beneficiaries from Moving to Oportunity	United States	2829
Sanbonmatsu et al., 2011	Housing Voucher	106827	Stress	10-15 years	Adults and adolescents beneficiaries from Moving to Oportunity	United States	4644
Baicker et al., 2013	Insurance Provision	7000	Happiness or Satisfaction, Depression	2 years	Adults beneficiaries of the Oregon Health Insurance experiment	United States	12229
Finkelstein et al., 2012	Insurance Provision	4083	Depression, Happiness or Satisfaction	14 months	Adults beneficiaries of the Oregon Health Insurance experiment	United States	23741
Haushofer et al., 2020	Insurance Provision, UCT	338	Depression, Stress, Happiness or Satisfaction	1 year	Metalworkers of the Kamukunji Jua Kali Association	Kenya	693
Burger et al., 2020	Lottery	10	Happiness or Satisfaction	1 week	Lottery winners	Netherlands	1097
Gardner & Oswald, 2001	Lottery	200	Stress, Happiness or Satisfaction	12 months	Lottery winners	United Kingdom	9493
Gardner & Oswald, 2007	Lottery	4303	Stress	2 years	Lottery winners	United Kingdom	12620
Kim, 2020	Lottery	254	Happiness or Satisfaction	1 year	Lottery winners	Singapore	5626

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Country	Sample size
Kuhn et al., 2011	Lottery	30000	Happiness or Satisfaction	6 montths	Lottery winners	Netherlands	1458
Lindqvist et al., 2020	Lottery	106000	Happiness or Satisfaction, Stress	5 to 22 years	Lottery winners	Sweden	3331
Banerjee et al., 2015	Poverty Graduation Program	6475	Happiness or Satisfaction, Anxiety, Stress	24 months, 36 months	Adults from ultra-poor households	Ethiopia, Ghana, Honduras, India, Pakistan, Peru	14595
Bossuroy, 2021	Poverty Graduation Program	127	Depression, Happiness or Satisfaction, Anxiety, Stress	6 months, 18 months	Women over 20 years old in extreme poverty	Niger	2409
Ismayilova et al., 2018	Poverty Graduation Program	100	Depression	24 months, 12 months	Children (10-15 years) of ultra-poor households	Burkina Faso	240
Alzua et al., 2021	UCT	384	Depression, Happiness or Satisfaction	6 months, 1 year	Older adults over 65 years old from vulnerable households	Nigeria	6059
Angeles et al., 2019	UCT	192	Depression	2 years	Young people from ultra-poor labor constrainded households	Malawi	1366
Bando, 2021	UCT	1104	Depression	1 year	Older adults (over 65) living under the poverty line	Paraguay	1939
Banerjee et al., 2020	UCT	675	Depression	30 months, 24 months	Adults from poor households	Kenya	4909
Blattman et al., 2011	UCT	374	Depression	24-30 months	Poor and unemployed adults aged 16-45	Uganda	1881
Blattman et al., 2014	UCT	374	Happiness or Satisfaction	24-30 months, 4 years	Poor and unemployed adults aged 16-35	Uganda	1996
Blattman et al., 2017	UCT	200	Depression	2-5 weeks, 12-13 months	High-risk men aged 18-35	Liberia	470
Blattman et al., 2019	UCT	374	Depression, Stress	9 years	Poor and unemployed adults aged 16-40	Uganda	1868
Egger et al., 2019	UCT	1000	Depression	11 months	Adults from poor households	Kenya	4121

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Table 1: Study Overview

Study	Intervention type	Average transfer value (USD)	Outcomes	Delay Intervention–Survey	Target population	Country	Sample size
Fernald et. al, 2011	UCT	360	Depression	2 years	Mothers of vulnerable households	Ecuador	1196
Ferrah, 2021	UCT	227	Happiness or Satisfaction, Depression	2 to 2.5 years	Women from vulnerable households	Tunisia	1356
Green et al., 2016	UCT	150	Depression	16 months	Young people in post-conflict settings (from 14 to 30 years old)	Uganda	1726
Haushofer & Shapiro, 2016	UCT	354	Stress, Happiness or Satisfaction, Anxiety, Depression	9 months	Heads of poor households	Kenya	1474
Haushofer & Shapiro, 2018	UCT	354	Stress, Happiness or Satisfaction, Depression	33 months	Heads of poor households	Kenya	1491
Haushofer et al., 2019	UCT	485	Happiness or Satisfaction, Depression, Stress	1 year	Adults in rural areas	Kenya	2140
Heath et al., 2020	UCT	324	Stress	18 months	Adults of poor households with a child aged 6-23 months	Mali	1143
Hjelm et. al, 2017	UCT	396	Stress	36 months	Mothers of poor households with a child under 5	Zambia	2490
Kilburn et al., 2018	UCT	85	Happiness or Satisfaction	17 months	Caregivers in ultra-poor, labor-constrainted households	Malawi	2919
McIntosh & Zeitlin, 2020	UCT	750	Depression	15 months	Young people aged 16-30 from poor households with less than secondary education	Rwanda	666
Molotskya, 2020	UCT	120	Stress, Happiness or Satisfaction	Combined Follow up waves	Caregivers in vulnerable households	Malawi	7551
Muller et al., 2019	UCT	1000	Happiness or Satisfaction	2.5 years	Young people aged 18 from 34 from vulnerable households	South Sudan	1495

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Table 1: Study Overview

Study	**Intervention type**	**Average transfer value (USD)**	**Outcomes**	**Delay Intervention–Survey**	**Target population**	**Country**	**Sample size**
Natali e al., 2018	UCT	816	Happiness or Satisfaction	36 months, 48 months	Mothers in poor households	Zambia	2203
Paxson & Schady, 2010	UCT	255	Depression, Stress	17 months	Mothers of vulnerable households	Ecuador	1046
Roy et al., 2019	UCT	456	Happiness or Satisfaction	4 years after	Adults in poor households	Bangladesh	1989
Stein et al., 2020	UCT	1000	Happiness or Satisfaction	7 months aprox	Refugee households	Uganda	1264
Baird et al., 2013	UCT, CCT	430	Stress	12 months, 24 months	Adolescent girls from vulnerable households aged 13 to 24 yeras old	Malawi	1820

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

We extracted 253 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). The average transfer value is USD 540, and the average delay between intervention and outcome measurement is two years.

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[13]. 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 0.100 SD of any transfer on well-being 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 asset transfers, which increase measures of well-being by 0.158 SD, significant at the 10 percent level,[14], closely followed by unconditional cash transfers, which increase measures of well-being by 0.150 SD, significant at the 1 percent level. Health insurance provision, studied in the Oregon Health Insurance Experiment and an RCT in Kenya, improved mental health by 0.093 SD, although this effect is not statistically significant at conventional levels. However, the effects of insurance provision are significant for depression and stress/anxiety outcomes. This pattern of results is driven by a reduction in stress and the stress hormone cortisol in the Kenya study, and by a reduction in depression in the US study. Poverty graduation programs have a positive but not statistically significant effect of 0.079 SD. Lotteries had an effect of 0.073 SD, significant at the 10 percent level. Housing vouchers, studied in an Ethiopian housing lottery, and in two US programs ("Moving to Opportunity", and a similar program in New York) have an effect of 0.070 SD, significant at the 1 percent level. Finally, conditional cash transfer (CCT) programs had an effect of 0.043 SD, significant at the 5 percent level.

Turning to different outcome variables, we observe the largest effect of transfers on happiness (0.131 SD) and depression (0.126 SD), both significant at the 1 percent level. Stress and anxiety show a 0.055 SD improvement on average, significant at the 1 percent level. The largest overall effects are generated by UCTs on happiness (0.180 SD), significant at the 1 percent level. Figure 3 presents a comparison of these effects by intervention type and well-being outcome.

13. The pooled result is estimated using the 'SJ' method for meta-analysis, weighted by the inverse of the standard error of the outcome.

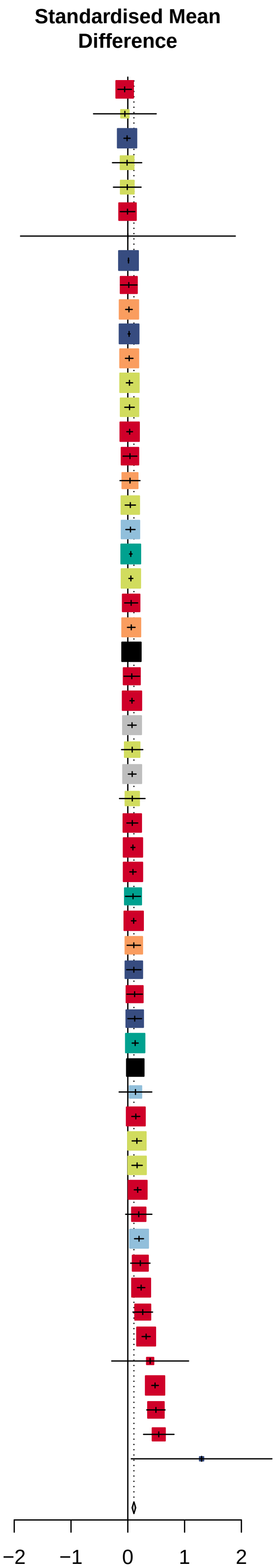
14. Note that the concept of significance at the 10 percent level is used in economics, but not psychology and medical science.

Figure 2: Forest plot of effects of economic transfers on well-being

Study	Treatment Effect	Standard Error	Intervention Type	Country
Fernald et. al, 2011	−0.06	0.06	UCT	Ecuador
Kessler et al., 2014	−0.05	0.28	Housing Voucher	United States
Kuhn et al., 2011	−0.01	0.03	Lottery	Netherlands
Osypuk et al., 2012	−0.01	0.13	Housing Voucher	United States
Nguyen et al., 2013	−0.01	0.12	Housing Voucher	United States
Paxson & Schady, 2010	−0.01	0.07	UCT	Ecuador
Muller et al., 2019	0.00	0.96	UCT	South Sudan
Kim, 2020	0.01	0.00	Lottery	Singapore
Blattman et al., 2017	0.02	0.07	UCT	Liberia
Kilburn et al., 2019	0.02	0.03	CCT	South Africa
Lindqvist et al., 2020	0.02	0.01	Lottery	Sweden
Macours, 2012	0.02	0.03	CCT	Nicaragua
Sanbonmatsu et al., 2011	0.03	0.03	Housing Voucher	United States
Leventhal & Dupere, 2011	0.03	0.04	Housing Voucher	United States
Blattman et al., 2019	0.03	0.03	UCT	Uganda
Hjelm et. al, 2017	0.04	0.06	UCT	Zambia
Morris et al., 2017	0.04	0.09	CCT	United States
Kling et al., 2007	0.04	0.05	Housing Voucher	United States
Edmonds & Theoharides, 2020	0.05	0.04	Asset Transfer	Philippines
Banerjee et al., 2015	0.05	0.01	Poverty Graduation Program	Multiple
Ludwig et al., 2013	0.05	0.02	Housing Voucher	United States
Green et al., 2016	0.06	0.06	UCT	Uganda
Blattman et al, 2019	0.06	0.03	CCT	Ethiopia
Finkelstein et al., 2012	0.06	0.02	Insurance Provision	United States
Roy et al., 2019	0.07	0.07	UCT	Bangladesh
Alzua et al., 2021	0.07	0.02	UCT	Nigeria
Baird et al., 2013	0.07	0.04	UCT, CCT	Malawi
Fauth et al., 2004	0.08	0.10	Housing Voucher	United States
Haushofer et al., 2020	0.08	0.04	Insurance Provision, UCT	Kenya
Andersen, 2021	0.08	0.12	Housing Voucher	Ethiopia
Blattman et al., 2011	0.08	0.05	UCT	Uganda
Natali e al., 2018	0.09	0.02	UCT	Zambia
Haushofer & Shapiro, 2018	0.09	0.03	UCT	Kenya
Ismayilova et al., 2018	0.09	0.07	Poverty Graduation Program	Burkina Faso
Banerjee et al., 2020	0.10	0.02	UCT	Kenya
Ohrnberger et al., 2020	0.11	0.06	CCT	Malawi
Gardner & Oswald, 2007	0.11	0.06	Lottery	United Kingdom
Egger et al., 2019	0.12	0.07	UCT	Kenya
Gardner & Oswald, 2001	0.12	0.06	Lottery	United Kingdom
Bossuroy, 2021	0.13	0.03	Poverty Graduation Program	Niger
Baicker et al., 2013	0.13	0.06	Insurance Provision	United States
Glass et al., 2017	0.13	0.15	Asset Transfer	Congo
Haushofer & Shapiro, 2016	0.14	0.04	UCT	Kenya
Leventhal & Brooks−Gunn, 2003	0.16	0.04	Housing Voucher	United States
Katz et al., 2001	0.16	0.05	Housing Voucher	United States
Haushofer et al., 2019	0.17	0.03	UCT	Kenya
Heath et al., 2020	0.19	0.12	UCT	Mali
Quattrochi, 2020	0.20	0.04	Asset Transfer	Congo
Stein et al., 2020	0.22	0.09	UCT	Uganda
Blattman et al., 2014	0.23	0.03	UCT	Uganda
McIntosh & Zeitlin, 2020	0.26	0.09	UCT	Rwanda
Angeles et al., 2019	0.32	0.04	UCT	Malawi
Ferrah, 2021	0.39	0.35	UCT	Tunisia
Bando, 2021	0.48	0.03	UCT	Paraguay
Molotskya, 2020	0.50	0.08	UCT	Malawi
Kilburn et al., 2018	0.55	0.14	UCT	Malawi
Burger et al., 2020	1.30	0.63	Lottery	Netherlands

Random effects model

Heterogeneity: $I^2 = 90\%$, $\tau^2 = 0.0148$, $p < 0.01$



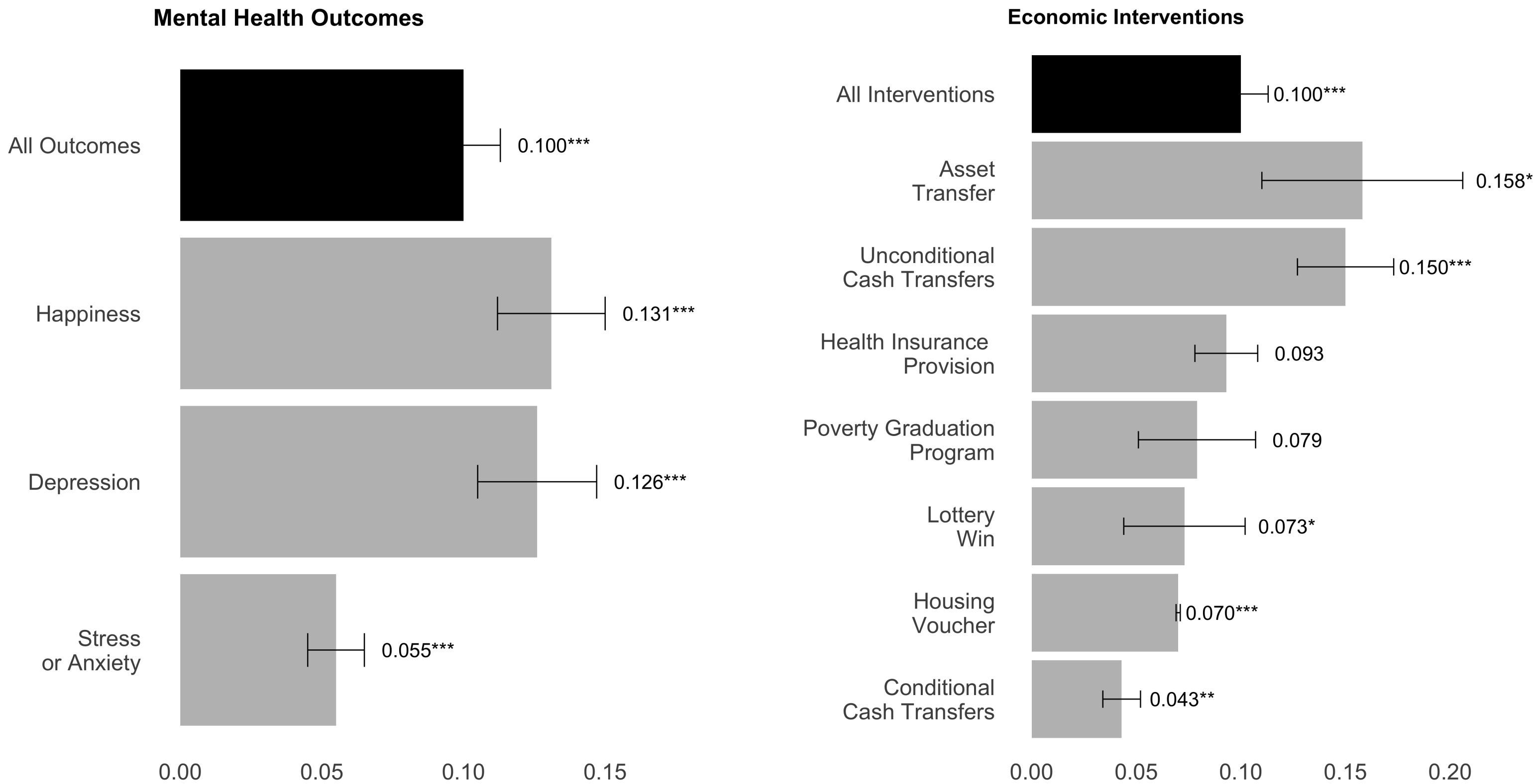
Notes: This forest plot shows point estimates and 95% confidence intervals for the main well-being outcome in each of the 57 studies in our sample. The size of each marker corresponds to the study's sample size, and the color corresponds to the intervention type.

Table 2: Meta-analytic estimates of effects of economic transfers on well-being

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.100	0.126	0.055	0.131
	(0.013)***	(0.021)***	(0.010)***	(0.019)***
	[253 / 57]	[89 / 34]	[98 / 28]	[66 / 28]
Unconditional Cash Transfer	0.150	0.163	0.081	0.180
	(0.023)***	(0.039)***	(0.024)**	(0.033)***
	[82 / 27]	[39 / 17]	[18 / 10]	[25 / 13]
Conditional Cash Transfer	0.043	0.028	0.053	0.106
	(0.009)**	(0.012)*	(0.013)*	(0.060)*
	[16 / 6]	[8 / 4]	[7 / 3]	[1 / 1]
Housing Voucher	0.070	0.104	0.041	0.089
	(0.001)***	(0.001)**	(0.005)**	(0.039)
	[66 / 11]	[25 / 6]	[34 / 10]	[7 / 3]
Poverty Graduation Program	0.079	0.115	0.047	0.124
	(0.028)	(0.005)**	(0.027)	(0.040)
	[51 / 3]	[6 / 2]	[28 / 2]	[17 / 2]
Lottery Win	0.073	—	0.082	0.068
	(0.029)*		(0.022)*	(0.050)
	[15 / 6]	[0 / 0]	[8 / 3]	[7 / 5]
Asset Transfer	0.158	0.137	0.156	0.181
	(0.048)*	(0.057)	(0.227)	(0.044)
	[12 / 3]	[6 / 3]	[1 / 1]	[5 / 2]
Health Insurance Provision	0.093	0.075	0.231	0.092
	(0.015)	(0.001)**	(0.067)***	(0.020)
	[11 / 3]	[5 / 3]	[2 / 1]	[4 / 3]

Notes: Meta-analytic estimates for the effect of different types of economic interventions (rows) on different well-being outcomes (columns), analyzed using a random effects model. The first row shows the impact of any intervention on the various well-being outcomes; the remaining rows show the impact of specific interventions. Similarly, the first column reports the effect of interventions on any mental health outcome, while the remaining columns focus on specific outcomes. Standard errors, clustered at the study level, are shown in parentheses. The number of treatment effects and studies on which the estimate in each cell is based is shown in brackets [number of treatment effects / number of studies]. *** 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: Meta-analytic estimates of effects of economic transfers on well-being



Notes: Meta-analytic estimates for the effect of different economic interventions on well-being outcomes. The left panel shows the impacts of any economic intervention on different well-being outcomes; the right panel shows the impacts of different economic interventions on any well-being outcome. Standard errors represent 95% confidence intervals.

3.3 Which variables correlate with effect sizes?

To understand how the observed effect sizes are related to specific characteristics of the intervention or the participants, we examined five variables: (1) average age of the beneficiaries; (2) a binary variable indicating if the intervention took place in a low-/middle-income country; (3) the share of female beneficiaries; (4) a binary variable indicating whether the treatment was given as a lump-sum or in repeated installments; (5) the value of the intervention as a percentage of GDP per capita in the study country; and (6) the delay in years between the beginning of the transfer and the outcome measurement. Results by intervention type are show in Table 3, and by outcome in Table 4. We find that effect sizes become smaller as the delay between intervention and outcome measurement increases; each year decreases the effect size by 0.006 SD. Perhaps surprisingly, we find no consistent effect of intervention value, possibly due to the considerable heterogeneity in intervention types and countries. Interventions in low- and middle-income countries have qualitatively larger effects, but this difference is not statistically significant. We observe little variation of effect sizes with age, share of women in the sample, and as a function of transfer modality (lump-sum vs. installments).

To mitigate the loss of statistical power that results from distinguishing between seven intervention types, we also conduct an exploratory analysis in which we divide the sample into studies of "in cash" vs. "in kind" transfers. Results are shown in Table A6. We confirm the finding that effect size decreases over time. In addition, in-kind transfers have larger effects when they are made to women than to men.

Table 3: Correlates of effect size, 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	0.099	0.134	0.199	0.011	0.163	-7.046	0.209	6.090
	(0.065)	(0.165)	(0.129)	(0.065)	(0.111)	(1.801)***	(0.277)	(20.999)
Age	0.000	0.001	-0.002	-0.001	-0.001	0.124	-0.010	-0.174
	(0.001)	(0.003)	(0.004)	(0.002)	(0.003)	(0.032)***	(0.022)	(0.603)
Low-/Middle-Income Country	0.032	—	0.062	-2.453	—	0.845	—	—
	(0.029)		(0.118)	(3.687)		(0.643)		
Female Share	0.034	-0.024	-0.153	0.185	0.026	-0.053	0.461	1.342
	(0.065)	(0.059)	(0.209)	(0.055)***	(0.112)	(0.062)	(1.050)	(5.093)
Lump Sum	-0.024	-0.016	0.045	—	—	—	—	—
	(0.036)	(0.058)	(0.093)					
Intervention Value (as % of GDP per capita)	-0.002	0.437	-0.015	0.259	-0.041	1.576	—	1.580
	(0.003)	(0.455)	(0.183)	(0.380)	(0.029)	(0.422)***		(1.738)
Delay Intervention-Survey (Years)	-0.006	-0.017	-0.022	-0.085	-0.028	-0.358	-0.147	—
	(0.002)**	(0.006)**	(0.017)	(0.118)	(0.021)	(0.097)***	(0.080)*	
Observations/Studies	[253 / 57]	[82 / 27]	[16 / 6]	[66 / 11]	[51 / 3]	[15 / 6]	[12 / 3]	[11 / 3]

Notes:
Correlates of effect size by intervention type. Each column is a meta-regression that estimates how the treatment effects of a specific set of interventions are related to a set of covariates. Age is the average age of the study sample; Low-/Middle Income Country is a binary variable if the intervention takes place in a low- or middle-income country; Female share is the percentage of women in the study sample; Lump Sum is a binary variable that indicates whether the intervention was a lump-sum transfer; Intervention Value is the value of the transfer as a percentage of the GDP per capita on the baseline year, in 1000s of USD; Delay Intervention-Survey is the delay, in years, between the start of the intervention and the outcome measurement. 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.

Table 4: Correlates of effect size, by well-being outcome

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
Constant	0.099	0.145	0.020	0.137
	(0.065)	(0.064)**	(0.095)	(0.104)
Age	0.000	-0.001	0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)
Low-/Middle-Income Country	0.032	0.098	-0.035	0.081
	(0.029)	(0.070)	(0.025)	(0.052)
Female Share	0.034	0.022	0.085	0.007
	(0.065)	(0.070)	(0.094)	(0.092)
Lump sum	-0.024	-0.064	-0.008	-0.024
	(0.036)	(0.070)	(0.045)	(0.050)
Intervention Value (as % of GDP per capita)	-0.002	0.045	-0.008	0.005
	(0.003)	(0.033)	(0.004)*	(0.005)
Delay Intervention-Survey (Years)	-0.006	-0.017	-0.003	-0.007
	(0.002)**	(0.010)*	(0.004)	(0.004)
Observations/Studies	[253 / 57]	[89 / 34]	[98 / 28]	[66 / 28]

Notes: Correlates of effect size by outcome type. Each column is a meta-regression that estimates how the treatment effects on a specific group outcome variables are related to a set of covariates. Age is the average age of the study sample; Low-/Middle Income Country is a binary variable if the intervention takes place in a low- or middle-income country; Female share is the percentage of women in the study sample; Lump Sum is a binary variable that indicates whether the intervention was a lump-sum transfer; Intervention Value is the value of the transfer as a percentage of the GDP per capita on the baseline year, in 1000s of USD; Delay Intervention-Survey is the delay, in years, between the start of the intervention and the outcome measurement. 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.

3.3.1 Effects in Low-Income, Middle-Income, and High-Income Countries

To be able to examine separately the impact of economic transfers in low- and middle-income countries (LMIC) and high-income countries (HIC), we repeat the analysis described in Section 3.2 separately for these two sets of countries. Results are shown in Tables 5 and 6. We find qualitatively larger overall effects in LMIC (0.115 SD) than in HIC (0.067 SD). However, note that this qualitative difference may reflect the different composition of intervention types: for example, unconditional cash transfers and asset transfers, which both produce large treatment effects, have been tested in LMIC but not in HIC.

Table 5: Meta-analytic effects: Studies in Low- and Middle-Income Countries

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.115	0.136	0.058	0.155
	(0.018)***	(0.028)***	(0.017)**	(0.024)***
	[167 / 39]	[59 / 25]	[56 / 15]	[52 / 20]
Unconditional Cash Transfer	0.150	0.163	0.081	0.180
	(0.023)***	(0.039)***	(0.024)**	(0.033)***
	[82 / 27]	[39 / 17]	[18 / 10]	[25 / 13]
Conditional Cash Transfer	0.043	0.032	0.045	0.106
	(0.010)**	(0.012)	(0.005)*	(0.060)*
	[14 / 5]	[7 / 3]	[6 / 2]	[1 / 1]
Housing Voucher	0.078	—	-0.039	0.196
	(0.115)		(0.036)	(0.036)***
	[2 / 1]	[0 / 0]	[1 / 1]	[1 / 1]
Poverty Graduation Program	0.079	0.115	0.047	0.124
	(0.028)	(0.005)**	(0.027)	(0.040)
	[51 / 3]	[6 / 2]	[28 / 2]	[17 / 2]
Lottery Win	1.299	—	—	1.299
	(0.632)**			(0.632)**
	[1 / 1]	[0 / 0]	[0 / 0]	[1 / 1]
Asset Transfer	0.158	0.137	0.156	0.181
	(0.048)*	(0.057)	(0.227)	(0.044)
	[12 / 3]	[6 / 3]	[1 / 1]	[5 / 2]
Health Insurance Provision	0.121	0.080	0.231	0.028
	(0.055)**	(0.100)	(0.067)***	(0.067)
	[5 / 1]	[1 / 1]	[2 / 1]	[2 / 1]

Notes: Meta-analytic estimates for the effect of transfers on well-being in low- and middle-income countries. 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: Meta-analytic effects: Studies in High-Income Countries

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.067 (0.005)*** [86 / 18]	0.099 (0.008)** [30 / 9]	0.051 (0.009)** [42 / 13]	0.043 (0.015)** [14 / 8]
Unconditional Cash Transfer	— [0 / 0]	— [0 / 0]	— [0 / 0]	— [0 / 0]
Conditional Cash Transfer	0.038 (0.090) [2 / 1]	-0.052 (0.088) [1 / 1]	0.129 (0.088) [1 / 1]	— [0 / 0]
Housing Voucher	0.069 (0.001)** [64 / 10]	0.104 (0.001)** [25 / 6]	0.044 (0.000)** [33 / 9]	0.057 (0.018)** [6 / 2]
Poverty Graduation Program	— [0 / 0]	— [0 / 0]	— [0 / 0]	— [0 / 0]
Lottery Win	0.046 (0.020)* [14 / 5]	— [0 / 0]	0.082 (0.022)* [8 / 3]	0.020 (0.009) [6 / 4]
Asset Transfer	— [0 / 0]	— [0 / 0]	— [0 / 0]	— [0 / 0]
Health Insurance Provision	0.080 (0.028)** [6 / 2]	0.081 (0.040)** [4 / 2]	— [0 / 0]	0.100 (0.036)** [2 / 2]

Notes: Meta-analytic estimates for the effect of transfers on well-being in high-income countries. 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.

3.4 Publication Bias

Recent years have highlighted the possibility of publication bias, i.e. selective publication of "positive", surprising, and statistically significant results Andrews and Kasy 2019. To assess to what extent our results may be affected by such selective publication practices, we adjust our meta-analytic effect size estimates using three methods: Vevea and Hedges (1995); the P-uniform* method proposed by Aert and Assen (2018); and Andrews and Kasy (2019) (see Appendix A2 for more information on these methods).

Tables 7 and 8 show the meta-analytic effect size estimates produced by each of these approaches, by intervention type and well-being outcome, respectively. The first column shows the "naïve" estimates for comparison. We find little evidence of publication bias; the corrected meta-analytic effect size estimates are numerically close to the overall results. In addition, only a small share of the corrected point estimates are numerically smaller than the naïve estimates.

The lack of evidence for 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, the well-being outcomes we analyze here are often not the primary outcomes of interest in the original studies, and thus may be less affected by publication bias.

Table 7: Meta-analytic effects adjusted for publication bias, by intervention type

	(1) Naïve Estimation	(2) P-uniform*	(3) Andrews & Kasy	(4) Veva & Hedges
All Interventions	0.100	0.112	0.100	0.138
	(0.013)***	(0.031)***	(0.010)***	(0.023)***
Unconditional Cash Transfer	0.150	0.192	0.161	0.227
	(0.025)***	(0.064)**	(0.022)***	—
Conditional Cash Transfer	0.043	0.068	0.048	0.092
	(0.009)**	(0.133)	(0.019)**	(0.064)
Housing Voucher	0.070	0.091	0.037	0.119
	(0.018)**	(0.069)*	(0.010)***	(0.027)***
Poverty Graduation Program	0.079	0.091	0.084	0.116
	(0.028)	(0.057)*	(0.017)***	—
Lottery Win	0.073	0.038	0.009	0.051
	(0.029)*	(0.074)	(0.004)**	(0.019)**
Asset Transfer	0.158	0.213	0.201	0.271
	(0.048)*	(0.179)	(0.053)***	—
Health Insurance Provision	0.093	0.113	0.057	0.145
	(0.023)*	(0.126)	(0.019)**	(0.403)

Notes: Meta-analytic effect size estimates adjusted for publication bias, by intervention type. Column 1 reproduces the results from the "naïve" analysis without adjustment. Columns 2–4 show adjusted results using the P-uniform* method; the Andrews & Kasy method; and and the Vevea & Hedges method. The different rows report results for different intervention types. 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.

Table 8: Meta-analytic effects adjusted for publication bias, by well-being outcome

	(1) Naïve Estimation	(2) P-uniform*	(3) Andrews & Kasy	(4) Vevea & Hedges
All Interventions	0.100	0.112	0.100	0.138
	(0.013)***	(0.031)***	(0.010)***	(0.023)***
Depression	0.126	0.148	0.136	0.173
	(0.022)***	(0.060)**	(0.020)***	(0.062)**
Stress or Anxiety	0.055	0.068	0.052	0.091
	(0.010)***	(0.047)*	(0.012)***	—
Happiness	0.131	0.152	0.116	0.237
	(0.019)***	(0.060)**	(0.015)***	(0.061)***

Notes: Meta-analytic effect size estimates adjusted for publication bias, by well-being outcome. Column 1 reproduces the results from the "naïve" analysis without adjustment. Columns 2–4 show adjusted results using the P-uniform* method; the Andrews & Kasy method; and and the Vevea & Hedges method. The different rows report results for different well-being outcomes. 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.

4. Discussion

In this systematic review and meta-analysis, 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, happiness, and life satisfaction. These improvements are most robustly documented for unconditional cash transfers (UCTs). Effects decay over time, but do not differ much depending on the gender of the recipient and the transfer amount.

Together, these results suggest that economic transfers are effective in improving mental health and subjective well-being, lending support to the "social causation" hypothesis familiar to public health researchers.

While the present study is hopefully a helpful step in better understanding the well-being effects of economic interventions, it has a number of limitations. First, the overall number of studies is not large, and especially for individual intervention types and outcome variables, many cells only contain 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. Secondly 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 an overall heterogeneity estimate of $I^2 = 93.1\%$, which is high.[15] In addition, the measure fluctuates between 0 and 97% for different intervention types and well-being outcomes across our sample. At the same time, however, we note that the almost uniformly positive treatment effects of the interventions we study, despite their heterogeneity, suggests that negative or zero impacts are unlikely.[16].

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 and subjective well-being.

15. I^2 describes the percentage of total variation across studies that is due to heterogeneity rather than chance. It is estimated as $I^2 = 100\frac{(Q-df)}{Q}$, where Q is Cochran's heterogeneity statistic (the weighted sum of squared differences between individual study effects and the pooled effect across studies, with the weights being those used in the pooling method), and df the degrees of freedom (Higgins et al. 2003).

16. Higgins et al. 2003 note that because systematic reviews bring together studies that are methodologically diverse, heterogeneity is to be expected, and "there seems little point in simply testing for heterogeneity when what matters is the extent to which it affects the conclusions of the meta-analysis"

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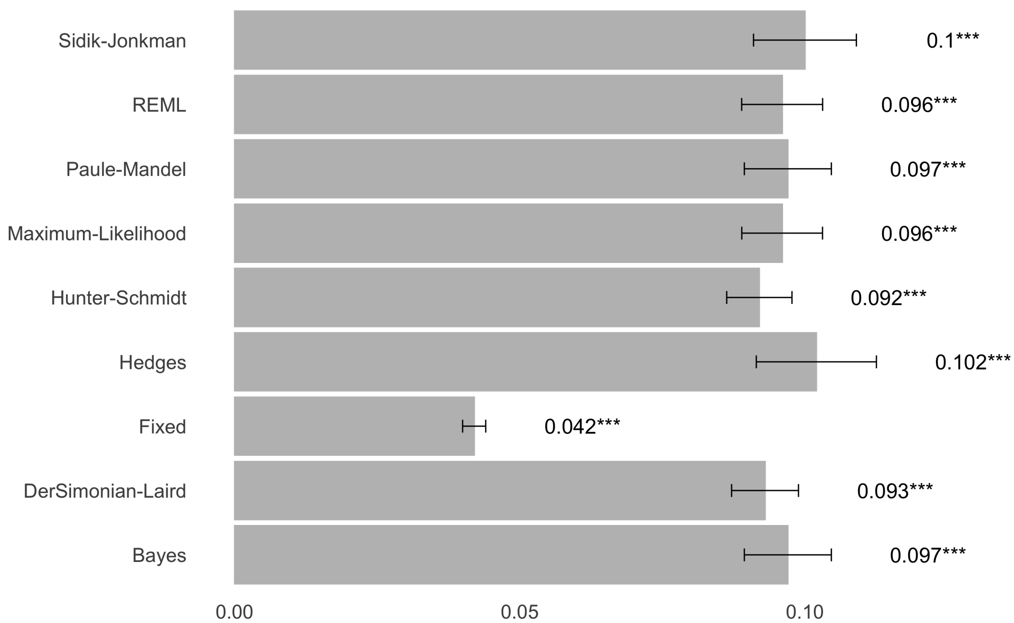
Appendix

A1. Pooled Estimates by Different Random Effects Methods

Figure A1: Meta-analytic effects using different Fixed and Random Effects estimators



Notes: Meta-analytic effect size estimates using different random effets and fixed effects methods. Each bar shows the meta-analytic effect of all interventions on all well-being outcomes using the respective estimator. The main analysis uses the Sidik-Jonkman estimator.

A2. Publication Bias

A common problem in meta-analyses (and in social science in general) is that effect size estimates may be affected by publication bias, i.e. a higher likelihood for studies to be published when the results are statistically significant, confirm some prior belief, or are surprising (Andrews and Kasy 2019). This can result in distorted meta-analytic effect size estimates. However, methods have recently become available to adjust meta-analytic estimates for publication bias. We use three of these methods: the method of Vevea and Hedges (1995); the P-uniform* approach of Aert and Assen (2018); and the method of Andrews and Kasy (2019).

A2.1 Vevea & Hedges (1995)

Vevea and Hedges 1995 propose model in which the likelihood for a study to be published depends on the p-value of a treatment effect. Specifically, publication probability is a step function of p-values. The model estimates probability of publication for each interval (Aert and Assen 2018). 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 a p-value cut-off of 0.05. Detailed results for our sample, using all intervention types and all well-being outcomes, are shown in Table A1.

Table A1: Meta-analytic effect adjusted for publication bias: Vevea & Hedges approach

Interval	Adjusted Estimate
Intercept	0.138
	(0.023)***
$0.05 < p < 1$	15.910
	(10.015)

Notes: Meta-analytic effect adjusted for publication bias using the Vevea and Hedges (1995) approach. The first row shows the adjusted pooled effect, controlling for the likelihood of publication in the p-value interval $0.05 < p < 1$. ones,

A2.2 Van Aert & Van Assen (2018)

Aert and Assen (2018) developed p-uniform* as an extension of the p-uniform model. The p-uniform approach is based on the fact that, under the null hypothesis, the p-values of hypothesis tests are uniformly distributed (Assen, Aert, and Wicherts 2015). The method therefore finds the effect size which generates a uniform distribution of p-values when it is used as the null hypothesis. The approach models all studies with statistically significant findings as equally likely to be published and included in the meta-analysis (Assen, Aert, and Wicherts 2015). The method has three drawbacks: (1) it uses only statistically significant effect sizes; (2) effect size estimates are positively biased when there is between-study variance in the true effect size; and (3) the approach does not estimate and test for presence of this between-study variance (Aert and Assen 2018).

The p-uniform* approach addresses these drawbacks. It assumes that the probability of publishing a statistically significant effect size and a non-statistically significant one are constant, but may be different around a cut-off value (for example, around $p = 0.05$; Aert and Assen 2018). 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.

Tables A2 and A3 compare the p-uniform and p-uniform* approaches. Each column shows the meta-analytic treatment effect estimate for a different method available within each of the two approaches. In Table **??**, the different methods to test the uniformity of the p-values are: P (Irwin-Hall), LNP (Fisher), LN1MINP (transforming p values to 1-P before applying Fisher's method), and Kolmogorov-Smirnov. In Table A3, the methods are P (Irwin-Hall), LNP (Fisher), and ML (maximum likelihood estimation of the effect size and the between-study variance). Using the p-uniform approach, we find very large meta-analytic effect sizes, that substantially exceed those of the naïve approach. In contrast, the estimates produced by the p-uniform* approach are very close to those produced by the naïve approach.

Table A2: Meta-analytic effects adjusted for publication bias: P-uniform approach

Estimation	P	LNP	LN1MINP	KS
All Interventions	0.396	0.303	0.480	0.396
	(0.414)*	(0.508)	(0.330)	-

Notes: Meta-analytic effect adjusted for publication bias using the p-uniform approach. Each column shows the meta-analytic effect size estimate corresponding 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.

A2.3 Andrews & Kasy

Andrews and Kasy (2019) propose a selection model that corrects the estimates for a known probability of publication, and this probability can be estimated around specific cut-offs of p-values. It makes the following distributional and functional form assumptions: publication bias is assumed be to a step function; the distribution of effect sizes is normal; and effect sizes and sample sizes are independent. While this latter assumption is common, it has been criticized as unrealistic, given that researchers often use power calculations to determine sample size (Lau et al. 2006).

Table A3: Meta-analytic effects adjusted for publication bias: P-uniform* approach

Estimation	P	LNP	ML
All Interventions	0.116	0.058	0.112
	(0.027)***	(0.033)*	(0.031)***

Notes: Meta-analytic effect adjusted for publication bias using the p-uniform* approach. Each column shows the meta-analytic effect size estimate corresponding to a different method to test uniformity of p-values: P (Irwin-Hall), LNP (Fisher), and ML (maximum likelihood estimator).

The 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.

Table A4 presents the results of applying this method to our dataset, assuming the probability of publication is symmetric and the cut-off is 1.96. We estimate a true effect (μ) of 0.104, with a standard deviation (τ) of 0.107. β is the relative publication probability of a significant vs. a non-significant result, and is estimated to be 1.000.

Table A4: Meta-analytic effects adjusted for publication bias: Andrews & Kasy approach

(1)	(2)	(3)
μ	τ	β
0.096	0.091	1.000
(0.009)***	(0.007)***	(0.182)***

Notes: Meta-analytic effect adjusted for publication bias using the Andrews & Kasy approach. μ is the meta-analytic effect size estimate; τ the variance estimate of the true effect; and β is the relative publication probability.

A3. Cash vs. In-kind transfers

Table A5: Meta-analytic effects for cash and in-kind transfers

	(1) All Mental Health Outcomes	(2) Depression	(3) Stress or Anxiety	(4) Happiness
All Interventions	0.100	0.126	0.055	0.131
	(0.013)***	(0.021)***	(0.010)***	(0.019)***
	[253 / 57]	[89 / 34]	[98 / 28]	[66 / 28]
In Cash	0.131	0.138	0.073	0.176
	(0.020)***	(0.036)**	(0.016)**	(0.032)***
	[98 / 32]	[47 / 21]	[25 / 12]	[26 / 14]
In Kind	0.080	0.111	0.051	0.104
	(0.012)***	(0.010)***	(0.011)**	(0.018)***
	[155 / 26]	[42 / 14]	[73 / 17]	[40 / 15]

Notes: Meta-analytic effect size estimates 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 delivered 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, and the numbers of effects and studies in brackets. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table A6: Correlates of effect size for cash and in-kind transfers

	(1) All Interventions	(2) In Cash	(3) In Kind
Constant	0.099	0.080	0.059
	(0.065)	(0.076)	(0.054)
Age	0.000	0.001	-0.001
	(0.001)	(0.002)	(0.001)*
LMIC	0.032	0.108	-0.012
	(0.029)	(0.072)	(0.028)
Female Share	0.034	-0.089	0.140
	(0.065)	(0.056)	(0.053)**
Lumpsum	-0.024	-0.022	0.002
	(0.036)	(0.055)	(0.031)
Intervention Value	-0.002	0.196	0.003
(as % of GDP per Capita)	(0.003)	(0.326)	(0.003)
Delay Intervention-Survey	-0.006	-0.015	-0.007
(Years)	(0.002)**	(0.006)**	(0.002)**
Obs/Studies	[253 / 57]	[98 / 32]	[155 / 26]

Notes: Correlates of effect size by intervention type. Each column is a meta-regression that estimates how the treatment effects of a specific set of interventions are related to a set of covariates. The first column shows the relationship between covariates and any intervention; the second and third columns show results for interventions delivered in-cash and in-kind, respectively. Age is the average age of the study sample; Low-/Middle Income Country is a binary variable if the intervention takes place in a low- or middle-income country; Female share is the percentage of women in the study sample; Lump Sum is a binary variable that indicates whether the intervention was a lump-sum transfer; Intervention Value is the value of the transfer as a percentage of the GDP per capita on the baseline year, in 1000s of USD; Delay Intervention-Survey is the delay, in years, between the start of the intervention and the outcome measurement. 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.

A4. Study 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 meta-analytic 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.