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ABSTRACT

We derive four facts from experiments on the spillover effects of cash and in-kind transfer programs onto non-recipients: (1) average spillovers on consumption and asset value are negligible; (2) negative psychological spillovers emerge in the longer term among households randomly excluded from transfers by within-village lotteries; (3) these spillovers are concentrated in consumption-poor households; and (4) they do not appear when the selection of transfer recipients is eligibility-based. These findings are consistent with household preferences against within-village randomization of transfers, and point to additional benefits of cluster-level randomized designs over household-level ones.

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1 Introduction

Cash and in-kind transfers are among the most common social safety net programs in the world (World Bank, 2018). A large literature documents improvements in economic and psychological outcomes for transfer recipients (Banerjee et al., 2015; Haushofer and Shapiro, 2016; McGuire et al., 2022). Yet the consequences of these programs extend beyond recipients themselves; the effects of transfers may spill over to non-recipients living nearby. These spillovers could be positive—for example, due to increased economic activity—or negative—for example, through recipients acquiring a greater share of scarce resources, or, relatedly, envy of recipients' good fortune. These spillovers should be accounted for in aggregate evaluations of transfer programs. In practice, however, these spillovers are often assumed to be zero due to limited evidence.[1]

We study the spillover effects of transfer programs, drawing on a systematically compiled set of randomized trials that separately identify direct and spillover effects. We first aggregate results using a random-effects meta-analysis framework for two categories of experimental designs, both of which use cluster randomization to assign higher-level units ("villages") into treatment or control conditions. In designs using *randomized selection* of beneficiaries, lower-level units ("households") within villages are randomly assigned to treatment—that is, receiving a transfer, or control. In designs using *targeted selection*, beneficiaries are identified non-randomly through mechanisms such as proxy-means testing or community-based selection. We find negligible economic spillovers onto non-recipients in both designs, no psychological spillovers in targeted designs, and negative psychological spillovers in randomized designs. These negative psychological spillovers only emerge after a year or more. By analyzing microdata from the four studies for which replication packages are available, we find support for two potential explanations for these findings: 1) household preferences against randomization of transfers within villages, and 2) better targeting of transfers to households that would otherwise suffer negative psychological spillovers. Both explanations support the use of targeted over randomized designs to minimize negative spillovers.

Our findings imply that the psychological benefits of randomized transfer programs are likely to be overstated. Evaluations that compare recipients to non-recipients in the same region will tend to over-estimate direct benefits due to negative indirect effects on the comparison group. If, instead,

[1]The Happier Lives Institute explicitly excludes spillover effects from their cost-effectiveness model, writing that "the evidence is extremely limited" (Happier Lives Institute, 2021). After GiveDirectly published estimates of negative spillovers from its cash transfer programs in 2018 (Haushofer and Shapiro, 2018), GiveWell updated their models to incorporate small negative consumption spillovers from cash transfers, but described that change as a "very uncertain and qualitative best guess" (GiveWell, 2018). See also GiveWell's updated discussion (GiveWell, 2025), as well as debates on the plausibility of economic spillovers (Özler, 2018; Sandefur, 2018).

an evaluation uses untreated villages as a control group, and does not separately examine the untreated in the treatment villages, the negative psychological spillovers on the untreated will be missed. Importantly, however, the allocation mechanism appears to matter: negative psychological spillovers appear only in designs using within-village randomization. Thus, our findings support targeting, rather than randomizing, transfers within small geographic areas in order to reduce these negative psychological effects.

In the short-term, we identify significant positive impacts on consumption, asset value, and psychological well-being among those who received a randomized transfer, consistent with the existing literature on direct effects of transfers. In the longer term, the direct impact on assets is reduced but remains positive and significant, while direct impacts on consumption and psychological well-being become statistically indistinguishable from zero. In contrast, we find that randomized transfers worsen the psychological well-being of non-recipients. These negative impacts appear only in the longer term, with no short-run effects. Short and long-run spillover impacts on consumption and assets are statistically indistinguishable from zero, suggesting that negative psychological spillovers are not driven by indirect economic impacts. Negative spillovers on psychological well-being appear only in experiments where recipient households are randomly selected; when recipients in treatment regions are targeted non-randomly, psychological spillovers are statistically indistinguishable from zero. The magnitude of long-run psychological spillover effects from randomized transfers is substantial, reducing well-being by 13% of a standard deviation. This is similar in magnitude to the 14–15% of a standard-deviation increase in psychological well-being we identify for direct recipients of transfers in the short run.[2]

To understand why negative spillovers appear in randomized but not targeted designs, we analyze microdata from four large-scale studies of graduation programs. In the three experiments of Banerjee et al. (2015) that use randomized targeting, we find that negative spillovers are driven by the poorest households in their villages. In contrast, the programs of Bandiera et al. (2017), which were community-targeted, but otherwise similar to those in Banerjee et al. (2015), did not create differential spillovers for the poorest and less-poor households. This finding indicates that the poorest households are not invariably worse off when they are excluded from transfers, as one might expect if they are more likely to know transfer recipients, or simply because of diminishing marginal utility. Rather, negative impacts arise specifically when transfers are randomized within villages. We argue that these findings, together, are consistent with preferences against ran-

[2]In the longer-term, direct psychological impacts are statistically indistinguishable from zero in randomized studies, and increase slightly for targeted studies; see Appendix Table A.3. Romero et al. (2021) conducts a meta-analysis of unconditional cash transfers and finds a similar 15% of a standard-deviation increase in psychological well-being for direct recipients, but does not distinguish between early and later impacts or randomized and targeted designs.

domization. We note that these findings are also consistent with targeted transfers reaching many spillover-sensitive households. In either case, our findings suggest that targeting should likely be preferred to randomization in transfer programs.

Literature. We contribute most directly to the limited literature studying the indirect effects of transfer programs. That literature shows that cash transfers can generate general equilibrium responses that can make non-recipients better off, for example, through labor-market adjustments, changes in local production, or insurance and credit markets (Angelucci and De Giorgi, 2009; Egger et al., 2022; Franklin et al., 2024). Cash transfer effects on prices are often small (Cunha et al., 2019; Egger et al., 2022; Allen and Gentilini, 2025), although in one study they induced meaningful economic inflation and worsened health among non-recipient children (Filmer et al., 2023). Evidence of negative psychological spillovers can be found in some of the individual studies we analyze, but the effects are estimated with considerable uncertainty and there has been little investigation into their potential causes.[3] We add to this literature a systematic meta-analysis of the economic and psychological spillover effects of transfer programs, along with new evidence for the likely mechanisms behind them. Our work builds on an earlier meta-analysis by McGuire et al. (2022) that finds no psychological spillovers. By expanding the studies considered, including longer-term follow-ups, and examining microdata when available, we show that both the allocation mechanism and timing of assessments modify this conclusion.

A much larger literature estimates the direct impacts of cash and in-kind transfers on recipient households. A central finding across this literature is that transfers generate sizable improvements in recipients' economic well-being (Schady et al., 2009; Banerjee et al., 2015; Bandiera et al., 2017; Crosta et al., 2024; Assaad et al., 2026), schooling outcomes (Baird et al., 2011), and psychological well-being (Haushofer and Shapiro, 2016; Romero et al., 2021)—among many other outcomes—without decreasing labor supply (Banerjee et al., 2017).[4] While this literature has been highly influential in policy-making, much of it continues to rely on designs that compare recipients to non-recipients within the same local economy, treating non-recipients as an unaffected counterfactual. Our finding that transfers can affect the psychological well-being of recipients' neighbors strengthens the case for estimating direct impacts using cluster-based experimental designs that avoid within-village comparisons.

We also contribute to the large literature studying the importance of allocation mechanisms in anti-poverty programs. The ways that aid is targeted—for example, by proxy-means test, community-based targeting, or self-targeting—trade off exclusion and inclusion errors, admin-

[3] See Baird et al. (2013); Banerjee et al. (2015); Haushofer and Shapiro (2018); and Haushofer et al. (2023).

[4] Long-term evidence on economic well-being is more mixed (see Blattman et al., 2020; Banerjee et al., 2021).

istrative costs, and perceived legitimacy (Alatas et al., 2012, 2016; Dupas et al., 2016; Hanna and Olken, 2018; Basurto et al., 2020; Sumarto et al., 2025). Community-based targeting produces higher satisfaction than proxy-means tests, possibly because communities apply a notion of neediness that differs from a proxy-means test (Alatas et al., 2012), or because of measurement error in survey-based consumption data (Gazeaud, 2020). We add to this literature by comparing the spillover impacts of randomized to non-randomized allocations. Our findings underscore the importance of the distribution mechanism in mediating overall welfare impacts.

A related theoretical and laboratory-based literature emphasizes the importance of procedural utility: the notion that people have preferences not only over allocations, but also over the processes by which those allocations are obtained (Bolton et al., 2005; Karni et al., 2008). There is growing evidence that randomization is perceived as less fair or less legitimate than allocation rules that appear to reflect merit, need, or deliberation, leading to lower acceptance and welfare even when outcomes are distributionally similar (Bouacida and Foucart, 2025; Bueno et al., 2025). These preferences appear to depend on inequality, with randomization being preferred to direct recipient selection when laboratory participants held equal endowments, but disliked compared to direct selection when endowments were unequal (Haushofer et al., 2019). Our paper links the study of procedural utility to that of transfer design, and argues that the randomization aversion documented in survey and laboratory settings may generate persistent negative psychological spillovers in the real world when high-stakes decisions are made randomly.

2 Design of the Meta-Analysis

This section describes the design of our meta-analysis, including how we identify studies, estimate impacts within each study consistently, and aggregate these estimates into average treatment effects.

2.1 Identifying Transfer Studies

We systematically reviewed the economics literature for cash or in-kind transfer experiments that measured both direct and spillover effects on consumption and psychological well-being.[5] We used *EconLit* to search the top 16 applied and general-interest economics journals for papers from any date with the words "transfer" and "randomized" in the text, using a built-in search tool to

[5]We use a broad definition of psychological well-being, encompassing measures of subjective well-being, mental health, and stress. For each study, we use a standardized index that aggregates all available measures of psychological well-being reported in that study. See Appendix Table A.2 for details.

include related word forms. This produced a set of 89 papers. Within that set, we selected all studies satisfying the following criteria: 1) it evaluated a cash or in-kind transfer program, 2) it used a cluster-randomized design, and 3) it collected data needed to measure direct and spillover impacts on consumption and a measure of psychological well-being.[6]

We count evaluations of programs across multiple countries as separate studies even if they are published in the same article, for example Banerjee et al. (2015). We count evaluations of the same program in the same country as a single study even when estimates are published in separate papers, for example Haushofer and Shapiro (2016) and Haushofer and Shapiro (2018). This produced a set of four studies. We added to this four additional studies we were also aware of that satisfied the same criteria, but which did not show up in the search because they were published in the journal *Science* (three studies) or in the NBER Working Paper Series (one study). For studies with only short-term results (around one year after transfers began), we searched for longer-term follow-up estimates and obtained them either from working papers or from the authors directly. Our final sample includes eight studies, each contributing point estimates from both the short- and longer-term.

2.2 Estimating Direct and Spillover Impacts

We consider two cluster-randomized designs that identify direct and spillover impacts of transfers, distinguished by how lower-level units are selected within clusters—by randomization or by a targeting rule. We analyze these two designs separately because they identify distinct spillover parameters: studies using lower-level randomization identify spillovers onto eligible households, while those using targeting identify spillovers onto ineligible households. In both designs, large geographic areas are assigned randomly to a treatment or a control condition. We describe these geographic areas as *villages*, reflecting the typical design, although they are larger than villages in some studies.

For designs using *randomized selection* of transfer recipients, some smaller units, which we refer to as *households*, again reflecting the terminology of a typical design, are randomly selected within treatment villages to receive a transfer. We identify direct effects by comparing treated households to control villages, and indirect effects by comparing untreated households in treated villages with control villages. We estimate the following regression on the full experimental sam-

[6]We excluded papers that identify spillovers through a saturation design—that is, a design that varies the share of treated units in a region. These designs identify spillover impacts of program intensity rather than the program itself.

ple:

$$Y_i = \alpha_1 \text{Treat}_i + \alpha_2 \text{Spillover}_i + X_i\Gamma + \varepsilon_i, \tag{1}$$

in which Treat_i equals 1 for households assigned to receive a transfer in a treatment village and 0 otherwise, and Spillover_i equals 1 for households assigned to not receive a transfer in a treatment village and 0 otherwise. An optional control vector X_i includes pre-intervention controls such as randomization-stratum fixed effects, controls for survey timing, or pre-intervention values of Y_i (in panel data). Direct effects are estimated through the coefficient α_1 which compares recipient households to all households in control villages. Indirect effects are estimated through the coefficient α_2 which compares non-recipient households in treatment villages to all households in control villages.

For designs using *targeted selection* of transfer recipients, eligible households are chosen based on criteria that are observable independent of treatment assignment. For example, eligible households may be those in the bottom of the distribution of baseline household consumption, or those that have a thatch roof. Within treatment villages, eligible households receive the transfer while ineligible households do not. We identify direct effects by comparing treated households in treatment villages to eligible but untreated households in control villages, and spillover effects by comparing ineligible households in treatment villages to ineligible households in control villages. We estimate the following regression:

$$Y_i = \beta_1 \text{Treated Village}_i + X_i\Omega + \nu_i, \tag{2}$$

in which Treated Village_i equals 1 for households residing in treatment villages and 0 otherwise. When the estimation sample includes only eligible households, the coefficient β_1 estimates direct effects of the transfer. When the estimation sample includes only ineligible households, the coefficient β_1 estimates spillover effects of the transfer.

We apply this estimation framework consistently across studies. For each of the eight studies we identified, we extracted the necessary impact estimates from the paper, or, if they were not available in the main text or appendices, from replication files. In most cases, authors followed the above estimation strategies. In cases where they implemented different strategies, we used replication files to re-estimate direct and spillover impacts consistently according to the equations above.[7]

[7]Specifically, Sedlmayr et al. (2020) includes non-recipient households in treatment villages when estimating direct impacts. Bandiera et al. (2017) uses difference-in-differences estimation, which reduces statistical power compared to cross-sectional specifications that control for the pre-intervention value of the outcome variable, sometimes

2.3 Aggregating Results Across Studies

We aggregate estimates using a random-effects meta-analysis estimated by restricted maximum likelihood.[8] For each study j, let $\widehat{\theta}_j$ denote the estimated treatment effect and $\widehat{s}_j$ its standard error. We assume that the observed effect sizes are drawn from a population distribution with mean θ and between-study variance τ^2, such that $\widehat{\theta}_j = \theta + u_j + \varepsilon_j$, where $\varepsilon_j \sim N(0, \widehat{s}_j^2)$ and $u_j \sim N(0, \tau^2)$. The pooled average effect θ is then obtained as a weighted mean of the study-level estimates, where the inverse-variance weights are given by $(s_j^2 + \widehat{\tau}^2)^{-1}$. This approach gives greater weight to studies with more precise estimates while allowing for heterogeneity across studies.[9] We estimate $\widehat{\tau}^2$ by restricted maximum likelihood (REML).[10]

3 Results of the Meta-Analysis

This section reports our estimates of the average direct and spillover impacts of transfers on consumption, asset value, and psychological well-being.[11]

3.1 Randomized Transfers

Direct effects. Randomized cash and in-kind transfers generate sizable improvements in economic outcomes for recipient households. As shown in Appendix Figure A.1, in the shorter term, we estimate positive and statistically significant effects on both consumption and asset value:

called ANCOVA regression (McKenzie, 2012). We re-estimate impacts using ANCOVA regression. Banerjee et al. (2015) includes additional control variables beyond those needed for efficient, unbiased estimation. We re-estimate impacts without these controls.

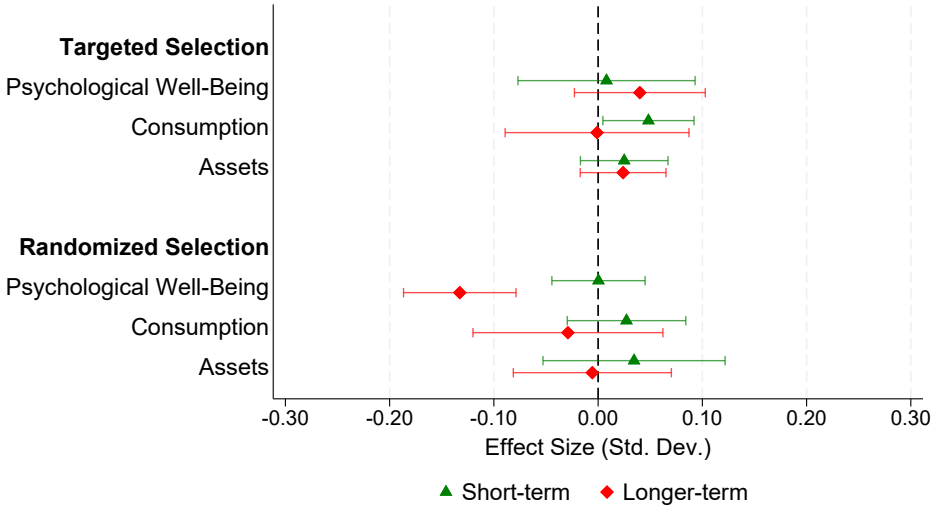
[8] Meta-analyses have been criticized for combining overly disparate estimates and for not excluding low-quality studies (Simonsohn et al., 2022). We mitigate the former concern by only selecting studies that used similar interventions—cash or in-kind transfer—and designs—cluster randomized controlled trials. Importantly, we accessed each paper's microdata as needed to apply a consistent estimation strategy and harmonize outcomes throughout. The latter concern is mitigated by analyzing large-scale, well-designed studies.

[9] Random-effects meta-analyses account for both within-study sampling variance and between-study heterogeneity. Compared to fixed-effects meta-analysis, random-effects meta-analysis relaxes the assumption that there is no heterogeneity across studies. It produces similarly precise estimates to a fixed-effect model when between-study heterogeneity is low, and expands standard errors when heterogeneity is high.

[10] See, for example, McKenzie (2020) for a related implementation.

[11] We present estimates of direct effects in the appendix. As few papers fit the strict screening criteria needed to identify spillover effects, we omit several studies that would be useful for obtaining more precise estimates of direct economic effects, such as many of those considered in the meta-analysis of Romero et al. (2021). Point estimates of direct and spillover effects are reported in Table A.3. Disaggregated effects by study are presented in the figures in Appendix B.

Figure 1: The negative psychological spillovers of randomized transfers emerge in the longer term.

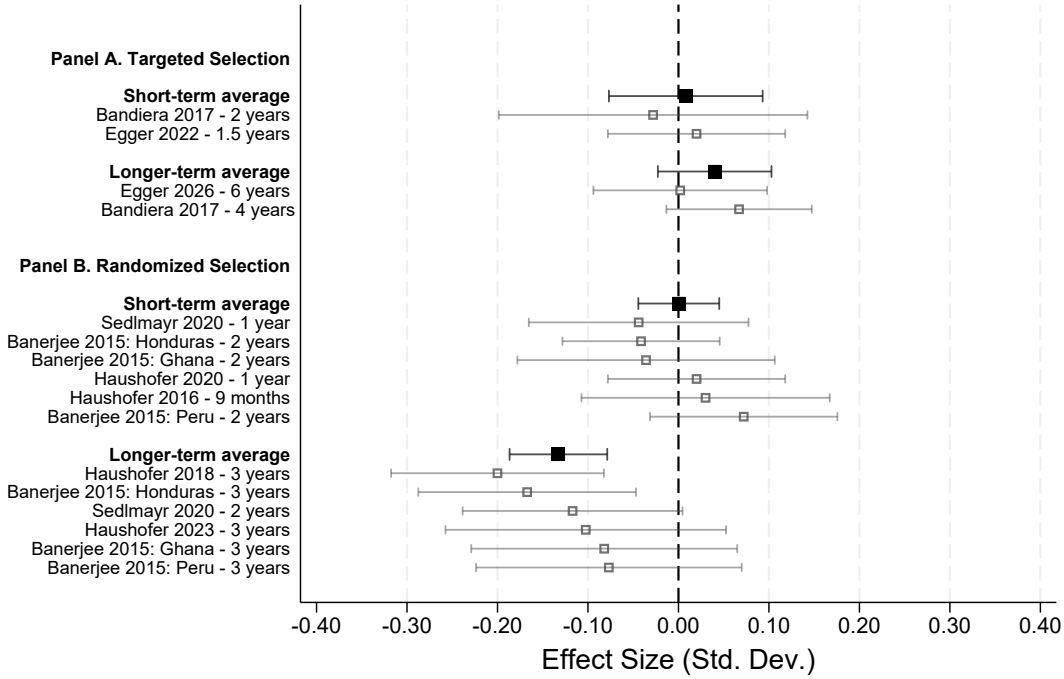


Notes: Average treatment effects estimated through REML. Short-term impacts are measured in the first post-treatment survey round within two years following treatment. Longer-term impacts show the latest available measures, typically three years after the treatment. Bars show 95% confidence intervals.

pooled effects are on the order of 10–30% of a standard deviation of the control-group outcome variable. Psychological well-being also improves in the short run, with an average increase of around 10–20% of a standard deviation. In the longer term, the direct effects on asset value attenuate but remain positive and statistically significant at $p < 0.10$, while the corresponding effects on consumption and psychological well-being are statistically indistinguishable from zero.

Spillover effects. For non-recipient households in villages with randomized transfers, we find little evidence of economic spillovers, but clear, negative, longer-term effects on psychological well-being, as shown in Figure 1. In the short term, the average spillover effects on consumption and asset value are close to zero and statistically insignificant, and we likewise estimate no meaningful short-run spillover on psychological well-being. In the longer term, the spillover effects on consumption and assets remain near zero, but the spillover effect on psychological well-being becomes negative and is precisely estimated. Lending credibility to this average negative effect is the fact that the point estimates on longer-term psychological effects are uniformly negative for studies employing randomized designs, as shown in Figure 2. The long-run pooled effect on non-recipients' psychological well-being is about -13% of a standard deviation, a comparable magnitude to the roughly 15% of a standard-deviation improvement in psychological well-being of unconditional cash transfer recipients in the meta-analysis of Romero et al. (2021). This effect

Figure 2: Longer-term negative psychological spillovers appear in all randomized studies.



Notes: Psychological well-being is a standardized index measure of subjective well-being, mental health, and stress measures available in each study. Average treatment effects estimated through REML. Short-term impacts are measured in the first post-treatment survey round within two years following treatment. Longer-term impacts show the latest available measures, typically three years after the treatment. Estimates for Egger et al. (2026) were shared privately and are preliminary. Bars show 95% confidence intervals.

does not appear to be driven by indirect economic impacts, as spillovers on consumption and asset value are statistically indistinguishable from zero. Moreover, there is no systematic correlation between economic and psychological spillovers across studies, see Appendix Figure A.2.

3.2 Targeted Transfers

Direct effects. When transfers are allocated using targeted selection, we observe direct impacts that are similar to studies using within-village randomization. On average, there are sizable direct effects for recipient households: pooling across follow-up rounds, targeted transfers raise consumption, asset value, and psychological well-being by 16–24% of a standard deviation, with all three effects precisely estimated.

Spillover effects. We find no substantial spillovers on consumption or asset value from targeted transfers in the longer-term, which is, again, consistent with studies using within-village randomization.[12] These pooled spillover effect estimates are small and generally not statistically different from zero. However, in contrast to randomized transfers, targeted transfers do not create negative spillovers on the psychological well-being of non-recipients living in treatment villages. The pooled spillover effect on our psychological well-being index is slightly positive—3% of a standard deviation—and not statistically different from zero. Our estimates are precise enough that we can rule out the possibility that the longer-term psychological spillover effects are the same across both designs.

4 Investigating the Causes of Negative Spillovers

Why do transfers worsen measures of psychological well-being for non-recipients? Economic spillovers do not appear to be responsible: there is no evidence of negative economic spillovers, and economic and psychological spillovers are uncorrelated across studies (see Appendix Figure A.2). The allocation mechanism is immediately focal, as significant spillovers occur, on average, only in studies using randomized—as opposed to targeted—beneficiary selection. However, other explanations are possible, as we outline in Section 4.1. To disentangle these possible explanations, we analyze microdata from the three cluster-randomized experiments reported in Banerjee et al. (2015), which used random selection of recipient households, and the one from Bandiera et al. (2017), which used targeted household selection.[13] Helpfully for our purposes, these four experiments study the impacts of very similar interventions, as we describe in Section 4.2.

4.1 Potential Mechanisms

Below we describe three classes of models that could potentially rationalize spillovers of randomized, but not targeted, transfers:

Explanation 1A: Preferences against randomization. Random selection of beneficiaries necessarily excludes some households who would have received a transfer based on need or some other notion of fairness. One possible explanation for our findings, which follows from the literature on procedural utility, is that exclusion is painful when it is based on randomization, but

[12] In these studies, ineligible households show a short-term increase of 4.8% of a standard deviation in consumption, but it does not persist.

[13] Complete microdata were not readily accessible for other studies.

not when based on "fair" targeting. In that case, we would expect those households most likely to believe they would receive a fairly allocated transfer—for example, poor households—to drive negative spillovers in studies using randomized selection.

Explanation 1B: Targeted designs reach spillover-sensitive households. An alternative formulation of Explanation 1A is that targeted transfers may be especially likely to reach households who would otherwise suffer from exclusion from transfers. In this formulation, negative spillovers emerge in studies using randomized selection not because of preferences over allocation mechanisms, but because certain types are excluded in randomized, but not targeted, designs. This explanation produces the same empirical prediction as preferences against randomization: households that suffer when passed over for transfers, such as the poorest households, will drive negative spillovers in studies using randomized selection.

Explanation 2: Utility cost of losing lotteries. Households might prefer not entering a lottery to entering one and losing, even without downside economic risk. If households view random assignment—but not targeted assignment—as being like a lottery, this preference could explain the negative psychological impacts that arise in randomized but not targeted designs. Unlike Explanations 1A and 1B, this theory predicts negative psychological impacts for both the poorest and less-poor households in studies using random beneficiary selection.

Explanation 3: Higher sensitivity to spillovers among the poor. A class of models can explain our findings if the poor are more sensitive to being excluded from transfers, and if they are more likely to be excluded in randomized compared to targeted designs. A higher sensitivity for the poorest households could arise if these non-recipients, compared to less-poor non-recipients, are more likely to interact with the poorest recipients. This is what one would expect if social networks are at least partly segregated by income. Or, being passed over for a transfer may be intrinsically more painful for the poor given that the transfer is worth relatively more to them. Unlike Explanations 1A and 1B, this theory predicts greater negative spillovers onto the poorest households, compared to the less-poor, in both randomized and targeted designs.

The above mechanisms give distinct predictions about how spillovers should vary by baseline household poverty. We will examine these predictions in the microdata from four graduation studies we describe in the next subsection.

4.2 Study and Transfer Design

Banerjee et al. (2015). This paper reports on the impacts of "graduation" programs (combining in-kind transfers with consumption support, training, and coaching) in six countries. We focus on the three countries in which two-stage randomization was used.[14] The study was conducted in poor regions of each country, with villages selected based on input from program staff. Villages were randomly assigned to treatment or control status. Within treatment villages, a sample of exceptionally poor households was identified using a wealth ranking based on statements from villagers, and verified during visits by program staff. Within this set of eligible, exceptionally poor households, households were randomly assigned to the program or not. Figure 3 shows the distribution of baseline household consumption in this sample. Median baseline monthly consumption is about $62 per capita, which is balanced across treatment and control.

The intervention involved a multifaceted graduation program. The core component was a productive-asset transfer, where households selected an income-generating asset valued between $450–850 (in PPP terms). The program also provided temporary consumption support through cash stipends. Households received business training before the transfer, followed by high-frequency coaching visits over two years. Additional components included savings facilitation, often through microfinance institutions or group savings schemes, and health-related education.

Bandiera et al. (2017). This paper also evaluates the impacts of a graduation program. The program targeted exceptionally poor households in rural Bangladesh using a two-stage, participatory ranking design. The study was first targeted at the district level based on food security measures. Then, 40 program branch offices were randomly assigned to treatment or control. Before implementation, a participatory wealth-ranking exercise was conducted in all villages to classify households into four wealth groups, with the exceptionally poor (6% of the population) being eligible for the intervention. Surveys were attempted with all exceptionally poor and near-poor households, as well as some wealthier ones.

The intervention offered recipients a choice of income-generating assets, primarily livestock, alongside training, ongoing support, and a short-term subsistence allowance. Each asset transfer was valued at $560 in PPP terms. Recipients also received classroom training, regular home visits from program officers and livestock specialists for up to two years, health support, training on legal and social rights, and encouragement to save or access microfinance after the program.

As shown in Figure 3, the eligibility rule, based on the exceptionally poor classification, creates

[14] These are Ghana, Honduras, and Peru. In the other three—India, Ethiopia, and Pakistan—only household-level randomization was used, so spillovers cannot be identified.

partial, but not total, separation of groups based on baseline wealth. While ineligible households are less poor on average (median of 87 USD compared to 28 USD in total wealth), there is considerable overlap. This is especially true for baseline consumption, where the eligibility rule created little separation across groups. The median eligible household has a baseline monthly consumption of 55 USD per capita, compared with 62 USD per capita for ineligible households. We exploit this feature of the experimental design to test whether spillover impacts on psychological well-being vary by initial household consumption and assets.

4.3 Empirical Approach

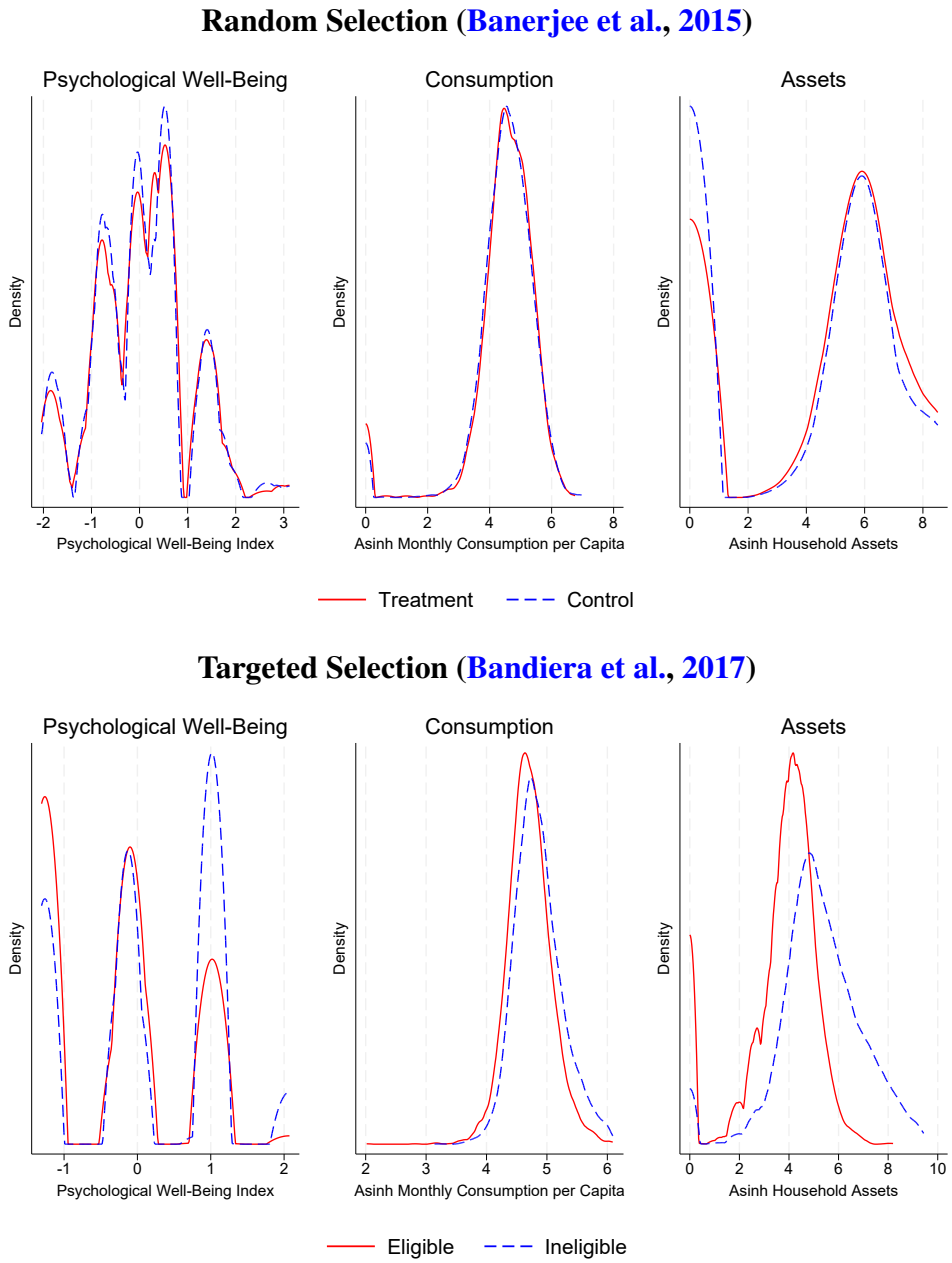
To identify spillover effects on untreated households, and test how these vary by the household's baseline poverty status, we estimate the following ANCOVA specification on untreated households:

$$Y_{it} = \gamma_1 \text{Treat}_i + \gamma_2 \text{Poor}_{i0} + \gamma_3 \text{Treat}_i \times \text{Poor}_{i0} + \theta Y_{i0} + \delta M_{i0} + \alpha_i + \varepsilon_{it} \tag{3}$$

in which Y_{it} is an outcome for household or individual i in survey-wave t—total asset value, monthly per capita household consumption, or a psychological well-being index—with $t = 0$ corresponding to pre-treatment (baseline) values; Treat_i equals 1 if the household is located in a village where other households were assigned to receive transfers; $Poor_{i0}$ equals 1 if the household is categorized as among the poorest at baseline—for example, residing within the bottom 50% of the consumption distribution in its village—M_{i0} is an indicator for a missing value of the baseline outcome Y_{i0} (we recode Y_{i0} to the average non-missing value when it is missing); and α_i is a randomization-stratum fixed effect, corresponding to a country in Banerjee et al. (2015) and a sub-district in Bandiera et al. (2017). We standardize outcomes to mean 0, standard deviation 1 by survey wave using data from control villages, and cluster standard errors at the village level in Banerjee et al. (2015)—corresponding to the unit of randomization—and the sub-district level in Bandiera et al. (2017)—corresponding to the randomization-pair level.[15]

[15]Equation 3 deviates from those used in the original papers. Bandiera et al. (2017) uses difference-in-differences specifications, but ANCOVA improves statistical power in experimental panel studies (McKenzie, 2012). The original papers cluster standard errors at the unit-of-randomization level, but clustering at the randomization-pair level improves error rates (de Chaisemartin and Ramirez-Cuellar, 2024). Banerjee et al. (2015) includes additional control variables beyond randomization-stratum fixed effects. We add only pre-randomization outcomes, which is preferable in most experimental settings to algorithmic control variable selection (Cilliers et al., 2024). We show in Appendix Tables A.8 and A.9 that employing the authors' original estimating equations does not qualitatively affect our results.

Figure 3: The spillover samples in the randomized and targeted designs have comparable consumption and asset distributions.



Banerjee et al. (2015) selects recipient households randomly within a set of poor households; Bandiera et al. (2017) uses a participatory wealth ranking to target recipient households. *Assets* is the value of household assets. *Consumption* is total monthly household expenditure per capita. *Psychological Well-Being* is a standardized index including happiness, anxiety, and stress measures in Banerjee et al. (2015) and happiness and anxiety measures in Bandiera et al. (2017). Monetary units are the inverse hyperbolic sine of 2014 USD PPP.

4.4 Results

Figure 4 shows our results graphically by examining quartiles of baseline consumption. Because psychological spillovers emerge in the longer-term, Figure 4 focuses on heterogeneity in longer-term impacts: Appendix Tables A.4 and A.5 show that psychological spillovers are not significantly different for the poorest compared to less-poor households in the short-run, nor are they statistically different from zero.

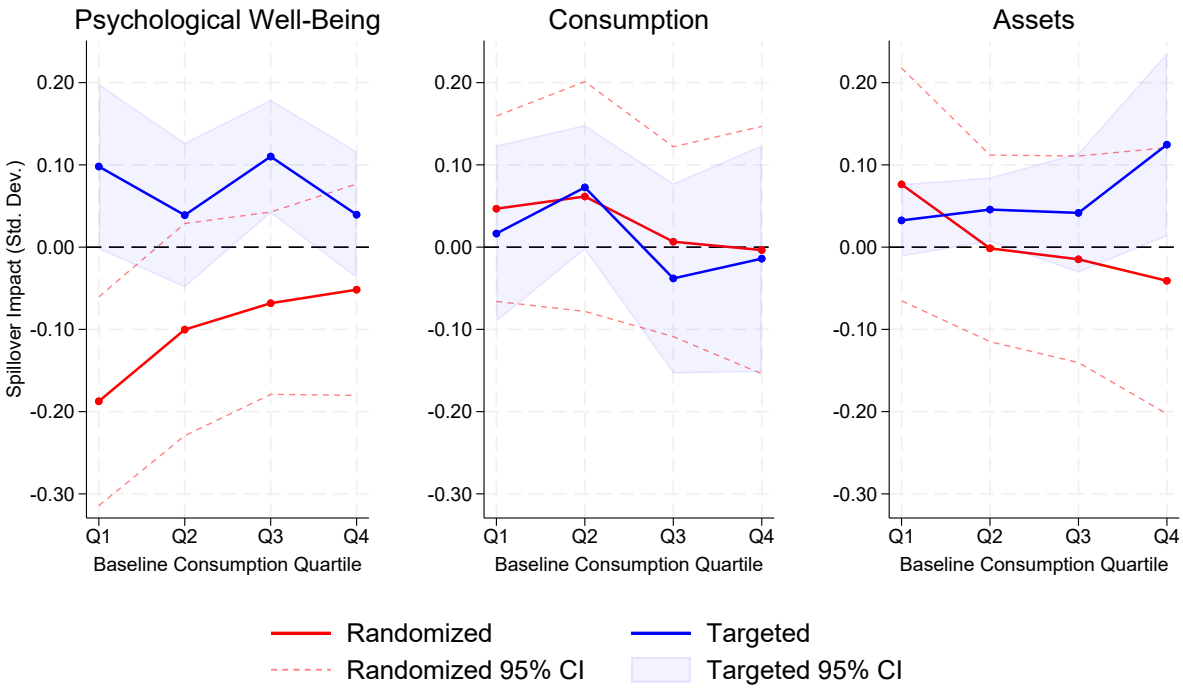
We find that negative psychological spillovers are greater in magnitude for consumption-poorer households in the three studies using randomized targeting, which is consistent with preferences against randomization. In the poorest quartile, the average impact is -19% of a standard deviation; in the least-poor, it is -5% of a standard deviation and is not statistically significant.[16] In contrast, we find no significant gradient in asset or consumption spillovers in these studies. If anything, consumption and asset spillovers are slightly more positive for the consumption-poorest households compared to the less-poor, suggesting that heterogeneity in economic spillovers is not behind this pattern. In the targeted transfers studied in Bandiera et al. (2017), we find no significant gradients by baseline household consumption for asset, consumption, or psychological well-being spillovers.

An additional test for preferences against randomization involves evaluating whether greater exposure to less-poor beneficiaries is correlated with larger psychological spillovers, especially in randomized studies. We construct a measure of each household's exposure to less-poor beneficiaries, defined as the average log difference between a non-recipient household's consumption and that of its less poor recipient neighbors, with differences to poorer recipient neighbors coded as zero.[17] This measure captures the logic that non-recipient households may dislike allocations in which either 1) there are many less-poor recipients in their village, and 2) those recipients are (much) less-poor than they are. We estimate versions of (3) that split the sample into highly exposed and less exposed households, and control for an indicator of whether the non-recipient's own baseline consumption is high or low. Results, shown in Appendix Table A.6, are consistent with those of Figure 4: households experience worse psychological spillovers when they are passed over for less-poor recipients, and to a greater degree when allocations are chosen randomly than when they are targeted.

[16] See Appendix Table A.5 for estimates of (3) and tests of statistical significance. The average difference between the poorest and least-poor halves is -0.122 ($p = 0.04$), as shown in that table.

[17] For each non-recipient household (i), we define exposure as $1/N_{iv} \sum_{j \in \mathscr{T}v} \max\left(\log C_{j0} - \log C_{i0}, 0\right)$, where C_{i0} is baseline consumption, $\mathscr{T}v$ is the set of recipient households in i's assignment area (village or branch), and $N_{iv} = |\mathscr{T}_v|$. Thus measured exposure is larger when a non-recipient is exposed to more recipients whose baseline consumption exceeded its own, and when those consumption gaps are larger.

Figure 4: The poorest households drive negative psychological spillovers in randomized, but not targeted, designs.



Red line shows estimates from Banerjee et al. (2015); blue line shows estimates from Bandiera et al. (2017). Vertical axis shows spillover impacts estimated on the final round of follow-up data separately in four subsamples defined by quartiles of baseline per capita household consumption. Quartiles are defined within the unit of program operation—a village in Banerjee et al. (2015) and a branch in Bandiera et al. (2017). All outcomes are standardized.

These results are difficult to reconcile with a utility cost of losing lotteries: less-poor households, as classified by consumption, do not experience significant negative spillovers from randomized transfers. They are also not consistent with an intrinsically greater sensitivity to spillovers among the consumption-poorest households, as these households do not experience negative spillovers from targeted transfers. However, they are consistent with a subset of spillover-sensitive households being more likely to receive the transfer in targeted studies. This explanation requires baseline consumption to be only partly correlated with spillover sensitivity. If consumption perfectly captured spillover sensitivity, we would expect to see negative spillovers among the consumption-poor excluded from targeted transfers, but we do not. If consumption was orthogonal to spillover sensitivity, we would expect spillovers from randomized studies to be uniform in baseline consumption, but we find that they are greater in magnitude for consumption-poor households.

We do not observe significant spillover heterogeneity by a household's position in the distribution of baseline assets, as shown in Appendix Table A.7. While the interaction coefficient for low-asset households is negative, it is not statistically significant. This result suggests that the type of household that suffers from transfer exclusion—whether because they prefer targeting to randomization, or because targeting does a good job of identifying them—is better proxied by baseline consumption than by baseline assets.[18]

5 Discussion

This paper aggregates evidence from eight experimental studies to assess whether cash and in-kind transfers generate spillover effects onto non-recipients. While transfers generally improve economic and psychological outcomes for recipients, we find negligible economic spillovers, and identify negative psychological spillovers among non-recipients in the long term—but only when recipients are chosen at random within villages. Overall, our findings suggest a natural channel through which transfer programs may affect non-beneficiaries: through dissatisfaction with the procedure used to allocate resources. Whether this dissatisfaction stems from procedural utility or from less successful targeting of exclusion-sensitive households is a question left for future research. In particular, we note that we can only evaluate the impacts of transfers targeted through community ranking or through eligibility based on the thatch-roof criterion used in Egger et al. (2022). Our results do not speak directly to transfers based, for example, on more opaque (to recipients) eligibility criteria such as regression-based proxy-means tests.

One remaining puzzle involves explaining why negative psychological spillovers arise only in the longer term: this finding holds both in Banerjee et al. (2015) specifically and in our meta-analytic results more broadly. The explanation is unlikely to be purely economic in nature, as consumption spillovers are small and statistically indistinguishable from zero in both the longer and shorter run. One potential explanation is that non-recipient households in treatment villages experience disutility from observing the benefits of transfers to their neighbors, which may take time to materialize. This is somewhat difficult to reconcile with the greater direct economic benefits to transfer recipients in the shorter term compared to the longer term, unless treatment dynamics differ greatly for observable and unobservable components of consumption. One plausible answer is that it takes time for households to discern their position in the consumption distribution of recipients. Only after a household observes many recipients who are richer than they are, can

[18]This explanation is arguably consistent with the greater within-village inequality in consumption than in assets, and with the stronger correlation between psychological well-being and consumption than between psychological well-being and assets.

they infer with confidence that the transfer was not targeted. Another plausible explanation is that non-recipient households who see their neighbors receive transfers hold out hope that they will eventually receive one too. After several years (or surveys), this hope fades.

The lack of negative spillovers among the poorest quartile in Bandiera et al. (2017) is potentially surprising given that eligible households in that study did not have substantially lower consumption than ineligible ones (see Figure 3). Because eligibility was determined using public, group-level meetings, it is highly unlikely that ineligible households were not aware that some higher-consumption households had been chosen for the transfer. We believe a likely explanation of these findings is that participatory wealth rankings produce an allocation that many households view as fair: the findings of Alatas et al. (2012) in a different setting support this view. Together, these findings suggest that allocations generally perceived as fair can help prevent the negative spillovers of randomized allocations, even when they offer transfers to low- and high-consumption households alike.

List of Studies Included in our Meta-Analysis

RCTs Using Random Household Selection. Banerjee et al. (2015); Haushofer and Shapiro (2016, 2018); Haushofer et al. (2020); Sedlmayr et al. (2020); Haushofer et al. (2023)

RCTs Using Targeted Household Selection. Bandiera et al. (2017); Egger et al. (2022, 2026)

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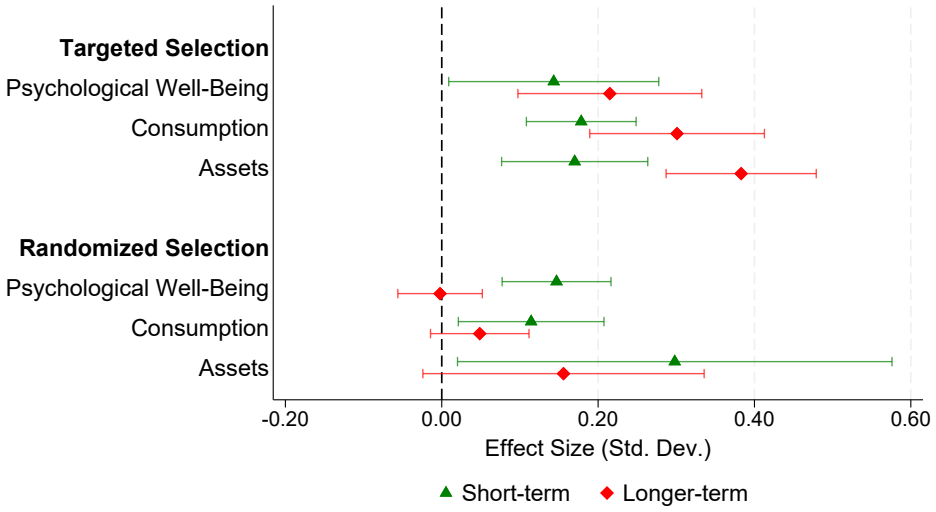
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Appendix for "Spillover effects of transfer programs"

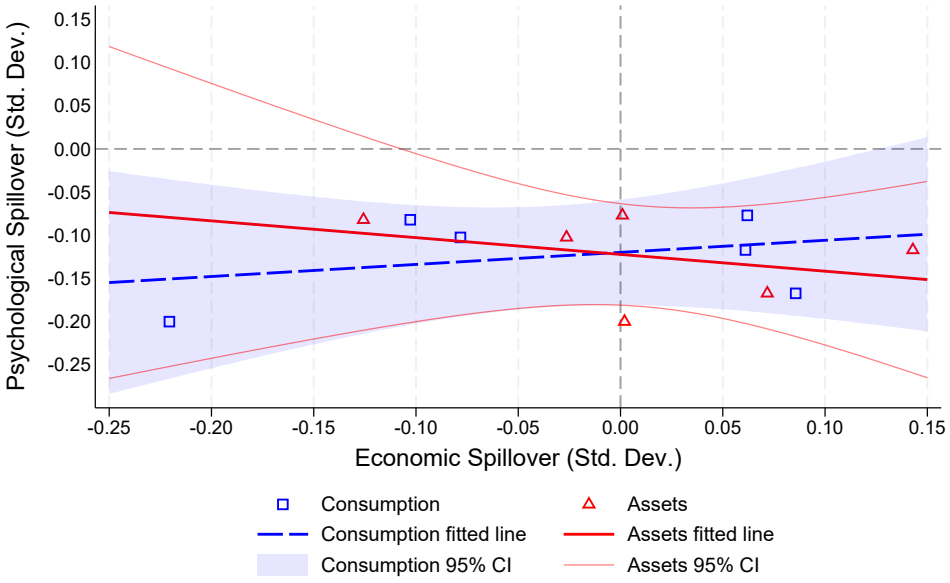
A Additional Tables and Figures

Figure A.1: Summary of Direct Impacts



Notes: Average treatment effects estimated through REML. Short-term impacts are measured in the first post-treatment survey round within two years following treatment. Longer-term impacts show the latest available measures, typically three years after the treatment. Bars show 95% confidence intervals.

Figure A.2: There is no correlation between economic and psychological spillovers across studies.



Notes: Each point shows the longer-term spillover impact (the latest available measure, typically three years after the treatment) for a single study. Only studies with randomized targeting are included.

Table A.1: Summary of Spillover Studies

	(1) Banerjee et al. (2015) – Ghana	(2) Banerjee et al. (2015) – Honduras	(3) Banerjee et al. (2015) – Peru	(4) Haushofer and Shapiro (2016, 2018) – Kenya
Journal	*Science*	Ibid.	Ibid.	*QJE*; working paper
Treatment	Graduation package: productive asset, subsistence allowance, training, home visits, savings promotion, and health/life-skills support	Ibid.	Ibid.	Unconditional cash transfer (monthly installments over 9 months or lump sum)
Transfer timing	2011/12 (asset)	2009/10 (asset)	2011 (asset)	2011/13
Transfer value (in PPP US$2017)	533 (asset)	635 (asset)	1,010 (asset)	757
Recipient eligibility	Participatory wealth ranking at village level	Participatory wealth ranking at village level; followed by verification survey	Participatory wealth ranking at village level; followed by verification survey	Thatched-roof homes
Design	Two-stage randomization (village then household randomization)	Ibid.	Ibid.	Two-stage randomization (village then household randomization)
Sample size (baseline)	2,606 households	2,403 households	2,284 households	1,440 households
Clusters	154 villages	80 villages	86 villages	120 villages

	(5) Bandiera et al. (2017) – Bangladesh	(6) Haushofer et al. (2020, 2023) – Kenya	(7) Sedlmayr et al. (2020) – Uganda	(8) Egger et al. (2022) – Kenya
Journal	*QJE*	NBER Working Papers	*JDE*	*Econometrica*
Treatment	Graduation package: productive asset, subsistence allowance, home visits, skills training	Unconditional cash transfer (lump sum or 5 weekly transfers)	Unconditional cash transfer (two transfers, seven months apart)	Unconditional cash transfers (delivered in three installments over eight months)
Transfer timing	2007 (asset)	2017/18	2014 on average	2014/17
Transfer value (in PPP US$2017)	662 (asset)	1,076	302	1,937
Recipient eligibility	Participatory wealth ranking; followed by verification survey	Homes without brick, stone, or metal walls	Participatory wealth ranking plus Poverty Probability Index score ≤ 39	Thatched-roof homes
Design	Branch randomization	Two-stage randomization (village then household randomization)	Two-stage randomization (village then household randomization)	Village randomization (with higher-level sub-locations assigned to high or low saturation)
Sample size (baseline)	21,839 households	3,519 households	2,088 households	7,845 households
Clusters	40 branches	120 villages	138 villages	653 villages

Notes: Haushofer et al. (2020, 2023) and Sedlmayr et al. (2020) include other treatment arms than the unconditional cash transfer (i.e., psychotherapy and microenterprise programs, respectively). We focus on the unconditional cash transfer arm in each study to ensure comparability across studies.

Table A.2: Measurement of psychological well-being indices across studies

Panel A. Studies with random selection	
Banerjee et al. (2015): Ghana	Standardized average of life satisfaction, stress, and worries
Banerjee et al. (2015): Honduras	Standardized average of life satisfaction and stress
Banerjee et al. (2015): Peru	Standardized average of life satisfaction and stress
Haushofer and Shapiro (2016, 2018)	Standardized weighted average of life satisfaction, stress, worries, depression, happiness, cortisol levels
Haushofer et al. (2020, 2023)	Standardized weighted average of life satisfaction, psychological distress and mental health disorders (GHQ-12; Cohen; only 2020), depression (only 2023), happiness
Sedlmayr et al. (2020)	Subjective well-being
Panel B. Studies with targeted selection	
Bandiera et al. (2017)	Standardized average of happiness and mental anxiety
Egger et al. (2022)	Standardized weighted average of life satisfaction, happiness, depression, perceived stress

Notes: All variables are appropriately signed so that positive values represent better psychological well-being.

Table A.3: Direct and Spillover Impacts by Design (in SD)

	(1) Psychological well-being	(2) Consumption	(3) Assets
Panel A. Studies With Random Recipient Selection			
A1. Direct effects			
Short-term	0.147***	0.114**	0.298**
	(0.036)	(0.048)	(0.142)
Longer-term	-0.002	0.049	0.156*
	(0.028)	(0.032)	(0.092)
Pooled effect	0.078**	0.081***	0.228***
	(0.031)	(0.028)	(0.084)
A2. Spillover effects			
Short-term	0.000	0.027	0.034
	(0.023)	(0.029)	(0.045)
Longer-term	-0.133***	-0.029	-0.006
	(0.028)	(0.046)	(0.039)
Pooled effect	-0.058**	-0.001	0.016
	(0.025)	(0.027)	(0.029)
Panel B. Studies With Targeted Transfers			
B1. Direct effects			
Short-term	0.143**	0.178***	0.170***
	(0.069)	(0.036)	(0.048)
Longer-term	0.215***	0.301***	0.383***
	(0.060)	(0.057)	(0.049)
Pooled effect	0.162***	0.216***	0.238***
	(0.050)	(0.044)	(0.074)
B2. Spillover effects			
Short-term	0.008	0.048**	0.025
	(0.043)	(0.022)	(0.021)
Longer-term	0.040	-0.001	0.024
	(0.032)	(0.045)	(0.021)
Pooled effect	0.029	0.039*	0.025
	(0.025)	(0.020)	(0.015)

Average treatment effects are estimated through REML. For studies with random recipient selection, short-term impacts are measured in the first post-treatment survey round within two years following treatment; longer-term impacts use the latest available measure, typically three years after treatment. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Average Spillover Impacts From Banerjee et al. (2015) and Bandiera et al. (2017)

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Impacts at End of Intervention						
Treatment Village/Branch	-0.002	0.036	0.059	-0.028	0.045*	0.018
	(0.034)	(0.038)	(0.044)	(0.087)	(0.025)	(0.018)
	[0.95]	[0.35]	[0.18]	[0.75]	[0.08]	[0.32]
Control Village/Branch Mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Observations	6,452	4,779	4,784	15,651	15,237	16,068
Impacts 1–2 Years Later						
Treatment Village/Branch	-0.106**	0.006	-0.004	0.067	-0.001	0.073**
	(0.042)	(0.037)	(0.040)	(0.041)	(0.045)	(0.027)
	[0.01]	[0.87]	[0.91]	[0.11]	[0.99]	[0.01]
Control Village/Branch Mean	-0.001	-0.000	-0.000	0.000	-0.000	-0.000
Observations	6,181	4,622	4,617	15,607	15,239	15,993

Columns 1–3 report estimates from the study with randomized household selection, Banerjee et al. (2015); columns 4–6 report estimates from the study with targeted household selection, Bandiera et al. (2017). Results are estimated using ANCOVA regressions separately by survey wave. All outcomes are standardized. All specifications control for *Low Consumption*, an indicator for whether the household is in the bottom 50% of the village-level distribution of per-capita baseline consumption (in Banerjee et al. (2015)) or of the branch-level distribution of baseline consumption (in Bandiera et al. (2017)). "End of Intervention" refers to the first endline survey, approximately 24 months after the start of interventions. "Impacts 1–2 Years Later" refers to the later follow-up survey: one year later in Banerjee et al. (2015) and two years later in Bandiera et al. (2017). Assets and consumption are measured at the household level. Psychological well-being is measured at the individual level in Banerjee et al. (2015) and for one individual per household in Bandiera et al. (2017). Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Spillover Impacts by Baseline Consumption

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Impacts at End of Intervention						
Treatment Village/Branch	-0.032	-0.010	0.087	-0.015	0.023	0.015
	(0.044)	(0.055)	(0.054)	(0.074)	(0.031)	(0.030)
	[0.47]	[0.86]	[0.11]	[0.84]	[0.46]	[0.63]
Treatment Village/Branch × Low Consumption	0.062	0.095	-0.059	-0.031	0.048	-0.005
	(0.058)	(0.063)	(0.056)	(0.053)	(0.030)	(0.029)
	[0.28]	[0.13]	[0.29]	[0.57]	[0.13]	[0.85]
Control Village/Branch Mean	0.001	-0.000	-0.000	-0.007	-0.019	-0.012
Observations	6,451	4,779	4,784	14,413	14,075	14,796
Impacts 1–2 Years Later						
Treatment Village/Branch	-0.047	-0.008	-0.021	0.076**	-0.023	0.084*
	(0.050)	(0.048)	(0.049)	(0.036)	(0.060)	(0.043)
	[0.34]	[0.87]	[0.66]	[0.05]	[0.70]	[0.06]
Treatment Village/Branch × Low Consumption	-0.122**	0.029	0.035	-0.011	0.067	-0.039
	(0.058)	(0.060)	(0.055)	(0.027)	(0.045)	(0.037)
	[0.04]	[0.63]	[0.53]	[0.69]	[0.16]	[0.31]
Control Village/Branch Mean	-0.001	-0.000	-0.000	-0.005	-0.018	-0.013
Observations	6,180	4,622	4,617	14,360	14,041	14,734

See Table A.4 for notes on specifications and definitions. All specifications control for *Low Consumption*, an indicator for whether the household is in the bottom 50% of the village-level distribution of per-capita baseline consumption (in Banerjee et al. (2015)) or of the branch-level distribution of baseline consumption (in Bandiera et al. (2017)). Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Long-Run Spillover Impacts by Exposure to Richer Beneficiaries

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Treatment Village/Branch	-0.049	-0.037	-0.058	0.085**	-0.014	0.113**
	(0.049)	(0.047)	(0.047)	(0.035)	(0.060)	(0.040)
	[0.32]	[0.43]	[0.22]	[0.03]	[0.82]	[0.01]
Treatment Village/Branch × High Exposure	-0.108**	0.087	0.098**	-0.031	0.042	-0.094***
	(0.053)	(0.054)	(0.047)	(0.020)	(0.037)	(0.032)
	[0.04]	[0.10]	[0.04]	[0.15]	[0.27]	[0.01]
Control Village/Branch Mean	-0.002	-0.001	0.004	-0.005	-0.018	-0.013
Observations	6,121	4,497	4,492	14,360	14,041	14,734

Columns 1–3 report estimates from the study with randomized household selection, Banerjee et al. (2015); columns 4–6 report estimates from the study with targeted household selection, Bandiera et al. (2017). Results are estimated using ANCOVA regressions. The sample includes untreated households only: pure-control households and non-beneficiary households in treatment villages or branches. *High Exposure* identifies non-beneficiary households in treatment villages or branches in the upper half of their study's distribution of exposure to richer beneficiaries. Exposure to richer beneficiaries is the mean across beneficiary households in the same village or branch of the difference between the beneficiary's log baseline consumption and the non-beneficiary household's log baseline consumption (coded as 0 if the beneficiary is poorer). All specifications control for the baseline value of the outcome, an indicator for a missing baseline value, and an indicator for whether the household has a value of baseline consumption higher than the study median. The Banerjee specifications include country fixed effects; the Bandiera specifications include sub-district fixed effects. All outcomes are standardized. Impacts are measured one year after intervention end in Banerjee et al. (2015) and two years after in Bandiera et al. (2017). Assets and consumption are measured at the household level. Psychological well-being is measured at the individual level in Banerjee et al. (2015) and for one individual per household in Bandiera et al. (2017). Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Spillover Impacts by Baseline Assets

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Impacts at End of Intervention						
Treatment Village/Branch	-0.010	0.008	0.030	-0.026	0.051	0.015
	(0.043)	(0.049)	(0.054)	(0.081)	(0.031)	(0.025)
	[0.82]	[0.87]	[0.58]	[0.75]	[0.11]	[0.56]
Treatment Village/Branch × Low Assets	0.031	0.089	0.094	-0.036	-0.016	-0.021
	(0.061)	(0.065)	(0.064)	(0.045)	(0.036)	(0.024)
	[0.61]	[0.17]	[0.14]	[0.44]	[0.67]	[0.39]
Control Village/Branch Mean	0.001	-0.000	-0.000	-0.012	-0.019	-0.021
Observations	6,451	4,779	4,784	15,497	15,110	15,998
Impacts 1–2 Years Later						
Treatment Village/Branch	-0.087*	-0.023	-0.034	0.058	-0.007	0.092**
	(0.047)	(0.046)	(0.050)	(0.049)	(0.052)	(0.036)
	[0.07]	[0.61]	[0.50]	[0.25]	[0.90]	[0.02]
Treatment Village/Branch × Low Assets	-0.052	0.094	0.095	0.007	0.010	-0.074**
	(0.067)	(0.065)	(0.070)	(0.035)	(0.029)	(0.029)
	[0.44]	[0.15]	[0.17]	[0.85]	[0.75]	[0.02]
Control Village/Branch Mean	-0.001	-0.000	-0.000	-0.010	-0.017	-0.023
Observations	6,180	4,622	4,617	15,454	15,110	15,916

See Table A.4 for notes on specifications and definitions. All specifications control for *Low Asset*, an indicator for whether the household is in the bottom 50% of the village-level distribution of baseline household assets (in Banerjee et al. (2015)) or of the branch-level distribution of baseline assets (in Bandiera et al. (2017)). Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Average Spillover Impacts, Original Specifications

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Impacts at End of Intervention						
Treatment Village/Branch	-0.005	0.036	0.046	-0.100	0.022	0.011
	(0.031)	(0.035)	(0.043)	(0.094)	(0.030)	(0.024)
	[0.87]	[0.30]	[0.29]	[0.30]	[0.47]	[0.65]
Control Village/Branch Mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Observations	6,452	4,779	4,784	46,603	45,440	48,200
Impacts 1–2 Years Later						
Treatment Village/Branch	-0.111***	0.003	-0.015	0.019	-0.002	0.048
	(0.040)	(0.036)	(0.037)	(0.046)	(0.047)	(0.032)
	[0.01]	[0.93]	[0.69]	[0.68]	[0.97]	[0.16]
Control Village/Branch Mean	-0.001	-0.000	-0.000	0.000	0.000	-0.000
Observations	6,181	4,622	4,617	46,603	45,440	48,200

See Table A.4 for notes on variable construction. Results use the original empirical specifications from each paper: the Banerjee et al. (2015) specifications add the authors' additional baseline controls, while the Bandiera et al. (2017) specifications use difference-in-differences regressions including all three survey waves. Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

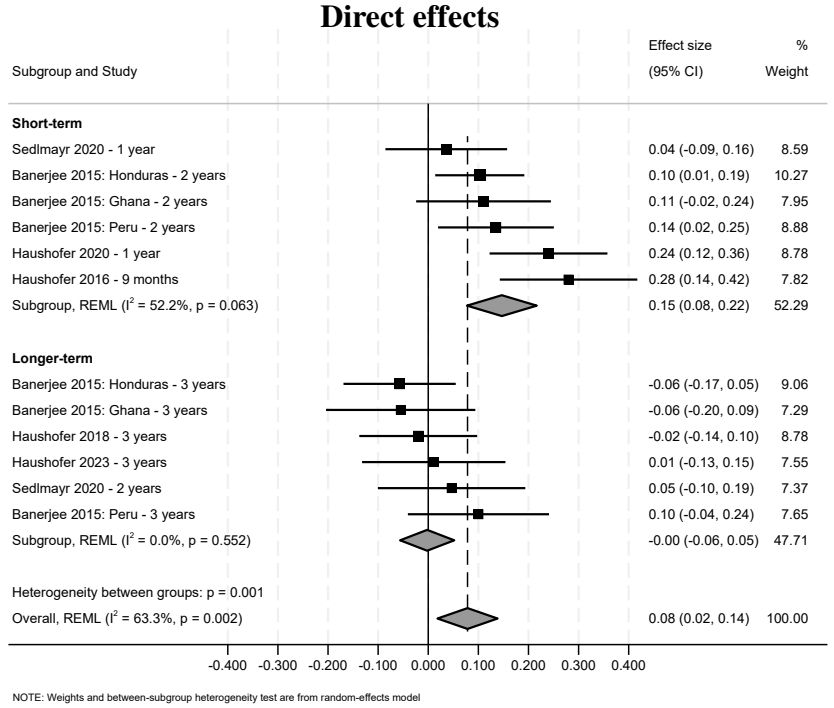
Table A.9: Spillover Impacts by Baseline Consumption, Original Specifications

	Random Selection (Banerjee et al., 2015)			Targeted Selection (Bandiera et al., 2017)		
	Psych. Well-Being	Consumption	Assets	Psych. Well-Being	Consumption	Assets
Impacts at End of Intervention						
Treatment Village/Branch	-0.040	-0.002	0.068	-0.089	0.009	-0.011
	(0.042)	(0.051)	(0.053)	(0.079)	(0.032)	(0.030)
	[0.34]	[0.97]	[0.20]	[0.27]	[0.79]	[0.71]
Treatment Village/Branch × Low Consumption	0.073	0.080	-0.045	-0.024	0.038	0.049
	(0.058)	(0.059)	(0.055)	(0.057)	(0.036)	(0.035)
	[0.21]	[0.18]	[0.41]	[0.67]	[0.31]	[0.18]
Control Village/Branch Mean	0.001	-0.000	-0.000	-0.005	-0.012	-0.013
Observations	6,451	4,779	4,784	43,737	43,080	44,375
Impacts 1–2 Years Later						
Treatment Village/Branch	-0.060	-0.013	-0.038	0.036	-0.022	0.037
	(0.049)	(0.048)	(0.047)	(0.043)	(0.059)	(0.045)
	[0.22]	[0.79]	[0.42]	[0.41]	[0.72]	[0.41]
Treatment Village/Branch × Low Consumption	-0.106*	0.032	0.048	-0.009	0.063	0.030
	(0.057)	(0.061)	(0.057)	(0.030)	(0.051)	(0.045)
	[0.07]	[0.60]	[0.40]	[0.76]	[0.23]	[0.51]
Control Village/Branch Mean	-0.001	-0.000	-0.000	-0.005	-0.012	-0.013
Observations	6,180	4,622	4,617	43,737	43,080	44,375

See Table A.4 for notes on variable construction. Results use the original empirical specifications from each paper: the Banerjee et al. (2015) specifications add the authors' additional baseline controls, while the Bandiera et al. (2017) specifications use difference-in-differences regressions including all three survey waves. Standard errors clustered at the village level for Banerjee et al. (2015) and at the sub-district/randomization-pair level for Bandiera et al. (2017) are in parentheses; two-sided p-values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Disaggregated Effects Per Study

Figure B.1: Effects on Psychological Well-Being—Random Selection



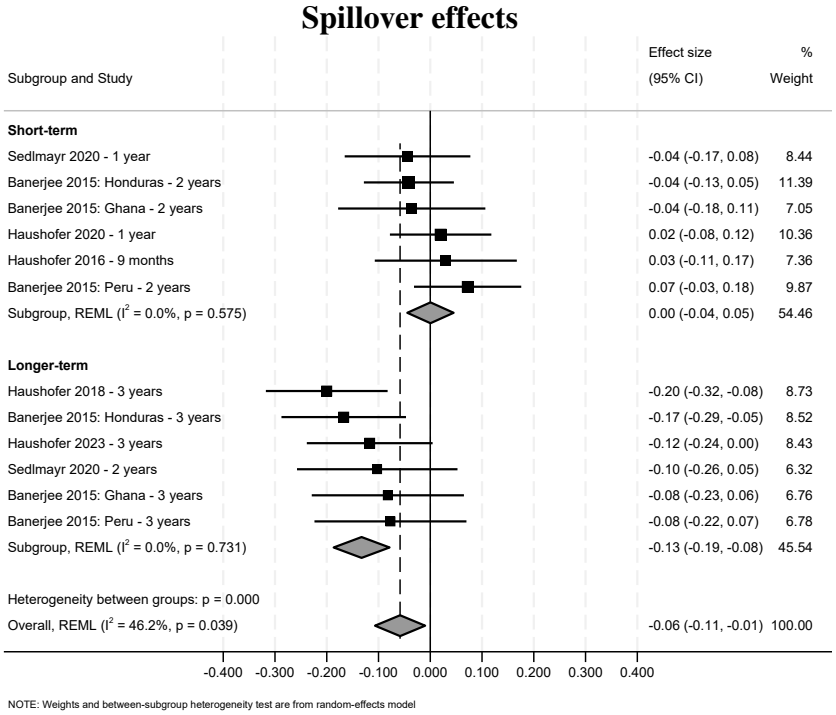
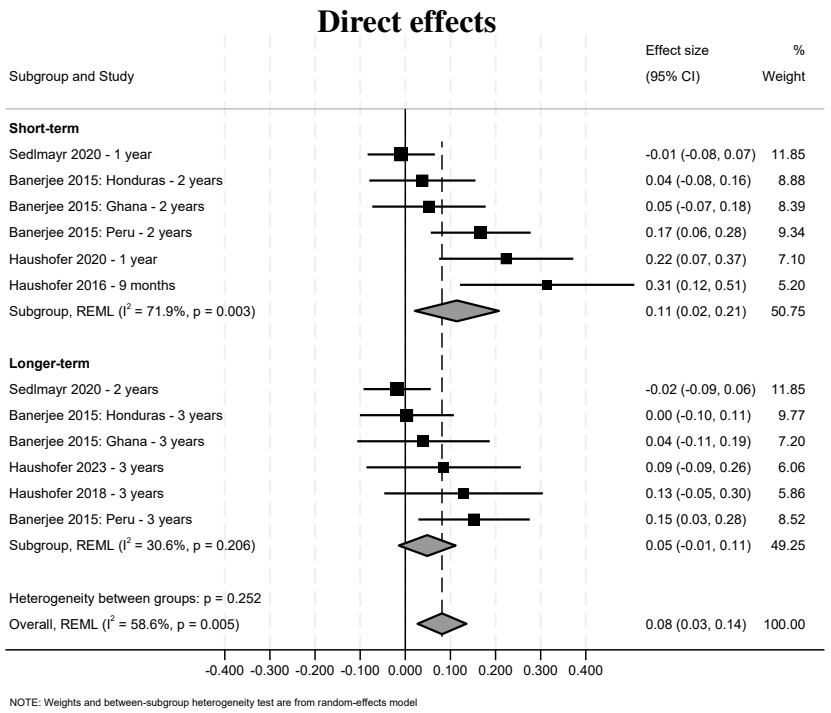
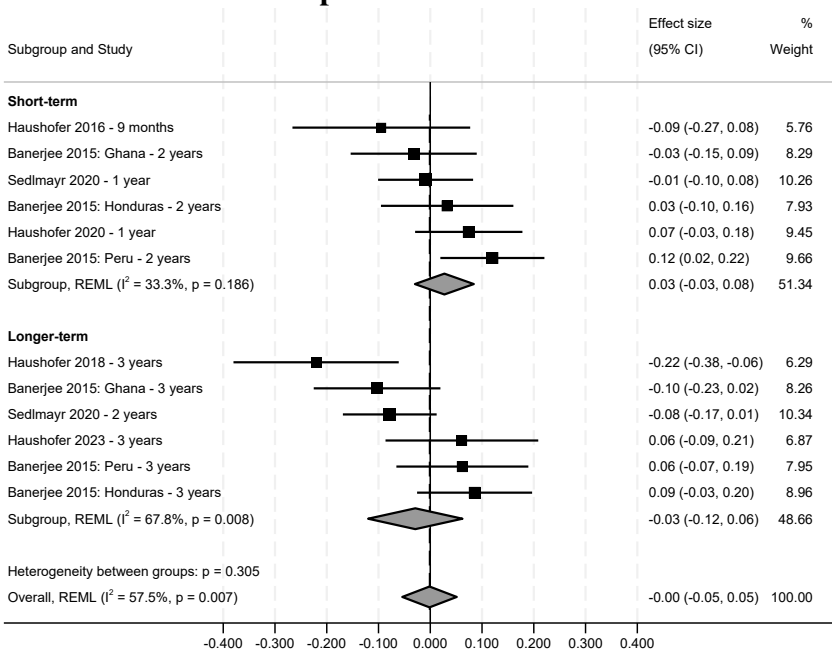


Figure B.2: Effects on Consumption—Random Selection

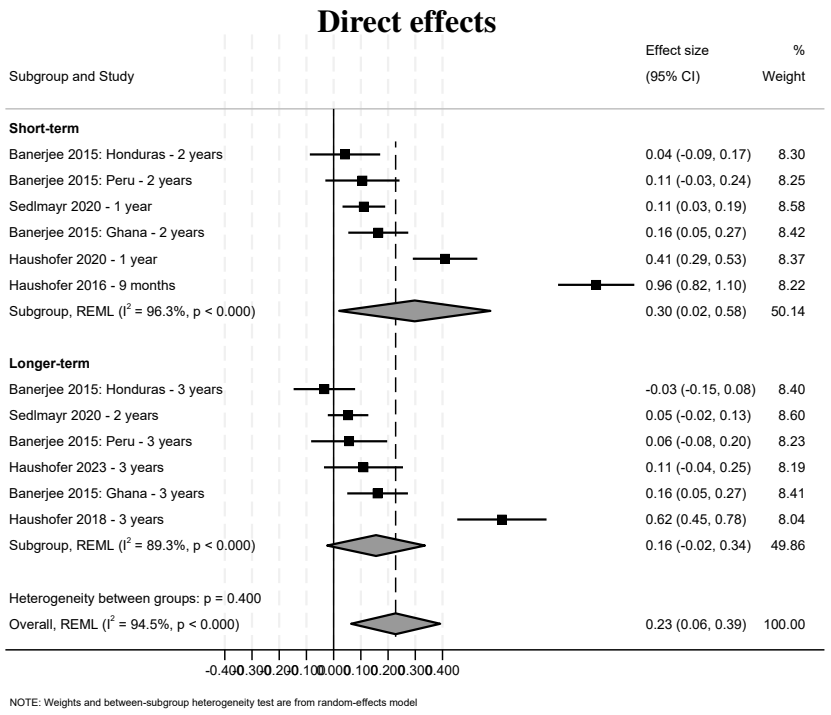


NOTE: Weights and between-subgroup heterogeneity test are from random-effects model

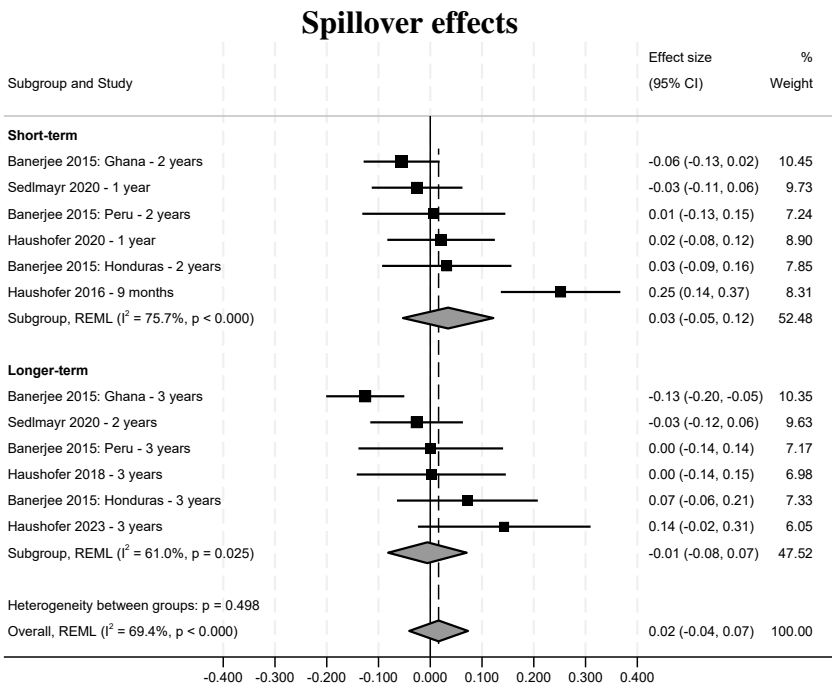


NOTE: Weights and between-subgroup heterogeneity test are from random-effects model

Figure B.3: Effects on Assets—Random Selection

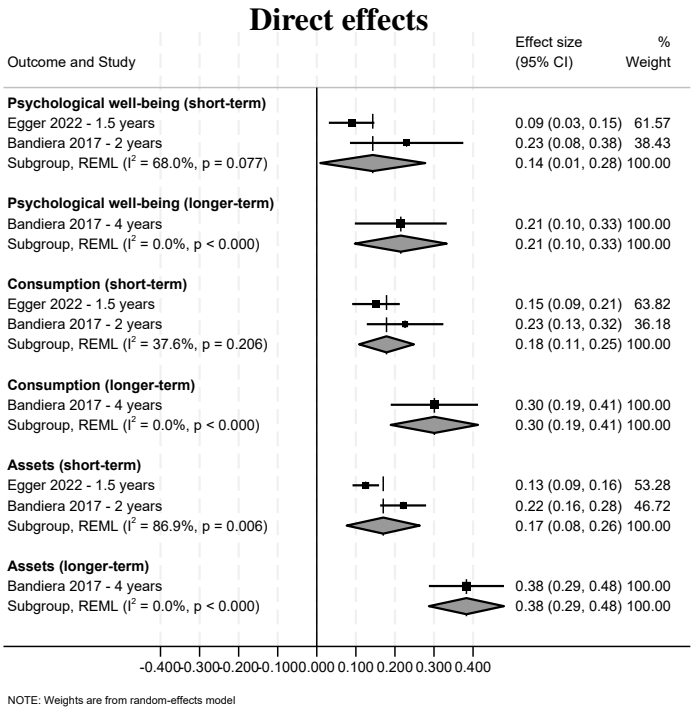


NOTE: Weights and between-subgroup heterogeneity test are from random-effects model

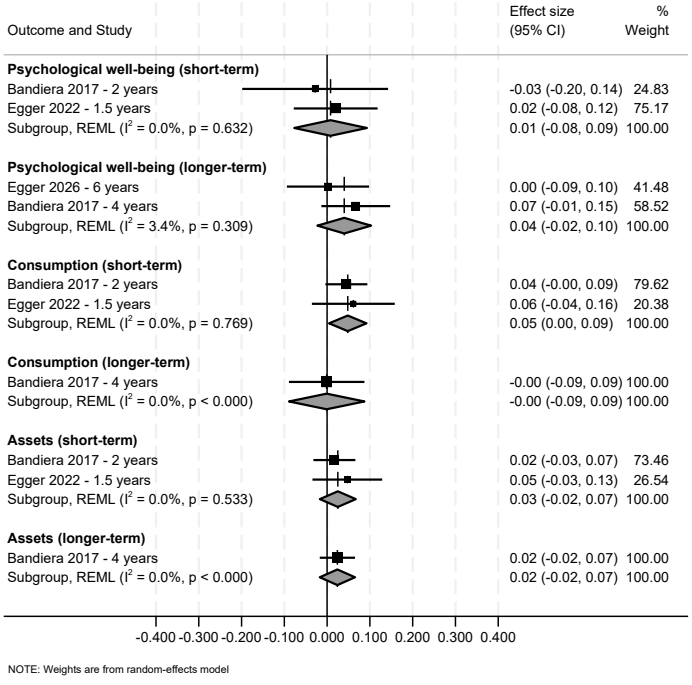


NOTE: Weights and between-subgroup heterogeneity test are from random-effects model

Figure B.4: Targeted Transfers



NOTE: Weights are from random-effects model



NOTE: Weights are from random-effects model