Introduction

- This document contains a rough set of notes to help readers to understand our rough cash spillovers model.
- This model is only intended to be a simple starting point to allow us to think about how much positive or negative spillover effects could matter, if they exist.
- The spreadsheet is here: https://docs.google.com/spreadsheets/d/1eCQbP56-7k9njEj0dCWWuZSBYf3G-LTJh8LgR42f_JA/edit#gid=515635846.
- As a short introduction to how GW models the direct effects of cash transfers: we assume that some proportion of the transfer is consumed immediately, the rest is invested, and the returns on that investment enable a higher level of consumption year on year, for a given number of years.
- Throughout our analysis, we focus only on spillover effects on consumption (as opposed to effects on prices, spillover effects on subjective well-being, etc.).

1. Best guess for spillover effect in GD's current program

See the "Weighted average spillover effect" tab.

- As a first step, we conduct a very simple meta-analysis to combine point estimates from five academic papers into a best guess for the across-village spillover effect of cash transfers on consumption.
- The five studies we focus on are: The new General Equilibrium study (GE18), Haushofer & Shapiro's studies: HS18, HS16, HRS15,[1] and the McIntosh & Zeitlin study (MZ18). These are the studies which look at the spillover effect of unconditional cash transfers on consumption in a sub-Saharan African country.
- We use point estimates for the spillover effect on consumption which are expressed as a % of consumption (typically baseline control-group consumption) (this is sometimes total consumption and sometimes consumption of non-durable goods specifically, we have so far treated the two the same). These are outlined in cells B3-B8.

[1] The spillover effects estimated in HRS15 are not directly comparable to the estimates in the other papers mentioned, as they estimate effects based on differences in treatment intensity across treated villages (averaged across both treated and control households). For the time being, we just use the point estimate for the effect of a mean increase in mean village wealth on consumption in that paper, as though it is comparable to the estimates in the other papers.

- We make several adjustments to the point estimates from the RCTs to make our guess as applicable as possible to GD's current program. Those adjustments are explained in the three steps (a), (b) and (c) below.
- In GD's current program: all households are treated in treatment villages, and the cash transfer = $1,085 in Kenya, $970 in Rwanda and $963 in Uganda (all nominal USD, not PPP-adjusted). We are therefore interested in our best guess for *across-village* spillover effects, even though most papers in the literature estimate effects *within* village.

a) Within-village spillover effect estimates are underestimates of the true effect if across-village spillover effects exist

- If across-village spillover effects exist, within-village estimates which compare control households in treatment villages against households in control villages will underestimate the absolute magnitude of the within-village spillover effect, as control villages are partially affected by the spillovers. I.e. the within-village spillover effect estimates suffer from attenuation bias (bias towards zero).
- We adjust for this in cells D3-D8. The size of this adjustment has a direct relationship to the size of another parameter in our model (the relative size of across compared to within-village spillover effects), as explained in the appendix (see end of this document).
- The one exception to this is the within-village spillover effect estimated in the GE study. The authors try to deal with the attenuation bias by controlling for sublocation saturation status in their within-village spillover regressions. If across-village spillover effects only operate within and not across sublocations, this perfectly controls for across-village spillovers and so deals with the problem. However, if across-village spillover effects occur across sublocation boundaries, there will still be some attenuation bias (likely of a smaller magnitude) since ineligible households in control villages experience spillovers from cash transfers made in other sublocations. To deal with this, we make an adjustment on third the size of the adjustment made to the other within-village spillover effect estimates in the literature.

b) Spillover effects estimated in the literature are based on smaller cash transfers than GD's program

- A larger cash transfer is likely to cause a larger spillover effect. We are unsure how much larger, but here we make an adjustment which assumes that the size of the spillover effect increases proportionally to the increase in the size of the transfer.
- We adjust for this in cells F3-F8, using the adjustment factors in cells E3-E8. E.g. HS18 mean transfer = $490 US nominal ($709 PPP); therefore adjustment factor is $1,085 (GD transfer in Kenya)/$490 = 2.2.

c) Best guess for across-village spillover effects based on within-village spillover effect estimates

- All papers estimate within-village spillover effect (the GE18 study also estimates across-village spillovers specifically). However, GD's current program treats all households in treatment villages, and so the relevant non-recipients for GD's program are all across-village. We therefore need to make inferences about the size of across-village spillover effects from the within-village spillover effect estimates.
- To do so, we assume that across-village spillover effects are some % of the size of within-village spillover effects in cell B11, which we use to scale the within-village treatment effects (in cells G3-G8).[2]
- We use the size of across- relative to within-village spillover effects estimated in the GE18 study to make an assumption for this value in cell B11.

Putting these estimates together in a meta-analysis

- Now that we have arrived at our best guesses for the GD-relevant (across-village) spillover effect using the results in each paper, we need to combine them across papers.
- We take the very simple approach of weighting each estimate by sample size (number of households in the regression from which each estimate derives). We do this in the "Meta-analysis weights" tab.
- This gives the final best guess for the across-village spillover effect of -0.48% in cell B15.

Things we have left out

This model is very rough, and there are several issues that we have not made adjustments for, as follows:

Best guess for spillover effect across RCTs:

- We do not account for the possibility that within-village spillover effects affect recipient households in treatment villages. For more discussion, see here.
- We do not adjust for differences in the proportion of households treated in RCTs compared to the GD program, or for differences in the spatial distribution of transfers in RCTs compared to the GD program. For more discussion, see here.
- We do not adjust for the eligibility of non-recipients in the RCTs compared to the GD program (e.g. only ineligible households are assigned as non-recipients in McIntosh & Zeitlin, as opposed to both eligibles and ineligibles in GD's program in non-recipient villages).

Meta-analysis weights:

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[2] Intuitively, across-village spillover effects on consumption are likely to be smaller than within-village spillover effects, since non-tradable goods prices will increase most in the locations where the cash transfer has been made.

- The HS18, HS16 and HRS15 studies are based on the same RCT and so are not independent. We have not decreased the weight placed on each of these three studies to deal with this.
- We do not adjust the weights for differences in methodological quality between the studies, or e.g. the extent of peer review.
- We have taken the simple approach of using sample size for the meta-analysis weights, but we could alternatively weight the studies based on the precision of the estimates (e.g. standard errors around the estimates).

2. Applying this best-guess spillover effect estimate to our CEA

See the "Cash" tab.

- The new rows (compared to our original cash model) are rows 28-49, and the -0.48% spillover effect enters in row 30.
- Row 32 models the change in ln(consumption) in the first year of the program as a result of the initial spillover effect which we assume is -0.48% of baseline consumption ($285.92). I.e. we assume that the estimates in the academic literature pertain to the immediate spillover effect in the first year (even though they are estimated at slightly different points in time ranging from ~1 month after the end of transfers to ~2 years after the end of transfers, for more discussion of the timing of the effect see here).
- We assume that there are long-term spillover effects on consumption. We assume that the timing and size of those long-term effects mirrors the direct effects of cash transfers on consumption for recipient households. I.e. lasts for the same length of time (row 16) as the direct effects, and that the relative size of long-term to immediate spillover effects is the same as the ratio of long-term to immediate direct effects on consumption in our model (that ratio is calculated in row 33).
- Row 36 calculates the present value of future changes in ln(consumption) through long-term spillover effects.
- Row 37 sums the immediate and long-term effects.
- We assume that for every one household that is treated by GD's program, there are seven non-recipient households that are affected by spillovers (row 38). We are quite uncertain about this parameter (e.g. it will depend upon the spatial distribution of GD's program, which we do not currently have much information on).
- The bottom-line effect of including spillovers in our model for cost-effectiveness of cash is in cell B49 (the difference in median cost-effectiveness excluding and including spillover effects).
- With the assumptions and framework that we have used so far, including spillover effects on consumption decreases the cost-effectiveness of cash transfers by ~8%.
- However, for all the reasons discussed in this post, we do not place much confidence in this model. Instead, in practise, we have applied a 15% negative discount to the cost-effectiveness of cash, for the reasons explained in this document.

Appendix - Supplementary notes

A1. Studies' within-village spillover effect estimates are underestimates of the true effect if across-village spillover effects exist

Two parameters, X and Y:

a) "Across-village spillover is X% as large as within-village spillover"
b) "True within-village spillover effect is a factor of Y times the within-village spillover effect estimated in the study"

Given some best guess for X, what is the value of Y?

Take a simple example:

- Suppose that $1,000 is given to 50% of households in village A, and no households are given the transfer in village B. The transfer has a within-village spillover effect which decreases consumption by W%, and an across-village spillover effect which decreases consumption by A%. Our study will compare the consumption of control households in village A to households in village B, and conclude that the spillover effect is (W% - A%).
- In that case:

  X = A/W
  Y = W/(W-A)

- Then:

  A = XW

  Therefore:

  Y = W/(W-A) = W/((1-X)W)
  = 1/(1-X)