02/11/15 - Sam, Denise, Kate, Joey

1. Plans and updates
    a. Denise: Masters / CEA / start a charity
    b. Joey&Kate:
        i. At least committed to research for charity entrepreneurship
        ii. ready to leave Charity Science
        iii. Going to a developing world country - India
        iv. Do mico-pilots
        v. 3 promising plans from May/June
2. Lots of benefits to starting effective charities
    a. no-one really doing this
    b. givewell thinks the odds of success are not astronomical
    c. the gap that is missing
3. Unanimous advice was to go spend time in the developing world.
4. Getting others to start charities too.
    a. the book idea
    b. the EA community
    c. Evidence Action
    d. Big charities - why GiveWell not give to big charities to do this?
        i. They tried talking to Unicef about vaccinations but put off by lack of transparency
        ii. (Should talk to Duncan Green - Oxfam)
5. Roles
    a. During research
        i. Advice or consulting type roles
    b. Motivating people to start charities
    c. Expect to generate plans for 3-5 good charities.
        i. So could start a charity
    d. Funding need - RCTs are expensive
6. will be a chraityentrepenurship blog
7. Prioritising GiveWell's ideas
    a. Metrics for choosing
        i. Chance of success Vs value of success
    b. Choosing a specific intervention
8. Running an organisation / charity
9. Values

    a. largely pluralistic
10. **Next steps**
    a. Joey and xio to do research in order to create new charities
    b. Sam to in short run agree to stay in touch and advice if he can

http://blog.givewell.org/2015/10/15/charities-wed-like-to-see/

Conversations prior to GiveWell's research

10/06/15 - Sam and Denise

1. GW are already doing this
    a. We should have a talk with Elie and getting a full picture
2. Funding research
3. What research do we want exactly
    a. health or all poverty
        i. No strong preference - want the best candidate
        ii. Probably broad covering all poverty interventions
    b. easy or easy-and-difficult interventions
        i. focus on easy
    c.

05/06/15 - Sam Denise Hauke

1. Next steps - an intern
    a. Do not have a concrete applicant
2. Keen on a good candidate even if that means waiting
3. GWWC
    a. recruitment skills
4. option
    a. Summer intern recruitment round
    b. Full recruitment process
    c. Sam's contacts
        i. run through recruitment process
    d. Charity science
        i. may have people
5. Funding any recruitment is an option
6. Logistics
    a. Desk space is available

        i. £1000 a month would cover desk space and £10 a day and housing
    b. Hauke could supervise
        i. Sam and Denise to give a very good idea to Hauke about what they are looking to do
        ii. Denise and Sam to remain involved
7. Research
    a. The idea is to generate a list
        i. interventions to work on and interventions to research further
        ii. maybe 60 interventions
        iii. not just for me and Denise
    b. See Hauke's post on the EA forum about promising interventions
    c. What kind of interventions
        i. Denise and Sam to discuss further
        ii. Global Poverty (but not just health)
8. Action points
    a. **Sam and Denise: To talk further about exactly what we expect out of the project.**
    b. **Hauke: Look at summer intern recruit rejects**
    c. **Sam: to provide Hauke with further candidates**
        i. From people in London
        ii. Joey and Xio double check
    d. **Hauke to look into the cost and benefit of recruitment**
        i. Benefit: Chance of finding someone better
        ii. Cost: What is the cost
    e. **Sam and Denise: To talk about funds**

TO do
Read: http://www.givewell.org/international/technical/programs

?/?/? – Sam, Denise, Elie

Meeting notes:

1. **Specific questions for Elie:**
    a. **GW's effort**: We were expecting a mediumly experienced intern to work full time on this for upto 6 months. At a best guess how much person-time will GW put into this? (? interns of ? experience for ? months).
        i. **Marginal returns**: If less than us, then any thoughts on the value of additional time?
    b. **Collaboration/duplication benefits**: If we go ahead and fund someone then would elie see this as a collaborative project or two

similar but independent projects? And how valuable does Elie think it is to have a [duplicator/collaborator]?

c. **Influence effects**: What kind of influence will it have on GW if we go ahead or do not go ahead with this project? (Eg. Do GW feel it would make them stop working on this and put time elsewhere?)
d. **Opportunity cost**: Perhaps it would be best we saved the funds and if we waited for GW to finish their research and then had a think about what we could add to it afterwards. What other options are there?
e. **Other**: Is there anything else Elie can say that we should take into account when considering the value of this?
f. **Overall**: What is Elie's view on if funding our own research into this is better or worse than funding AMF/ GW / CEA / etc?

Questions above are based on Sam's thoughts:

2. GW is starting an intern on their own version of this project with a low priority. We would like to get a better understanding of if it is valuable for us to put time and money into making this project happen.
3. We are keen to know if funding research into this is better or worse than funding AMF/ GW / CEA / etc?
4. Measuring the value of this project is:
    a. We were expecting a mediumly experienced intern to work full time on this for 6 months. Let's call the value of such an project = 100
    b. Roughly if GW would do y% (wherey<100) as much effort then the value we will create will be:
        i. 100*(1-y%)*(some measure of diminishing marginal returns) + collaboration/duplication benefits ± influence effects – opportunity costs ± other
        ii. some measure of diminishing marginal returns. so if GW were to fund someone for 80% as much time assume they would create like 95% of the value to be created. Best guess ≈ (1-(y%^2))
    c. Collaboration/duplication benefits. If GW would do this:
        i. if GW not work with us then value created is 100 + value of duplication. What is value of duplication? I suspect it is very low. Best guess ≈ 3%
        ii. if GW not work with us then value created is 100 + value of collaboration. What is value of collaboration? I suspect it is reasonable. Best guess ≈ 35%
    d. Influence effects:
        i. What kind of influence will it have on GW if we go ahead or do not go ahead with this project? For example if us funding it means that GW stops working on it is that likely and is that a good thing or bad thing? GW could work on other stuff but also GW working on this is very high value. Best guess ≈ 0%
    e. Opportunity cost
        i. For example we could save the funds and then wait for GW to finish and fund someone to add value to the project if needed. Best guess ≈ 70%

        f. Other. What other factors are there? Best guess ≈ 0%
5. Specific questions that Elie may be able to answer:
    a.

21/05/15 – Sam, Denise

1. It is a good idea to talk to GiveWell
    a. This will help us work out exactly what we want out of this project.
    b. Should be a well prepared relatively short conversation.
    c. Sam cannot do Tues 26 May, Mon 8 June, Tues 16 June,
    d. **Denise to email**
2. Other next steps **-** working with CEA or Charity Science
    a. **Sam to email to keep them updated**
3. Talk through comments on EA post (see below)

Comments from EA Forum post

http://effective-altruism.com/ea/ib/request_for_feedback_researching_global_poverty/
Summary of comments from post are in black
Sam and Denise conclusions are in green

1. *Ben Todd*
    a. Good idea overall
    b. Distinguish interventions that need RCTs Vs do not need RCTs
        i. When doing research we should have a quantifiable metric on how much evidence / RCTs are needed
        ii. Are RCT options really worth it?
            1. **Add to agenda for GW**
            2. Decide after have collated data on all interventions
    c. Difficult to find interventions
        i. talking to experts seems valuable
    d. Difficult to run a charity
        i. could get someone else to run charity
            1. funding from GW and EAV and us?
2. *Peter McIntyre (and Ben Todd)*
    a. Not do RCT
        i. Arg is that GW decided not a valuable thing to fund
            1. **Talk to GW about at some point**
3. *Owen Cotton Barratt*
    a. Will take time
    b. Already some of this happening
    c. Distinguish
        i. (1) The interventions with the highest potential aren't the ones which get properly evaluated by RCTs (or there isn't enough of this).
        ii. (2) The interventions with very good evidence from RCTs aren't implemented by charities or aren't scaled up appropriately.
            1. **conclusion to be reached further down line - this is something to bare in mind when doing the project**

    d. GW may see spaces where interventions can be scaled up
4. *Tom Stocker*
    a. Overall a good idea and is a value to pulling things in one place
5. *David Nash*
    a. Replicating existing interventions in different ways or locations may be high value
    b. Geography is important
        i. **conclusion to be reached further down line - this is something to bare in mind when doing the project**
6. *Nekoinentr (and Joey)*
    a. starting a charity may be time consuming
        i. 1-3 years with 1-2 employees (~2000-12000 hours)
7. *Ryan*
    a. I'd rather see a charity get recommended by the Open Philanthropy Project than by GiveWell classic.
        i. **Ask why?**

16/04/15 – Sam, Hauke, Denise

1. Is effective is valuable
    a. As can find a charity that is better than them
    b. Not as effective as if consider the couter-factual of funding effective charities that already exist
    c. Definitely interventions better than AMF
        i. Some of Gws analyses are superficial
        ii. Maybe a bit surprised
    d. Are basically charities in all interventions
2. AMF (and SCI)
    a. Not expanding as much as they could
    b. How difficult to spend money is u shaped.
        i. If little money easy to spend it all
        ii. If get more it becomes hard as need to find lots of distribution partners
        iii. If get more easier can get more leverage and can work with states
    c. Can send reports on funding gaps
3. Prioritisation research is never really a waste of money
4. Practical issues
    a. funding
    b. £1000 a month
    c. Timing
        i. No hurry

Sam, Denise
1. [LINK]

What should be done

1. Make a list of other organisations to get in contact with
2. Make a list of questions to ask
3. Interview / Skype with other organisations
    a. Generate a list of possible very high impact charities to start
    b. Grow the list of organisations to talk to
    c. Generate a list of resources to use to, to research interventions
4. Do more in depth of each intervention for example literature reviews and further conversations with the people who suggested each intervention
5. Generate a list of interventions to start (and interventions considered and now dismissed)
6. Choose an intervention
7. Find funding to start a charity doing that intervention
8. Organise a RCT to test intervention

GWWC
GW
Charity Science
Sam to have an intern in London
Crowd source

Sam to talk to GWWC

THE PROCESS FOR INITIAL RESEARCH
29/03/25 - SAM, DENISE, JOEY AND XIO

1. **Why do this:**
    a. Starting effective charities is a good thing to do according to many EAs (GiveWell think it is good, 80K have recommended).
    b. **But why not just work for Evidence Action instead**
        i. There is still room for multiple orgs in this area
        ii. Evidence Action would not do anything weird
        iii. The counterfactual sucks
            1. Evidence Action could just hire more people if they wanted.
            2. Eg they may just do less research themselves if you worked for them
                a. [Aside what if gave them earmarked funding for research]
2. **Start with a research project**
    a. Joey's best guess is this would take 6 month of work
    b. This research project should initially be based on talking to people and then looking in more depth at the various options
    c. Eventually want:

        i. To find and compile a list of ideas - from which can pick a charity to start
            1. Should be public
            2. Who recommended each one
        ii. Also want to have a list of considered no-go ideas and why they have been ruled out
            1. Want to use this research to rule out options too
        iii. Perhaps the list can also start comparing between the options

3. **Who to talk to:**
    a. GiveWell.
        i. They are approachable. Email Elie (elie@givewell.org)
    b. CEA
        i. GWWC
        ii. GPP
    c. Evidence Action
        i. They can be hard to get in contact with.
        ii. Ask GiveWell for an introduction
    d. JPAL
    e. IPA
    f. Centre for Global Development
    g. DCP
    h. Whoever created this: http://www.beguide.org/
        i. GWWC know
    i. (Esther Duflo)
    j. Development for Innovation Ventures
    k. Experts in their field
4. **What to ask:**
    a. Explain general approach and ask: Do they think this is a good approach? Why?
    b. What charities would be good to start?
        i. what interventions look promising? Why? etc?
        ii. NOTE: Be wary of people having pet ideas.
    c. Who else should we talk to about this?
        i. Can you provide an introduction?
    d. Can we
        i. come back and speak further when we have a more specific idea?
        ii. make the information you have shared with us public?

5. The **further initial research**
    a. Look into happiness amounts. May find some underdeveloped ways of improving welfare in welfare economics research
        i. Eg. See level mean happiness map at http://worlddatabaseofhappiness.eur.nl/hap_nat/nat_fp.php?mode=8
    b. Use Gap Minder
        i. Look for areas of the world where have most things needed to be happy but something is missing

    c. For each intervention suggested talk to experts in their field
        i. Ask: Why has this not happened yet?

6. The second step would be to **carry out an RCT** on the chosen area
    a. Unless a very well proven area
    b. This would likely need funding
    c. This generates useful additional evidence
    d. This sets you up to fail early

7. **General running a charity advice**
    a. Transparency FTW
    b. Set self up to fail
        i. Check to see if idea is theoretically falsifiable
        ii. Joey recommends being super empirical and pessimistic about ideas working well.
    c. If **Outsourcing**
        i. Want at least some oversight / some well known EA keeping an eye on project

DEFINING THE PROBLEM & FIRST STEPS

Alex & Hauke email exchange on the value of this project

HAUKE CRITICISMS OF PROJECT IDEA

------ Forwarded Message --------
From: Hauke Hillebrandt <hauke.hillebrandt@gmail.com>
Date: 07/04/2015 17:16:18
Subject: Re: Re: Super effective new charities
To: Samuel Hilton <weeatquince@gmail.com>
Cc: Jonathan Courtney <jonathan.e.courtney@gmail.com>, Denise Melchin <denisemelchin@gmail.com>, Michelle Hutchinson <michelle.hutchinson@givingwhatwecan.org>, Pascal Zimmer <zimmer.pascal@gmail.com>
Hi all,

thanks so much clarifying! It's great that you're all so motivated about this!

After reading the document however, I think your motivation to start another global poverty charity is based on a false premise (this is not to say that you shouldn't do it! just wanted to clarify and give some feedback on the proposal). You write:

Over-funding in 5 years time for EA global poverty projects
No charity better than GiveDirectly in 5 years and this is off-putting to donors

It's highly unlikely that effective global health charity will be overfunded within so little time.

There's this meme going around the EA that the Gates foundation could just fund every EA project, but they really couldn't by a long stretch. The room for more funding projections that you read on Givewell, which are always in the low millions, are based on what they could absorb to fund already existing projects, that they don't ever have much money sitting around, to give their limited funds to other organisations to diversity risks, and so in order to keep them on their toes to not rely on Givewell and apply for other grants. It does not account however for potential scale ups of their operations. So for instance, they estimate that SCI's funding gap is in the low millions this year, however, they could easily intensify their efforts of existing projects and expand to other countries and have a very similar impact. They have in the past successfully absorbed >$30 mil Gates grants and if you were to just hand them 10 million dollar, they would 'Do another country'.
You can read more about the overall funding gap of schistosomiasis as an example over the next years 10s of millions every year in my report here:

https://www.givingwhatwecan.org/blog/2015-03-31/charity-update-ii-schistosomiasis-control-initiative-sci

And this is only for schistosomiasis. Take a look at the WHO report which I cite in this report, which outlines the funding gap for other neglected tropical diseases (Note that these big funding gaps already take into account that lower-middle income will take cover most of the costs and just need technical assistance, but lower income countries cannot fund mass drug administrations, bednet distributions etc themselves, because they're too poor and it is politically not feasible. Even taking this and increasing western aid budgets into account there's a big funding gap and nobody knows where the money need to achieve somewhat universal coverage from NTDs is supposed to come from.) It's similar for the Against Malaria foundation, which could hand out many more bednets than they currently do. Then you could distribute many more condoms, for which organisations already exists, etc etc. And then take a look at the recent DCP report on 'first-level' hospitals, which do very basic things. They actually very cost-effective too. So Medicines Sans Frontiers, could help with such hospitals in developing countries.

"A subsequent study in a pure trauma first-level hospital in Cambodia found a cost-effectiveness of US$78 per DALY averted (Gosselin and Heitto 2008). Another study comparing two Médecins Sans Frontières trauma hospitals in Haiti and Nigeria found a cost-effectiveness ratio of US$223 and US$172 per DALY averted, respectively, with almost all the difference attributable to pay scales and employee-benefit schemes (Gosselin, Maldonado, and Elder 2010).
http://www.dcp-3.org/sites/default/files/chapters/DCP3_Essential%20Surgery_Ch3.pdf"

This is probably an underestimate, but also not very far off (hospitals are pretty good at estimating DALYs averted, because you come in with a problem, say appendicitis, you know the associated burden, you fix it, and you know how much it costs). In comparison, Givewell estimates that combination deworming is $177 per DALY averted.

So in sum, I think the actual room for funding for very effective health interventions (<$1000 per DALY averted) is at least in the hundreds of billions per year (I could do some more exact calculations of this). But think about how much it would cost to set up at least rudimentary hospitals everywhere. And I have the feeling that there are already very many good, transparent organisations that we don't currently recommend that fall in this category, because SCI and AMF have room for more funding.

So just like with EA job considerations, where you should ask a nonprofit whether they'd rather have you work for them or donate to them and most would say 'donate to me', because they have lack of resources and not talent, the same applies to starting a charity: if you start a charity that averts a DALY for 200 dollars, through some clever intervention, this would just mean that EAs will fund the 210 dollar per DALY averted charity a bit further up the list a bit later. So, I do believe it might be more valuable, for the 'entrepreneurial EA' to set up a business that makes a lot of money, and then donate the money to already existing charities.

Having said this, I think it might be good expected value to go ahead and trying to find an intervention that is super effective for which no charity currently exists. In a way this is what the DCP is already working on. I'm also definitely up for skyping about this on Thursday afternoon. Let me know!

Hauke :)

ALEX GB RESPONSE

---------- Forwarded message ----------
From: **Alex Gordon-Brown** <alexander.gb.gordy@googlemail.com>
Date: 2015-04-07 19:28 GMT+02:00
Subject: Re: Super effective new charities
To: Denise Melchin <denisemelchin@gmail.com>

Thanks.

"There's this meme going around the EA that the Gates foundation could just fund every EA project, but they really couldn't by a long stretch. The room for more funding projections that you read on Givewell, which are always in the low millions, are based on what they could absorb to fund already existing projects, that they don't ever have much money sitting around, to give their limited funds to other organisations to diversity risks, and so in order to keep them on their toes to not rely on Givewell and apply for other grants. It does not account however for potential scale ups of their operations."

I think this is roughly true as far as it goes, with the possible exception of AMF where Givewell of course considered them to not have room for more funding last year due to the issues they were having using the flood of money they were receiving. Also, it might be worth clarifying that it's the upcoming growth of EA funding (a near-certainty for some magnitude, with an outside chance of being a massive uptick) which makes this a concern, rather than the possibility for intervention by Gates/Good Ventures, which I agree is unlikely.

" So for instance, they estimate that SCI's funding gap is in the low millions this year, however, they could easily intensify their efforts of existing projects and expand to other countries and have a very similar impact. They have in the past successfully absorbed >$30 mil Gates grants and if you were to just hand them 10 million dollar, they would 'Do another country'."

You can always 'do another country'. You can't always do it successfully; my understanding was that the literature on what happens when interventions are ported out of their original successful environments is not optimistic. I didn't know about SCI absorbing such a large sum in the past; it would be interesting to know whether Givewell was aware of this and if/how they were accounting for it.

"Take a look at the WHO report which I cite in this report, which outlines the funding gap for other neglected tropical diseases (Note that these big funding gaps already take into account that lower-middle income will take cover most of the costs and just need technical assistance, but lower income countries cannot fund mass drug administrations, bednet distributions etc themselves, because they're too poor and it is politically not feasible. Even taking this and increasing western aid budgets into account there's a big funding gap and nobody knows where the money need to achieve somewhat universal coverage from NTDs is supposed to come from.)"

Are the other NTDs as tractable/efficient to treat? I literally don't know; but obviously we shouldn't assume that they are just as good as deworming; we (by which I mean Givewell) selected deworming because it appeared to be the best!

"It's similar for the Against Malaria foundation, which could hand out many more bednets than they currently do."

AMF is still sitting on a big pile of cash. If they received significantly more funding, their bottleneck would once again become ability to find distributions. Handing out bednets in an efficient manner, i.e. with follow-up, corruption/theft-protection, high coverage, etc., is not quite as simple as he seems to believe. Of course, it rather depends what you define as 'efficient'. Which brings me to what I suspect the true disagreement is:

"So in sum, I think the actual room for funding for very effective health interventions (<$1000 per DALY averted) is at least in the hundreds of billions per year (I could do some more exact calculations of this). But think about how much it would cost to set up at least rudimentary hospitals everywhere."

$1000 per DALY is literally an order of magnitude more than the current top charities; the gold standard so far has been ~$100 per DALY, which is roughly where AMF is. I think I agree with him that numbers quickly run into the tens or hundreds of billions if you're willing to accept such a large loss of efficiency.

The idea of funding developing world hospitals is interesting based on those preliminary numbers and would indeed be a plausible candidate for a massive money sink, but presumably is exactly the type of intervention your intern was hopefully going to identify? The very fact that these things probably exist and we don't currently have EA-aligned charities for them is kinda the whole point. If we instead identify a bunch of already-existing transparent accountable charities in good areas, that seems even more awesome, though I have some skepticism about the ability to actually do that given it's basically Givewell's entire focus. It might be worth talking to Givewell about whether they do indeed feel like there's a glut of plausible charities just behind AMF/SCI (say aBround $200 per DALY) that they currently don't bother with. That wasn't my understanding, but it seems to be Hauke's.

"So, I do believe it might be more valuable, for the 'entrepreneurial EA' to set up a business that makes a lot of money, and then donate the money to already existing charities."

Your comparative advantage in setting up a charity as an EA is your focus on results, which sets you apart from most of the sector. Said focus is not an advantage in the business world because there everyone focuses on results (i.e. profit). So there's no direct reason that EAs capable of starting a very good charity would also be able to run a very good business, though it probably is true that (like Rob Mather) they would at least be competent in business. But I think Rob was right to switch into his currrent job as AMF CEO because of the comparative advantage argument above; if it's right for him it is likely right for at least some others.

9/03/15 - SAM AND DENISE

1. Brainstorm a list of questions that we want answered
2. Order list
3. Look for answers by asking others or doing our own research

4. Send email to several people to ask where they see problems (global poverty) that might be overlooked

BROAD PROBLEM

5. Suffering in the world and not enough happiness

SO

6. Starting charity (rather than not starting one)
    a. Start a charity that does direct good (rather than a meta-charity)
        i. A charity that tackles global poverty (rather than another issue)

WHY THIS

7. You think this is the most effective thing to be doing based on some sort of utility calculation
8. There is no way to decide for sure the most effective action and this seems a plausible candidate

SPECIFIC PROBLEM

9. Suggestions
    a. Over-funding in 5 years time for EA global poverty projects
    b. No charity better than GiveDirectly in 5 years and this is off-putting to donors
    c. There is a possible missed opportunity to do vast amounts of good
10. There is a possible missed opportunity to do vast amounts of good

KEY QUESTIONS

11. What is the opportunity cost
12. What is the best-guess likelihood of all the factors that you would need to no to do utility calculation of this project
    a. Chance of creating something so that there is at some point something better than what would exist
        i. Do GW and GWWC have ideas for charities that do not yet exists
        ii. Where do EAs see the future effectiveness of charities
        iii. Total EA funding

    b. How much better it would be

13. Overfunding for highly effective global poverty charities within years so that most effective charity is gd

14. Expand evidence action

15. What should be outsourced

16. Partner up
    a) create an EAV sub-project

b) do research in a 'branded way'

17. approach charities and convince them to be more effective

17. Looking for opportunity

    a. opportunities suggested by GW, GWWC etc
        i. But in practice may be less good than expected
            1. A way of telling this would to be to look to see if these things have been tried and what problems charities trying these had faced
    b. Maybe opportunities that no one has ever researched etc
        i. Why has no one thought of them
            1. Technology dependent
            2. Disgusting (physically / morally)
                a. Adding medicine into water supplies
            3. Innovative - new ideas - contrarian
            4. Relies on cutting edge top of field academic research
            5. requires more heterogenous memes people usually aren't exposed to (e.g a technological solution to a problem only sociologists are able to see)
        ii. Ask people with good knowledge of development
        iii. => contact contrarian people?
18. How long did it take? (Ask AMF and Co.) Also, ask them for advice.
19. What are the limiting factors AMF & Co experience?
20. What has been done already in this area?

NEXT STEPS

21. What has been done in the EA comunnity and elsewhere to look into this
    a. Joey Xio
    b. GWWC
    c. GW
    d. Holden's contacts
    e. Evidence Action
    f. Poverty action lab
22. Engage more with EAV
    a. Letting them know we would fund someone to do this
    b. Considering starting some sort of EAV sub-project
23. Ask specific people if they thing this would be a good idea
    a. Write down a thought process
    b. Specific questions
    c. Tailor the message to those people

24. Research
    a. Research in more depth the opportunities that people have already thought of
    b. Research the areas and opportunities that have not been thought of.

20/08/14 - SAMS OLD NOTES

1. INITIAL RESEARCH TO CREATE A LIST OF HIGHLY EFFECTIVE INTERVENTIONS

Things current top charities do:

1. Deworming in schools
2. Distribute malaria nets
3. Direct cash transfers
4. Adding micro-nutrients to foods
5. Working with governments to ensure that the above is done

Sources:
- http://www.givewell.org/charities/top-charities
- http://www.givingwhatwecan.org/top-charities

Other highly effective interventions according to Giving What We Can (GWWC):

1. Health Education (like Development Media International and Medical Aid Films)
    1. Hygiene education - handwashing with soap is really effective
2. Increasing labour mobility in developing world
3. Preventing people dying from inhaling soot from fires
4. Preventing / pushing back child marriages - Eg. with financial incentives

5. Microsavings - as opposed to Microloans ()

Sources
- 1.a recomeded by GWWC's Director in conversation
- Rest of the suggestions are from https://www.facebook.com/groups/602287509820703/permalink/682336405149146/

Sam's ideas
1. Research - monitoring and evaluation for existing interventions

Other highly effective interventions according to GiveWell (GW):

1. effective salt iodization charity? (mentioned here)

????

Other highly effective interventions according to Evidence Action:

????

Research needs to be done here
- contacting GiveWell and Evidence Action and asking them for suggestions
- reading the GiveWell website to see their past research

Note: GiveWell's view of Evidence Action and of other similar organisations can be found at: http://files.givewell.org/files/conversations/Zwane%202-18-14%20(public)%20.pdf