Summary

Developing-world direct aid is the area in which we have the most tangible idea of how we're going to add value, and the most confidence that we'll be able to produce high-quality, useful research for donors. It may be the area least likely to "expand our reach" and draw in new donors, since we have already identified two strong organizations (and would better draw in new donors by exploring areas we've done no work on).

Global warming is a cause with strong and broad appeal to donors, and we are highly confident that we could add value by getting specifics on what different organizations are doing, as well as making it clear *how much* support there is (in terms of how broad the consensus is, if nothing else) for the many different claims advanced about possible problems and solutions.

Disease research is a cause with broad appeal that could bring in many new donors, and we find the cause very philosophically compelling. We have ideas for how to add value, but we are not highly confident that we'll be able to produce much.

We do not plan on further researching **developed-world direct aid** in the near future. We believe that we have already made significant progress and identified standout organizations in this cause, and we see more potential to improve donors' options in the three areas listed above.

We lean toward developing-world direct aid as the best fit with both our interests and our abilities, despite the disadvantage of continuing with an area we've already done work on.

Developing-world direct aid

What we would research

Taking a more "top-down" approach than last year – identifying high-priority problems and interventions before contacting specific charities – would enable us to find strong programs *within and across* organizations. For 2007, we recommended only those organizations that we could form a *full* picture of, but we don't believe this is necessary – many of the organizations we investigated are better thought of as "alliances" of programs (or even as grantmakers in their own right), and an organization whose top-level organization is weak may still have programs that are extremely strong.

We would begin our research by use existing academic literature to get as full a picture as possible of what sorts of lives people live in different parts of the world (broadly) and which regions suffer from which problems (specifically). This sort of broad, literature-based overview would allow us to:

- Give donors a picture of *what sorts of lives* they're helping to enable – a picture that could potentially make developing-world aid much more tangible and appealing to them.
- Identify parts of the world where people both *need* help and are *well positioned to benefit* from it. For example, when choosing between malaria programs in different areas, it isn't just relevant which area has higher malaria prevalence; it also matters which area allows higher quality of life *apart from* malaria (on dimensions such as hygiene and education, for example), such that reductions in malaria-related deaths translate more directly into increases in high-quality, high-opportunity lives.
- Gain a better understanding of how different problems in the developing world connect to each other (for example, the contribution of malnutrition to vulnerability to malaria, pneumonia, etc.), in order to better understand the full effects of a program targeted at a particular problem.

We would investigate the following specific interventions, which we have preliminary reason to believe are extremely promising:

- **Vaccination programs**, particularly those of the Measles Initiative and the GAVI Alliance. Preliminary evidence suggests that these programs could be significantly more cost-effective (and scalable) ways to save lives than those of our current strongest applicants.
- **Programs focusing on neglected tropical diseases (NTDs)**, which cause extreme debilitation including severe skin disease and permanent blindness and can be treated relatively simply and cheaply, but generally receive little attention from donors and the media. Preliminary evidence suggests that these programs could be significantly more cost-effective (and scalable) ways to bring about significant life change than those of our current strongest applicants.
- **Malnutrition-centered programs.** We believe that malnutrition is extremely widespread in the developing world, and is strongly connected to other health and economic problems, but that many food aid programs could be doing more harm than good. A good malnutrition program could make enormous and highly cost-effective differences in quality of life.
- **Microfinance and economic empowerment programs.** Intuitively (and according to very limited and preliminary evidence), we believe that microfinance programs may be uniquely cost-effective ways of accomplishing a goal that our current recommended charities do not: helping people escape poverty and permanently raise their standard of living. Our initial research focused on large international organizations, but we now believe there is more potential – for this particular cause – in smaller organizations with a more direct relationship to their population served.
- **Programs focused specifically on empowering women.** Women in the developing world face unique problems related to social oppression. We believe that charities may be able to help in a variety of ways, from corrective surgery for obstetric fistula (helping women gain re-entrance into society) to women-centered economic empowerment programs (helping them go beyond culturally imposed

limitations). Research on these issues could reach a set of donors that our current developing-world research does not.

- **Programs focused on fighting child slavery and sex slavery.** We know little about the role of nonprofits in fighting child slavery and sex slavery, but if we can find great organizations accomplishing this goal, they could appeal emotionally to many donors who are not compelled by our health-centered causes.
- **Programs facilitating adoption.** We know little about the role of nonprofits in facilitating adoption, but if we can find great organizations accomplishing this goal, we can accomplish truly drastic positive life change: helping abandoned and extremely disadvantaged children to find well-supported lives of full opportunity in the U.S.
- **Comprehensive aid programs.** We will be sure to seek out programs, such as the Millennium Villages Project (which we were not able to assess in 2007), that take a *by-region* rather than *by-problem* approach; we believe such programs are promising in terms of their potential economies of scale (addressing as many problems as possible after having dealt with the fixed cost of setting up offices) and in terms of their ability to adjust quickly to changing conditions.

Advantages

- **We have a very clear sense of what we would research.** We believe it is almost certain that we would find interesting, useful information for donors, and that this cause will provide a good illustration of the general potential of research to inform giving.
- **We are confident that we would be able to find many excellent options for donors,** in addition to the two we've already identified.
- **We believe that the people most interested in supporting GiveWell also tend to be very interested in developing-world aid.** It was the most popular cause according to our online survey, and is popular with the major contacts we've spoken with.
- **We would guess that developing-world aid represents the best overall opportunity for an individual donor with a global humanitarian orientation (our target market).** If this is true, even moderate improvements on what we already have would do more good for our donors than identifying the "best of an inferior bunch" in other causes.

Disadvantages

- We've already done a good deal of work in this area and identified two excellent organizations. **More research in this cause would probably not bring as many *new* donors** as researching other causes that appeal to entirely different sets of people.
- Related to the above point: **bringing in new donors is likely worth more** – in terms of the difference between what donors can accomplish with our research and what they can accomplish without our research – than improving the options of people who can already benefit from our research.

- The fact that there is so much research to do could end up meaning that this cause overwhelms us; it may be easier to get to the point of "knowing what there is to know" with other causes.

Global warming

What we would research

We would go through the large body of academic literature on global warming, examining the possible consequences of global warming and the amount of change in CO_2 reductions change necessary to mitigate it. **We would focus on distinguishing between arguments that have *isolated, partial, or full* academic consensus behind them**, to give a full picture of where experts' opinions do and not differ, rather than simply another set of projections. To our knowledge, we would be the first to cover this issue in this way.

We would then look to get a sense – likely through economic literature – of how much reduction in carbon emissions could reasonably expected from **citizen-focused measures (encouraging carpooling, reduced energy use, etc.)** and **policy-focused measures (restricting and/or taxing carbon emissions).** This "top-down" approach might lead us to favored solutions, and we could specifically seek out organizations (of any kind) pursuing these.

We would use our grant application process to collect detailed information from major organizations about how they use donated funds, and provide this information along with what we know about the likely effectiveness of their activities.

Advantages

- **The large body of academic literature means that we would likely be able to add significant value**, without relying on organizations' own abilities to make their case.
- **This is a relatively popular cause** (2nd-most popular in our survey) that could draw in a large set of donors uninterested in our currently covered causes.

Disadvantages

- **We are not confident that we'd be able to find excellent options for donors.** We find it plausible that the only good way to slow global warming is through policy, and we don't have a good sense of how effective donor dollars can be in pursuing policy change. We also know little about the magnitude and importance of the risks posed by global warming.

Disease research

What we would research

We would provide estimates of the number and severity of cases of each major disease, helping donors to get a sense of relative importance.

We would then target what we saw as the most important diseases, using our grant application process to collect **specific information from major organizations** on:

- **What specific goals they're pursuing:** what diseases they target, and the extent to which they're focused on finding a cure vs. improving treatment.
- **What role money plays in their activities**. We would be more focused on this than we have been in our past research, looking for specifics on what equipment they would buy, whom they would hire, etc., with an increase in funds.
- **Credentials of top researchers.**

Advantages

- This cause could **draw in a new set of donors** not interested in our current causes.
- This cause has **high upside** in that the basic goal appeals a great deal to us and would likely appeal to just about anyone. We don't believe that averting future disasters or helping extremely unfortunate people in limited ways is as broadly or as strongly appealing as helping otherwise fortunate people to live full lives.

Disadvantages

- **We think there is a strong chance that we would fail to improve donors' options –** that we would conclude that the basic approach of funding large numbers of small projects, as large coalitions likely do, is the best one and that we have little insight to add.
- We would have to strike a careful balance between focusing on diseases we found most important and focusing on diseases likely to draw in many donors.