This is an edited version of a document that was shared with the Board. It has been redacted, in alignment with the guidelines on our [Approach to Transparency page](). We do not indicate each redacted item. However, we may indicate specific places where redactions were made if they improve the readability of the document (for example, clarifying that a link has been made confidential, or explaining the jump from one topic to another) or may make minor clarifying edits.

Board Review Questions

December 15, 2020 Board Meeting

Attachment A: Update on GiveWell's major goals, progress, and plans

Section: Summary → Our goals

1. *GiveWell (GW) said: goal 2 = Find additional cost-effective giving opportunities.*

   Holden asks: At some point I wonder whether it would be good to broaden this a bit to include things like "helping donors time their giving to have more impact." Not pressing/crucial.

   GW response:
   It's been a long time since we've reconsidered our mission statement and this is another nudge to do that.

   That may mean incorporating timing of giving. We're also considering revising our mission statement to better reflect the work that outreach is doing to raise additional funds or to be framed in a way that better serves our marketing efforts.

2. *GW said: Goal 3 = Influence institutional funders of global health and development.*

   Norma asks: Have you considered making it a goal to influence very high net worth individuals who share many GiveWell values, but might not donate to Givewell or top charities? My sense is that there's a substantial number of them who want higher risk opportunities than Givewell currently recommends, and/or to feel they have a personalized opportunity. My sense is that some of these individuals are substantially easier to influence than longstanding institutional funders. To be clear, I don't think this should be in the top three goals now.

   GW response:
   We currently believe that there are still a lot of very large donors who will give to GiveWell-recommended opportunities if we can reach them and build relationships with them. As a result, we have not yet meaningfully explored trying to influence very large donors to give to *non*-GiveWell-recommended opportunities.

We have seen that sharing specific giving opportunities can be a good way to connect with large donors, and so we've used that approach to fill some GiveWell-recommended funding gaps. (The distinction between a GiveWell-recommended and a non-GiveWell-recommended opportunity is that in the absence of specific very large donors giving to a GiveWell-recommended opportunity, we would look to make that grant via Open Philanthropy or another source of funds. We've been explicit with donors about the fact that their giving is not necessary for the project to be funded.)

We also believe that working with donors on opportunities that aren't GiveWell-recommended may be a good way to build relationships that eventually lead them to support GiveWell-recommended opportunities. In several cases we've taken this approach, most notably this project we did with a donor in early 2019 to identify giving opportunities in Southeast Asia.

In order to consider prioritizing influencing very large donors giving to non-GiveWell-recommended opportunities, we'd have to believe that:

- We could provide input that would materially improve the impact their giving had.
- The improvement in their giving would be worth the cost (i.e., staff time) that would go into advising them.
- The above two bullets would lead to greater impact than what we could accomplish by prioritizing GiveWell-recommended opportunities.

Section: What are our major goals? What progress have we made, and what will we do next? → Increasing money moved → Where are we now?

3. *GW said: We are on track to meet this goal [moving $100 million excluding Open Philanthropy (OP) by 2022]; we may reach $100 million moved excluding OP in 2021. We have reached this point through a combination of organic growth and the work of our relatively new outreach team.*

Holden asks: I'd find it helpful to see a quantitative breakdown here:

- What was this figure in 2019 (I see #'s on the website but am not sure what you're using) and 2020 (est)?
- What has accounted for the growth?
- Where are you expecting the rest of the growth to come from?

GW response:
Our 2020 estimate, **$195 million**, is a very rough estimate:

- **Open Philanthropy is giving slightly more than $100 million in 2020**.

- Other donors gave $83.3 million in 2019**.** GiveWell's money moved was $43.4m in 2015, so we've grown by ~$40m over the last four years. We extrapolate that trend and forecast ~$10m in growth in 2020. That leads to an expected rough total of **$93.3 million in 2020 from non-Open Philanthropy donors.**

We just published our 2019 metrics report here. In 2019, our headline money moved was **$152 million.**

- **In 2019, Open Philanthropy giving was approximately $68 million. Open Philanthropy gave $54.7 million to our recommended charities, and an additional $14 million given for Incubation Grants was almost entirely funded by Open Philanthropy.**
- **$83.3 million was donated by other donors.**

We don't know what's driving our non-Open Philanthropy growth. Our impression is that the vast majority of the growth is organic, i.e., not directly driven by outreach. That's the case for the largest single donations we know about. We haven't analyzed this carefully to date. One of our top priorities for outreach in early 2021 is to better understand the drivers of our money moved to date. This exercise will inform our overall plan, including which channels we pursue and the relative investment we make in different approaches (e.g., marketing vs. development).

Section: What are our major goals? What progress have we made, and what will we do next? → Increasing money moved → How are we working towards this goal?

4. *GW said: Going into 2021, we have several lightly-held hypotheses [including]...*
   - *Maintaining and enhancing GiveWell's long-term brand... is likely the most important driver for attracting new large dollar donors.*

Tim says: I don't understand the meaning of "long-term" here. Is there a different brand or view that donors have in the short-term? Or other features of GiveWell that we are currently trying to emphasize?

GW response: We didn't intend to imply different short-term and long-term brands, so shouldn't have used the word "long-term" here. What we mean is simply that brand takes time to build and that some activities or actions (deep commitment to truth-seeking and transparency in our research) build our brand over time but may not be immediately visible or brand-beneficial in the short term. So to clarify, we are not saying that there is a different brand in the short-term vs. long-term.

5. *GW said: Going into 2021, we have several lightly-held hypotheses [including]...*

- *Long-term brand enhancing actions (e.g., high-profile earned media coverage; wide dissemination of GiveWell intellectual outputs; books like Peter Singer's, Will MacAskill's, and Larissa MacFarquhar's) take time to develop; and the benefits to these actions often largely accrue several years down the line. A focus on long-term brand and public profile enhancing actions (e.g., thought leadership) is likely to be the most meaningful new area of exploration for outreach.*

Tim says: I think this sets up the wrong contrast. There are brand enhancing actions that take a long time (and these are good examples) and can exert an influence over a long period of time. But there are also possible actions that enhance the brand in a shorter amount of time, and a series of those could have similar effects. And one brand destroying action can wipe out lots of actions that take a long time.

GW response:
We agree. There are brand-enhancing actions that can exert influence in a shorter period of time (e.g., positioning campaign or advertising, if done well) and other actions that take longer to have an effect. I think the main point we want to emphasize here is that we want to be very aware of any marketing or other outreach activities that might return money in the short-term but could jeopardize our brand, since our hypothesis is that the strength of GiveWell's brand as trustworthy, transparent and rigorous is the most important driver of our long-term impact.

Julia asks: What kind of thing does this [thought leadership] mean?

GW response:
By "thought leadership," we mean activities that would position GiveWell as a public, well-regarded voice in philanthropy and/or global health and development. Activities in this area could include writing op-eds, giving talks, or publishing a book.

6. *GW said: [In marketing]… We aim to continue to increase the amount we spend on advertising while maintaining a strong ROAS. [footnote: We do not expect to maintain a 1.5x first-year directly measured ROAS going forward. Based on data we've pulled from existing donors, we currently estimate that a 1x return estimated over a three-year period would be sufficiently strong to keep advertising.]*

Cari asks: I'm not following this [footnote]. What does "a 1x return estimated over a three-year period" mean?
Tim says: Seconded. And if it means what I think it means, does that not imply something about the value of a life saved 3 years from now being more valuable than one saved now?

GW response:
To Cari's question: A 1x return estimated over a three-year period means that based on: (1) donations we are able to directly observe coming in as a result of advertisements and (2) the

retention rate we expect on those donations, we estimate that within 3 years we will have brought in as much money as we spent. That estimated return *does not* include: (1) donations that will come in more than three years from now or (2) donations coming in as a result of advertisements but are not directly attributable to the advertisements. Because the actual returns will include the above amounts not formally attributed to it, we believe a 1x return estimated over a three-year period is a good rate of return even though it appears that we would be estimating a return that only just recovers our advertising costs.

To Tim's question: We don't mean to imply that about the value of a life saved now vs. three years from now, and we aren't sure we understand where that implication comes from. Please let us know if the above explanation doesn't clarify and if we should discuss further.

Tim Follow up question: This is mostly addressed by the response above, but simply we could give money to top charities now rather than spend it on advertising. If it takes 3 years to get back what we spent on advertising a choice to spend on advertising rather than donation implies a value for spending that money in the future rather than today.

Based on the response, I understand this as a 1x measured return over 3 years is the minimum necessary to convince us that the actual return is sufficiently high to justify advertising rather than donating. If that's not the case, then we should discuss more.

GW Response: That understanding is correct.

Section: Finding additional cost-effective giving opportunities → What was our goal?

7. *GW shared a footnote with the full text of our previous goals. It included "*Broader scope at greater levels of cost-effectiveness: *We aim to evaluate opportunities that go beyond the scope of the types of charities GiveWell has evaluated to date (more in [this blog post](#)). This area is still new for us and the goal we have in mind is intended to communicate our direction; it's likely that we revise our goal as we learn more about this area. Currently, our goal is: By the end of 2022, either (a) direct at least $25 million per year to opportunities in these new areas that we believe are significantly more cost-effective than the top charity we would have otherwise directed funds to or (b) conclude that the areas we're exploring are likely less cost-effective than our top charities (or not sufficiently more cost-effective to be worth the effort of exploring this new area)."*

Norma asks: ***Can you clarify goal b? Do you mean that you would rule out the areas you've done deep dives on, the areas that you've done deep and shallow dives on, or that you would rule out opportunities with a broader scope entirely? Given how broad this is and the capacity constraints that you note in the research update, I'd recommend against the latter.

GW response:
We included this language (i.e. our old goal for high-leverage areas) for transparency, but it no longer reflects how we're approaching high-leverage areas. We have already found some high-leverage opportunities we're excited about – the Centre for Pesticide Suicide Prevention, Vital Strategies for alcohol control – and our goals are now to make $10 million in grants to high-leverage areas in 2021 and $30 million in grants in 2023. We can share more if you have particular questions about how we're approaching exploring and deprioritizing areas.

Section: Finding additional cost-effective giving opportunities → How has our goal evolved?

8. Tim has the following overarching comments/questions related to finding additional giving opportunities:

My prior is to expect diminishing returns, and so the targets for more cost effective giving continue to surprise me. It would be useful to have more conversation not just about where those figures are coming from, but also:

- Are these aspirational goals, in the sense that we are setting a hard-to-reach target to inspire action?
- Are these necessity goals, in the sense that we expect to have demand for this much giving and so to keep our "customers" happy and keep GiveWell relevant we need to have this much product?
- Are these operational goals, in the sense that we have a specific plan for capacity that would generate these amounts and figures if we continue to execute our (well-founded) operational plans?

And to round that off, a useful question to hear some thinking on would be: what will happen if we don't reach these goals? What would that mean for GiveWell's future organization and operations? What trade-offs are worth making to reach these goals (possibly none)?

GW response: Here are a few thoughts; we're happy to discuss further in the meeting:

- The next section (on the specific goals) provides more discussion of these goals. In short:
    - The goals are ambitious "stretch goals." We aim to achieve them but may not and don't consider it a failure if we don't hit these targets.
    - Depending on how our money moved grows over the next few years (and we'll have more information about that as time goes on), we might need to reach these goals or we might not; these RFMF goals are set to meet our high-end projections of potential money moved.

    - We may need to modify our benchmark for "highly cost-effective" (e.g. move it from 10x to 8x or 6x cash) if we need to identify more RFMF than we can at 10x cash in order to meet our money moved.
- These aren't operational goals, in that we don't yet have a concrete plan that we feel confident will enable us to reach these goals if we just follow said plan. High-leverage areas and the intervention pipeline are both new enough that we need to learn more before we can develop an operational plan.
- It would be a big problem for GiveWell if we had more money moved than we have RFMF – it would mean we couldn't make recommendations to all our would-be donors. That said, we have options that we believe will allow us to meet our money moved even if these headline goals prove difficult to reach, such as accepting lower cost-effectiveness (as mentioned above).

Tim follow up: A question for future discussion is whether we are setting up a conflict in the way we are thinking about cost-effective opportunities. Essentially, should we just be improving the effectiveness of the giving field or should we only make recommendations to orgs that meet the 10X cost-effectiveness bar?

GW response: We're happy to return to this topic in a future session.

Tim follow up: We should follow up on questions about the impacts we have on charities we don't recommend.

GW response: We are thinking about how to update our approach to the information we provide and how that might affect charities we do not recommend.

9. *GW said: In 2021, we aim to find $60 million of additional highly cost-effective RFMF, and by 2023, we aim to find at least $310 million in additional, annual, highly cost-effective RFMF.*[1] *This RFMF will come from a mix of opportunities from our intervention pipeline and of high-leverage opportunities (more on this distinction in the next section). Our goal for 2021 is to find $50 million in annual RFMF via our intervention pipeline work and $10 million in annual RFMF in high-leverage opportunities. In 2023, our goal is to find $280 million via our intervention pipeline work and $30 million in high-leverage opportunities.*

9a. Tim said: I think this paragraph would be much better as a table. It's hard to keep track of the levels and changes between years and categories when presented this way.

GW response:

[1] We model giving opportunities' cost-effectiveness relative to the cost-effectiveness of cash transfers. Currently, we consider 10x cash to be highly cost-effective. We would not be surprised if our threshold falls over time (e.g., to 8x or 6x), especially if we're unable to find as much RFMF at 10x cash as we hope to.

We've added a table here. Note also that **we've adjusted these figures since we put the original document together for the Board** – the intervention pipeline goal for 2023 is lower ($230 million rather than $280 million), and we've added some additional room for more funding that we expect to find from current top charities.

	Additional[2] room for more funding (RFMF) goals, by area and year			
	Current top charities	Intervention pipeline	High-leverage	Total
Year 1 2021	$0 million	$50 million	$10 million	$60 million
Year 3 2023	$20 million	$230 million	$30 million	$280 million

9b. Holden asks: Can you share the reasoning/source for these estimates? (Also for the above RFMF tables if easy)

GW response:
Note that these estimates are extremely rough and we do not have high confidence in them. The money moved estimates are entirely based on the simple reasoning below:

- What Open Philanthropy has told us about its best guess about how its giving via GiveWell may grow.
- Extrapolating the trend of money moved growth forward (~$10 million per year over the past four years – this is an uncertain forecast).
- Adding additional money moved due to our proactive outreach efforts. We don't have data to make accurate forecasts; these additions are guesses.

The research targets for identifying RFMF are based on our goal of having enough RFMF to meet the demand that comes from money moved.

- Our median projection is $323 million in money moved in 2023 but our high-end projection is $470 million.
- We have $190 million in highly cost-effective RFMF in 2020, and we estimate that we'll continue to have at least an equal amount of yearly room for more funding in programs we've already identified going forward.
- Identifying $280 million *additional* room for more funding in 2023 would give us $470 million total, the high-end of our forecast for money moved.

[2] "Additional" meaning beyond the ~$190 million in yearly RFMF that we expect to continue to have going forward because of our current recommended charities.

Note: These numbers are intended to be directional. We don't intend them to be precise estimates of what we will (or need to) achieve. We expect to learn substantially more as time goes on about the likely path our money moved will take, and that path will inform how much additional RFMF we need to identify. Concretely, at this point, we see limited distinction between finding $450 million and $500 million of RFMF by year-end 2023: both represent a very large increase in RFMF and are on the high-end of what we foresee directing.

We don't know how realistic these targets are. 2021 is the first year where we'll be able to put a reasonable amount of research capacity against the goal of finding lots more RFMF. We'll know a lot more by year-end about how reasonable these targets are. So, we see these as ambitious targets that we aim to achieve but would not consider it a failure if GiveWell has ~$350 million of RFMF in 2023 and has $320 million in money moved.

The intervention pipeline vs. high-leverage areas breakdown (e.g. $50 million from the intervention pipeline and $10 million in high-leverage areas in 2021) is a guess at what we might be able to find in each; we expect the bulk of the RFMF to come from the intervention pipeline.

Norma follow up: Given the uncertainty around the intervention pipeline, have you considered spreading your bets? Are there ways you could help top charities build the capacity to have more RFMF at their current levels of cost effectiveness, and would that be worth the cost? Or are there light-touch ways to do more to explore high leverage areas (e.g. through relationship building?) I think it will take a while to find investable opportunities in high-leverage areas, so if you want to spread your bets for 2022 and 2023, it may be worth starting now.

9c. (With regard to: "find $60 million of additional highly cost-effective RFMF, and by 2023, we aim to find at least $310 million") Tim asks: within 24 months we're going to increase rfmf by 4x? I'm struggling with what "additional" means here. Is that on top of the $100M moved this year, or on top of the $280M projected to be moved in 2022. It's not clear how these RFMF numbers relate to the money moved targets.

GW response:
For reference, here is a table showing money moved forecasts and research targets for the next five years. Figures that are discussed here or in other responses are bolded and underlined.

	Money moved (millions)			Research RFMF targets (millions)			
	Low	Medium	High	Total RFMF target	Top charities	Intervention pipeline	High leverage
2019	-	**$150**	-	-	-	-	-

2020	$155	**$195**	$230	**$190**	-	-	-
2021	$180	**$238**	**$310**	**$250**	**$190**	**$50**	**$10**
2022	$200	$280	$390	$330	$200	$125	$15
2023	$225	**$323**	**$470**	**$470**	**$210**	**$230**	**$30**
2024	$245	$365	$550	$550	$220	$290	$40
2025	$265	$393	$630	$630	$230	$350	$50

To reframe:

- We have identified ~$190 million in RFMF this year.
- Our high end projections for money moved by the end of 2021 is $310 million. To match that projection, we would ideally also want to have identified $310 million in highly cost-effective RFMF. We don't believe we can achieve that target in 2021. We believe we can identify $250 million in highly cost-effective RFMF in 2021 (including RFMF at our current top charities). We expect that the opportunities we've already identified will continue to have ~$190 million in RFMF next year, so that means we need to find an additional $60 million. This would surpass our median expectation for money moved in 2021 ($238 million).
- To meet our high-end, 3-year projections of money moved, we ideally want to have identified $470 million in highly cost-effective RFMF in 2023. Minus the ~$190 in yearly RFMF we've already identified, we would need to find an additional $280 million by 2023 to hit that target.

So yes, that means we want to find almost 5x as much RFMF from the intervention pipeline and high-leverage areas in 2023 as we want to find in 2021, which is a steep increase. We think that might be possible because we are searching for programs more broadly than we have in the past, looking at technical-assistance-oriented programs, and we believe that GiveWell is now better able to engage with large charities than we were in the past. In 2020, we saw some very early, small signs supporting the notion above, via a small grant to Evidence Action for its syphilis screening and treatment technical assistance program and the initial response to a request for information for a new program: intermittent preventive treatment of infants (for malaria). But, we will know much more a year from now.

In addition, historically, we have regularly made relatively small grants that have led to much larger grants in later years. For example, we directed $5 million to Malaria Consortium's seasonal malaria chemoprevention program the first year it was recommended (2016); $30 million the following year (2017), and $52 million in 2019. We are searching for opportunities that have a similar ability to grow significantly over time.

Finally, these goals are adjustable in two ways:

1. If our annual money moved doesn't appear to be increasing so rapidly that it would reach our high-end projections (which is likely, as those are our high-end rather than median projections), then we don't need to reach such an ambitious RFMF goal.
2. If we try but ultimately aren't succeeding in finding enough RFMF that is 10x as cost-effective as cash to meet the increasing demand for GiveWell's recommendations, we may lower our benchmark to something like 6x or 8x to be able to have enough giving opportunities to meet our money moved.

Julia follow up: What will happen if the money moved this year is more like the "high" number and exceeds the RFMF?

Cari follow up: On such a short time horizon, is there enough time for this kind of iteration [the 2nd adjustment proposed]? i.e. If you aren't successful in finding enough 10x RFMF, will you have time to identify and evaluate those 6x or 8x opportunities?

9d. Tim asks: Per the footnote ["Additional" meaning beyond the ~$190 million in yearly RFMF that we expect to continue to have going forward because of our current recommended charities."], is 10x the floor for "highly cost effective", and 6x to 8x would just be "cost effective"? or are those "highly" as well?

GW response:
Yes, we're currently considering 10x to be the bar for "highly cost-effective" giving opportunities. However, our rule of thumb is that our cost-effectiveness analyses can easily shift by up to 25% in either direction, so we would be cautious about deprioritizing something that appears to be e.g. 8x without further investigation. Also, the bar for "highly cost-effective" may shift depending on our money moved and the opportunities we're able to find.

Section: Influencing institutional funders of global health and development → What will we do next?

10. *GW said: In 2021, our Managing Director plans to build deep relationships with ~100 leaders at carefully-selected institutions in global health and development.*

Tim says: Building "deep relationships" with 100 people in a year is not a credible goal.

GW response:
You're right that building deep relationships with 100 people in one year is not credible; we should have phrased our goal more accurately. The current thinking is that we will identify a relatively large number (~100) of potential individuals and institutions with whom we might want to build deep relationships. Based on an initial set of meetings with this relatively large

group, our aim would be to build deep relationships with some smaller subset. A few additional thoughts:

1. The number of “100” is a placeholder, to be refined in January. We are creating a list from the bottom up, looking for individuals and institutions with whom we believe having more meaningful relationships will enable us to:
   a. source new giving opportunities,
   b. get valuable input into our research process,
   c. better recruit exceptional candidates, and
   d. potentially influence large institutions.

   As we build this list, we will prioritize a shortlist of these individuals/institutions based both on how valuable we think those connections will be and on our internal capacity. The ultimate number of individuals with whom we build deep professional relationships will be an output of this process, and will be meaningfully lower than ~100.
2. This goal does not only refer to relationships that our Managing Director will personally build (we should have worded the above goal differently). One component of our broader engagement strategy will be for our Managing Director to cultivate and own relationships. But an additional and equally important component of the strategy will be to equip and support other GiveWell staff to maintain meaningful relationships with appropriate networks.

Attachment B: Update on GiveWell's research – December 2020

Section: Intervention pipeline → Context

11. *GW said: In 2020, we have added additional programs to [the intervention pipeline] list as they come across our radar, and have reviewed dozens of the most promising-seeming programs in shallow depth (i.e. ~week-long assessments by individual researchers, which we call "quick evidence assessments" or "interim intervention reports")*

Holden asks: Is anything from this work published? (Not that it necessarily should be, but wondering)

GW response: Historically we haven't published things at the quick evidence assessment (QEA) level, but we are planning to in the future. We're publishing an updated dashboard, hopefully before year-end but it may slip to early 2021. The first version will contain 10 QEAs as well about 40 more detailed, intervention-report-style program reviews (not all of which were completed this year). We plan to add the backlog of programs to it and keep this updated going forward.

12. *GW said: We continue to add to the [intervention pipeline] list of potentially promising programs and shallowly review the most promising-seeming ones. Then, we take the programs that look most promising after our initial review and move forward with more detailed evaluation of those, scrutinizing them further and searching for opportunities we could fund or create.*

Tim comments: This is a stretch but it occurs to me that using [redacted] might be a useful way to effectively prioritize these lists.

GW response: Thanks, Tim! We've passed this along to the staff that are leading this work for consideration.

13. *GW said: We do not anticipate finding [intervention pipeline] opportunities that are ready to be added to our top charity list as-is… Instead, we think promising programs may need to be created, strengthened, or scaled up, via vehicles like Evidence Action's Accelerator, via evidence generation like funding randomized control trials, or via other means.*

Norma comments: You may already be planning on this, but I'd recommend tracking & assessing what kind of incubation and scale up tactics work. For example, it's conventional wisdom in certain development circles that scale-ups only work if an NGO or firm truly owns the program; to inform future work, you might wish to assess to what extent that's true in your work and/or others' experiences.

GW response: Thanks, Norma! We've passed this idea along to the staff member who is leading this work for consideration. She noted that she recently spoke to a group that is working on analyzing factors that make incubation/scale-up successful in general and she plans to stay in touch with them.

Tim follow up: Are we creating space for other organizations that are playing into a GiveWell model? Are we creating space for more collaboration (or competition)?

GW response: We'll follow up on these with Directors.

Section: Intervention Pipeline → Recent Progress and Future Plans

14. *GW said: As discussed above, in 2021 we aim to find an additional $50 million in promising giving opportunities that are similarly cost-effective to our current top charities. We anticipate those may be spread across five to ten different opportunities in a few different cause areas, and are looking to build a portfolio of programs that can double in size by 2023 to support our goal of identifying $280 million in highly cost-effective RFMF by then.*

Holden asks: Can you share sources/explanations for these estimates?

GW response: See answer here.

Section: Exploring high-leverage, more cost-effective areas → Future Plans

15. *GW said: Even though we have been working on this for nearly two years, we have devoted limited capacity to do it. Only one research staff person has been dedicated to it, and we roughly estimate they allocated ~⅓ of their time to it in 2018 and 2019.*

Norma asks: To what extent (if at all) is growing capacity in this area a priority? Why or why not?

GW response: Adding capacity in new areas, specifically, isn't currently our priority. Our top research priority is increasing our RFMF, and we expect to do more to increase RFMF via the intervention pipeline than new areas given the relative size of the funding gaps we expect to find.

We haven't yet determined how we'll approach high-leverage areas from an organizational perspective once that work advances, i.e. whether we'll take an approach that leads us to make domain-specialized hires (like Program Officers) or generalists (like the senior researchers we've been hiring).

Hiring additional senior researchers is our top capacity-building priority, and it's possible that a new hire will end up working on high-leverage areas.

16. *GW said: Our rough goal is to have a developed program of grantmaking in this [high leverage] area in which we are directing $30 million annually by 2023.*

Holden asks: Can you share source/reasoning for this estimate?

GW response: See answer here.

Attachment C: Resolutions for Board Vote

No comments or questions

Attachment D: Draft Investment Policy

Section: Responsibility

17. *GW said: The Director of Operations or Chief Executive Officer is responsible for the maintenance and revision of this policy. Further, the Director of Operations or CEO and his/her authorized delegates are responsible for securing and managing investments and cash for operations.*

Holden asks: What does the "or" here mean? I generally think of responsibility as belonging to one party, would appreciate clarification.

GW Response: The intent was joint responsibility but the wording was not clear. We have updated to clarify that responsibility rests with the Director of Operations and have revised this wording accordingly. The relevant sentences now read: “The Director of Operations is responsible for the maintenance and revision of this policy. Further, the Director of Operations and their authorized delegates are responsible for securing and managing investments and cash for operations.”