WILLIAM MACASKILL
COFOUNDER OF THE EFFECTIVE ALTRUISM MOVEMENT

DOING GOOD BETTER

EFFECTIVE ALTRUISM AND HOW YOU CAN MAKE A DIFFERENCE

PRAISE FOR
DOING GOOD BETTER

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"William MacAskill shows that we can make a surprisingly large life-changing difference to those in disadvantaged parts of the world—provided that our altruistic impulses are intelligently channeled. This fascinating and clearly written book deserves wide readership: It can in itself do great good if its message is heeded."

—**Lord Martin Rees,** former president of the Royal Society

"Think philosophers are dry, inaccessible, and obsessed with mere ideas, not actions? Think again. MacAskill puts forward an eye-popping claim: By combining heart and mind, one can make the world a better place. And then he shows you how."

—**Dean Karlan,** professor of economics at Yale University and coauthor of *More Than Good Intentions*

"This is a challenging and thought-provoking book. We are encouraged first to ask ourselves whether we are altruistic enough, and second whether we practice effective altruism, a much tougher and more contested ask. This book will guarantee lively debates and should encourage us all to ask searching questions."

—**Fiona Reynolds,** former director-general of the National Trust

"*Doing Good Better* is a must-read for anyone with both a heart and a brain. MacAskill demolishes the lazy myths of nothing-you-can-do-ism and demonstrates the power of asking the right questions. This is an important book. It's also surprisingly fun. Figuring out what really helps people is a challenging scientific puzzle, and these pages are full of unexpected twists—enlightening and invigorating."

—**Joshua Greene**, director of Harvard's Moral Cognition Lab and author of *Moral Tribes*

DOING GOOD BETTER

HOW EFFECTIVE ALTRUISM CAN HELP YOU MAKE A DIFFERENCE

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To Toby Ord, Peter Singer, and Stanislav Petrov, without whom this book would not have been written

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INTRODUCTION

Worms and Water Pumps

How can you do the most good?

Until 1989, Trevor Field was a typical middle-aged South African man who had lived a fairly normal life. He enjoyed fresh steaks, cold beer, and fishing with his friends. Working in advertising for magazines like *TopCar* and *Penthouse*, he had never thought seriously about using his skills for the greater good. When he discovered the PlayPump, however, everything changed.

That year, Field and his father-in-law, a farmer, visited an agricultural fair in Pretoria. There, he met a water engineer named Ronnie Stuiver who was demonstrating a model for a new type of water pump. The demonstration reminded Field of a fishing trip he'd taken years before, during which he had watched the women of a rural village wait for hours next to a windmill-powered water pump. There had been no wind that day, but the women, who had trekked for miles, still needed to bring water back to their homes. So they simply sat and waited for the water to flow. Field had been struck by how unfair this was. *There simply must be a better way to do this*, he thought. Now he was witnessing a potential solution.

Stuiver's invention seemed brilliant. Instead of the typical hand pump or windmill pump found in many villages in poor countries, Stuiver's pump doubled as a playground merry-go-round. Children would play on the merry-go-round, which, as it spun, would pump clean water from deep underground up to a storage tank. No longer would the women of the village need to walk miles to draw water using a hand pump or wait in line at a windmill-powered pump on a still day. The PlayPump, as it was called, utilized the power of playing children to provide a sustainable water supply for the community. "African [kids] have almost nothing—not even books in school let alone playground equipment—and access to water is a huge problem," Field later told me. "I thought it was just the

best idea I've ever seen."

Field bought the patent from Stuiver and worked in his spare time over the next five years to improve the design. Using his experience in advertising, Field came up with the idea of placing billboard advertisements on the sides of the water tank as a way to generate revenue to pay for pump maintenance. In 1995, he secured his first sponsor, Colgate Palmolive; installed the first PlayPump; and quit his job in order to focus full-time on the project, now a registered charity called PlayPumps International. Progress was slow at first, but he persevered, paying for several pumps with his own money. At the same time, he developed connections with corporations and government bodies across South Africa to pay for more pumps. By the turn of the millennium, he had installed fifty pumps across the country.

His first major breakthrough came in 2000 when, out of three thousand applicants, he won a World Bank Development Marketplace Award, given to "innovative, early-stage development projects that are scalable and/or replicable, while also having high potential for development impact." That award attracted funding and attention, which culminated in a site visit from Steve Case, CEO of AOL, and his wife, Jean. "They thought the PlayPump was incredible," Field said. "As soon as they saw it in action, they were sold." In 2005, the Cases agreed to fund the project and worked with Field to set up an American arm of PlayPumps International. Their aim was to roll out thousands of new PlayPumps across Africa.

The PlayPump became the center of a massive marketing campaign. Steve Case used his expertise from running AOL to pioneer new forms of online fund-raising. The One Foundation, a British fund-raising charity, launched a bottled water brand called One Water and donated the profits to PlayPumps International. It was a huge success and became the official bottled water of the Live 8 concerts and the Make Poverty History campaign. The PlayPump became the darling of the international media, who leaped at the opportunity to pun, with headlines like PUMPING WATER IS CHILD'S PLAY and THE MAGIC ROUNDABOUT. In an article for *Time* in 2006, Bill Clinton called the PlayPump a "wonderful innovation."

Celebrities, too, jumped on the bandwagon. Jay-Z raised tens of thousands of dollars through his "The Diary of Jay-Z: Water for Life" concert tour. Soon after, PlayPumps International secured its biggest win: a $16.4 million grant awarded by then First Lady Laura Bush, launching a campaign designed to raise $60 million to fund four thousand PlayPumps across Africa by 2010. By 2007,

the PlayPump was the hottest thing in international development, and Trevor Field was at the center of it all—a rock star of the charity world.

"It has just gone berserk! . . . When I first looked at this water pump . . . I could never imagine that this is something that could possibly change the world," Field said in 2008, reflecting on PlayPump International's startling success. "It really rocks me to know we're making a difference to a lot of people who are nowhere near as privileged as I am or my family is." By 2009, his charity had installed eighteen hundred PlayPumps across South Africa, Mozambique, Swaziland, and Zambia.

Then things went sour. Two damning reports were released, one by UNICEF and one by the Swiss Resource Centre and Consultancies for Development (SKAT). It turned out that, despite the hype and the awards and the millions of dollars spent, no one had really considered the practicalities of the PlayPump. Most playground merry-go-rounds spin freely once they've gained sufficient momentum—that's what makes them fun. But in order to pump water, PlayPumps need constant force, and children playing on them would quickly get exhausted. According to the UNICEF report, children sometimes fell off and broke limbs, and some vomited from the spinning. In one village, local children were paid to "play" on the pump. Much of the time, women of the village ended up pushing the merry-go-round themselves—a task they found tiring, undignified, and demeaning.

What's more, no one had asked the local communities if they wanted a PlayPump in the first place. When the investigators from SKAT asked the community what they thought about the new PlayPump, many said they preferred the hand pumps that were previously installed. With less effort, a Zimbabwe Bush hand pump of the same cylinder size as a PlayPump provided thirteen hundred liters of water per hour—five times the amount of the PlayPump. A woman in Mozambique said, "From five A.M., we are in the fields, working for six hours. Then we come to this pump and have to turn it. From this, your arms start to hurt. The old hand pump was much easier." One reporter estimated that, in order to provide a typical village's water needs, the merry-go-round would have to spin for twenty-seven hours per day.

Even when communities welcomed the pumps, they didn't do so for long. The pumps often broke down within months, but unlike the Zimbabwe Bush Pump, the mechanics of the pump were encased in a metal shell and could not be repaired by the community. The locals were supposed to receive a phone number to call for maintenance, but most communities never received one, and those

who did never got anyone on the phone. The billboards on the storage tanks lay bare: the rural communities were too poor for companies to be interested in paying for advertising. The PlayPump was inferior in almost every way to the unsexy but functional hand pumps it competed with. Yet, at $14,000 per unit, it cost four times as much.

Soon, the media turned on its golden child. PBS ran a documentary exposing the PlayPump's many shortcomings. (One thing that didn't change was the media's love of puns: the documentary was called *Southern Africa: Troubled Water*; *The Guardian* repeatedly referred to the PlayPump as "money down the drain.") In an admirable response to this criticism, the US arm of PlayPumps International shut down and its sponsor, the Case Foundation, publicly acknowledged that the program had been a failure. Yet, despite its fall from grace, the PlayPump lives on. Under the name of Roundabout Water Solutions, Field's nonprofit continues to install the same model of PlayPumps across South Africa, funded by corporations like Ford Motor Company and Colgate Palmolive.

• • •

Most people want to make a difference in their lives and, if you're reading this book, you're probably no exception. As Trevor Field's story illustrates, however, good intentions can all too easily lead to bad outcomes. The challenge for us is this: How can we ensure that, when we try to help others, we do so as effectively as possible? How can we ensure that we avoid accidentally causing harm, and succeed in having the greatest positive impact we can?

This book tries to help answer these questions. I believe that by combining the heart and the head—by applying data and reason to altruistic acts—we can turn our good intentions into astonishingly good outcomes. To illustrate, let's look at a story with a very different ending from the one you just read about.

• • •

In 2007, at the peak of the PlayPump's popularity, Michael Kremer and Rachel Glennerster launched an organization of their own, the culmination of decades of research into how to improve the lives of the poorest people in the world.

Glennerster had studied economics at the University of Oxford, graduating in 1988. She was interested in learning about poverty relief, so she decided to

live in a developing country and spent a summer in Kenya. She spoke to people working in development, many of whom were deeply disillusioned. When she asked why, they told her to look at some of the ways development projects had backfired.

"I got sent down to big projects that had failed," Glennerster told me. "I went to Lake Turkana, up in the north of Kenya. The Turkana people are basically nomadic, and various development projects had hoped to improve their quality of life by settling them on the lake, so they built a big factory for fish. They managed to get them to settle and fish in the lake, but then the lake got overfished, and the fish stock collapsed. . . . It was depressing." Disenchanted about the potential to have an impact in global development, she moved into domestic policy, taking a job at the British Treasury.

Michael Kremer also spent some of his young adulthood in Kenya, living there for a year after finishing his undergraduate degree. Like Glennerster, he was concerned by extreme poverty and wanted to learn more, so he lived with a local family, teaching English at a secondary school. He also saw some dramatic ways in which attempts to improve conditions there were failing. When he returned to grad school, he decided to figure out how things could be done better.

Kremer and Glennerster met at Harvard University in 1990. Kremer was a PhD student; Glennerster was visiting on a Kennedy Scholarship, having taken a sabbatical from her work at the Treasury. By the time Kremer became a professor at MIT in 1993, he and Glennerster were married. As a vacation, they returned to Kenya to visit the family Kremer had lived with several years prior.

While there, Kremer spoke to Paul Lipeyah, a friend who worked for the Dutch charity International Christian Support (now called Investing in Children and Their Societies, or ICS). ICS's main program was child sponsorship, in which a donor paid a regular amount to help an individual child or a small community. ICS had been trying to improve school attendance and test scores. They provided a package of different things: new textbooks and additional teachers to schools, and free school uniforms to the students. ICS had received new funding, and Paul Lipeyah was about to roll out the program to seven new schools.

Kremer urged Lipeyah to test his program using what's called a randomized controlled trial: he would monitor and collect data for fourteen local schools, implementing the program in seven of them, while letting the other seven go about business as usual. By collecting data from all fourteen schools to see

which fared better, he could find out if his program actually worked.

In hindsight, Kremer's idea seems obvious. Randomized controlled trials are the gold-standard method of testing ideas in other sciences, and for decades pharmaceutical companies have used them to test new drugs. In fact, because it's so important not to sell people ineffective or harmful drugs, it's illegal to market a drug that hasn't gone through extensive randomized controlled trials. But before Kremer suggested it, the idea had never been applied to the development world.

With the help of collaborators, Kremer tested the different ICS programs one by one. First, he looked at the efficacy of providing schools with additional textbooks. Classrooms would often have only one textbook for a class of thirty, so it seemed obvious that providing more textbooks would help students learn. However, when Kremer tested this theory by comparing test scores between schools that received books and those that didn't, he found no effect for all but the most high-achieving of students. (He suggests the textbooks were written at too high a level for the children, especially considering they were in English, the pupils' third language after Swahili and their local languages.)

Next, Kremer looked at providing flip charts. The schoolchildren couldn't understand the textbooks, but having flip charts would allow teachers to tailor lessons to the specific needs of the students. Perhaps these would work better. Again, however, no effect.

Undaunted, he took a different approach. If providing additional materials didn't work, maybe increasing the number of teachers would. After all, most schools had only one teacher, catering to a large class. But, again, he found no discernible improvement from decreasing class sizes.

Over and over again, Kremer found that seemingly obvious programs to improve education just weren't working. But he persisted. He refused to believe there was simply no way to improve the education of children in Kenya. At that point, a friend at the World Bank suggested he test deworming.

Few people in developed countries know about intestinal worms: parasitic infections that affect more than one billion people worldwide. They aren't as dramatic as AIDS or cancer or malaria, because they don't kill nearly as many people as those other conditions. But they do make children sick, and can be cured for pennies: off-patent drugs, developed in the 1950s, can be distributed through schools and administered by teachers, and will cure children of intestinal worms for a year.

Kremer did an experiment to see whether treating children for these

intestinal worms had an impact on education. The results were striking. “We didn’t expect deworming to be as effective as it was,” Kremer told me. “It turned out to be one of the most cost-effective ways of increasing school participation.”

Absenteeism is a chronic problem in schools in Kenya, and deworming reduced it by 25 percent. In fact, every child treated spent an extra two weeks in school, and every one hundred dollars spent on the program provided a total of ten years of additional school attendance among all students. Enabling a child to spend an extra day in school therefore cost just five cents. It wasn’t merely that deworming children “worked” at getting children into school. It worked incredibly well.

What’s more, deworming didn’t merely have educational benefits. It had health and economic benefits, too. Intestinal worms can cause a variety of maladies, including anemia, intestinal obstruction, and a suppressed immune system that can increase the risk of other diseases like malaria. Deworming decreases all these risks. Moreover, when Kremer’s colleagues followed up with the children ten years later, those who had been dewormed were working an extra 3.4 hours per week and earning an extra 20 percent of income compared to those who had not been dewormed. In fact, deworming was such a powerful program that it paid for itself through increased tax revenue.

By the time his work on deworming was published, Kremer’s revolutionary new approach to development had spawned a following, with dozens of the brightest young economists running hundreds of trials of different development programs. Meanwhile, Glennerster had quit her job and become the executive director of the newly founded Poverty Action Lab at MIT, where she used her knowledge of policy to ensure the research Kremer and his colleagues were conducting would have real-world impact.

In 2007, on the basis of this research, Kremer and Glennerster cofounded the nonprofit Deworm the World Initiative, which provides technical assistance to the governments of developing countries, enabling them to launch their own deworming programs. The charity has provided more than forty million deworming treatments, and the independent charity evaluator GiveWell regards them as one of the most cost-effective development charities.

• • •

When it comes to helping others, being unreflective often means being ineffective.

The PlayPump is the perfect example. Trevor Field and everyone who supported him were driven by emotions—the appeal of seeing happy children provide their communities with clean water through the simple act of playing—rather than facts. The Case Foundation, Laura Bush, and Bill Clinton supported the PlayPump not because there was good evidence to believe it would help people but because it had the thrill of a revolutionary technology. Even critics of the campaign would stop short of accusing Field and his supporters of bad intentions—they no doubt genuinely wanted to help the people of rural Africa. But relying on good intentions alone to inform your decisions is potentially disastrous.

It would be nice if the PlayPump were an isolated example of unreflective altruism, but sadly it's just an extreme example of a much more general trend. We very often fail to think as carefully about helping others as we could, mistakenly believing that applying data and rationality to a charitable endeavor robs the act of virtue. And that means we pass up opportunities to make a tremendous difference.

Imagine, for example, that you're walking down a commercial street in your hometown. An attractive and frighteningly enthusiastic young woman nearly assaults you in order to get you to stop and speak with her. She clasps a tablet and wears a T-shirt displaying the words *Dazzling Cosmetics*. You agree to let her speak, and she explains that she's representing a beauty products company that is looking for investment. She tells you about how big the market for beauty products is, and how great the products they sell are, and how, because the company spends more than 90 percent of its money on making the products and less than 10 percent on staff, distribution, and marketing, the company is extremely efficient and therefore able to generate an impressive return on investment. Would you invest?

Of course you wouldn't. If you wanted to invest in a company, you would consult experts or investigate different companies and compare Dazzling Cosmetics' performance with the rest of them. Either way, you would look at the best available evidence in order to work out where you will get the most bang for your buck. In fact, almost no one is foolish enough to invest in a company that is pitched to them on the street—which is why the imaginary situation I described here never occurs. Yet, every year, hundreds of thousands of people donate to charities they haven't heard of simply because a well-spoken fund-raiser whom they didn't know asked them to. And they normally have no way of knowing what happened to the money they donated.

One difference between investing in a company and donating to a charity is that the charity world often lacks appropriate feedback mechanisms. Invest in a bad company, and you lose money; but give money to a bad charity, and you probably won't hear about its failings. Buy a shirt that's advertised as silk when it's really polyester, and you'll realize pretty quickly; but buy coffee that has a Fairtrade stamp on it, and you never know whether doing so helped people, harmed them, or did nothing. If it weren't for the independent investigations by UNICEF and SKAT, PlayPumps International would have looked like a terrific success to those who supported it. Because we don't get useful feedback when we try to help others, we can't get a meaningful sense of whether we're really making a difference.

Kremer and Glennerster succeeded in part because they didn't assume they knew what the most effective way of helping people was. Instead, they tested their ideas before putting them into action. They were willing to revise their beliefs about what worked in light of the evidence they received and then went out and did what the evidence suggested they should do. In contrast with the PlayPump, the most effective program turned out to be remarkably boring: Grace Hollister, now the director of Deworm the World Initiative, told me that "deworming is probably the least sexy development program there is." But by focusing on what was effective rather than what was emotionally appealing, they produced outstanding results, significantly improving the lives of millions of people.

Kremer and Glennerster exemplify a way of thinking I call *effective altruism*. Effective altruism is about asking, "How can I make the biggest difference I can?" and using evidence and careful reasoning to try to find an answer. It takes a scientific approach to doing good. Just as science consists of the honest and impartial attempt to work out what's true, and a commitment to believe the truth whatever that turns out to be, effective altruism consists of the honest and impartial attempt to work out what's best for the world, and a commitment to do what's best, whatever that turns out to be.

As the phrase suggests, effective altruism has two parts, and I want to be clear on what each part means. As I use the term, *altruism* simply means improving the lives of others. Many people believe that altruism should denote sacrifice, but if you can do good while maintaining a comfortable life for yourself, that's a bonus, and I'm very happy to call that altruism. The second part is *effectiveness*, by which I mean doing the most good with whatever resources you have. Importantly, effective altruism is not just about making *a*

difference, or doing *some* amount of good. It's about trying to make the *most* difference you can. Determining whether something is effective means recognizing that some ways of doing good are better than others. The point of this isn't to lay blame, or to claim that some ways of doing good are "unworthy." Rather, it's simply to work out which ways of doing good are best, and to do those first. This project is crucial because, as we'll discuss, the best ways of doing good are very good indeed.

I helped to develop the idea of effective altruism while a graduate student at the University of Oxford. I had begun donating to charity and wanted to ensure that my donations did as much to help others as possible. Along with Toby Ord, a postdoctoral researcher at Oxford, I began to investigate the cost-effectiveness of charities that fight poverty in the developing world. The results were remarkable. We discovered that the best charities are hundreds of times more effective at improving lives than merely "good" charities. In 2009, Toby and I cofounded Giving What We Can, an organization that encourages people to donate at least 10 percent of their income to these most cost-effective charities. Around the same time, two New York hedge fund analysts, Holden Karnofsky and Elie Hassenfeld, quit their jobs to start GiveWell, an organization that does extraordinarily in-depth research to work out which charities do the most good with every dollar they receive.

From there, a community developed. We realized that effective altruism could be applied to all areas of our lives—choosing a charity, certainly, but also choosing a career, volunteering, and choosing what we buy and don't buy. On the basis of this, in 2011, I cofounded 80,000 Hours (a name that refers to the number of hours you typically work in your life), which provides advice and coaching on how to choose a career that will allow you to make the most difference.

In this book, I'll present in more depth effective altruism's approach to making a difference. What I hope to convey is not a series of facts but a new way of thinking about helping others, which you can take with you and apply in your own life. The first part of this book outlines effective altruism's way of thinking, enabling us, in the second part of the book, to apply that way of thinking to specific issues.

In the first part, I dedicate each chapter to exploring one of effective altruism's five key questions:

1. How many people benefit, and by how much?

2. Is this the most effective thing you can do?
3. Is this area neglected?
4. What would have happened otherwise?
5. What are the chances of success, and how good would success be?

Asking these five key questions can help us avoid common pitfalls when thinking about doing good. The first question helps us to think concretely about how different actions improve people's lives, so that we don't squander our time or money on activities that don't, ultimately, make people better off. The second question ensures we try to spend our efforts not on "merely good" activities but on the *very best* activities. The third question directs us to focus on those areas that receive comparatively little attention, and for which others haven't taken the outstanding opportunities to make a difference. The fourth question helps us to avoid trying to do good works that would happen with or without our involvement. The fifth question helps us to think about uncertainty correctly, so that we can know when to pursue activities that have low odds of success but large potential payoffs instead of activities with guaranteed smaller benefits.

Taken together, these five questions help us to answer the guiding question of effective altruism: "How can I do the most good?" They form the core of effective altruism's approach to making a difference.

In the second part of this book, I apply these questions to specific areas: How can I figure out which charities will do the most good with my donations? How can I choose a career or volunteering opportunity with the biggest impact? How much of a difference can I make through ethical consumption? Of the many problems in the world, how can I decide which to focus on? In each case, I provide a framework for thinking about the issue, a checklist of questions to help you ensure that you think through all the most important considerations. I hope to show how effective altruism can help us to have a greater impact in all aspects of our lives. For ease of reference, the frameworks and the five key questions are all restated in the appendix.

Before we begin, let me emphasize why these considerations are so important. In the next chapter, I'll explain why each and every one of us has the power, if we so choose, to do extraordinary things.

ONE

YOU ARE THE 1 PERCENT

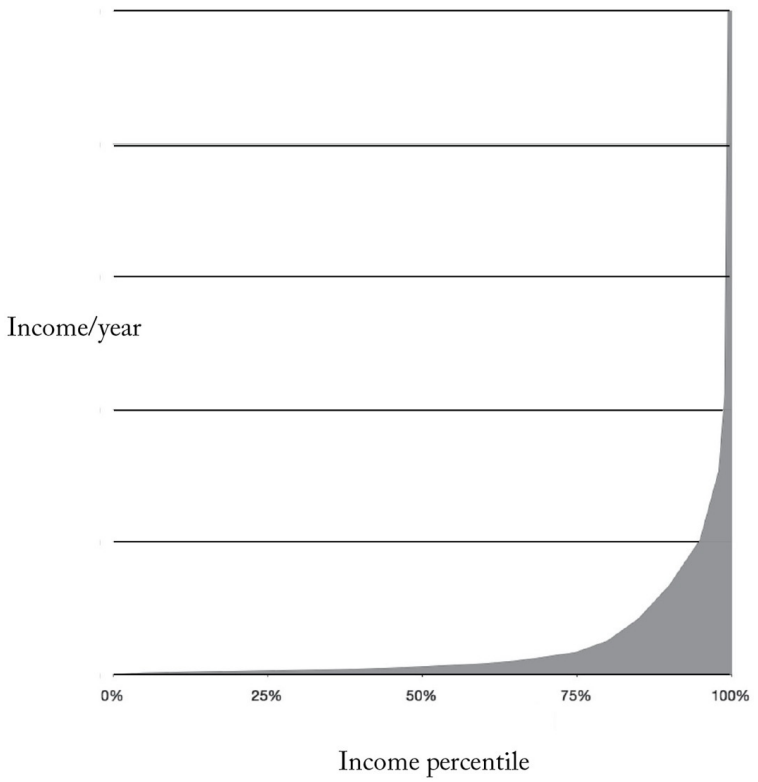
Just how much can you achieve?

When the Occupy Wall Street movement gained traction in the fall of 2011, disaffected citizens of the Western world quickly adopted the term *the 1 percent* to refer to the top 1 percent of income earners in wealthy nations, primarily the United States. The term came from a popular statistic that the richest 1 percent of the population receives 24 percent of total income—that's more than $340,000 per year, twelve times the $28,000 earned by the typical American worker. References to the 1 percent versus the 99 percent—i.e., the rest of the population—quickly became shorthand for the income gap in America.

Inequality in America is getting starker over time: while typical household income grew by less than 40 percent between 1979 and 2007, the income of the richest 1 percent grew by 275 percent in that same time period. The French economist Thomas Piketty, who gained international fame for his 2014 book *Capital in the Twenty-First Century*, has suggested that the level of income inequality in the United States is "probably higher than in any other society at any time in the past, anywhere in the world."

This can lead those of us who aren't in that 1 percent to feel powerless, but this focus neglects just how much power almost any member of an affluent country has. If people focus exclusively on American inequality, they're missing an important part of the bigger picture. Consider this graph of global income distribution:

GLOBAL INCOME DISTRIBUTION



Source: Branko Milanovic, PovcalNet

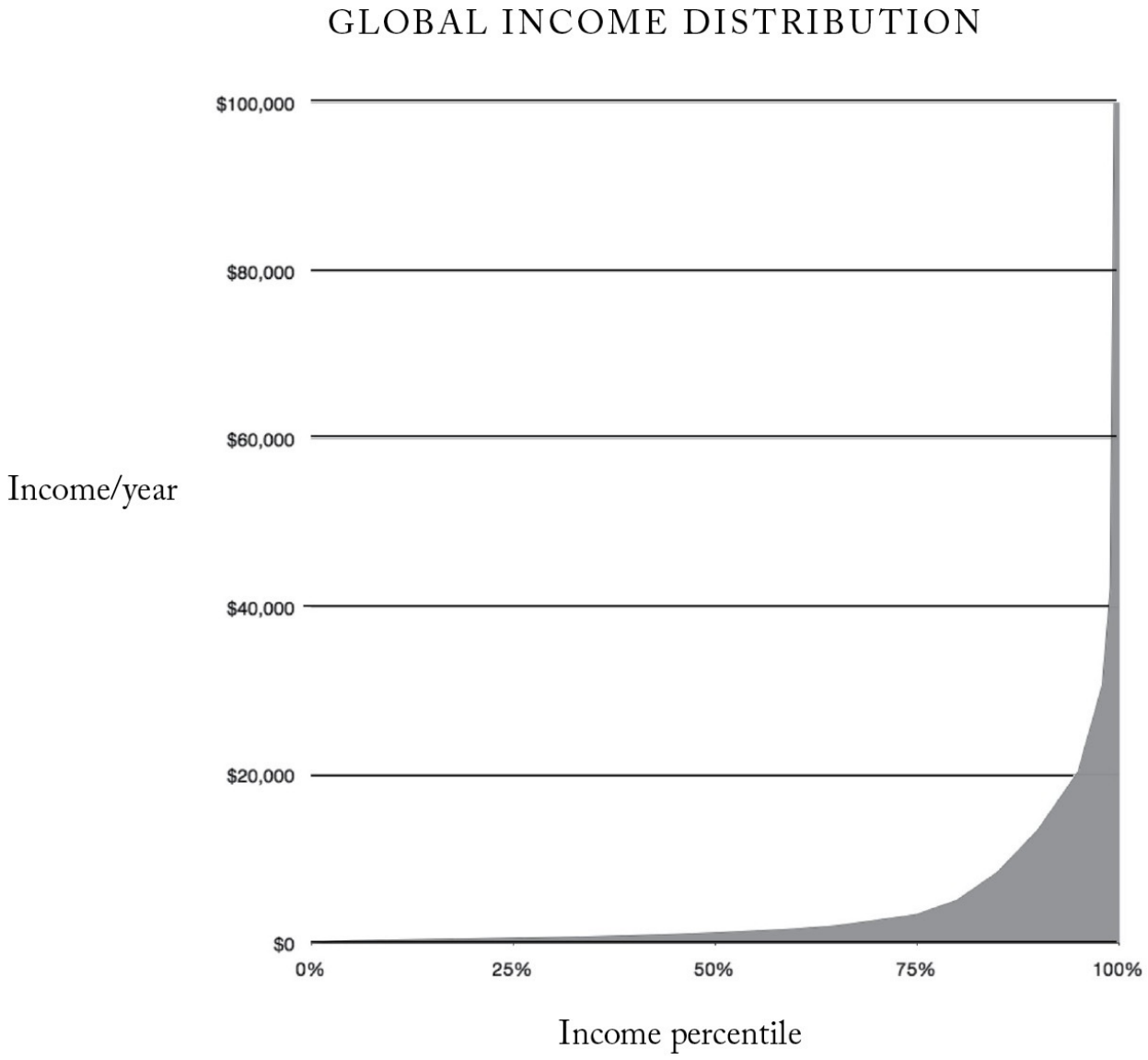
This graph lines up everyone in the world, ordered by their income. The space between 0 and 25 percent represents the 25 percent of the world with the smallest incomes; the space between 75 and 100 percent represents the 25 percent of the world with the largest incomes. If everyone had the same income, the line would be flat, forming a neat rectangle under it. But they don't. The poorest people in the world barely even register on the graph. Income soars when you hit the top 10 percent. And the richest 1 percent? That spike goes off the chart. If I wanted to draw the whole of this graph so that you could see where the spike ends, this book would have to be as tall as a twenty-three-story building, taller than the original Godzilla.

Where do you fall on this graph? You obviously won't know for sure, since I've deliberately left the vertical axis unlabeled, but have a guess. What percentage of the world's population is above you in income, and what percentage is below?

When I ask residents of the United States or the United Kingdom this question, they typically guess they fall into the seventieth or eightieth percentile.

They know they're from an affluent country, but they also know they're not like those bankers and CEOs who make up the global elite. They therefore guess that they're at the corner of the curve, peering up at the megarich who sit atop that spike. That's what I used to think, too.

Here's that graph with the vertical axis labeled.



Source: Branko Milanovic, PovcalNet

If you earn more than $52,000 per year, then, speaking globally, you are the 1 percent. If you earn at least $28,000—that's the typical income for working individuals in the United States—you're in the richest 5 percent of the world's population. Even someone living below the US poverty line, earning just

$11,000 per year, is still richer than 85 percent of people in the world. Because we're used to judging ourselves in comparison with our peers, it's easy to underestimate just how well off those of us in rich countries are.

You might be feeling skeptical at this point. I certainly was when I first heard these facts. "Sure," you might say, "the poor in developing countries might not have much money, but that money can pay for so much more because the cost of living in those places is cheaper."

It's true that money goes further overseas. When I was in Ethiopia, I ate at one of Addis Ababa's fanciest restaurants, and the bill came to about ten dollars. I even once stayed in a hotel room (albeit, a nasty one) for a night for one dollar. However, that graph of income inequality has already taken into account the fact that money goes further overseas. Let's look at that bottom 20 percent of the world's population: that's 1.22 billion people who earn less than $1.50 per day, and thereby count as members of the "extreme poor." You might assume that "$1.50 per day" means that every day the extreme poor live on the equivalent of $1.50 in their local currency. But it actually means they live on an amount of money equivalent to what $1.50 could buy in the United States in 2014. What can $1.50 buy you in the United States? A candy bar? A bag of rice?

You might still be skeptical. Perhaps, you think, people in poor countries can live on less than $1.50 a day because they produce a lot of their own goods. They don't have much money but they don't need that much money because they farm their own land and mainly live off what they grow. Again, however, this has already been taken into account in that graph. Suppose Annette is a farmer who earns $1.20 per day from selling her produce, but who also eats forty cents' worth of what she grows per day. According to the way these figures are calculated, she lives on $1.60 per day and is therefore above the $1.50-per-day poverty line.

You might wonder: How can anyone live on such little money? Surely they'd die? And the answer is . . . they do. At least, they die much more regularly than those of us who live in developed countries do. Even though average life expectancy in developing countries has skyrocketed over the last few decades, in poor countries in sub-Saharan Africa it is only fifty-six years, compared to over seventy-eight years in the United States. In other dimensions, their lives are just as lacking as you'd expect given their earnings. In order to get a full picture of what life is like for the extreme poor, Professors Abhijit Banerjee and Esther Duflo, economists at MIT, conducted a survey of more than thirteen countries. They found that the extreme poor consume an average of

fourteen hundred calories per day—about half of what is recommended for a physically active man or a very physically active woman—while spending most of their income on food. The majority are underweight and anemic. Most households own radios but lack electricity, toilets, or tap water. Less than 10 percent of households possess a chair or a table.

There is, however, one way in which the $1.50-per-day figure can't quite be cashed out as "what $1.50 could buy in the United States in 2014." In the United States, because there is no extreme poverty, there is no market for extremely cheap goods. The lowest-quality rice you can buy in the United States is far better than what you could buy in Ethiopia or India. The room I rented in Ethiopia for one dollar a night was far worse than anything I could rent in the United States. (Trust me on this.) The very worst housing you can buy in the United States is *far* better than the mud-brick houses typical for those living below the $1.50-per-day extreme-poverty line. This explains how someone living in extreme poverty can still have a "home," but it doesn't do much, if anything, to improve life in extreme poverty.

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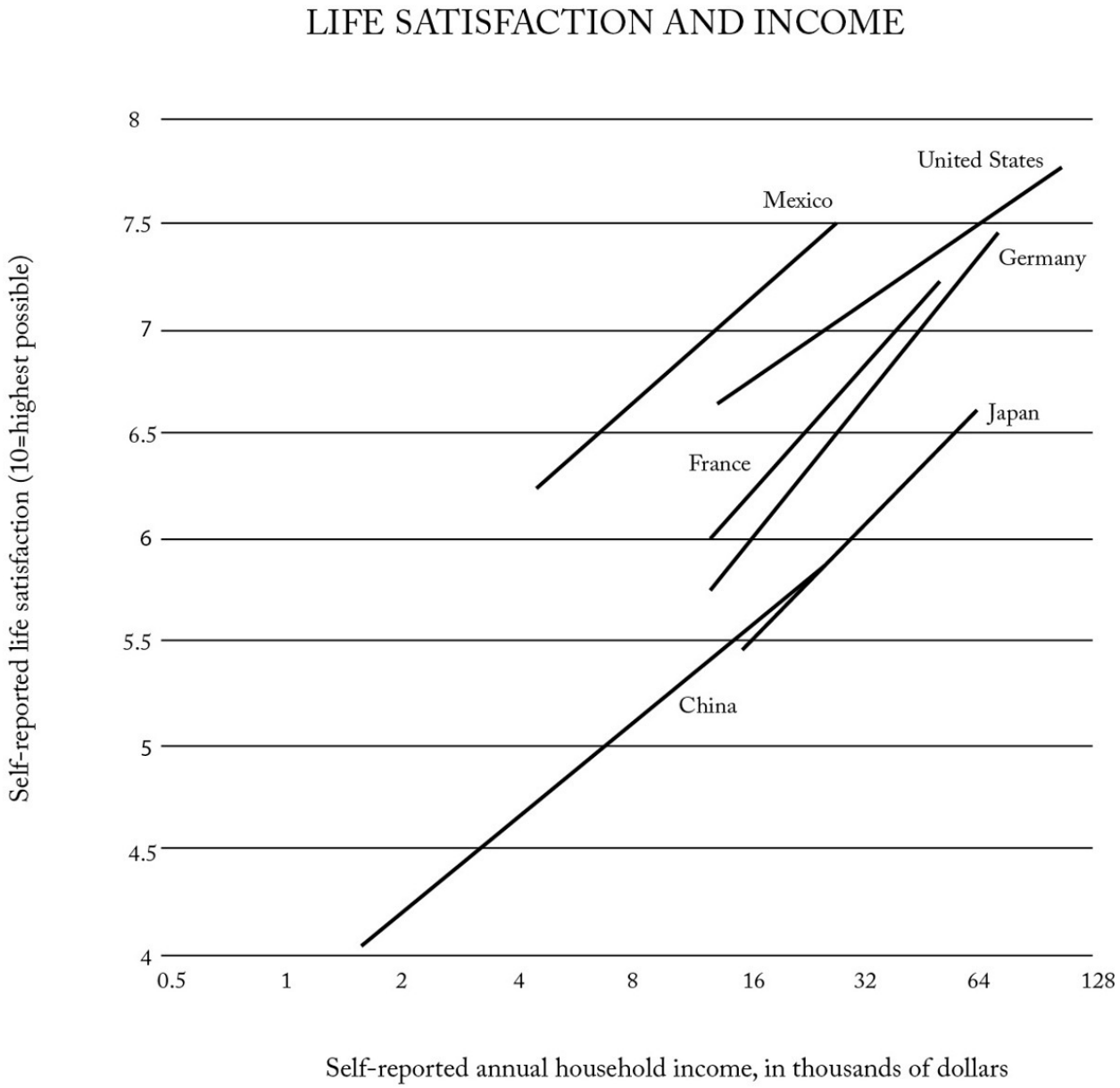
The fact that we've found ourselves at the top of the heap, globally speaking, provides us with a tremendous opportunity to make a difference. Because we are comparatively so rich, the amount by which we can benefit others is vastly greater than the amount by which we can benefit ourselves. We can therefore do a huge amount of good at relatively little cost.

Just how much good should we expect to be able to do? Let's very simplistically suppose that by some social action—giving to a development charity, buying fair-trade items, or something else—we make ourselves a dollar poorer and thereby make an Indian farmer living in extreme poverty a dollar richer. How much greater a benefit would that dollar provide the poor Indian farmer than it would provide for ourselves? It's a basic rule of economics that money is less valuable to you the more you have of it. We should therefore expect a dollar to provide a larger benefit for an extremely poor Indian farmer than it would for you or me. But how much larger?

Economists have sought to answer this question through a variety of methods. We'll look at some of these in the next chapter, but for now I'll discuss just one method, which is to ask people directly about their well-being. (Estimates via other methods would support my conclusion at least as well as

this one does.)

In order to work out the relationship between level of income and level of subjective well-being, economists have conducted large-scale surveys of income levels and the subjective well-being of people in each of them. Their results are given in this graph, which shows the relationship between income and subjective well-being both within a country and across countries.



Source: Betsey Stevenson and Justin Wolfers

The vertical axis of this graph represents self-reported well-being. Those

interviewed had to say how satisfied they were with their lives on a scale from 0 to 10. Rating yourself at 10 means you consider yourself maximally happy: you think that, realistically, life couldn't get any better. Rating yourself at 0 means you consider yourself maximally unhappy: you think that, realistically, life couldn't get any worse. Most people fall in the middle of this range. The horizontal axis represents annual income.

What's interesting about this graph is that a doubling of income will always increase reported subjective well-being by the same amount. For someone earning $1,000 per year, a $1,000 pay rise generates the same increase in happiness as a $2,000 pay rise for someone earning $2,000 per year, or an $80,000 pay rise for someone already earning $80,000 per year. And so on.

This graph allows us to determine just how much greater a benefit the extreme poor receive from one dollar than you or I do. Imagine if your boss called you into her office and told you your salary would double for the next year. You'd be pretty pleased, right? What the conclusions from the economic studies suggest is that the benefit you get from having your salary doubled is the same as the benefit an extremely poor Indian farmer gets from having his salary doubled. If you're on the typical US wage of $28,000 per year, the benefit you'd get from an additional $28,000 in income is the same as the benefit a poor Indian farmer would get from an additional $220.

This gives us a good theoretical reason for thinking that the same amount of money can do one hundred times as much to benefit the very poorest people in the world as it can to benefit typical citizens of the United States. If you earn as much as the typical American worker, then you are one hundred times as rich as the very poorest people in the world, which means additional income can do a hundred times as much to benefit the extreme poor as it can to benefit you or me. This isn't to say that income is all that matters to well-being—of course other factors like safety and political freedom are involved. But income certainly plays a critical role in how enjoyable, long, and healthy your life is. Looking at how much we can benefit people via increasing their income gives us a particularly robust way of assessing how much we can benefit others compared to ourselves.

It's not often you have two options, one of which is one hundred times better than the other. Imagine a happy hour where you could either buy yourself a beer for five dollars or buy someone else a beer for five cents. If that were the case, we'd probably be pretty generous—next round's on me! But that's effectively the situation we're in all the time. It's like a 99-percent-off sale, or getting 10,000 percent extra free. It might be the most amazing deal you'll see in your

life.

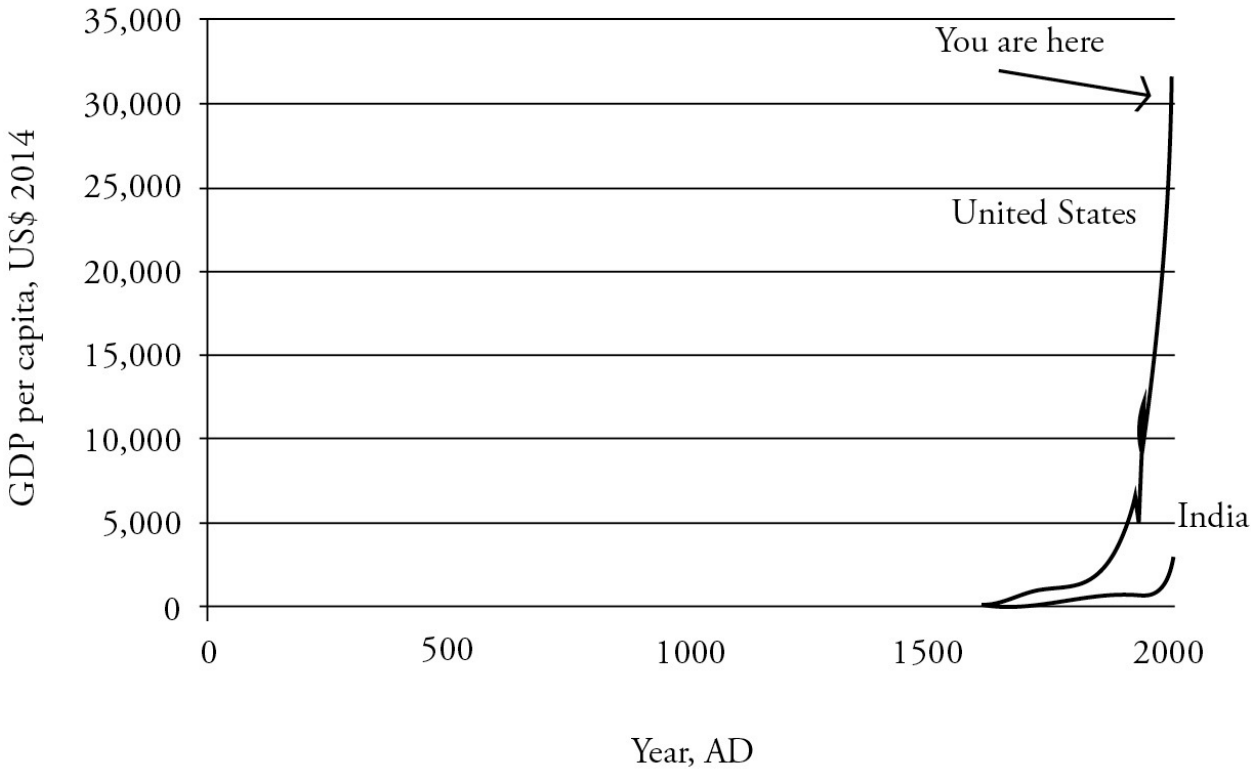
This idea is important enough that I've given it a name. I call it the 100x Multiplier. For those of us living in rich countries, you should expect to be able to do *at least* one hundred times as much to benefit other people as you can to benefit yourself.

The 100x Multiplier should surprise us. We shouldn't expect to be able to do so much to benefit others at such little cost to ourselves. But we live in an unusual place during an unusual time.

It's an unusual place because, if you're reading this book, then, like me, you're probably lucky enough to be earning $16,000 per year or more, putting you in the richest 10 percent of the world's population. That's a remarkable situation to be in.

It's an unusual time because it comes after a period of remarkable economic progress, which has led to some of the world experiencing what is, historically, fabulous wealth. In 1800, the gross domestic product per person per year in America was only $1,400 (in today's money), whereas now it's more than $42,000. In a mere two hundred years, we've become thirty times richer. But it is a time following remarkably *unequal* economic progress. Despite the riches of people like us, there are still billions living in abject poverty. This is highly unintuitive, as can be seen by this graph of gross domestic product per person, over the last two thousand years.

GDP OVER TIME



Source: Angus Maddison

For almost all of human history—from the evolution of *Homo sapiens* two hundred thousand years ago until the Industrial Revolution 250 years ago—the average income across all countries was the equivalent of two dollars per day or less. Even now, more than half of the world still lives on four dollars per day or less. Yet, through some outstanding stroke of luck, we have found ourselves as the inheritors of the most astonishing period of economic growth the world has ever seen, while a significant proportion of people stay as poor as they have ever been.

Moreover, because of that economic progress, we live at a time in which we have the technology to easily gather information about people thousands of miles away, the ability to significantly influence their lives, and the scientific knowledge to work out what the most effective ways of helping are. For these reasons, few people who have ever existed have had so much power to help others as we have today.

Sometimes we look at the size of the problems in the world and think,

“Anything I do would be just a drop in the bucket. So why bother?” But, in light of the research shown in these graphs, that reasoning doesn’t make any sense. It’s the size of the drop that matters, not the size of the bucket, and, if we choose, we can create an enormous drop. We’ve already seen that we have the opportunity to provide a benefit for others that is one hundred times greater than the benefit we could provide for ourselves. That we can’t solve all the problems in the world doesn’t alter in any way the fact that, if we choose, we can transform the lives of thousands of people.

THE FIVE KEY QUESTIONS OF EFFECTIVE ALTRUISM

TWO

HARD TRADE-OFFS

Question #1: How many people benefit, and by how much?

June 21, 1994. Kigali, Rwanda. Two months into one of the most horrific genocides the world has ever witnessed, James Orbinski manned a small Red Cross hospital, a tiny wellspring amid a moral wasteland.

The problems in Rwanda began to build up decades before, when the early Belgian colonialists had decreed that, of the native population, the minority Tutsi were racially superior to the more numerous Hutu. Under this regime, the Tutsi assisted the colonial rulers while Hutu were used as forced labor. This situation changed radically in 1959, when the Tutsi monarchy was overthrown and replaced with a Hutu republic and Rwanda became independent of Belgium. But things did not get better. The country's new leaders imposed dictatorial military rule and harvested the little wealth the country had for their own ends. Many of Rwanda's Tutsi fled to neighboring countries as refugees, and the country soon became one of the poorest in the world.

As the prosperity of the country declined, the Hutu's resentment toward the Tutsi grew. As time passed, the extremist ideology known as Hutu Power, explicitly based around racist anti-Tutsi principles, gained popularity. By 1990, Rwanda's leaders had begun arming Hutu citizens with machetes, razor blades, saws, and scissors; a new radio station had been set up to broadcast propaganda and hate speech; and attacks from the Tutsi refugee army, the Rwandan Patriotic Front, were being used to catalyze fear among the Hutu populace. By 1994, anti-Tutsi sentiment reached its zenith. On April 6, 1994, the Rwandan president was assassinated. The extremist Hutus blamed the Rwandan Patriotic Front, giving them the perfect opportunity to initiate their long-planned genocide.

By the time Orbinski found himself at that Red Cross hospital, hundreds of thousands of Tutsi had been killed. The UN was stalling, not wanting to admit

that a genocide was happening, and had provided almost no support. Only a handful of nonprofit workers remained in the country. Later in his life, Orbinski would become the president of Doctors Without Borders and accept the Nobel Peace Prize on its behalf, but at this time his role was simply to provide care for those who needed it, and with so many casualties, what could he do? He later recalled:

> There were so many, and they kept coming. Patients were taped with a 1, 2, or 3 on their foreheads: 1 meant treat now, 2 meant treat within twenty-four hours, and 3 meant irretrievable. The 3s were moved to the small hill by the roadside opposite the emergency room and left to die in as much comfort as could be mustered for them. They were covered with blankets to stay warm and given water and whatever morphine we had. The 1s were carried by stretcher to the emergency room or to the entrance area around it. The 2s were placed in groups behind the 1s.

I cannot comprehend what it was like for James Orbinski to see so many in pain at once and know he could help so few of them. I can only be thankful that I will never witness suffering of that magnitude. I imagine you feel the same.

However, there is a way in which Orbinski's situation is similar to ours. With so many casualties coming in, Orbinski knew he could not save everyone, and that meant he had to make tough choices: whom did he save, and whom did he leave to die? Not all could be helped, so he prioritized and engaged in triage. If it were not for that cold, calculating, yet utterly necessary allocation of 1s, 2s, and 3s, how many more lives would have been lost? If he had made no choice—if he had put his hands in the air and claimed defeat, or if he had simply tried to treat whoever came in first—he would have made the worst choice of all.

The reality of our world is such that, if we want to make the world a better place, we must make choices similar to those of Orbinski.

Suppose you have money you want to donate to charity. If you donate to Haiti earthquake relief, you help disaster victims. That means you have less money to fund antiretrovirals to fight HIV in Uganda, or to help the homeless in your hometown. As a result of your choices, someone is made better off and someone else is not. Confronted with the choice, you might be inclined to give to all these causes: make more room in your budget for increased charitable giving or divide your donation among several causes. But your time and money are limited and you cannot solve all the problems in the world. This means you need

to make some hard decisions: Whom do you choose to help?

Exactly the same problem arises for our use of time. If you have a spare couple of hours per week that you're happy to dedicate to helping others, how should you use them? Should you work at a soup kitchen? Join a mentorship program for troubled youth? Organize fund-raisers for your favorite charity? Again, there are far too many problems in the world and not enough time to solve them all. We need to prioritize.

Orbinski's situation was more salient than ours, since the potential beneficiaries were there in front of him, crying out for help. The fact that he *had* to make a choice, and that not choosing would itself be a decision, was inescapable. That we are not directly confronted with the competing beneficiaries of our charitable efforts and donations may lead us to take our situation less seriously than we would if we were in Orbinski's shoes, but it makes the situation no less real. There are literally billions of potential recipients of our help. Each one is a worthy beneficiary, someone who has real problems and whose life could be made better by our actions. We therefore need to make decisions about whom we choose to help, because failure to decide is the worst decision of all.

Effective altruism, at its core, is about confronting Orbinski's dilemma and trying our best to make hard trade-offs. Of all the ways in which we could make the world a better place, which will do the *most* good? Which problems should we tackle immediately, and which should we leave for another time? Valuing one action over another is difficult both psychologically and practically, but it is not impossible. In order to make comparisons between actions, we need to ask: How many people benefit, and by how much? This is the first key question of effective altruism.

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To begin to answer this question, we need to know the consequences of our actions. To illustrate, let's think about choosing which charity to give to. In order to assess your potential impact by donating to a charity, you need to know what exactly that charity will do with your donation.

For many charities, it's not clear what the answer is. For example, if you go onto the Salvation Army's website, you can read about the many programs they run, like soup kitchens, community support for veterans, emergency shelters for the homeless, and summer camps and after-school programs for children from

low-income families. If you dig into their website, you can see what proportion of their revenue they spend on broad categories of expenditure like "rehabilitation," "corps community center," and "other social services." Nowhere, however, do they tell you how much any of their programs cost, and therefore what your donation would achieve. Perhaps fifty dollars is enough to pay for one person's meals at a soup kitchen for a whole year, but from the information provided on the website, we can't know for sure.

This is such a typical state of affairs that you might not have thought about how astonishing it is. But imagine if you went into a grocery store and none of the products had prices on them. Instead, the storekeeper asks: "How much would you like to spend at this grocery store today?" When you give the storekeeper some money, he hands over a selection of groceries chosen by him.

This, of course, would be absurd. If this was how things worked, how could we figure out if one grocery store was better than another? One store could charge ten times the amount for the same produce, and, prior to actually paying for the products, we wouldn't be able to tell.

If it would be absurd to buy groceries this way, why is donating to charity any different? In the case of charity, you're buying things for other people rather than yourself, but if you're interested in using your money to help other people effectively, that shouldn't make a difference.

Sometimes, charities do tell you what you're getting with your money. For example, on its "Donate Now" webpage, the New York City branch of the United Way tells you that a donation of fifty dollars is enough to provide five books with parent guides to a family. This is a step in the right direction. But, even assuming that the "fifty dollars for five books" figure is accurate, it's still not that useful because we don't know what benefits those books provide. We care about providing books only if doing so will lead to things that really are of value. Do the books help children do better in school? Do they enrich a family's lives through a better understanding of the world? If those extra books don't actually improve anyone's lives, then your fifty-dollar donation is worthless.

We can overcome this problem by thinking in terms of *improving lives* rather than in terms of intermediate metrics like number of schoolbooks provided. In order to truly make comparisons between different actions, we need to measure impact in terms of the size of the benefits we confer through those actions.

In some cases, it's relatively clear which action will provide a larger benefit. Let's think about Orbinski again. Saving someone from dying provides a larger

benefit than saving someone from losing a limb, so if Orbinski had to choose between one or the other, he should save the life. Similarly, he would provide a larger benefit by saving five lives than by saving one. So, if, for example, he could provide five simple lifesaving surgeries in the time it would take to provide one more-complicated lifesaving surgery, it's clear he should opt to perform the simpler surgeries.

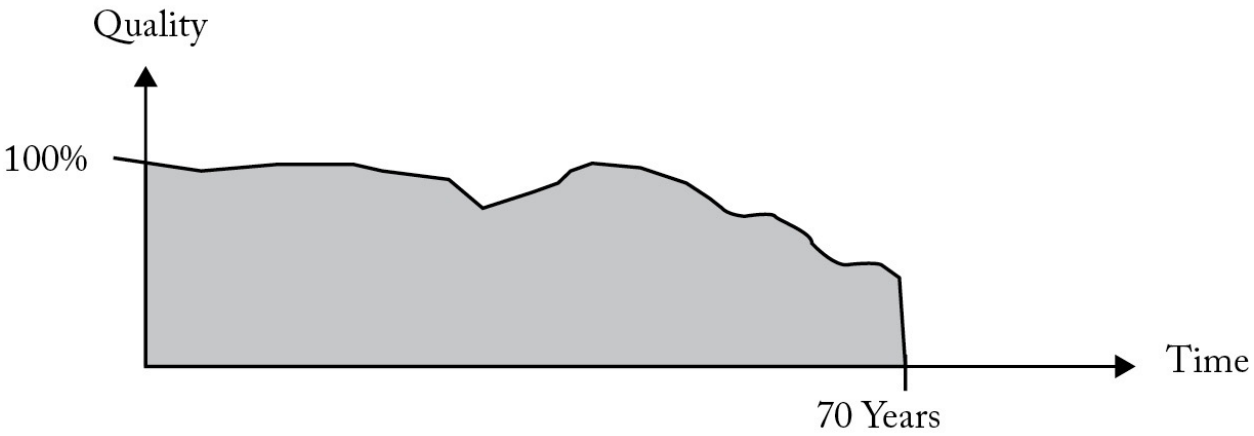
However, there are many harder cases: If you can prevent the death of a five-year-old or a twenty-year-old, which should you do? What if you can prevent ten people from suffering from AIDS or one hundred people from suffering from severe arthritis? What about preventing one woman from being domestically abused versus enabling one child to go to school?

For health benefits, economists have spent decades conducting research in order to answer questions like these. They have developed a metric called the quality-adjusted life year, or QALY (pronounced "kwalee"), in order to help make decisions about how to prioritize among different health programs.

The idea behind the QALY is that there are two ways you can give a health benefit to someone. First, you can "save someone's life." (I use quotes here because "saving" a life, of course, only ever means extending someone's life.) The second way to benefit someone is to improve the quality of their life during the time they are alive. Migraines don't kill people, but, as someone who occasionally suffers from them, I know that life is better without them.

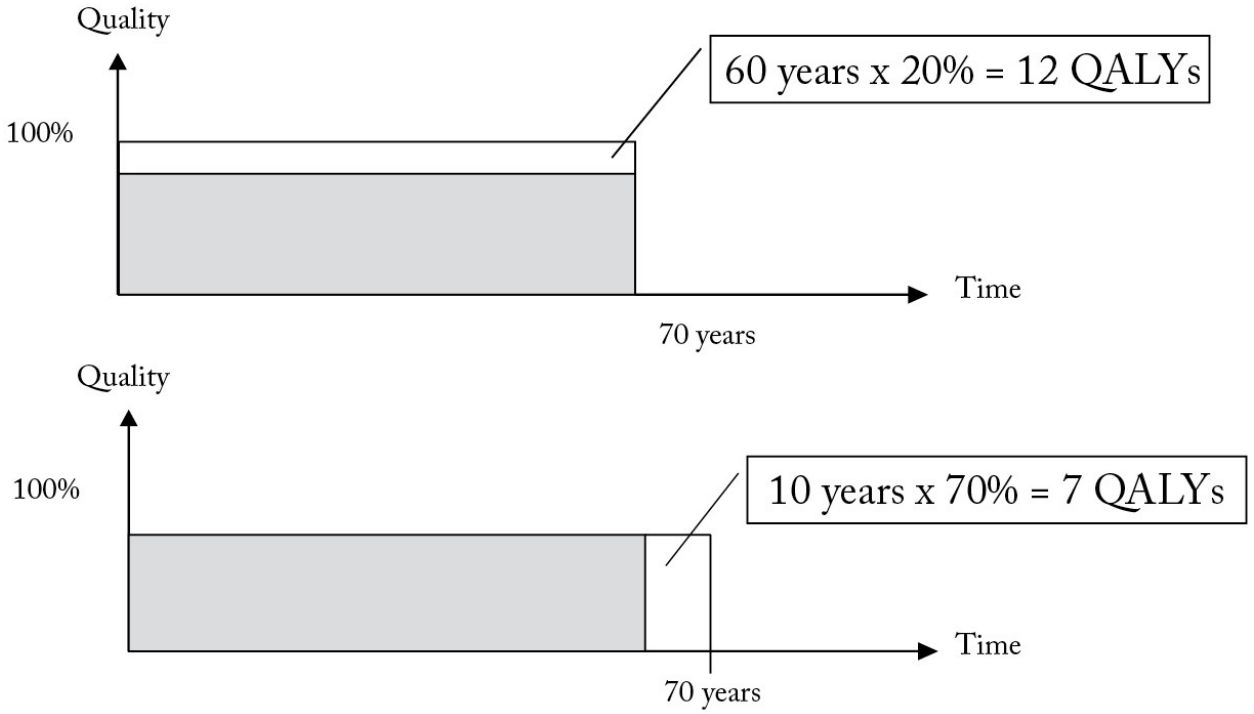
The QALY combines these two benefits into one metric, using survey data about the trade-offs people are willing to make in order to assess how bad different sorts of illnesses or disabilities are. For example, people on average rate a life with untreated AIDS as 50 percent as good as life at full health; people on average rate life after a stroke as 75 percent as good as life at full health; and people on average rate life with moderate depression as only 30 percent as good as life in full health.

We can illustrate the QALY metric using graphs. Here's a graph that illustrates a fairly typical life, representing how well the person's life goes over time.



This graph shows a person who lives most of his life very healthy, who has some mild health troubles at age thirty-five but then gets better. His health begins to deteriorate as he enters old age, until his death at seventy-two.

The next two graphs show the two ways you can benefit someone's life.



The first graph illustrates improving the quality of someone's life by 20 percent for the sixty years of their life. That would amount to twelve QALYs in total (60 x 20% = 12). The second graph illustrates extending the life of someone who is currently at 70 percent health by ten years. That amounts to seven QALYs overall (10 x 70% = 7). The QALY metric therefore allows us to measure the size of benefits that different people receive.

If you want, you can come up with your own personal quality weights. Think of an ailment that you have suffered from at some point in your life. Suppose you suffer from back pain, as I sometimes do. You could think to yourself: If 10 represents how good my life is when I'm perfectly healthy, how good is my life on a day when I'm suffering from back pain? This can be difficult to answer, so a way to make your judgments more precise is to ask yourself about what sorts of trade-offs you'd make. If you could live one extra day at perfect health or a certain larger number of days living with back pain, at what point would you be indifferent? In my own case, I'd be indifferent between living an extra four days perfectly healthy, or an extra five days with back pain. That suggests that I think life with back pain is 80 percent as good as life pain-free. In the endnotes, I link to some official lists of quality-of-life estimates, to help you assess the severity of different conditions yourself.

Economists have used the QALY metric to assess the cost-effectiveness of different health treatments. They test a certain program, assess how much it costs and what health improvements it provides, and then translate those health improvements into QALYs. After doing this for a number of different programs, they can make comparisons between these programs to see which provides the largest benefit for a given amount of money. If you have limited resources, then, other things being equal, you should spend those resources in whatever way will provide the most QALYs.

To illustrate, consider a simple hypothetical example. Suppose that you have $10,000, and with that money you could pay for antiretroviral therapy for a forty-year-old who has AIDS or a surgery to prevent blindness in a twenty-year-old, but not both. Without the antiretroviral therapy, the forty-year-old would die in five years' time; with the antiretroviral therapy, the forty-year-old would die in ten years' time. The twenty-year-old will live to the age of seventy whether or not she receives the blindness-preventing surgery. (Of course, in the real world we can never know exactly how long people will live, so to perform this calculation we'd have to use average life expectancy.) Should you pay for the antiretroviral therapy or for the surgery? QALYs can help us to make that decision. First, we assess the size of the benefit from the antiretroviral therapy. People rate the quality of life with AIDS while not receiving antiretroviral therapy at 50 percent, and rate the quality of life with AIDS while receiving antiretroviral therapy at 90 percent. By providing antiretroviral therapy to the forty-year-old, you'd therefore increase her quality of life from 50 percent to 90 percent for five years, and give her an extra five years of life at 90 percent

health. That equals 6.5 QALYs (because [90% – 50%] × 5 + [90% × 5] = 6.5).

Second, we assess the size of the benefit from the blindness-preventing surgery. People rate the quality of life while blind at 40 percent. By preventing the blindness of the twenty-year-old, you'd therefore increase her quality of life from 40 percent to 100 percent for fifty years. That equals thirty QALYs (because [100% – 40%) × 50 = 30). This tells us that you'd provide a larger benefit by paying for the surgery than for paying for the antiretroviral therapy. All other things being equal, that suggests you should pay for the surgery rather than the antiretroviral therapy.

QALYs are imperfect, as any measure of health benefit will be. For example, on average, people who have never been on dialysis estimate that if they were on dialysis, their health-related quality of life would be 39 percent, whereas people who actually are on dialysis on average rate their health-related quality of life at 56 percent. The same is true for other medical conditions: patients tend to regard their conditions as less bad than the general public does. Is this because the general public doesn't really understand what life with the medical condition is like and overestimates how bad it is? Or is it because patients have subconsciously lowered the standard for what they regard as 100 percent health? It's difficult to know, and academics continue to debate the topic. Similarly, some people think that we should give special importance to preventing the deaths of younger people, or give special weight to those who are particularly badly off. These are contested issues, and they are unlikely to be resolved any time soon.

For our purposes, however, it's often not important for us to have precise numbers on how good or bad different conditions are. As we'll see in the next chapter, programs differ dramatically in how great an impact they have, so even a rough idea of how many people are affected, and by how much, is often enough to show that one program has a much larger impact than another.

In this book, I'll talk about QALYs quite a lot. That's not because I think the only way to make a difference is to improve someone's health. Rather, it's because, as I'll explain in the coming chapters, many of the best, most concrete, and easiest-to-measure ways of doing good involve improving global health. We've also got much better data for health programs than for many other sorts of activity. Since the goal of effective altruism is to do the most good we can, health is a good place to start.

Moreover, in principle, the same methods that were used to create the QALY could be used to measure the costs and benefits of pretty much anything.

We could use these methods to estimate the degree to which your well-being is affected by stubbing your toe, or by going through a divorce, or by losing your job. We could call them well-being-adjusted life years instead. The idea would be that being dead is at 0 percent well-being; being as well off as you realistically can be is at 100 percent well-being. You can compare the impact of different activities in terms of how much and for how long they increase people's well-being. In chapter one we saw that doubling someone's income gives a 5-percentage-point increase in reported subjective well-being. On this measure, doubling someone's income for twenty years would provide one WALY.

Thinking in terms of well-being improvements allows us to compare very different outcomes, at least in principle. For example, suppose you were unsure about whether to donate to the United Way of New York City or to Guide Dogs of America. You find out that it costs Guide Dogs of America approximately $50,000 to train and provide one guide dog for one blind person. Which is a better use of fifty dollars: providing five books, or a 1/1,000th contribution to a guide dog? It might initially seem like such a comparison is impossible, but if we knew the impact of each of these activities on people's well-being, then we could compare them.

Suppose, hypothetically, that we found out that providing one guide dog (at a cost of $50,000) would give a 10-percentage-point increase in reported well-being for one person's life over nine years (the working life of the dog). That would be 0.9 WALYs. And suppose that providing five thousand books (at a cost of $50,000) provided a 0.001-percentage-point increase in quality of life for five hundred people for forty years. That would be two WALYs. If we knew this, then we'd know that spending $50,000 on schoolbooks provided a greater benefit than spending $50,000 on one guide dog.

The difficulty of comparing different sorts of altruistic activity is therefore ultimately due to a lack of knowledge about what will happen as a result of that activity, or a lack of knowledge about how different activities translate into improvements to people's lives. It's not that different sorts of benefits are *in principle* incomparable.

Not everyone agrees with this. For example, in 2013, the CEO of Charity Navigator (a charity evaluation service that I'll discuss in chapter seven), Ken Berger, and his colleague Robert M. Penna wrote a critical piece on effective altruism for the *Stanford Social Innovation Review* blog. They objected that the comparing of one cause to another "amounts to little more than charitable

imperialism, whereby ‘my cause’ is just, and yours is—to one degree or another—a waste of precious resources.” As they clarified in correspondence, Berger and Penna believe that “it’s impossible to weigh one person’s interests against another’s”; they therefore think it’s immoral to try to determine which causes are most effective.

However, their view simply cannot be correct. If Berger and Penna were right, then we couldn’t say that giving someone an extra dessert is a smaller benefit than saving someone’s life. Nor could we say that you do more good by saving one million lives than by saving ten. We would have to conclude that nurses who engage in triage—ensuring that doctors don’t spend their time treating mild coughs when they could be treating heart attacks—have no basis for their decisions. But that would be absurd. It might be difficult, both emotionally and practically, to weigh different people’s interests against each other, but it’s not impossible in principle.

Let’s consider a different objection. Doesn’t the focus on trying to benefit others as much as possible neglect the fact that you might have a closer personal connection to some causes rather than others? If a family member died of cancer, isn’t it natural to want to direct your energies to fighting cancer? Shouldn’t you focus on that cause, even if you could theoretically do more good elsewhere?

I feel the pull of this objection. For example, in 2009, when setting up Giving What We Can, I was trying to find those charities that do the most good with every dollar they receive. In the course of doing this, I came across the Fistula Foundation. Obstetric fistulas are truly awful conditions: a hole between a women’s vagina and bladder or rectum, through which urine or feces leak uncontrollably. They are generally caused by prolonged labor during childbirth, though they can be caused by rape or sexual abuse. They occur almost entirely in poor countries, where malnutrition results in women having an underdeveloped pelvis, and where there are insufficient medical resources available to perform a cesarean. The fistula causes incontinence, and the women who suffer from them often end up ostracized from their communities, unable to get work.

The primary recipient of the Fistula Foundation’s revenue is the Hamlin Fistula Hospital in Addis Ababa, Ethiopia. They treat fistulas through surgery and provide follow-up care, counseling, and education. It was clearly a highly worthy cause, doing a huge amount of good. Ultimately, however, I concluded that you’d probably have an even bigger impact on people’s lives by donating elsewhere. (I’ll discuss what causes I believe to be most effective later in the book.)

But there was a catch. When I'd visited Ethiopia several years before, I'd visited this hospital. I'd hugged the women who suffered from this condition, and they'd thanked me for visiting them. It had been an important experience for me: a vivid firsthand demonstration of the severity of the problems in the world. This was a cause I had a personal connection with.

Should I have donated to the Fistula Foundation, knowing I could do more to help people if I donated elsewhere? I do not think so. If I were to give to the Fistula Foundation rather than to the charities I thought were most effective, I would be privileging the needs of some people over others merely because I happened to know them. That would be unfair to those I could have helped more. If I'd visited some other shelter in Ethiopia, or in any other country, I would have had a different set of personal connections. It was arbitrary that I'd seen this problem close up rather than any of the other problems in the world.

Similar thoughts apply to deciding what cause to focus on more generally. For example, if an uncle dies of cancer, you might naturally want to raise money for cancer research. Responding to bereavement by trying to make a difference is certainly admirable. But it seems arbitrary to raise money for one specific cause of death rather than any other. If that family member had died of a different illness, it would have been no less tragic. What we care about when we lose someone close to us is that they suffered and died before their time, not that they died from a specific cause. By all means, we should harness the sadness we feel at the loss of a loved one in order to make the world a better place. But we should focus that motivation on preventing death and improving lives, rather than preventing death and improving lives in one very specific way. Any other decision would be unfair to those whom we could have helped more.

• • •

If we want to do as much good as we can, we need to think about what the consequences of our actions will be. Moreover, we need to think about how our actions will turn into improvements to people's lives. When making decisions, whether it's in volunteering, choosing a career, or deciding to buy "ethical" produce, we should therefore ask: How much does this activity cost, in terms of time or money? How many people does it affect? And, crucially: By how much does it improve people's lives?

This is the first step toward addressing the hard question of how to allocate our limited time and money. The crucial second step is to realize the importance

of focusing on the *best* activities. Let’s turn to that.

THREE

HOW YOU CAN SAVE HUNDREDS OF LIVES

Question #2: Is this the most effective thing you can do?

In 2009, a Zambian-born economist, Dambisa Moyo, published a book called *Dead Aid: Why Aid Is Not Working and How There Is a Better Way for Africa*, in which she argued that "aid is malignant" and should stop. She summed up her views early on in the book: "So there we have it: sixty years, over US$1 trillion dollars of African aid, and not much good to show for it." Her message rang true for many, and her book was a bestseller.

She's not alone in her anti-aid sentiment. In 2006, William Easterly, an economist at New York University, wrote a book entitled *The White Man's Burden*. Easterly's book, which popularizes the view that aid has been ineffective at best and harmful at worst, has become the bible for aid skeptics—those who believe international aid efforts have been a waste of time and energy. He writes: "The other tragedy of the world's poor . . . is the tragedy in which the West spent $2.3 trillion on foreign aid over the last five decades and still had not managed to get twelve-cent medicines to children to prevent half of all malaria deaths. The West spent $2.3 trillion and still had not managed to get six-dollar bed nets to poor families. The West spent $2.3 trillion and had still not managed to get three dollars to each new mother to prevent five million child deaths."

I endorsed something like aid skepticism for quite a long time. After I graduated college, I decided against applying to jobs at nonprofits partly because of stories I'd heard about food aid being stolen and sold by corrupt governments, and assumed there was no way I could have an impact under these conditions. I donated to development charities, but I always felt uneasy about whether I was actually helping others or merely alleviating my own sense of guilt about being

born privileged in a world with so much need.

I've since realized that I was thinking about development in entirely the wrong way. The picture that aid skeptics paint is highly misleading and, even more important, isn't particularly relevant for people who want to do good.

One error skeptics make is emphasizing in their critiques the amount of money spent. A trillion dollars of aid spending, which Moyo appealed to in her book, sounds like a lot of money, but, to the average person, it's too great a sum to comprehend, so we need to put it into context. The total annual economic output of the world is $87 trillion; the United States spends about $800 billion on social security every year; a decade of cosmetics sales amounts to $1.7 trillion; and in 2001, Donald Rumsfeld mentioned that the US military had simply *lost track* of $2.3 trillion. In global terms, therefore, $1 trillion is not very much money. We can see this even more clearly once we translate this figure into more meaningful terms. Over sixty years of aid spending, $1 trillion is slightly less than $17 billion per year. Divided by 412 million people—the average population of sub-Saharan Africa during that time period—that's only forty dollars per recipient per year. When we take into account the fact that the $1 trillion in aid spending must be divided among a very large number of people over many decades, we see that the amount of aid spent per recipient is very small indeed.

Second, the claim that there is "not much to show for it" is simply false. Even among the "bottom billion"—the population of countries that have experienced the weakest economic growth over the last few decades—quality of life has increased dramatically. In 1950, life expectancy in sub-Saharan Africa was just 36.7 years. Now it's 56 years, a gain of almost 50 percent. The picture that Dambisa Moyo paints is inaccurate. In reality, a tiny amount of aid has been spent, and there have been dramatic increases in the welfare of the world's poorest people.

Of course, correlation is not causation. Merely showing that the people's welfare has improved at the same time the West has been offering aid does not prove that aid *caused* the improvement. It could be that aid is entirely incidental, or even harmful, holding back even greater progress that would have happened anyway or otherwise. But in fact there's good reason to think that, on average, international aid spending has been incredibly beneficial. Moyo points to aid's inefficiencies by focusing on *typical* aid programs. But to get a true picture of how much benefit the developing world has received from aid, one needs to focus instead on the *best* aid programs.

A good contender for the best aid program ever is the eradication of smallpox. Smallpox was a horrific disease. The infection would present itself initially like the flu, resulting in fever, muscle pain, malaise, and headaches. After two weeks, small lesions appeared on the mouth, tongue, and throat. Soon afterward, fluid-filled blisters developed on the skin: first the forehead, then the face, then the rest of the body. Those who were infected would be badly disfigured for the rest of their lives, and about 30 percent would die. In the twentieth century alone, smallpox killed more than three hundred million people. Fortunately, in 1977, we eradicated it.

It's difficult to comprehend just how great an achievement this was, so let's make a comparison. Suppose we'd achieved world peace in 1973. How many deaths would have been prevented? That timescale includes the killings of Cambodia's Khmer Rouge, the Rwandan genocide, the two Congo wars, the 9/11 attacks and the wars in Afghanistan and Iraq. If you add up all the wars, genocides, and terrorist acts that occurred since 1973, the death toll is a staggering twelve million. Prior to its eradication, smallpox killed 1.5 to 3 million people every year, so by preventing these deaths for over forty years, its eradication has effectively saved somewhere between 60 and 120 million lives. The eradication of smallpox is one success story from aid, saving five times as many lives as world peace would have done.

Just for the sake of argument, let's be generous to the aid skeptics. Let's suppose that, over the last six decades, foreign aid achieved absolutely nothing except eradicating smallpox. A simple calculation shows that *even if this were true*, foreign aid would still be a bargain. The total aid spending of all countries over the last five decades is $2.3 trillion (Moyo's $1 trillion figure was aid just to Africa). That means that, using the low estimate of the benefits of smallpox, at sixty million lives saved, foreign aid has saved a life with every $40,000 spent. In comparison, government departments in the United States will pay for infrastructure to improve safety if doing so costs less than about $7 million per life saved; the precise figures are $9.1 million for the Environmental Protection Agency, $7.9 million for the Food and Drug Administration, and $6 million for the Department of Transportation. This means that, even if aid had achieved absolutely nothing except eradicating smallpox, it still would have prevented a death for 1/150th of the cost that the United States is currently willing to spend to save the lives of its own citizens.

This calculation not only shows that aid has "worked," but also that it's been cost-effective on average. Moreover, this calculation significantly

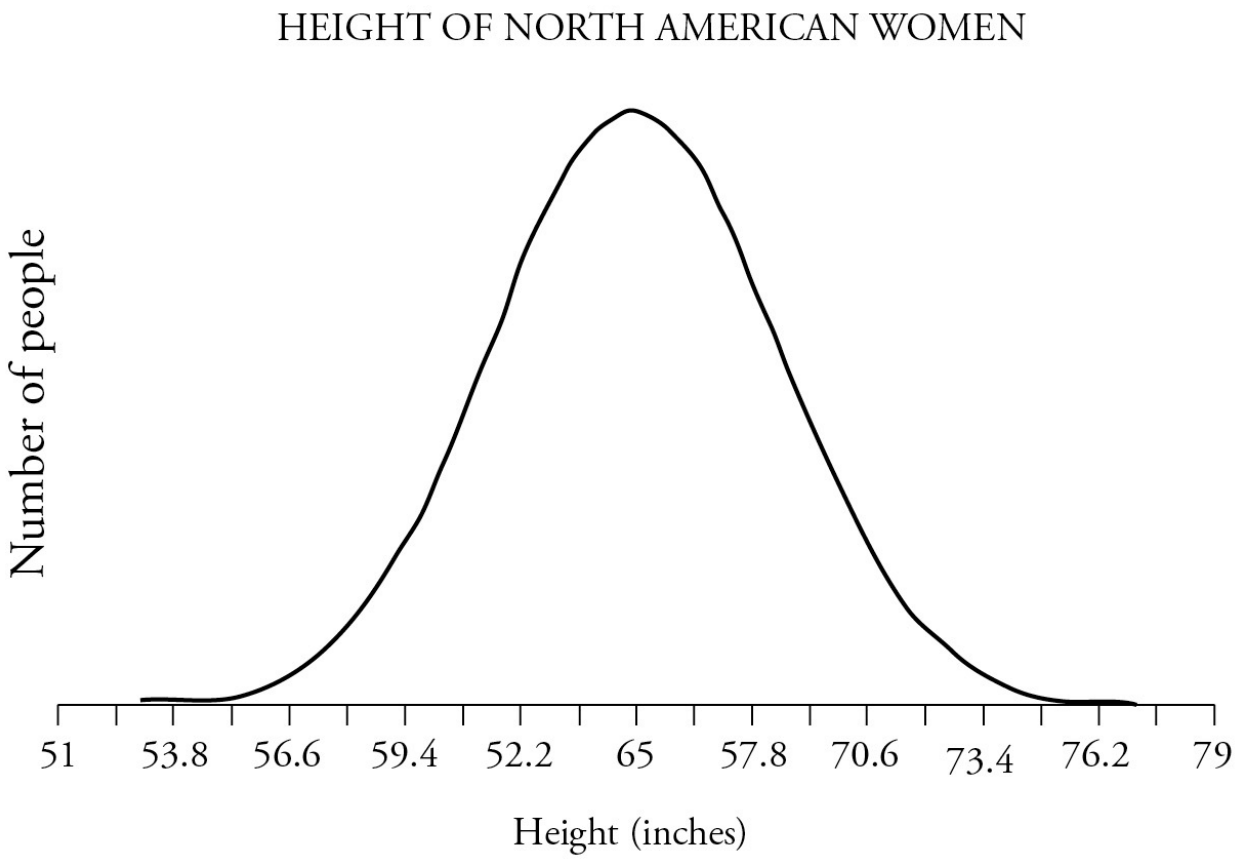
underestimates the positive impact of aid. Thanks to immunization, annual deaths from preventable illnesses have declined from 5 million in 1960 to 1.4 million in 2001, despite world population doubling in that time. Annual malaria deaths have declined from 3.8 million to about 0.7 million. Annual diarrheal deaths have declined from 4.6 million to 1.6 million. Aid isn't responsible for all of these reductions, but it is responsible for a proportion of them, and that's despite the fact that the amount of money spent on aid is tiny in global terms.

Indeed, even aid skeptics agree that the best sorts of development programs, especially those within global health, are very effective. For example, William Easterly (the author of *The White Man's Burden*) notes that "there are well-known and striking donor success stories, like the elimination of smallpox, the near-eradication of river blindness and Guinea worm, the spread of oral rehydration therapy for treating infant diarrheal diseases, DDT campaigns against malarial mosquitoes (although later halted for environmental reasons), and the success of WHO vaccination programs against measles and other childhood diseases." He summarizes his view by commenting that "even those of us labeled as 'aid critics' do not believe aid has been a universal failure. If we give you aid agencies grief on failures, it is because we have seen some successes, and we would like to see more!"

There are certainly many examples of attempts to help those that do little good: PlayPumps is just one example. But when evaluating whether aid has worked *on average*, it's not enough to look at *typical* cases of aid; you also need to look at the *best* cases. In the context of doing good, this is vital, because the best activities are often far superior to typical ones, which can make the average benefits of aid spending very high, even if typical benefits are small.

• • •

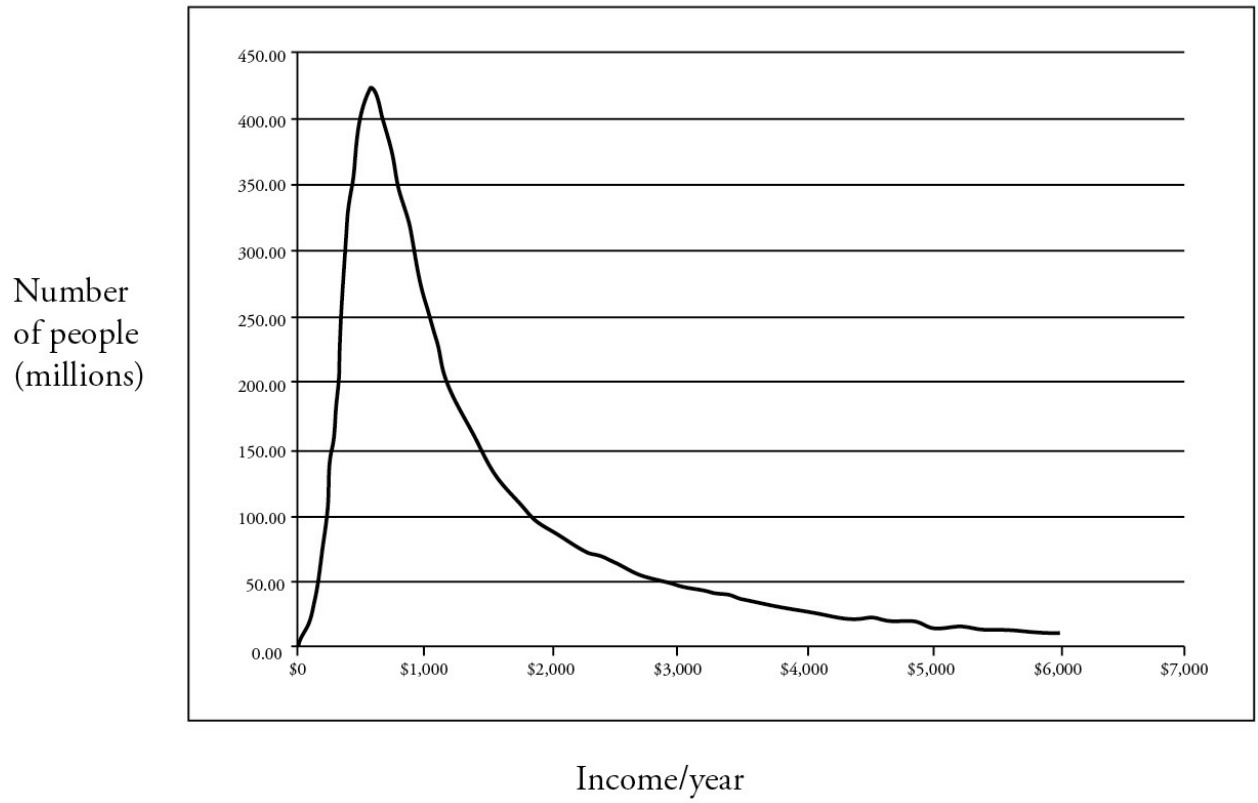
We're used to thinking of what's typical and what's average as being the same. For example, if you measured the height of all women in North America and plotted those heights on a graph, you get this:



The height of a typical woman in North America (that is, a woman who is taller than 50 percent of people and shorter than 50 percent of people) is five feet five inches; the height of the average woman (which is equal to the total height of all women divided by the number of women) is also five feet five inches. In the case of height, what is typical and what is average is the same. This sort of distribution is what we're most familiar with, so it's aptly called a *normal* distribution.

But this isn't always true. Look at the following graph, which, like the graph in chapter one, represents global income distribution.

GLOBAL INCOME DISTRIBUTION



This graph shows how many people live in various income brackets. Notice how different this is from the distribution of height. In this graph, the right-hand "tail" of the curve just keeps going. In fact, in order to make the shape of the curve visible on the page, I had to cut off the graph at $6,000 per year, even though 20 percent of the world earns more than that.

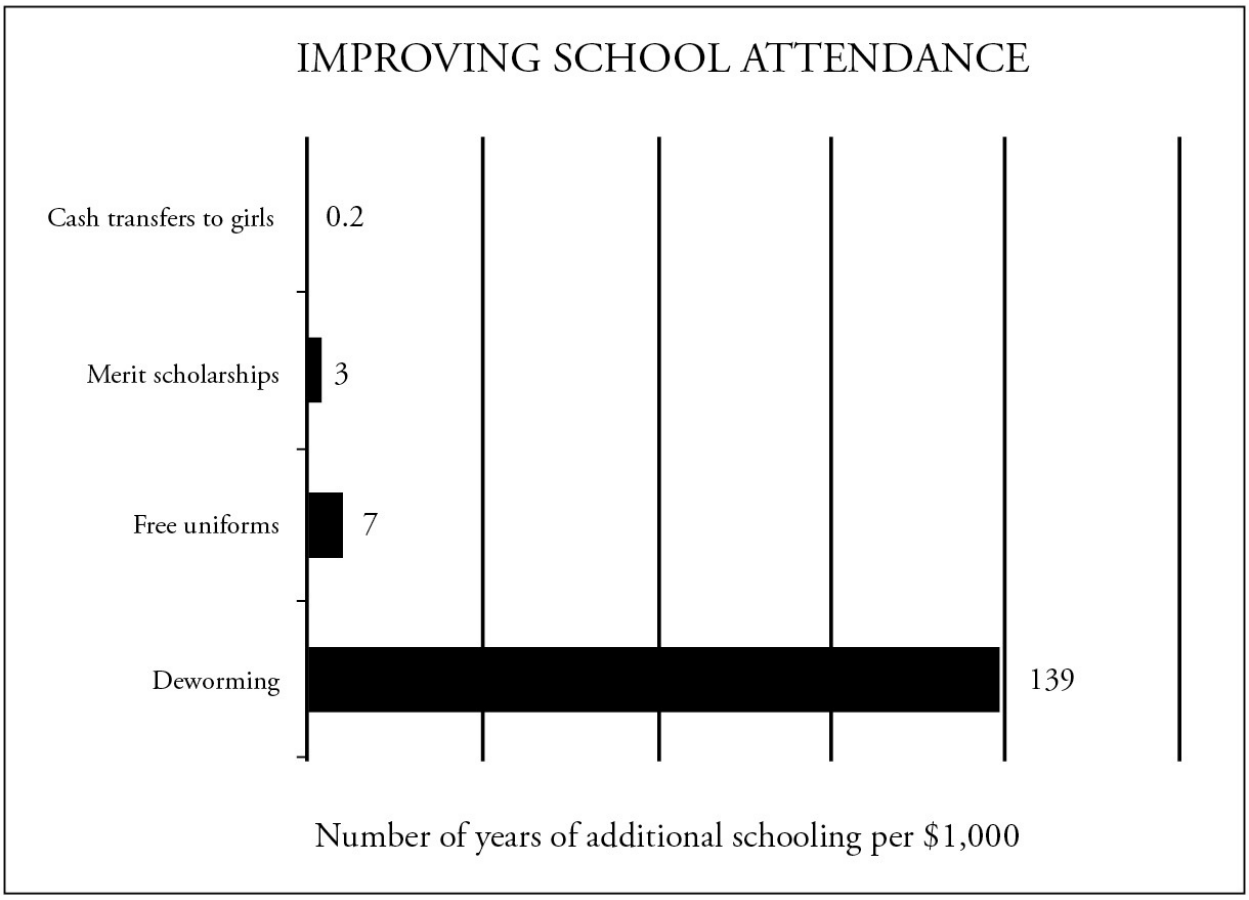
Distributions that look like this are called *fat-tailed* distributions. (You might have heard of the 80/20 rule: that 80 percent of the value of a set of activities comes from the best 20 percent of activities. That rule describes a fat-tailed distribution.) Fat-tailed distributions are interesting because they're marked by extreme events. Whereas there are very few extremely small or extremely tall people, there are a relatively large number of extremely rich people. (If height were distributed like income is, we would regularly see people towering 270 feet tall, peering over skyscrapers.) That's why the world's average income, which is $10,000 per year, is so much higher than the typical income, which is only $1,400 per year: the richest people bring up the average.

For this reason, fat-tailed distributions are unintuitive. That's partly why it's so difficult to understand income inequality. We don't realize that we're extreme outliers. In fact, fat-tailed distributions are fairly common. For example, most

people live in a small number of cities; most people who have died in an earthquake died in one of the relatively rare catastrophic ones; a small number of words make up the majority of most printed text (which means that, if you want to learn a language, you're better off learning the one thousand or so most common words first). When it comes to doing good, fat-tailed distributions seem to be everywhere. It's not always true that exactly 80 percent of the value comes from the top 20 percent of activities—sometimes things are even more extreme than that, and sometimes less. But the general rule that most of the value generated comes from the very best activities is very common.

The effectiveness of different aid activities forms a fat-tailed distribution, and this fact is very important if we want to make a difference. In response to Dambisa Moyo, I pointed out that, because the best programs are so good, they make aid very effective on average. But we don't need to fund programs of merely average effectiveness. We can deliberately choose to fund only the very best programs, which allows us to do a *tremendous* amount of good.

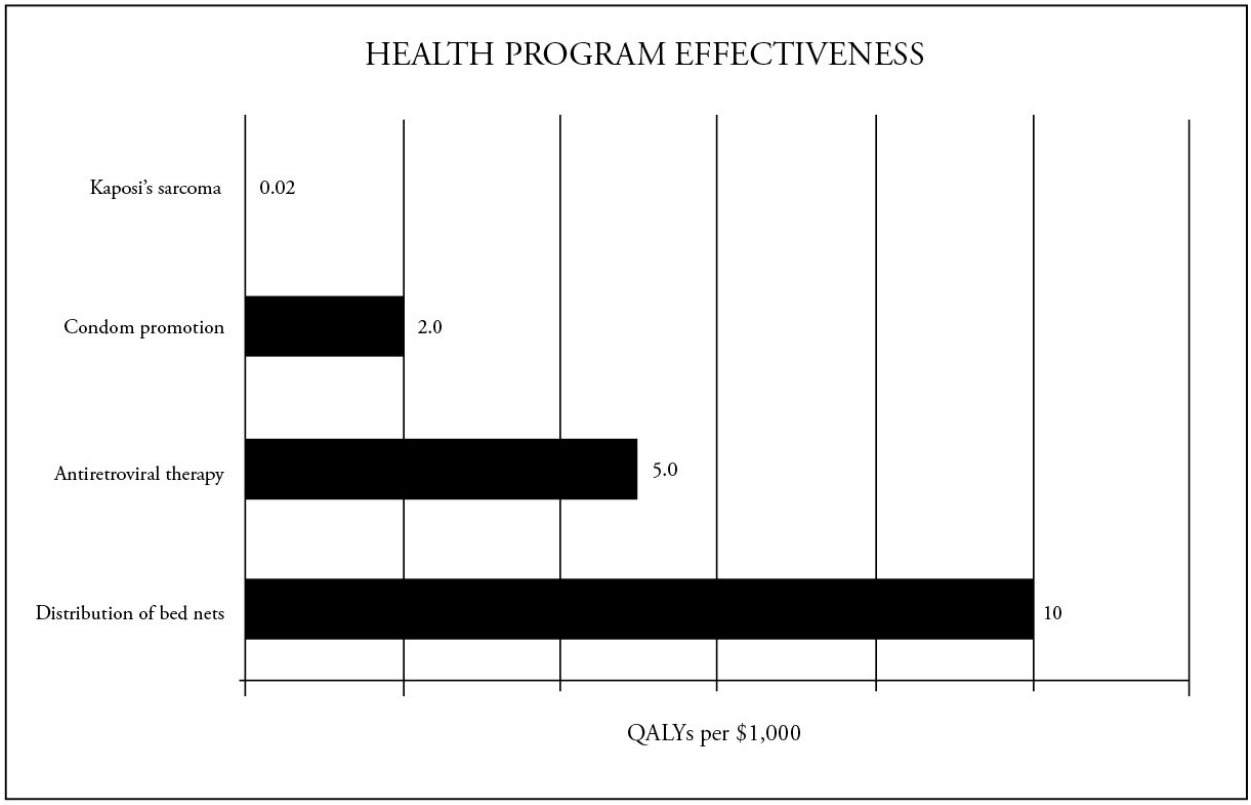
To see how this plays out, let's consider two types of aid programs. First, developing-world education:



All the programs listed in this graph are programs that "work," in the sense that they have a measurable positive impact. But the differences between these four estimates are enormous. Providing cash rewards to girls who stay in school yields an additional 0.2 years of school attendance with every $1,000 spent. Providing free primary-school uniforms does ten times better, resulting in 7.1 additional years of school attendance for every $1,000 spent. But deworming schoolchildren does fifteen times better than that, with 139 total years of school per $1,000.

In the context of helping others, the difference between a good use of money and a great use of money is huge. We shouldn't just ask: Is this program a good use of money? We need to ask: Is this program the *best* use of money?

The same phenomenon occurs with respect to developing-world health. This graph lists the estimated cost-effectiveness of different health programs, measured in QALYs (where one QALY, remember, represents a benefit equivalent to giving one person one year of life in perfect health).



These results are even more amazing than those for school attendance. Consider Kaposi's sarcoma, a cancer that occurs in those with HIV and typically causes disfiguring purple tumors on the skin and in the mouth. Kaposi's sarcoma

can cause painful swelling of the legs and feet and it can be life-threatening if the tumors occur in the lungs, liver, or digestive tract. One estimate puts the cost-effectiveness of surgery to remove Kaposi's sarcoma, which produces mainly cosmetic benefit, at about $50,000 per QALY.

Spending money to treat Kaposi's sarcoma is clearly a good deal, costing less than the governments of the United States or the United Kingdom are willing to spend to provide one QALY and less than I would be willing to spend to give myself an extra year of perfect health. But treating Kaposi's sarcoma is clearly not the best use of money if we wish to help people in the developing world. On these estimates, by promotion of condom use, we can do one hundred times as much to benefit people than we can by treating Kaposi's sarcoma; by providing antiretroviral therapy we provide two and a half times the benefit again. Moreover, the QALY allows us to make comparisons across very different programs that combat very different illnesses. By donating to the Against Malaria Foundation, which buys and distributes long-lasting insecticide-treated bed nets, you would, by this estimate, provide five hundred times the benefit as you would by spending the same amount of money treating Kaposi's sarcoma.

Once again, we see the importance of focusing on the very best activities. We need to ensure we're making not just *a* difference but the *most* difference we can.

Importantly, the cost-effectiveness estimates given are just that: estimates. The figures for Kaposi's sarcoma, condom distribution, and antiretroviral therapy are individual estimates based on specific contexts and may therefore be optimistic. The figure for bed-net distribution is more robust—the calculation behind it tries to correct for biases in favor of optimism, and takes into account the specific context in which the charities work—but even this estimate should not be taken as gospel. However, in the context of fat-tailed distributions, even rough estimates are vitally important for decision making. In the health-care graph, the best program is estimated to be five hundred times more effective than the worst program (which, remember, is still a good program). Even if the highest estimates were too optimistic by a factor of fifty, it would still be vitally important to focus on the best programs rather than merely good ones.

• • •

What we've seen is that thinking carefully about how you can do the most to

benefit others doesn't just allow you to do a *bit* more good—it enables you to do vastly more than you might have done otherwise.

Imagine saving a single person's life: you pass a burning building, kick the door down, rush through the smoke and flames, and drag a young child to safety. If you did that, it would stay with you for the rest of your life. If you saved several people's lives—running into a burning building one week, rescuing someone from drowning the next week, and diving in front of a bullet the week after—you'd think your life was really special. You'd be in the news. You'd be a hero.

But we can do *far* more than that.

According to the most rigorous estimates, the cost to save a life in the developing world is about $3,400 (or $100 for one QALY). This is a small enough amount that most of us in affluent countries could donate that amount every year while maintaining about the same quality of life. Rather than just saving one life, we could save a life every working year of our lives. Donating to charity is not nearly as glamorous as kicking down the door of a burning building, but the benefits are just as great. Through the simple act of donating to the most effective charities, we have the power to save dozens of lives. That's pretty amazing.

• • •

In this chapter, we've seen the importance of focusing on the best charitable programs, and we've seen just how good those programs can be. In the next, we'll look at one rule of thumb to help us find those most effective programs, and we'll begin to look at how we can best use our time as well as our money.

FOUR

WHY YOU SHOULDN'T DONATE TO DISASTER RELIEF

Question #3: Is this area neglected?

Greg Lewis was fourteen when he decided to become a doctor. Born and raised in the quiet rural city of Salisbury, England, his reasons were typical of countless others who decide to pursue medicine. "I want to study medicine because of a desire I have to help others," he wrote in his university application.

Indeed, medicine is the banner career for people who want to make a difference. Every year, about twenty thousand people in the United States and eight thousand people in the United Kingdom go to medical school, and the number is growing year after year. Even for those for whom medicine isn't a good fit, the desire to pursue a career that makes a difference is widespread. According to one study, 70 percent of young people regard ethical considerations as "crucial" in their choice of employer. Enterprises like Teach for America have grown dramatically, explicitly targeting students who care more about making a difference than about making a high salary. Organizations like Net Impact, Idealist, and ethicalcareers.org all offer advice on choosing a vocation that does good. Even Oprah Winfrey, on her website, provides examples of "jobs that make a difference."

But since effective altruism holds that we should test our assumptions about how to do good before putting them into action, we should look at this more critically. Are the most popular ways to make a difference through one's work really the most effective ones?

If anyone was going to make a big difference through medicine, it was Greg Lewis. After acing his high school classes and representing his country in the British Biology Olympiad, he pursued his dream and went to Cambridge to

study medicine. He excelled there, too, publishing his first paper at the age of twenty-one. But as Greg began life as a doctor, he started to wonder what impact he was really having.

Wasn't it obvious? He was in the ward, in the midst of the action, saving lives and healing the sick on a daily basis. He could see the beneficiaries of his actions—they were right there in front of him!

It wasn't obvious enough for Greg, though, and he started to use the research skills he'd honed in the lab to analyze a new question: How much good was he really doing by choosing medicine as a career rather than some other? As a result of that research, he developed a different view on how he could best make a difference in the world. To explain the reasoning behind that view, we need to look at the third of effective altruism's key questions: Is this area neglected?

• • •

Which is more valuable: water or diamonds?

I imagine this question has divided my readership into two camps. Team Water will say: *Obviously* water is more valuable. If we didn't have water, we'd all be dead. If we didn't have diamonds, we'd just have slightly less attractive jewelry. No big loss. In contrast, Team Diamonds will say: *Obviously* diamonds are more valuable. If you think water is more valuable, then how about we make a little trade? I'll give you a gallon of water and you give me a twenty-carat diamond. Sound fair?

So which team is right? They both are, depending on what exactly we mean. Drinkable water is in one sense extremely valuable because it's necessary for us to keep living. This makes the *average* value of drinkable water high. But we've already got a lot of water, so the value of an *additional* gallon of water (in developed countries) is very low. If I, a citizen of a developed Western nation, have one additional gallon of water, that means I may simply have a slightly deeper bath one evening. This is why the cost of a gallon of water from the tap in New York City, where I'm writing these lines, is just $0.015—less than two cents.

In contrast, even though the average value of diamonds is much lower than that of water, the value of an additional (or "marginal") diamond is much higher. The reason, simply, is that there aren't that many diamonds available on the market: they are therefore scarce in the way that water isn't. If I had no possessions at all, and couldn't sell what I gained, I'd rather have a gallon of

water than a twenty-carat diamond. In contrast, given the easy access to water that I currently have, I'd prefer the diamond if given the choice.

This "water and diamonds" paradox shows the importance of what economists call *thinking at the margin*: assessing the value of an additional thing—what is known in economics as its *marginal utility*—rather than thinking about the average value of that thing.

We think on the margin all the time. Suppose you receive a new sweater for Christmas. How good is that sweater? The answer depends on how many sweaters you already have. If it's winter, you're homeless, and you have no warm clothes, that sweater might prevent you from getting hypothermia, so an additional sweater would be extremely valuable. If you've got a place to live but are low on sweaters, that extra cable-knit might give you something new to wear on a cold day and would therefore still be pretty valuable. If you already have too many sweaters, though, one more might just be a nuisance—one extra thing to pack when you move—and therefore be bad overall.

The value of a new sweater decreases the more sweaters you already have. The value can even become negative if you already have lots of sweaters. In fact, it's true of most good things (though not all of them all the time), that their value diminishes as their quantity increases. The first slice of cake is delicious, but by the third, you're feeling a little sick. Having one copy of this book might provide you with an interesting and entertaining experience, but having a second might just provide you with a makeshift doorstop. This is what economists call the law of diminishing returns.

So far we've only compared different sorts of programs within a specific cause—like developing-world education, or developing-world health. If we want to do as much good as we can, we've also got to ask which cause to focus on. The law of diminishing returns provides a useful rule of thumb for comparing causes. If a specific area has already received a great deal of funding and attention, then we should expect it to be difficult for us to do a lot of good by devoting additional resources to that area. In contrast, within causes that are comparatively neglected, the most effective opportunities for doing good have probably not been taken.

Consider disaster relief. On March 11, 2011, the Tohoku region of Japan was hit by the fourth most powerful earthquake since recording began in 1900. Tsunamis reached heights of 130 feet and traveled six miles inland. The earthquake was so large that the entire main island of Japan was moved 2.4 meters (7.9 feet) east. Millions of people were left without electricity or water.

Thousands died.

On January 12, 2010, just one year before, an earthquake hit Haiti. The epicenter was near Léogâne, sixteen miles west of the country's capital, Port-au-Prince. An estimated 280,000 buildings collapsed, including the National Palace, the National Assembly, the Port-au-Prince Cathedral, and the main jail. Cholera broke out. Thousands died.

In both cases, there was massive international media attention and a huge humanitarian relief effort. The disaster dominated the news. Aid agencies were mobilized, and individuals around the world reached for their wallets. In each case, the total international aid raised in the immediate aftermath came to about $5 billion.

The two disasters seem very similar. Both were caused by earthquakes. Both resulted in destruction on a massive scale. But in two ways they were very different, which should make us wonder why the international aid response was so similar. First, the human scale of the two disasters differed dramatically. Including deaths in the aftermath, the Japanese earthquake caused fifteen thousand deaths. The Haitian earthquake, by comparison, caused 150,000. Second, Japan is the fourth richest country in the world and had the resources to deal with a disaster on that scale. Haiti didn't. Per person, Japan was thirty times richer than Haiti. As a whole, the country was a thousand times richer. For that reason, on the fifteenth of March, just four days after the earthquake hit, the Japanese Red Cross made the following statement:

> The Japanese Red Cross Society, with the support of the International Federation of Red Cross and Red Crescent Societies, has determined that external assistance is not required, and is therefore not seeking funding or other assistance from donors at this time.

If the international response to natural disasters was rational, we would expect a greater amount of funding to be provided to larger disasters and to disasters that occur in poorer countries, which are less able to cope. But that's not what happens. Funding seems to be allocated in proportion with how evocative and widely publicized the disaster is, rather than on the basis of its scale and severity.

I'm using this example because it illustrates why, if we want to have an impact, we should donate to less widely publicized disasters rather than to the ones that make the news. For example, in 2008, an earthquake hit Sichuan,

China. You probably haven't heard of it: I hadn't before I started writing this book. This earthquake struck fifty miles northwest of Chengdu, right in the center of China. It killed eighty-seven thousand people: five times as many as the Japanese earthquake, and half as many as the Haitian earthquake. Yet it only raised $500 million in international aid—one-tenth that of Haiti or Japan. For some reason, it wasn't as widely publicized as the other earthquakes, so it received fewer funds. Because it received so much less, donations would have probably made a bigger impact.

The law of diminishing returns also explains why *in general*, it makes less sense to donate to disaster relief than it does to donate to the best charities that fight poverty. Every day, people die from easily preventable diseases like AIDS, malaria, or tuberculosis. This is a disaster far beyond that of Haiti, or Tohoku, or Sichuan. Every day, eighteen thousand children—more than the number of people who perished in Tohoku—die from preventable causes. For every death the Japanese earthquake caused, aid organizations received $330,000 in donations. In contrast, for every person who dies from poverty-related causes worldwide, only $15,000 on average is spent in foreign aid and philanthropy. Partly for this reason, experts from the World Health Organization and World Bank concluded that "emergency health interventions are more costly and less effective than time-tested health activities."

Our response to natural disasters is one of the clearest cases of how, when it comes to charity, most people follow their gut and respond to new events rather than ongoing problems. When a disaster strikes, the emotional centers of our brain flare up: we think—*emergency!* We forget there is an emergency happening all the time, because we've grown accustomed to everyday emergencies like disease and poverty and oppression. Because disasters are new and dramatic events, they inspire deeper and more urgent emotions, causing our subconscious to mistakenly assess them as more important or worthy of attention.

Ironically, the law of diminishing returns suggests that, if you feel a strong emotional reaction to a story and want to help, you should probably resist this inclination because there are probably many others like you who are also donating. By all means, you should harness the emotion you feel when a natural disaster strikes, but remind yourself that a similar disaster is happening all the time—and then consider donating to wherever your money will help the most rather than what is getting the most attention.

Diminishing returns also provides a powerful argument for focusing your

altruistic efforts on people in poor countries rather than those in rich countries.

For example, it costs about $50,000 to train and provide one guide dog for one blind person, something that would significantly improve that person's quality of life. However, if we could use that $50,000 to completely cure someone of blindness, that would be an even better use of money, since it provides a larger benefit for the same cost. Not only is $50,000 enough to cure one person of blindness in the developing world, it's enough to cure five hundred people of blindness if spent on surgery to prevent blindness from sufferers of trachoma (a bacterial infection that causes the eyelids to turn inwards, causing the eyelashes to scratch the cornea). Any program that costs one hundred dollars to cure blindness would have been fully funded in rich countries decades ago. The same is not true in poor countries, which means we can do so much more to help those people than we can at home.

Similar considerations apply to which sorts of health treatments do the most good with additional funding. Every year, cancer kills 8.2 million people and is responsible for 7.6 percent of all deaths and ill health worldwide (measured in terms of QALYs lost). Per year, $217 billion is spent on cancer treatment. Malaria is responsible for 3.3 percent of QALYs lost worldwide. In terms of its health impacts, cancer is about twice as bad as malaria, so if medical spending were in proportion to the scale of the problem, we would expect malaria treatment to receive about $100 billion per year. In reality, only $1.6 billion per year is spent on malaria treatment—about sixty times less than we would expect.

Cancer treatment receives so much more funding than malaria treatment because malaria is such a cheap problem to solve that rich countries no longer suffer from it. (It was eliminated from the United States in 1951.) The fact that cancer treatment receives so much more funding than malaria treatment means that, on the margin, each of us can provide a far greater benefit for other people by funding the most effective malaria treatments in the developing world than we can by funding the most effective cancer treatments in the developed world. In the United States, public health experts regard any program that provides one QALY for less than $50,000 as a good value, and health programs will often be funded even if the cost per QALY is much higher than $50,000. In contrast, providing the same benefit in poor countries (such as by distributing insecticide-treated bed nets to prevent the spread of malaria) can cost as little as one hundred dollars. That means that, with a given amount of money, you can benefit people in poor countries five hundred times more than people in rich countries.

Again, we see the 100x Multiplier at work. We're about one hundred times richer than the poorest billion people in the world, and we can do several hundred times more to help them than we can to help others in the rich countries we live in.

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So far we've looked at how money diminishes in value, but similar reasoning applies to time. One subtle implication of the principle of diminishing returns is how it applies to career choice. Let's come back to Greg Lewis, the idealistic medical student we encountered at the beginning of this chapter, and consider the question: How much good does a doctor do?

"I had hoped that this question would already be extensively researched," Greg told me. "Because you'd think that if you were trying to run a health service, you'd really want to know what the returns are on having more doctors. So I was quite surprised to find that no one had really looked at this question at all."

You might think this is easy to work out and that's why no one had needed to research it. To ascertain how many lives a doctor saves, all you have to do is add up how many lifesaving surgeries they perform and lifesaving treatments they administer over the course of their lives. To figure out how much sickness they heal, you add up all the occasions when they've done something to improve a patient's life. Add up the benefits of both activities and that tells you how much good they've done. Easy, right?

This is what Greg did initially. He looked at the data for a large number of countries. For the United States, he divided the total benefit of American healthcare by the number of doctors in the United States. The National Area Health Education Center Organization estimated there to be 878,194 doctors in the United States, and Greg found work by an epidemiologist named John Bunker, who estimated that the total benefits from medicine in the United States is about 7 QALYs per person, or 2.2 billion QALYs in total. (Remember that a QALY is equivalent to providing someone with one additional year of healthy life.) On this calculation, each doctor is estimated to provide 2,500 QALYs (2.2 billion ÷ 878,194). That includes both benefits through saving lives and benefits through improving quality of life. It can be hard to get an intuitive sense of that, so we could think of this in terms of the equivalent number of "lives saved." Health economists estimate that, on average, the benefit of "saving a life" is the

same as the benefit of providing 36.5 QALYs. Based on this calculation, therefore, a doctor provides health benefits equivalent to saving seventy lives over the course of his or her career. Greg knew this was an overestimate insofar as it didn't account for the impact of nurses, hospital administrators, and so on, so he scaled down that number, figuring the real figure might be something like twenty-five or thirty lives saved per doctor. Still, pretty impressive, right?

Let's call this the Simple View. Greg realized that, even though this was the view most people intuitively held, it wasn't the right way to think about things, because it made the mistake of assessing the *average* value of a doctor. As we've discussed throughout this chapter, this is the wrong calculation to consider when trying to determine how much impact you can have. Instead, young people wanting to make a difference through their careers should determine the *marginal* value they would provide by becoming a doctor.

To see why the Simple View doesn't work, imagine you're in a small town, isolated from the rest of the country. That town has the resources to employ in its hospital three doctors—Alice, Bob, and Charlotte. There are three categories of activities that they do: (i) lifesaving operations and treatments, like heart surgery; (ii) major health improvements, like anxiety treatments; (iii) treating minor ailments, like coughs and colds. In this hospital, Alice, Bob, and Charlotte each spend about one-third of their time on each of these categories, and the health-care needs of the town are met. Each doctor performs one hundred lifesaving surgeries per year, so, according to the Simple View, each saves approximately one hundred lives per year.

Now let's suppose that the clinic loses resources and can no longer afford to employ Charlotte. How bad would that be for the residents of the town? According to the Simple View, it's a disaster, because Charlotte will not be available to perform those one hundred lifesaving operations, which means one hundred people will die.

However, when we think about this realistically, we realize this is not what would happen. If Charlotte was no longer employed, Alice and Bob would do some reprioritizing. They'd neglect or delegate to other health-care personnel all the minor ailments they could have treated so they can focus solely on major health improvements and lifesaving operations and treatments. In Charlotte's absence, Alice and Bob now each save 150 lives per year. So, even though Charlotte was performing lifesaving operations, the difference she made by working for that clinic was really in treating minor ailments like coughs and colds that could not have been treated with fewer doctors.

Saving lives is the most important task a doctor can do—and it's a task that would be taken care of in the absence of almost any individual doctor we could point to. As stated earlier, there are an estimated 878,194 doctors in the United States. Suppose you become the 878,195th doctor. What's the difference you make as a result? Well, those 878,194 will have already plucked all the low-hanging fruit in terms of easy ways to save lives, so you, as the 878,195th doctor, will have only hard-to-realize opportunities to improve health. That's unlikely to include performing heart surgeries and more likely to involve treating minor ailments.

Using this idea, we can revise our estimates of how much good a doctor really does. The good that you would do by becoming a doctor (effectively becoming the 878,195th doctor in the United States) is the difference between (i) the total benefit from US health-care given that the United States has 878,194 doctors, and (ii) the total benefit from US healthcare given that the United States has 878,195 doctors. How big is that difference?

Greg used statistics to work out the answer. He looked at both how good the quality of health is in many different countries and how many doctors there are in each of those countries, and then plotted the relationship between those two factors (while also taking into account the effect of things like wealth and education). This enabled him to answer his question. He worked out that adding one doctor to the United States adds about four QALYs per year to the population as a whole. Over a forty-year career, that's 160 QALYs. Once we take into account the fact that nurses and other health-care workers also generate some of this benefit, we get the conclusion that one additional doctor in the United States provides a benefit equivalent to about four lives saved over the course of their career. That is still awesome. But it's also less than you probably thought before, all because of diminishing returns. Of course, the good that a doctor does will vary from specialty to specialty; this estimate is an average across all specialties. Unless some specialties do far more good than others, however, this won't make much difference to our assessment of the good that doctors do.

If you aim to become a doctor in a rich country, you're adding only your labor to the already very large pool of doctors who are working in that country. That means that becoming a doctor probably does less good than you'd intuitively think. The same consideration explains why doctors have a much bigger impact if they work in poor countries than in rich ones. Greg did some more statistics to work out how much good he'd do if he upped roots and went to

work in a very poor country like Ethiopia. He found that he'd make a much larger difference, providing an extra three hundred QALYs per year, or about three hundred lives over a forty-year career. That's more than one hundred times as big an impact than if he worked in the United Kingdom. Once again, we see the 100x Multiplier in effect: because far fewer resources are spent on healthcare in poor countries, Greg could do far more good working in a poor country than in a rich one.

Asking, "Is this area neglected?" and trying to focus only on those areas that truly are neglected can be counterintuitive. It means that the most popular causes are, precisely for that reason, the ones where it will be difficult to have a big impact. Because of diminishing returns, we can make a much bigger difference if we focus our efforts on areas on which comparatively fewer resources have been spent, like less-publicized disasters, or global poverty rather than domestic poverty.

You might be wondering what Greg ultimately decided to do with his career: Did he move to work in a poor country? In fact, he concluded that he should continue to work in the United Kingdom. We'll find out why in the next chapter.

FIVE

THE BEST PERSON WHO EVER LIVED IS AN UNKNOWN UKRAINIAN MAN

Question #4: What would have happened otherwise?

Out of everyone who ever existed, who has done the most good? In researching this question, I came across a list that *Esquire* had published called "The 75 Best People in the World." The writers suggested that the number one spot should go to . . . Matt Damon. Which seems unlikely.

In chapter three, I suggested that smallpox eradication was one of humanity's greatest achievements. If we're looking for the Best Person Ever, we could start by looking at those who helped in this effort. In fact, much of the responsibility of smallpox eradication can be attributed to just one man.

In 1966, Ohio-born doctor D. A. Henderson became the leader of the WHO's Global Smallpox Eradication campaign. At only thirty-eight years old, and with only ten years' clinical experience, he was fifteen years younger than everyone else in the program, but he excelled at what he did. When he took charge of the campaign, he proposed an ambitious goal: to completely wipe smallpox off the face of the planet within ten years. Astoundingly, the campaign succeeded, and between 1967 and 1971 the number of smallpox-endemic countries plummeted from thirty-one to five. Henderson pioneered the novel technique of ring-vaccination in which, rather than vaccinating an entire population—a costly and time-consuming procedure—his team used large-scale reporting to identify outbreaks of the disease, contain those who had it, and vaccinate everyone else within a certain radius. It exceeded everyone's expectations, and in 1977, the last naturally occurring case of smallpox was diagnosed in Somalia. Smallpox was the first disease ever to have been eradicated.

Henderson's success resulted in a string of accolades. He won more than a dozen major awards, including the Public Welfare Medal, the National Medal of Science, and the Presidential Medal of Freedom—the highest civilian award in the United States. He received honorary degrees from seventeen different universities. Immediately after 9/11, he became President George W. Bush's lead expert on bioterrorism. He was even knighted by the king of Thailand.

But D. A. Henderson is not the person I'm talking about.

By the time Henderson was hired, the political will to eradicate smallpox already existed. There was a job opening—a job he didn't even want initially—and Henderson filled it. This isn't to say he didn't rise to the challenge or that he wasn't a hero, but if he had never existed, someone else would have been in his shoes and eradicated smallpox eventually. This person might not have been as good or as quick as Henderson, but as long as he or she was good enough, smallpox would have been eradicated.

Instead, we should look to a much more unlikely hero: Viktor Zhdanov, a Ukrainian virologist who died in 1987. At the time of this writing, he has a mere four-paragraph *Wikipedia* page, and there are only a few grainy black-and-white photos of him available online. I'm not aware of any major accolades for his work.

In 1958, Zhdanov was a deputy minister of health for the Soviet Union. In May of that year, at the Eleventh World Health Assembly meeting in Minneapolis, Minnesota, during the Soviet Union's first appearance in the assembly after a nine-year absence, Zhdanov described a visionary plan to eradicate smallpox. At the time, no disease had ever before been eradicated. No one knew if it could even be done. And no one expected such a suggestion to come from the Soviet Union. But he conveyed his message with passion, conviction, and optimism, boldly suggesting that the disease could be eradicated within ten years. Since smallpox was an exclusively human disease, he argued, it would be easier to eradicate than mosquito-borne infections such as malaria. He pointed to the Soviet Union's success at eliminating smallpox, despite its vast territory and poor transportation networks. He referenced Thomas Jefferson's letter to the inventor of the smallpox vaccine, Edward Jenner: "I avail myself of this occasion of rendering you a portion of the tribute of gratitude due to you from the whole human family. Medicine has never before produced any single improvement of such utility. . . . Future nations will know by history only that the loathsome small-pox has existed and by you has been extirpated."

By the force of his arguments, Zhdanov was successful. For the first time in

its history, the WHO agreed to form a campaign to completely eradicate a disease.

To assess how much good Zhdanov did, we should bear in mind that, even if he had not lobbied the WHO, smallpox would probably have been eradicated anyway. The problem was serious enough that someone would have started a campaign to fix it. Many of those 120 million lives that have been saved by smallpox eradication would therefore have been saved anyway. But there would probably have been a considerable delay in the smallpox eradication campaign. Suppose, therefore, that Zhdanov moved forward the eradication of smallpox by a decade. If so, then he alone prevented between 10 and 20 million deaths—about as much as if he'd achieved three decades of world peace.

We don't usually think of achievements in terms of what would have happened otherwise, but we should. What matters is not *who* does good but whether good is done; and the measure of how much good you achieve is the difference between what happens as a result of your actions and what would have happened anyway.

Suppose, for example, that I see a woman collapse on the ground. She's had a heart attack. There's no one else around, so I run up to her and start performing CPR. Suppose I've never performed CPR before, but I manage to restart the woman's heart. She recovers but, as a result of the poor-quality CPR, is left with a disability. Even so, it's clear that I have done a great thing.

Now suppose there had been a paramedic around when the woman collapsed. This paramedic would have surely restarted her heart without causing injury, but, while I was running toward the woman, I pushed the paramedic out of the way and started performing CPR myself. In this case, I still saved her life, but if I hadn't, the paramedic would have been able to do the same thing without causing damage. In this case, how should I feel about my actions? Am I a hero? After all, I "saved a life!"

Of course I'm not a hero. The good I do is not a matter of the direct benefits I *cause*. Rather, it is the *difference* I make. Even though I technically saved this woman's life, I actually did harm overall.

Looking at what would have happened otherwise is a fundamental piece of scientific reasoning, referred to as assessing the counterfactual. But the mistake of neglecting the counterfactual is rife within the world of altruism, and this mistake can have terrible consequences.

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Sixteen-year-old Brandon lived in the northwest of Detroit. He had already been in trouble with the law for armed robbery, home invasion, and drug-related offenses, and he was being brought into Oakland County Jail so he could see how awful prison life was. The goal was to make him reconsider his life choices before he ended up in jail for good. Brandon is the central character in an episode of *Beyond Scared Straight*.

Scared Straight! began in 1978 as a documentary by Arnold Shapiro. It told the true story of a group of teenage delinquents who were taken by correctional officers to spend three hours at a prison. The inmates screamed at, intimidated, and threatened the juveniles, telling stories of the horrors of life in prison, of rapes and beatings, in order to scare them out of a life of crime. The documentary ends by revealing that most of the juveniles have turned away from their troubled paths, though acknowledging that some did go on to reoffend. The documentary was highly successful, wining an Oscar and eight Emmys, and begat a succession of follow-ups: *Scared Straight! Another Story*, *Scared Straight! 10 Years Later*, *Scared Straight! 20 Years Later*. The latest incarnation, *Beyond Scared Straight*, broadcast on A&E in the United States, is as of this writing in its eighth season, airing weekly and attracting millions of viewers.

In this episode, when Brandon enters the prison, he's cocky and defiant. He faces a wall of inmates through the bars. They taunt him, yelling, "We got a hard-ass? Let's do this!" and "You want to be a real fucking tough guy, is that it?" They threaten him, they jeer, and the guards emphasize the seriousness of their threats.

Through all this, Brandon smiles. He thinks it's funny. When he gets taken aside by a correctional officer, he continues to play tough guy: "I ain't tripping. I ain't scared of none of these shitheads . . . they breathe just like I breathe and they bleed just like I bleed." Time and again, he shrugs off the intimidation.

Things change when he sees an "extraction": a prisoner tased, bound to a chair, and forcibly sedated. Out in the corridor he breaks down, tears in his eyes. The guards tell him they are on his side. They don't want him to end up in here.

At the end of the show, we visit Brandon one month later. He's smiling again, but this time there's hope in his eyes, not defiance. He's got a personal tutor, and he's stopped hanging out with the friends who got him into trouble. He goes back to the prison to apologize to the guards he stood up to. Brandon says he now recognizes he was wrong. "I'm happy I went to the jail tour because it changed my life and made me a better person," he says. "And it made me realize that some of the stuff I was doing wasn't good . . . the future look[s] bright

now."

Beyond Scared Straight introduces us to a world most of us will never experience. It's a mix of pop entertainment and uplifting coming-of-age narrative. The producers say it's a highly effective social program, with cases like Brandon's, in which a once-troubled teen turns his or her life around, being the norm rather than the exception. Since the original release of *Scared Straight!*, hundreds of prisons across the United States have adopted similar programs. The program seems to be a win-win: It reduces the recidivism rate among juvenile offenders, and it makes for great TV.

But, as you've probably guessed based on my discussion of it in this chapter, there's a darker side to *Scared Straight*. Those who tout the program's effectiveness are wrong. Not only is the program ineffective, it's downright *harmful*.

Nine high-quality studies have been done on the program, assessing the progress of one thousand juveniles overall. The Cochrane Collaboration, a nonprofit institute that rigorously assesses the evidence behind health and social programs, looked at these studies and found that two of them had no significant effect, while the remaining seven showed *increased* rates of criminality among juveniles. The authors of the review estimated that the Scared Straight programs that had been studied increased the odds of offending by about 60 percent. "The analyses show the intervention to be more harmful than doing nothing," they concluded. "The program effect, whether assuming a fixed or random effects model, was nearly identical and negative in direction, regardless of the meta-analytic strategy." In the jargon of academia, that's just about as harsh a criticism as you can get, claiming that, no matter what way they looked at it, Scared Straight caused more crime than it prevented. In a separate study, the Washington State Institute for Public Policy estimated the value for society, per dollar invested, of a range of preventative social policies, such as psychotherapy and anger management. Of the sixty interventions studied, the vast majority were shown to produce more value than they cost. Only three were harmful, and only one of these egregiously so—the Scared Straight program. The researchers concluded that, because Scared Straight was increasing rates of crime, with associated penitentiary costs and costs to the local community, every dollar spent on Scared Straight cost society $203.

Yet Scared Straight programs still continue and are still touted as effective. How can a program that has been proven to cause harm thrive?

The problem is that those who tout its effectiveness aren't thinking in terms

of what would have happened otherwise. They see delinquent kids come to the Scared Straight program, they see them go on to commit fewer crimes than they'd been committing in the past (only one-third of kids who go through the program go on to commit a crime in the following year), and they conclude it's a success. But you can't conclude a certain program *causes* things to get better based solely on the fact that they have gotten better. In the case of Scared Straight, studies show that rates of delinquency would have decreased even without the Scared Straight program. In fact, they show that rates of delinquency would have decreased by a greater amount if the Scared Straight program had never been run. Scared Straight actually impedes progress that is happening anyway.

I suspect the apparent effectiveness of Scared Straight can be explained by a phenomenon called regression to the mean. If you play a truly excellent round of golf one day, you'll probably play worse the next time you play because that excellent round was statistically unlikely and you should expect to see a more typical performance the next time. Similarly, people who are undergoing a bout of particularly severe depression will on average be happier three months later, because they are likely closer to their average level of happiness. And, similarly, if you select a group of juveniles to go through a reform program because they've committed an unusually high number of misdemeanors in a given time period, they're likely to exhibit something closer to typical behavior in the following months.

But that only explains why Scared Straight can appear to be effective when it really does nothing. Why, then, does the program increase rates of criminality? No one knows for sure, but one hypothesis is that the inmates—who play up how tough they are for surviving life in prison—act as role models rather than deterrents for the delinquents. The delinquents identify with the inmates and then imitate their behaviors. Watching the show again, this hypothesis seems plausible. The inmates tell the kids they should try to avoid prison not because it's an awful place to be, or because it's shameful to have broken the law, but because they're not tough enough for life inside.

The example of Scared Straight shows the importance of ensuring, wherever possible, that large-scale social programs undergo rigorous testing through controlled trials before they are put into practice. If an amateur chemist created a pill he claimed would reduce crime, we would never administer it to thousands of children without rigorous testing because it would be dangerous, not to mention illegal, to do so. Yet new social programs like Scared Straight can be

rolled out without any good evidence behind them. Without rigorous testing, we can't know if a social program is making things better, making things worse, or achieving nothing at all. Of course, sometimes programs are too small in scale for testing to be a good use of money, and sometimes rigorous trials are impossible. But our default attitude should be that, if a social program is going to be rolled out on a large scale, then it should have been proven to be effective first.

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The most subtle and interesting way in which we neglect to think about what would have happened otherwise is when we think about career choice. This takes us back to Greg Lewis, our statistically inclined doctor from the previous chapter, and his estimate of how much good doctors do.

In the previous chapter, we estimated the benefit of one additional doctor in the United States at four lives saved per doctor. But that doesn't yet measure how good becoming a doctor is, because, by becoming a doctor, you aren't simply adding one extra doctor to the supply of doctors. The number of spots at medical school is fairly rigid, so if you decide not to go to medical school, someone else will take your place and become a doctor in your stead. Thus, by becoming a doctor, you're really just changing who works as a doctor, not adding to the overall amount of talent out there. The difference you make isn't equal to the difference between the United States having 878,194 doctors and the United States having 878,195 doctors (which is how we analyzed it in the previous chapter). It's the difference you make by becoming a doctor as compared to the difference someone else would make if he or she took your place.

This consideration means that our previous estimate that each doctor saves approximately four lives over the course of their careers is too high. There are still benefits to becoming a doctor: if you get into medical school, then you increase the average quality of doctors (assuming the selection process selects the best applicants); and adding yourself to the labor pool may decrease doctors' wages slightly, allowing more doctors to be employed. But the contribution won't be as much as we suggested earlier. Factoring this consideration into his calculations, Greg estimated that, rather than saving four lives over the course of your career, you might actually save only an additional one or two that would not have been saved otherwise. This is still a very valuable contribution to

society, but less than one might expect.

This consideration is widely applicable. For example, in my teenage years, I used to work as a care assistant at a nursing home. How much of an impact was I making? When I initially thought about this, I thought about the direct benefits of the work: the improvements in the lives of the people who were living at that nursing home. However, I should have thought about whether I was doing a better job than whoever would have taken my place. Though enthusiastic, I was slow and inexperienced, and I probably needed the money I was paid less than whoever would have been in my shoes, who might have had a family to support. So it's not clear that, on balance, I was doing any good at all.

This explains, in part, why Greg didn't go to Africa. If he took a job in a nonprofit, he'd be taking the place of someone else who wanted to do the same. The impact of an additional doctor in a developing country is about three hundred QALYs per year, which is very large, but the difference he'd make by taking someone else's position would be less than that. He therefore chose a different path, one that brings together many of the considerations we've covered so far. I call it *earning to give*.

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Earning to give means exactly what it sounds like: rather than trying to maximize the direct impact you have with your job, you instead try to increase your earnings so you can donate more, improving people's lives through your giving rather than your day-to-day work. Most people don't consider this option when choosing a career that "makes a difference." But time and money are normally interchangeable—money can pay for people's time, and your time can be used to earn money—so there's no reason to assume the best careers are only those that benefit people directly through the work itself. If we're serious about doing good, earning to give is a path we should consider.

Let's look at Greg Lewis's options. If he worked as a doctor in a rich country and didn't donate a portion of his income, he would do an amount of good equivalent to saving two lives over the course of his career. If he went to work as a doctor in a very poor country, he would do an amount of good equivalent to saving four lives every year, or 140 lives over a thirty-five-year career. But how many lives could he save if he stayed home and donated his earnings?

The average salary of a doctor in the UK is about £70,000 per year before

taxes. In dollars, that's about $110,000 or $4.6 million over a forty-two-year career. By pursuing a particularly lucrative specialty—medical oncology—Greg could earn almost double that, earning about $200,000 per year on average. Earlier I said that one of the most cost-effective ways to save lives is by distributing antimalarial bed nets: $3,400 pays for 560 nets, which on average will prevent one death due to malaria. By pursuing medical oncology, Greg could therefore donate 50 percent of his $200,000 per year earnings while still having a very comfortable $100,000 per year pretax salary (donations are tax-deductible). His donations would save dozens of lives a year, considerably more than he could have done if he'd worked directly in a poor country.

Because of this, Greg decided to earn to give, planning to specialize in medical oncology. "I found it pretty humbling, when I looked at the difference. I'd save a few lives through my direct impact as a doctor," Greg told me. "Which was less than I thought, but still great. But through my donations, I could save hundreds of lives." For Greg, the same reasons that made him want to go into medicine made it clear to him that he should start donating, too. "I started out giving around ten percent. But I've gradually been increasing that, as I found that I really didn't miss the money. Now I'm donating about fifty percent and my life is, if anything, better than it was. I feel that I'm doing justice to my seventeen-year-old self who wanted to make the world a better place." In 2014, Greg donated £20,000, enough to save ten lives.

Importantly, by earning to give, Greg is making a difference that wouldn't have happened otherwise. If he weren't a doctor, someone else would take his place, but that doctor would probably donate very little (the average is about 2 percent). In contrast, by working for a nongovernmental organization (NGO) in a poor country, Greg would be using money from the NGO that would otherwise have been spent on a different doctor's salary, or on medical supplies. Because he's making a difference that wouldn't have happened anyway, Greg will do even more good by earning to give than he would have done if he worked directly in poor countries. And he can do so without having to give up the comforts of home.

It's worth reflecting on this. In 2007, Louis Theroux, a British documentary filmmaker, released a documentary called *Under the Knife* in which he explored the world of cosmetic surgery in Beverly Hills. In the culmination of the show, he accused the cosmetic surgeon he'd been interviewing of wasting his talent and skills to make wannabe movie stars more attractive, rather than saving lives. What we've seen so far shows that Louis Theroux's sentiment, while

understandable, is misplaced. It's the cosmetic surgeon's decision about how to spend his money that really matters.

Earning to give seems to be an enormously powerful way of doing good. It exploits the fact that even typical workers in developed countries are among the top income earners in the world and that there are some charities that do huge amounts to help the world's poorest people for relatively little money. Moreover, unlike the conventional "ethical" careers guidance, earning to give is a path that's open to everyone. The conventional advice is that if you want to make a difference you should work in the nonprofit or public sector or work in corporate social responsibility. But many people struggle to get *a* job, let alone find a job in a specific sector. However, many more people have the option to work overtime in order to earn more, or to work harder in order to get a raise or promotion, or to move toward a higher-paying career, or just to live on less. By doing this, and being smart about where you give, almost anyone in rich countries can do a tremendous amount to help others.

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There is plenty more to be said about doing good through your career, and the whole of chapter nine will be devoted to it. But before we can address this question properly, and before we can see why earning to give is merely one path and not always the most effective career choice, we need to look at another key question of effective altruism.

SIX

WHY VOTING IS LIKE DONATING THOUSANDS OF DOLLARS TO CHARITY

Question #5: What are the chances of success, and how good would success be?

"The possibility of a severe accident occurring is so small that from an engineering standpoint, it is practically unthinkable." This is a quote from the comprehensive accident management plan of the Fukushima Daiichi nuclear power plant, and it helps us see the importance of thinking correctly about risk.

Fukushima Daiichi was located seventy kilometers away from the epicenter of the massive earthquake that hit Japan in March 2011. All operating nuclear reactors shut down automatically following the earthquake, a safety precaution designed to prevent a meltdown, which would result in hazardous radioactive material leaking out into the environment. However, an ensuing tsunami hit and disabled the plant's cooling system, causing the meltdown of three of its reactors. Although no one died from radiation exposure, about 160,000 people had to be evacuated from their homes, and sixteen hundred people died during the evacuation because of conditions such as hospital closures. The Fukushima disaster remains the worst nuclear accident since Chernobyl.

Four months after the disaster, the Japanese government formed an investigation committee, made up of a panel of ten independent experts in fields including radiation protection, medicine, and law, which presented a 450-page report to Prime Minister Yoshihiko Noda. Panel chairman Yotaro Hatamura, an emeritus professor of engineering at the University of Tokyo, told a news conference that "the root cause of the Fukushima crisis is that [the regulatory bodies and the Tokyo Electric Power Company] selfishly assumed that natural disasters that are beyond their imagination would not occur." In a closing note,

Hatamura wrote that Japan "should take the accident as a reminder from nature that humans' way of thinking can be defective."

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So far in this book we have focused on measurable, concrete ways of helping others. The real world, unfortunately, is not always so simple. Often we don't know whether our actions will be successful, and, given the difficulty of knowing what would have happened otherwise, we will usually never know whether our actions really make a difference. When it comes to effecting political change, this problem becomes particularly severe. Even if you run a campaign and the policy you've campaigned for is put into place, there are usually other forces at work, making it difficult to measure your individual impact.

We shouldn't dismiss more speculative or high-risk activities out of hand, though, because when successful, they can have an enormous impact. We therefore need a way to compare higher-risk but higher-upside actions with actions that are certain to have an impact.

Within economics and decision theory the standard way to do this is to look at an action's *expected value*. To take a simple example, suppose I offer you a bet. I'll flip a coin and if the coin lands heads, I'll give you two dollars; if the coin lands tails, you give me a dollar. Should you take the bet? According to the idea of expected value, you should.

To calculate the expected monetary value of each bet, you look at all the possible outcomes of that bet. For each outcome, you take the monetary gain or loss and multiply it by the probability of the outcome. In this case, there are two possible outcomes, heads and tails. Each has a 50 percent chance of occurring. The expected monetary value of taking the bet is therefore (50% × +$2) + (50% × –$1) = $0.50. The expected monetary value of refusing the bet is zero. Taking the bet has the higher expected value, so you should take the bet.

Maximizing expected value is generally regarded as the best strategy for making decisions when you know the value and the probabilities of each option. It's the strategy used by economists, statisticians, poker players, risk-management experts, and pretty much anyone else who needs to regularly deal with uncertain outcomes. To see why, suppose that I offer you the same bet over and over again. In the long run, you're almost guaranteed to make more money if you accept my bets than if you don't: in fact, on average you'll make fifty

cents for every bet that you take.

In the flipping-a-coin example we talked about expected monetary value. When thinking about buying things for yourself, however, expected monetary value isn't always what you care about. Most people would prefer to keep $1,000 than to bet it on a one-in-a-hundred chance of receiving $100,001, even though the expected monetary value of the bet is positive. That's rational because money has diminishing returns: if you're like most people, the improvements you'd make to your life by spending the first $1,000 of your $100,001 lottery winnings will bring you more joy than the improvements you make to your life by spending the last $1,000. The same isn't true, however, when we're thinking about philanthropy. If you donate $1,000 to the Against Malaria Foundation, they will buy and distribute one hundred and sixty bed nets. If you donate $100,000 to the Against Malaria Foundation, they will buy and distribute ten thousand six hundred bed nets. Because the social problems in the world are so big, additional resources directed to them diminish in value very slowly. Additional resources do diminish in value *somewhat*—if the Against Malaria Foundation received $50 million (several times its current budget), it would struggle to spend all that money as quickly or as well as it has spent money so far. And after spending billions of dollars on bed nets, spending more money on bed nets wouldn't be effective because everyone who needs a bed net would have one. And, as we've seen, it's more effective to spend money on malaria treatment than cancer treatment in part because malaria treatment receives a tiny fraction of the resources that cancer treatment receives. But when thinking "merely" in terms of millions of dollars, one can often assume the altruistic value of money stays the same no matter how much of it you have.

To see how useful the idea of expected value is, let's consider a morbid but important application: assessing the risk of death from different activities. Smoking, riding a motorbike, scuba diving, taking ecstasy, eating peanut butter: all these things increase your risk of death. How much should you worry about each of them? Public health experts use the concept of a "micromort" to compare the risks, where one micromort equals a one-in-a-million chance of dying, equivalent to thirty minutes of expected life lost if you're aged twenty, or fifteen minutes of expected life lost if you're aged fifty. By comparing different activities in terms of micromorts, we can easily assess their relative dangers. The results can be surprising. Based on reported cases of deaths from these activities, one ecstasy session (two tablets) is only about one micromort, whereas going scuba diving is five micromorts and going skydiving is nine micromorts. Flying

in a space shuttle is seventeen thousand micromorts, or a 1.7 percent chance of dying, which is about as dangerous as attempting to climb Mount Everest beyond base camp, at thirteen thousand micromorts, or a 1.3 percent chance of dying.

The same concept can be applied to things that increase risk of death later in life as well. Eating forty tablespoons of peanut butter gives you one micromort because you risk ingesting aflatoxin, a fungal toxin that increases your risk of liver cancer later in life. Smoking a single cigarette gives you 0.7 micromorts, increasing your chance of dying of lung cancer many years down the line. Taking this into account, smoking one cigarette reduces life expectancy by five minutes—about the same length of time it takes to smoke it.

When thinking about risk from transport, you can think directly in terms of minutes of life lost per hour of travel. Each time you travel, you face a slight risk of getting into a fatal accident, but the chance of getting into a fatal accident varies dramatically depending on the mode of transport. For example, the risk of a fatal car crash while driving for an hour is about one in ten million (so 0.1 micromorts). For a twenty-year-old, that's a one-in-ten-million chance of losing sixty years. The expected life lost from driving for one hour is therefore three minutes. Looking at expected minutes lost shows just how great a discrepancy there is between risks from different sorts of transport. Whereas an hour on a train costs you only twenty expected seconds of life, an hour on a motorbike costs you an expected three hours and forty-five minutes.

In addition to giving us a way to compare the risks of different activities, the concept of expected value helps us choose which risks are worth taking. Would you be willing to spend an hour on a motorbike if it was perfectly safe but caused you to be unconscious later for three hours and forty-five minutes? If your answer is no, but you're otherwise happy to ride motorbikes in your day-to-day life, you're probably not fully appreciating the risk of death.

Thinking explicitly about expected value is important because humans are often terrible at assessing low-probability high-value events. Psychologists have found that people either give too much weight to low-probability events (as, perhaps, when people choose to play the lottery), or they simply ignore them all together.

This takes us back to the Fukushima safety report.

The authors of the comprehensive accident management plan were correct that the probability of a catastrophe occurring was very small. However, they didn't think correctly about how they should deal with that probability. They

assimilated “very small” to zero and promptly forgot all about it. Their mistake was failing to consider that *if* a catastrophe happens at a nuclear power plant, the costs are huge—in this case, more than a thousand deaths. Even though the chance of this catastrophe was small, it was clearly worth taking substantial safety precautions.

The Fukushima safety engineers were trying to prevent harm with their safety assessment, and they failed by ignoring an important but low-probability event. In just the same way, when trying to do good, we need to be sensitive *both* to the likelihood of success *and* to the value of that success. This means that low-probability high-payoff activities can take priority over sure bets of more modest impact. It also shows that people are often confused when they say that “one person can’t make a difference.” Voting provides a vivid illustration.

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Most people believe you should vote in government elections. But many economists argue that, if your concern is to actually affect the outcome, it’s a waste of time. Steven Levitt, professor of economics at the University of Chicago and coauthor of *Freakonomics*, wrote a blog post reiterating a sentiment he’d previously made in *The New York Times*:

> Nobody in their right mind votes because they think they’re going to affect the outcome of an election. If you look over the last hundred years of, say, elections for the US House of Representatives, I think there’s been maybe one [very close] election that’s been decided by votes. . . . The reasons for voting have to be something very different: it’s fun, your wife will love you more if you do it, it makes you feel like a proud American—but never should anyone delude themselves into thinking that the vote they cast will ever decide an election. . . . Just about anything you do with your time would be more productive.

Given the concept of expected value, however, Levitt’s reasoning is too quick. We can’t just say that the chance of affecting the outcome by voting is so small as to be negligible. We need to work out how large the benefit would be if we did indeed affect the outcome.

Luckily, some statisticians have done the hard work for us, including political pundit extraordinaire Nate Silver, who correctly predicted the winner of

the 2012 election in all fifty states and the District of Columbia. Along with Columbia University professor of statistics Andrew Gelman and Berkeley professor of law Aaron Edlin, Silver calculated the odds of an individual vote swaying the outcome of the 2008 presidential election and found that, on average, a voter had approximately a one-in-sixty-million chance of affecting the outcome—small odds to be sure.

Next, we have to work out what the stakes are. This will necessarily involve some guesstimation. You should start by asking yourself: How much do I personally expect to gain by having my preferred party in power? If your preferred party is the Republicans, then you might expect to benefit because you'll pay fewer taxes. If your preferred party is the Democrats, you might expect to benefit because you'll receive more government-funded public services. Suppose for the sake of argument you conclude that your preferred party getting into power is worth $1,000 to you. Although this $1,000-per-citizen figure is hypothetical, it seems plausible to me. The total spending of the US government is $3.5 trillion per year: that's $14 trillion over four years, or $44,000 per person. If that money is spent 2.5 percent more effectively, then the benefits amount to $1,000 per person. The government also, of course, makes people better or worse off in other ways, such as through regulation.

An economist like Levitt might say that, for you, the expected value of voting is only one in sixty million times $1,000, which equals 0.0016 cents. With such a low expected value, it's clearly not worth it to vote.

But this line of reasoning assumes that the value of voting is only the value to *you*. Instead, we should think about the *total* benefit of the better party being in power. Let's keep using this hypothetical $1,000 figure of the benefit per person of the better party being in power. If so, the total benefit to all Americans is $1,000 multiplied by the US population of 314 million, so $314 billion. The average expected value of voting for the better party, therefore, is the probability of success (one in sixty million) multiplied by the benefit to Americans (which I'm supposing to be $314 billion), which equals about $5,200 of value to the people of the United States. That's the sense in which voting is like donating thousands of dollars to (developed-world) charities. For all but the ultrarich, that's a much better use of time than you could get, for example, by working the hour it takes you to vote and donating your earnings.

Some caveats on this conclusion are required. First, the total benefit per American was a purely hypothetical number, so it should be taken with a grain of salt. If you're uncertain about which party is really better, you might

reasonably think that it's an overestimate: your expected value of voting will be lower due to a greater chance of voting for the worse party; and if you're completely unsure which party is better, the expected value of voting drops to zero. If so, that's fine; you should make your own estimate of the benefits of your preferred party getting into power, and then run the calculation. Only if you think the expected benefit of one party over another is very small (perhaps less than twenty dollars of benefit per person) will you conclude that voting isn't a reasonable altruistic activity.

A more important caveat is that the probability of swinging the election varies dramatically by state. In swing states such as Colorado, New Hampshire, and Virginia, the probability goes as low as one in ten million, which makes the case for voting much stronger. Using my hypothetical number of $1,000 of benefit per person, the value of the benefit to the people of the United States of you voting for the better party is $30,000. However, in safe states, the probability of your vote making a difference is much less. In Massachusetts, for example, the probability of your vote making a difference is only one in a billion. Given my estimate of the benefit of the better party being in power, that would still mean that voting has an expected value of $300, which seems well worth it. In the District of Columbia, however, the probability of your vote making the difference is less than one in one hundred billion, giving an expected value of voting of only three dollars.

We used the idea of expected value to show why voting for the better party is often an (expected) high-impact altruistic activity. The same sort of reasoning, however, can be applied to other areas. On many issues, I find that people hold the following two views:

- If *many* people did this thing, then change would happen.
- But any *individual* person doesn't make a difference.

Holding that combination of views is usually a mistake when we consider expected value.

Consider ethical consumption, like switching to fair-trade coffee, or reducing how much meat you buy. Suppose someone stops buying chicken breasts, instead choosing vegetarian options, in order to reduce the amount of animal suffering on factory farms. Does that person make a difference? You might think not. If one person decides against buying chicken breast one day but the rest of the meat eaters on the planet continue to buy chicken, how could that

possibly affect how many chickens are killed for human consumption? When a supermarket decides how much chicken to buy, they don't care that one fewer breast was purchased on a given day. However, if thousands or millions of people stopped buying chicken breasts, the number of chickens raised for food would decrease—supply would fall to meet demand. But then we're left with a paradox: individuals can't make a difference, but millions of individuals do. But the actions of millions of people are just the sum of the actions of many individual people. Moreover, an iron law of economics is that, in a well-functioning market, if demand for a product decreases, the quantity of the product that's supplied decreases. How, then, can we reconcile these thoughts?

The answer lies with expected value. If you decline to buy some chicken breast, then most of the time you'll make no difference: the supermarket will buy the same amount of chicken in the future. Sometimes, however, you will make a difference. Occasionally, the manager of the store will assess the number of chicken breasts bought by consumers and decide to decrease their intake of stock, even though they wouldn't have done so had the number of chicken breasts bought been one higher. (Perhaps they follow a rule like: "If fewer than five thousand chicken breasts were bought this month, decrease stock intake.") And when that manager does decide to decrease their stock intake, they will decrease stock by a large amount. Perhaps your decision against purchasing chicken breast will have an effect on the supermarket only one in a thousand times, but in that one time, the store manager will decide to purchase approximately one thousand fewer chicken breasts.

This isn't just a theoretical argument. Economists have studied this issue and worked out how, on average, a consumer affects the number of animal products supplied by declining to buy that product. They estimate that, on average, if you give up one egg, total production ultimately falls by 0.91 eggs; if you give up one gallon of milk, total production falls by 0.56 gallons. Other products are somewhere in between: economists estimate that if you give up one pound of beef, beef production falls by 0.68 pounds; if you give up one pound of pork, production ultimately falls by 0.74 pounds; if you give up one pound of chicken, production ultimately falls by 0.76 pounds.

This same reasoning can be applied when considering the value of participating in political rallies. Suppose there's some policy that a group of people want to see implemented. Suppose everyone agrees that if no one attends a rally on this policy, the policy won't go through, but if one million people show up, the policy will go through. What difference do *you* make by showing

up at this rally? You're just one body among thousands of others—surely the difference you make is negligible. Again, the solution is to think in terms of expected value. The chance of you being the person who makes the difference is very small, but if you do make the difference, it will be very large indeed. This isn't just a speculative model. Professors of political science at Harvard and Stockholm Universities analyzed Tea Party rallies held on Tax Day, April 15, 2009. They used the weather in different constituencies as a natural experiment: if the weather was bad on the day of a rally, fewer people would show up. This allowed them to assess whether increased numbers of people at a rally made a difference to how influential the rally was. They found that policy was significantly influenced by those rallies that attracted more people, and that the larger the rally, the greater the degree to which those protestors' representatives in Congress voted conservatively.

For our purposes, the most important use of expected value reasoning is in comparing concrete, measurable ways of doing good with more speculative but potentially higher-payoff strategies. One example of this concerns how to compare different careers. Whereas earning to give in order to donate to charities like Deworm the World Initiative provide a reliable way of doing good, others, like politics, are much less certain. How should we compare these?

"I expect to fail," Laura Brown told me while putting down her cup of coffee. We were sitting at the Grand Café, the oldest coffee-house in the United Kingdom, and I was there to discuss Laura's career plans. A second-year student of philosophy, politics, and economics (PPE), Laura had recently read a newspaper article discussing the chances of a PPE student being elected to Parliament. Intrigued, she traced the original source, found work by my organization 80,000 Hours, and studied the calculations carefully. The research persuaded her to pursue a career in politics. "I'm most likely going to fail to become a high-flying politician. But I could do so much good if I did succeed that I think it's worth taking the chance," Laura told me.

Before making her decision, Laura had been unsure about whether to go into politics or whether to enter a lucrative career and earn to give, but she knew she wanted to pursue whatever would make the bigger difference. You might think it would be impossible to make this comparison, and it is certainly very difficult to do so. However, the idea of expected value allows us to come up with a reasonable answer. At 80,000 Hours, we did a very rough calculation to see if entering politics could plausibly be competitive with earning to give.

First, one has to work out the odds of success. The most naive estimate of

the odds of becoming an member of Parliament (MP) would be to work out how many people alive in the United Kingdom today will serve as an MP at some point in their life (which we estimated at about 3,100, with five of those becoming prime minister at some point), and divide by the total population of the United Kingdom (sixty-four million), giving a one-in-twenty-thousand chance of becoming an MP, and a one-in-twelve-million chance of becoming prime minister. However, most people don't try to enter politics, and the representation of politicians in the United Kingdom is highly skewed toward people of certain backgrounds. In particular, graduates of Oxford are dramatically overrepresented within UK politics, with graduates of PPE especially overrepresented. For example, both the current prime minister, David Cameron, and the leader of the opposition, Ed Miliband, earned degrees in PPE from Oxford. Of the 650 members of Parliament, more than one hundred studied at Oxford (despite there being only three thousand graduates every year), of which thirty-five studied PPE (despite there being only two hundred PPE graduates each year). Thirty-two percent of cabinet ministers (those MPs who hold executive power) studied PPE at Oxford, and of the thirteen prime ministers since 1945, nine studied at Oxford and three of those studied PPE.

These statistics represent some disappointing facts about political mobility and equal representation in the United Kingdom. However, for someone who is altruistically minded and happened to study PPE at Oxford, it represents a powerful opportunity. Laura was such a person. We worked out that, for a graduate in PPE from Oxford who chooses to enter politics, the historical chances of his or her becoming an MP was one in thirty, with the chances of becoming prime minister at one in three thousand. Laura's background gave her remarkably good odds of entering Parliament, but, even given this, it was still far more likely than not that she would fail, so entering politics was still a high-risk venture.

In order to determine the expected value of her pursuing politics, we next needed to work out how big an impact she would have if she did manage to enter Parliament.

This is very difficult to estimate, so we'll use what I call *lower-bound reasoning*: given that it's impossible to get a precise estimate of her potential impact in politics, we'll try to create an estimate that we feel confident is an underestimate (or a "lower bound"). Then we can say that, even though we don't know how much influence she'll have, it is *at least* as much as the lower-bound estimate. If Laura's expected impact from going into politics is greater than that

through earning to give even based on this lower-bound estimate, then we should think it likely that her expected impact really is greater through politics than through earning to give.

In the spirit of being conservative, therefore, we'll first assume that Laura's impact would only come through becoming an MP (or serving in the cabinet or as prime minister), and not through other jobs she would take within politics even if she failed to become an MP, such as being a special adviser to an MP, or working at a think tank. Second, we'll assume that the impact of being an MP comes only through government expenditure and not at all through legislation or through other forms of influence via the public platform that a political position would give her. Both of these assumptions are false, of course, but they help to ensure that we can be reasonably confident that the number we end up with is an underestimate of her expected impact.

We're therefore only trying to estimate her potential influence over governmental spending as an MP. The way we estimated this was to think on an annual basis: per year, how much influence do PPE-graduate MPs have, and how many people from PPE in Oxford try to enter politics? Again, we'll try to remain conservative throughout.

First, the annual influence that all MPs have (including the cabinet and prime minister). Total UK government spending in 2014–2015 was £732 billion. Legally speaking, MPs and ministers decide government policy and spending. However, in practice, they are restricted in how they can allocate that spending by other political actors, international bodies, and the views of the electorate. We want to be conservative, so we'll assume that each of these reduce MPs' and ministers' influence by half. Using these assumptions, we conclude that MPs and ministers influence one-eighth of government spending. However, it is civil servants who actually implement the policies that MPs and ministers decide on. This reduces the MPs' and ministers' effective influence yet again. We'll estimate that this decreases their influence by half, giving us our final estimate of the annual influence of all MPs at one-sixteenth of government expenditure, or approximately £45 billion.

Next, we estimate what proportion of that is influenced by PPE graduates. Currently, 5 percent of MPs and 32 percent of cabinet ministers studied PPE at Oxford. We'll assume that the influence of all 628 noncabinet MPs is the same as the influence of the twenty-two MPs in the cabinet (including the prime minister): so each group influences 50 percent of the approximately £45 billion that MPs in total influence. (Again this seems conservative; PPE has even higher

representation at cabinet and prime minister level than it does at MP level, and we suspect that the cabinet and prime minister really have even more influence than the other MPs combined.) We therefore estimate that people who studied PPE at Oxford influence 5% × £22.5 billion + 32% × £22.5 billion = £8 billion annually. Every year, two hundred people graduate from Oxford in PPE; however only about one-quarter of them pursue a career relevant to party politics. Therefore fifty graduates are responsible for that £8 billion of influence. The expected influence of each of those graduates (such as Laura Brown) is therefore 1 / 50 × £8 billion = £160 million. That expected influence might come, for example, from the chance of causing a change in allocation of spending between defense and overseas development aid; or it might come from the chance of improving the effectiveness of money spent on healthcare.

That's her expected financial influence, but how much is that influence worth? Because she will be restricted in how that money is used, it won't be worth as much as if she simply had £160 million that she could target to the most effective causes. Moreover, we must compare how much good that money would do given she is the person influencing it, compared to how much good that money would do if someone else were in her shoes. Again, being conservative, let's assume that the money she can influence will only do 1/50th as much good as money that was donated directly to the most effective causes. If so, then our estimate of Laura's expected impact of entering politics is as great as £8 million donated to the most effective causes.

In coming up with this number, we made conservative assumptions at every stage, assuming no impact if she didn't become an MP or cabinet minister, and assuming that her impact as an MP would come only through government expenditure rather than through legislation. We should therefore think that the £8 million figure is an underestimate of her expected impact. However, £8 million is considerably more than she could donate if she earned to give. Even given our pessimistic assumptions, for Laura, politics therefore seems to have a greater expected impact than earning to give. She wanted to have as big an impact as she could, so, in part on the basis of this reasoning, she chose to enter a political career.

The conclusion that earning to give isn't always the highest-impact career path isn't a conclusion that's unique to Oxford PPE graduates. In chapter nine we'll discuss several other career paths that have a low probability of success but a very high upside given success, such as research and entrepreneurship, that seem highly competitive with earning to give.

As well as assessing careers, the concept of expected value can be used to assess efforts to effect political change. Donating to highly effective charities provides a comparatively concrete, reliable, and measurable way of doing good. But the potential gains of systemic change are even greater: if you can find the right area, funding or participating in political campaigns could potentially do even more good. It's not generally necessary to work out an exact numerical estimate for the expected value of that activity; rough estimates based on reasonable numbers can show you approximately how great the expected value is. The point is simply that long shots can be worth it if the payoff is big enough.

When assessing a potential course of action, one should therefore not dismiss it as ineffective by saying "that'll never happen." Many ethical ideas that are now commonsense seemed highly radical in the past. The idea that women, or black people, or nonheterosexual people should have equal rights was considered ridiculous up until very recently, historically speaking. In 1790, Benjamin Franklin, for example, wrote a letter petitioning the US Congress to end slavery. Congress debated the petition for two days. The defenders of slavery had no shortage of objections. "Who is going to compensate the slave owners?" they asked, and, "What is the mixing of races going to do to American values and character?" Yet slavery was abolished wholesale, and we now find those objections indefensible. Those activists who campaigned for equal rights for women, black people, and the LBGT community were right to do so, not because they had a good chance of succeeding in the short term, but because the benefit was so great if they did succeed.

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Climate change is another issue where the concept of expected value proves useful, in three different ways. First, it shows that the debate over whether man-made climate change is happening is pretty irrelevant when it comes to what we ought to do. In these debates, one group points to the scientific consensus that man-made climate change is happening while the other argues that the jury is still out. To be clear, there really is near consensus among scientists that man-made climate change is happening. A UN-backed panel of thousands of climate scientists, the Intergovernmental Panel on Climate Change has said that "it is extremely likely that human influence has been the dominant cause of the observed warming since the mid-twentieth century," where they define "extremely likely" to mean at least 95 percent probability. One article reviewed

four thousand papers that discuss global warming and reported that "97.1 percent endorsed the consensus position that humans are causing global warming."

However, this debate is strange for another reason: *even if* scientists had not already shown that man-made climate change is happening, the mere fact that man-made climate change *might* be happening is enough to warrant action. As an analogy, suppose you have a carbon monoxide detector in your home that is a bit sensitive. It gives a false alarm every four out of five times it goes off but is otherwise accurate. You're watching TV one evening, and the alarm goes off. What do you do? If you reason to yourself, "It's probably a false alarm, so I won't bother doing anything," you'd be making a grave mistake. If it's a false alarm but, to be safe, you turn off the boiler and open the windows, the worst that happens is you've made yourself chilly for an evening and had to pause your favorite TV show unnecessarily. In contrast, if there really is a carbon monoxide leak and you do nothing, you may die. Given this, the sensible thing is to turn off the boiler. The one-in-five chance of death clearly outweighs the four-in-five chance of being a little cold temporarily for no reason.

Our predicament with climate change is no different. If climate change is happening and we don't take action, millions of lives will be lost and the world economy will lose trillions of dollars. If climate change isn't happening and we *do* take action, the costs are much lower. We would have wasted some amount of resources developing low-carbon technology and slowed economic progress a bit, but it wouldn't, literally, be the end of the world.

The second way in which expected value is relevant when figuring out how to respond to the threat of climate change is that it shows why individuals have reason to mitigate climate change just as much as governments do. Over your lifetime, your individual greenhouse gas contribution will only increase the temperature of the planet by about half a billionth of a degree Celsius. That, you might think, is such a small difference as to be negligible, so you shouldn't bother trying to reduce your personal emissions.

This reasoning, however, doesn't consider expected value. It's true that increasing the planet's temperature by half a billionth of a degree probably won't make a difference to anyone, but sometimes it will make a difference, and when it does, the difference will be very large. Occasionally, that increase of half a billionth of a degree will cause a flood or a heat wave that wouldn't have happened otherwise. In which case the *expected* harm of raising global temperatures by half a billionth of a degree would be fairly great. We know that something like this has to be the case because we know that, if millions of

people emit greenhouse gasses, the bad effects are very large, and millions of people emitting greenhouse gasses is just the sum of millions of individual actions.

Finally, expected value is important when assessing just how bad climate change is and how much of a change we should make. When I first looked into economic assessments of the damages of climate change, I was surprised to find that economists tended to assess climate change as being not all that bad. Most estimate that climate change will cost only a couple percent of global gross domestic product (GDP). This, of course, is huge when measured in the trillions of dollars lost. But it's not that large when compared to somewhat typical rates of slow economic growth. Economic growth per person over the last decade has been about 2 percent per year, so a loss of 2 percent of global GDP due to climate change is therefore equivalent to going one year without economic growth. The thought that climate change would do the equivalent of putting us back one year economically isn't all that scary—2013 didn't seem that much worse than 2014.

We can see the same thing on an individual level. Carbon dioxide equivalent, or CO_{2eq}, is a way of measuring your carbon footprint that includes greenhouse gasses other than carbon dioxide, like methane and nitrous oxide. For example, one metric ton of methane produces as much warming as twenty-one metric tons of carbon dioxide, so one metric ton of methane is twenty-one metric tons of CO_{2eq}. On typical estimates, the social cost of one metric ton of CO_{2eq} is about thirty-two dollars: incorporating costs both now and in the future, emitting one metric ton of CO_2, or an equivalent amount of greenhouse gasses like methane or nitrous oxide, costs all people a total of thirty-two dollars. The average American emits about twenty-one metric tons of CO_{2eq} every year. So the social cost of one American's greenhouse gas emissions is about $670 every year. Again, that's a significant cost, but it's also not the end of the world. For those living in other countries, the cost of greenhouse gas emissions is significantly less again; people in the United Kingdom, for example, only emit about nine metric tons of CO_{2eq} every year, so the harm they cause every year is only $275.

However, this standard economic analysis fails to faithfully use expected value reasoning. The standard analysis looks only at the effects from the most likely scenario: a 2 to 4ºC rise in temperature. It doesn't consider what the consequences would be if our best-guess estimates are wrong. This is especially important because the climate is an incredibly complex system that is difficult to

predict, so we can't be sure that our estimates are correct. When climate scientists make estimates about temperature rise, they have to acknowledge that there is a small but significant risk of a temperature increase that's much greater than 2 to 4ºC. The Intergovernmental Panel on Climate Change gives more than 5 percent probability to temperature rises greater than 6ºC, and even acknowledges a small risk of *catastrophic* climate change, of 10ºC or more. To be clear, I'm not saying that this is at all likely, in fact, it's very unlikely. But it is possible, and if it were to happen, the consequences would be disastrous, potentially resulting in civilizational collapse. It's difficult to give a meaningful answer to the question of how bad that would be, but if we think it's potentially catastrophic, then we need to revise our evaluation of the importance of mitigating climate change. In that case, the true expected social cost of CO_2 could be much higher than thirty-two dollars per metric ton, justifying much more extensive efforts to reduce emissions than the estimates the economists first suggested.

Just as most of the value from aid programs comes from the very best aid programs (which we discussed in chapter three), it's often the case that most of the expected harm from disasters come from the very worst disasters. (That is: the death tolls from disasters form a fat-tailed distribution. Nassim Taleb describes these as Black Swans: very rare events that have a very great impact.) For example, most people who've died in war have died in the very worst wars: of the four hundred wars in the last two hundred years, about 30 percent of deaths were from World War II alone. This means that if we're concerned by war, we should spend most of our efforts trying to prevent the very largest wars from occurring, or to limit their scope. Similar considerations are true of earthquakes, floods, and epidemics.

In cases where people seem to neglect the risks of worst-case outcomes, helping to prevent these outcomes might be a particularly effective altruistic activity. This is what the Skoll Global Threats Fund focuses on, trying to reduce the chances of global catastrophes arising from climate change, pandemics, and the proliferation of nuclear weapons. The charity evaluator GiveWell is currently investigating these sorts of activities in an attempt to work out how effective donations in these areas can be.

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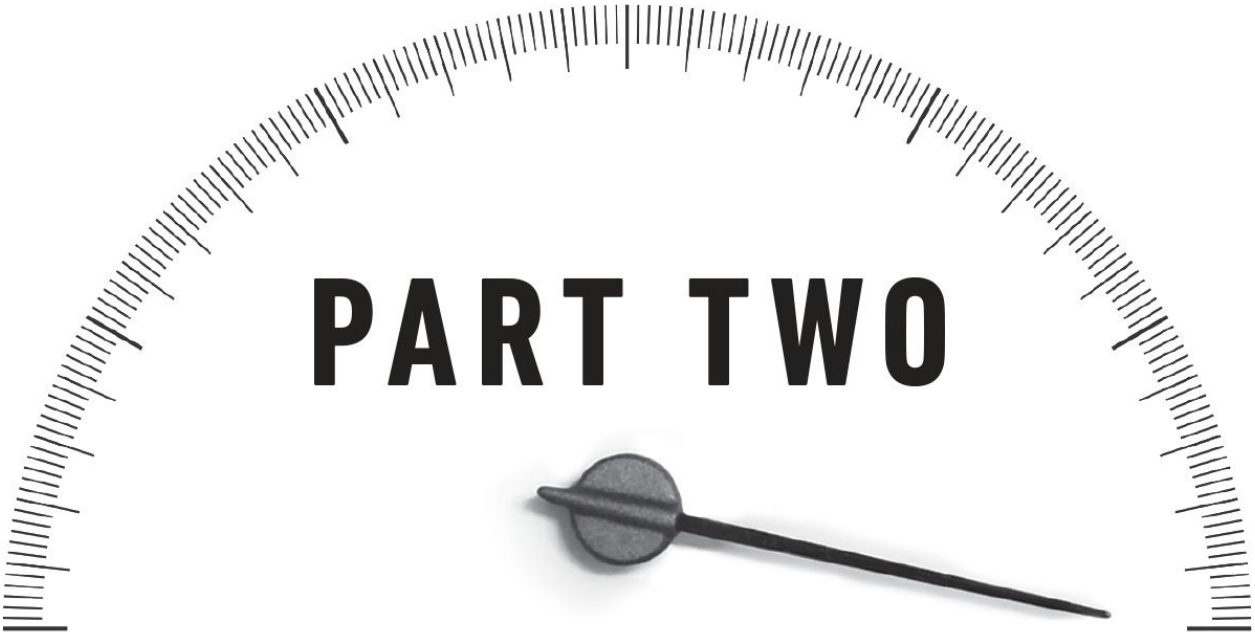
From the previous chapters, you might have felt that effective altruism is limited

only to those activities with comparatively easy-to-quantify benefits, like deworming schoolchildren or distributing antimalarial bed nets. I hope that this discussion of expected value has helped to show why that's not the case. Even in what seem like "unquantifiable" areas like political change and disaster prevention, we can still think rigorously, in an evidence-based manner, about how good those activities are. We just need to assess the chances of success and how good success would be if it happened. This, of course, is very difficult to do, but we will make better decisions if we at least try to make these assessments rather than simply throwing up our hands and randomly choosing an activity to pursue, or worse, not choosing any altruistic activity at all.

I've now introduced the key questions to help you think like an effective altruist: How many people benefit, and by how much? Is this the most effective thing you can do? Is this area neglected? What would have happened otherwise? What are the chances of success, and how good would success be? Now it's time to examine how we apply those questions to the real world and put effective altruism into action. That is the subject of the next part of this book.



EFFECTIVE ALTRUISM IN ACTION

SEVEN

OVERHEAD COSTS, CEO PAY, AND OTHER CONFUSIONS

Which charities make the most difference?

Suppose I give you a hundred dollars and tell you to donate the whole sum to one of three charities, each of which is attempting to address a different problem facing poor countries in Africa. Which would you choose?

First, Books For Africa (BFA). BFA's mission is to improve education by shipping donated books from the United States to the African continent, where they are distributed by nonprofit partners. Founded in 1988, it has shipped more than twenty-eight million books to forty-nine different countries. On their website, they vividly describe the problem and their solution:

> Most African children who attend school have never owned a book of their own. In many classrooms, 10–20 students share one textbook. . . . Although Books For Africa has made tremendous progress in its mission, the book famine in Africa remains a reality. Where books are available, there are still very few. Empty library shelves are a constant reminder of Africa's desperate need for printed materials. If we are to see the day when African school children are to have the books they need to learn the skills necessary to provide for themselves and others, Books For Africa must continue to send millions of books.

Former UN secretary-general Kofi Annan has personally endorsed BFA, saying, "Books For Africa is a simple idea, but its impact is transformative. For us, literacy is quite simply the bridge from misery to hope."

The second organization is Development Media International (DMI). Their

focus is on preventing deaths of African children under the age of five. They aim to do this by designing and broadcasting radio and TV programs that provide health education, such as encouraging breast-feeding (to improve child health), proper hand washing (to decrease rates of diarrheal disease), and the use of antimalarial bed nets. Sometimes these take the form of minute-long radio ads, broadcast several times a day. Other times they create stand-alone soap operas with educational messages built in. In their words:

> 6.3 million children worldwide die under the age of five every year. In 2013 one in 11 children in sub-Saharan Africa died before their fifth birthday. . . . Many people cannot recognize when their child has a potentially dangerous illness, or do not know what to do about it, so many deaths are due to lack of knowledge rather than lack of healthcare services. If a mother can recognize that her baby has diarrhea and is able to provide her child with oral rehydration therapy [a piece of advice that DMI promotes], then the child is far more likely to reach the age of five.

DMI currently operates in Burkina Faso, and has plans to run similar programs in the Democratic Republic of the Congo, Mozambique, Cameroon, and the Ivory Coast.

The third organization is GiveDirectly. Its program is simple: it transfers money from donors directly to some of the poorest people in Kenya and Uganda who are then free to use that money however they wish. Using what is called the M-Pesa system, cell phones are used as makeshift bank accounts, thereby enabling an easy transfer of money from foreign bank accounts to the poor. GiveDirectly uses satellite images to find households with thatched roofs (a strong indicator of poverty, compared to iron roofs) and then contacts those households to discuss the program. If the household is willing, GiveDirectly transfers them a lump sum of $1,000, which is equal to a little more than one year's total income for that household. In their words:

> Recipients use transfers for whatever is most important to them; we never tell them what to do. An independent evaluation of our work in Kenya by Innovations for Poverty Action found that recipients use transfers for a wide variety of purposes that on average generate large income gains. Common uses range from buying food to investing in tangible assets such as housing and livestock to investing in children's

education.

Of these three charities, which do you think can do the most good with your one-hundred-dollar donation? In this chapter, I'll provide a framework that will help you determine the answer.

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One popular way of evaluating a charity is to look at financial information regarding how the charity spends its money. How much does the charity spend on administration? How much is its CEO paid? What percentage of donations are put directly to the charity's main programs? This is the approach that Charity Navigator, the oldest and most popular charity evaluator, has taken for the last fifteen years. According to Charity Navigator, "Savvy donors know that the financial health of a charity is a strong indicator of the charity's programmatic performance. They know that in most cause areas, the most efficient charities spend 75 percent or more of their budget on their programs and services and less than 25 percent on fund-raising and administrative fees."

Using these metrics, let's see how the three charities compare.

Books For Africa's overhead costs are a tiny 0.8 percent of their total expenditure (which was $24 million in 2013), and their CEO is paid $116,204, which is only 0.47 percent of that total expenditure. For these reasons, and for their general financial transparency, Charity Navigator has given BFA its highest four-star rating for seven years running. As I write this book, it's rated number three on Charity Navigator's list of 10 Top-Notch Charities and carries a near-perfect score of 99.93 out of 100.

GiveDirectly isn't rated by Charity Navigator but would also do well by these metrics. So far, of every dollar that's been donated to GiveDirectly, between eighty-seven cents (in Uganda) and ninety cents (in Kenya) has been transferred to the poor, with the rest spent on enrollment and follow-up and transfer costs. With every dollar GiveDirectly spent on fund-raising, they raised one hundred dollars in donations, a remarkable number compared to the average of four dollars of donations raised for every dollar spent on fund-raising. They had an overhead ratio of 6 percent, spending just $124,000 on administration costs out of an expenditure of $2.2 million. Much of the administration spending was on fixed costs, so as GiveDirectly increases the amount of cash they transfer, the percentage spent on overhead is likely to go down considerably.

In contrast, Development Media International's overhead amount to 44 percent of its total budget, and there is little financial information on its website. Charity Navigator evaluates only US-based charities, so it does not evaluate DMI, which is based in the United Kingdom. However, DMI clearly performs much worse than the two other charities according to Charity Navigator's metrics.

The idea that you should use financial information to compare charities has been highly influential, and Charity Navigator has seen major success as a result. In 2012, their site received a total of 6.2 million visits and they claimed to have influenced approximately $10 billion of charitable donations. Their metrics have become the gold standard for determining whether a charity is efficient, and intuitively it makes some sense. If we're donating our hard-earned money to a charity that supports a cause we believe in, we want to feel confident that our donation is actually working to advance that cause rather than being wasted. Based on this metric, you would choose Books For Africa as the best charity, then GiveDirectly, then Development Media International.

Given the lessons of the previous chapters, however, we should already understand that this approach to evaluating a charity's effectiveness is seriously misguided.

For starters, think about the logic behind this reasoning if you apply it to personal spending. Suppose you're deciding whether to buy a Mac or a PC. What factors would you consider? You'd probably think about the design and usability of the two computers, the hardware, the software, and the price. You certainly wouldn't think about how much Apple and Microsoft each spend on administration, and you wouldn't think about how much their respective CEOs are paid. Why would you? As a consumer you only care about the product you get with the money you spend; details about the financials of the companies who make the products are almost always irrelevant. If Apple spent a lot of money to attract a more talented management team, you might even consider that a good sign that their products were the best on the market!

If we don't care about financial information when we buy products for ourselves, why should we care about financial information when we buy products for other people? Take a silly example: imagine I set up a charity that distributes doughnuts to hungry police officers and I am so enthusiastic about the mission that I manage to spend only 0.1 percent of the charity's money on overhead, with the rest spent on doughnuts and distribution. Suppose, moreover, that I, as the CEO of this charity, don't take a salary at all. Would I really have

created an amazing charity? Surely, as we discussed in chapter two, what we should ultimately care about is the *impact* charities have. When you give a hundred dollars to a charity, what does that charity *do* with it? How are people's lives improved as a result?

We can begin to answer this question by comparing the concrete outcomes that donating to each charity achieves. Books For Africa ships one book with every fifty cents donated to them. GiveDirectly gives the poor ninety cents with every dollar donated to them. Development Media International spends $1.5 million to run a mass media campaign promoting health education in a particular country. But those numbers alone don't tell us that much. Is it better to ship three million schoolbooks, transfer $1.35 million to poor people, or educate a country's populace about how they can stay healthy (each of which would cost $1.5 million)? To answer that, we have to know how these different expenditures affect people's lives.

The first thing to explore is whether there's high-quality evidence regarding the impact of the program that the charity implements. It might seem obvious that distributing textbooks is beneficial, but there's surprisingly little good evidence in favor of this idea and some evidence against. Development economists have tested textbook distribution (remember Michael Kremer?) and have found that, in the absence of teacher training, providing textbooks has either no discernible effect on children's school performance, or only a limited effect on the very most able students.

This should discourage us from choosing Books For Africa. It's supposedly "efficient" for having low administrative costs, but what's really important is how much good is done per dollar spent on the program the charity implements. Books For Africa is a promising charity insofar as it focuses on the very poorest people in the world, and it implements a program that at least seems like it should work. But the evidence in favor of the other two charities' programs is much stronger. In my view, therefore, BFA is the least effective of our three candidates.

We clearly need a better set of standards. Here are the five questions I think any donor should ask before deciding where to give. They are based on the criteria used by the charity evaluator GiveWell, which has spent the last eight years trying to figure out which charities improve lives the most with the donations they receive.

1. **What does this charity do?** How many different types of

programs does it run? For each of these programs, what exactly is it that this charity does? If it runs more than one program, why is that?

2. **How cost-effective is each program area?** Is the charity focused on one of the most important causes? How cost-effective does the evidence suggest the program to be?
3. **How robust is the evidence behind each program?** What is the evidence behind the programs that the charity runs? Are there trials showing that the program is effective? Does the charity rigorously monitor and evaluate the success of its programs?
4. **How well is each program implemented?** Do the leaders of the charity have demonstrated success in other areas? Is the charity highly transparent? Does it acknowledge mistakes that it's made in the past? What are the alternative charities you could give to? Are there good reasons for supposing that this charity is better than others?
5. **Does the charity need additional funds?** What would additional funding be used to do? Why haven't other donors already funded the charity to the point it can't use extra money?

This framework enables us to genuinely assess charities in terms of their impact, rather than on flawed metrics like administrative overhead. I'll explain each aspect of the framework in turn, in every case using the framework to compare our two remaining charities—GiveDirectly and Development Media International—against each other.

What does this charity do?

This might seem like an obvious question, but often what most people think a charity does is quite different from what it actually does. For example, I was surprised to find out that many developed-world medical charities spend only a small fraction of their money on research, with the rest spent on other programs, even though research is what they emphasize in their marketing and websites. The American Cancer Society spends 43 percent of its program expenses on patient support, 21 percent on prevention, 14 percent on detection/treatment, and just 22 percent on research. The ALS Association (of ice-bucket-challenge fame)

spends 41 percent of its program expenses on public and professional education, 24 percent on patient and community services, and just 35 percent on research. That's not a reason in itself against donating to any of these charities, and that's not to say that any of these charities have been misleading in their marketing, but you would assess them differently knowing that donations support numerous programs rather than just research.

I've already explained what GiveDirectly and DMI do. What we need to know then is how good each of these programs are.

How cost-effective is each program area?

We want to estimate what a charity achieves with a given amount of money, so our focus should always be on *cost-effectiveness* rather than just *effectiveness.* Charity A and Charity B might both be *effective* at distributing deworming drugs (that is to say, they successfully distribute them), but if Charity B can do so at half the cost, then a donation to them will do twice as much good.

The first step in estimating cost-effectiveness is to find out how much the charity spends per person to run their program. For example, it costs about six dollars to deliver one antimalarial bed net, which on average protects two children for two years, so it costs $1.50 to protect one child for one year. It costs GiveDirectly one dollar to give someone in extreme poverty ninety cents; it costs DMI between forty and eighty cents per listener per year to run their education campaigns. Ultimately, however, we should try to figure out how this converts into impact on people's well-being. These figures don't yet tell us whether GiveDirectly or DMI does more good. To do that we need to assess how these programs actually affect people's lives.

The obvious first question to ask about GiveDirectly is: What do the recipients of these cash transfers do with the money? If they spend it on education, that sounds pretty good; if they spend it on drugs and alcohol, that's worrying. It turns out that the most common use of the transfer is to buy assets, typically farm animals, or to convert thatched roofs into iron ones; on average, recipients spent 39 percent of the transfer on assets. These purchases seem to have very high returns, potentially as high as 14 percent per year for at least a period of several years.

Cash transfers also seem to have several less-tangible effects. Recipients of the transfer reported significant increases in subjective well-being, reported

significant decreases in number of whole days gone without food, and scored significantly higher on an index of female empowerment, though there weren't significant increases in health or education during the time period studied.

The estimated cost-effectiveness of GiveDirectly's programs is very impressive, but the estimated cost-effectiveness of DMI's program is even more so.

There's a lot of health knowledge that we have without even realizing it. For example, everyone in the United States knows to wash their hands regularly; it's a lesson that's drilled into us from childhood. Moreover, we know to use soap and that just because our hands *look* clean that doesn't mean they *are* clean. In many poor countries, however, people have never been taught this, or they regard soap as a precious commodity and are therefore reluctant to use it for hand washing. This can have severe consequences. Diarrhea is a major problem in the developing world, killing 760,000 children every year, primarily through dehydration. (For comparison, that's a death toll equivalent to five jumbo jets crashing to the ground every day, killing everyone on board.) A significant number of those deaths could be avoided through simple improvements to sanitation and hygiene, like more regular hand washing with soap.

The ads that DMI runs are terribly corny. (In one, a baby has a conversation with a group of diseases that are ultimately defeated by breast-feeding.) Through those ads, however, DMI can teach people crucial pieces of information, like the importance of breast-feeding immediately after childbirth or the proper use of bed nets, for pennies per person.

When looking at DMI's impact, we can use QALYs as a measurement (where one QALY, remember, represents one year of perfect health). Studies and models of mass media education have estimated that it costs about ten dollars to provide one QALY. In chapter three, I mentioned that by providing insecticide-treated bed nets, you could provide one QALY for just one hundred dollars (equivalent to saving a life for $3,400), and I pointed out that this was an astonishing fact. If the estimate of ten dollars per QALY is correct, however, we could do ten times as much good, the equivalent of saving a life for just $360, by donating to DMI.

It's difficult to know how to value the increases in income, psychological well-being, and empowerment that DMI provides compared with QALYs and lifesaving, but under any reasonable assumptions about how to make the comparison, on the basis of these estimates, DMI looks like the clear winner.

To see this, note that a $1,000 cash transfer represents approximately a

doubling of annual household income, where a household consists of about five people. Supposing, very generously, that that doubling of income lasts for ten years thanks to returns on investment, the entire household will therefore be twice as rich as they would otherwise have been for ten years following the transfer. We can then ask which is the larger benefit: saving approximately three lives (as you would by donating $1,000 to DMI), or doubling the income of five people for ten years (as you would by donating to GiveDirectly). It seems clear that saving three lives is providing a larger benefit with the same amount of resources.

Based on estimated cost-effectiveness, therefore, DMI seems like a better bet. In order to fully compare DMI with GiveDirectly, however, we need to complete our framework.

How robust is the evidence behind each program?

Often we should prefer a charity that has very good evidence of being fairly cost-effective to a charity that has only weak evidence of being very cost-effective; if the evidence behind an estimate is weak, it's likely that the estimate is optimistic, and the true cost-effectiveness is much lower.

For example, the evidence behind claims made on charities' websites or in marketing materials is often very shaky, and sometimes even downright misleading. On its website, the charity Nothing But Nets says that, "one $10 bed net can mean the difference between life and death." In one sense, this is true: bed nets do save lives, so a single bed net *can* save a life. But not every child who is protected by a bed net would otherwise die of malaria, so it's not true that $10 spent distributing bed nets *will* save a life; and if you weren't reading Nothing But Nets' message closely, you might form the impression that that's what they mean. It's cheap to save or improve lives in poor countries, but it's not *that* cheap. (For a sanity check, remember that even the very poorest people in the world live on sixty cents per day; if it cost ten dollars to save a life, then we'd have to suppose that they or their family members couldn't save up for a few weeks, or take out a loan, in order to pay for the lifesaving product.)

Claims of a program's effectiveness are more reliable when grounded in academic studies. If there's been a meta-analysis—a study of the studies—that's even better. Even then, there can be cause for concern because the program that a charity implements might be subtly different from the programs that were

studied in the meta-analysis. Knowing that, it's even better if the charity has done its own independently audited or peer-reviewed randomized controlled evaluations of its programs.

Robustness of evidence is very important for the simple reason that many programs don't work, and it's hard to distinguish the programs that don't work from the programs that do. If we'd assessed Scared Straight by looking just at before-and-after delinquency rates for individuals who went through the program, we would have concluded it was a great program. Only after looking at randomized controlled trials could we tell that correlation did not indicate causation in this case and that Scared Straight programs were actually doing more harm than good.

One of the most damning examples of low-quality evidence concerns microcredit (that is, lending small amounts of money to the very poor, a form of microfinance most famously associated with Muhammad Yunus and the Grameen Bank). Intuitively, microcredit seems like it would be very cost-effective, and there were many anecdotes of people who'd received microloans and used them to start businesses that, in turn, helped them escape poverty. But when high-quality studies were conducted, microcredit programs were shown to have little or no effect on income, consumption, health, or education. Rather than starting new companies, microloans are typically used to pay for extra consumption like food and healthcare, and the rate of interest on them is usually very high. There's even concern that they can cause harm by providing a tempting short-term income boost at the expense of longer-term financial security: people take out a loan in order to pay for food or health-care costs of family members but then enter debt that they are unable to repay. The latest evidence suggests that, overall and on average, microlending does have a small positive improvement on people's lives, but it's not the panacea that the anecdotes portray.

With these warnings at the top of our minds, how should we compare GiveDirectly and DMI? Here, GiveDirectly clearly has the edge. Cash transfers are one of the most well-studied development programs, having been shown to improve lives in many different countries around the world. They also easily pass a sanity check as to whether they will be effective: the recipients of the transfers are very well-placed to know their most pressing needs, and they can use additional resources to fill those needs in ways they know will benefit them. Finally, the independent development think tank Innovations for Poverty Action has run a randomized controlled trial on GiveDirectly, so we can be confident

not just about the efficacy of cash transfers in general but also about cash transfers as implemented by GiveDirectly.

Because cash transfers is such a simple program, and because the evidence in favor of them is so robust, we could think about them as like the "index fund" of giving. Money invested in an index fund grows (or shrinks) at the same rate as the stock market; investing in an index fund is the lowest-fee way to invest in stocks. Actively managed mutual funds, in contrast, take higher management fees, and it's only worth investing in one if that fund manages to beat the market by a big enough margin that the additional returns on investment are greater than the additional management costs. In the same way, one might think, it's only worth it to donate to charitable programs rather than simply transfer cash directly to the poor if the other programs provide a benefit great enough to outweigh the additional costs incurred in implementing them. In other words, we should only assume we're in a better position to help the poor than they are to help themselves if we have some particularly compelling reason for thinking so.

In the case of mass media education, we *do* have a plausible explanation for why it could be more effective than cash transfers: mass media health education isn't something individuals can buy, and even if they could, they probably wouldn't know just how valuable it is. Markets alone cannot provide mass media health education, so it needs to be funded and implemented by governments or nonprofits.

However, the mere fact we have a plausible explanation for how mass media education could be more cost-effective than cash transfers doesn't show that it is more cost-effective. When we look at the evidence for supporting mass media education, we find it's weaker than the evidence for cash transfers.

There are three main sources of evidence, each of which provides an estimate of approximately ten dollars per QALY for mass media campaigns. First, there are some published studies of mass media health education programs, but these are of far lower quality and relevance than the studies behind GiveDirectly's program. Second, there is a mathematical model created by DMI prior to rolling out its interventions. But a model is only as good as its assumptions, and the assumptions put into this model may be optimistic. Finally, there are the midline results from the randomized controlled trial that DMI is performing on its own program. Their program is based in Burkina Faso, which has many local radio services. This means that they can implement their program in seven districts, monitor health indicators in an additional seven districts, and then compare mortality and disease prevalence in order to see what effect their

program has. These midline results are very promising, but they are based on self-reports, which are not as accurate as indicators like mortality.

The fact that the evidence for the ten-dollars-per-QALY figure is weaker than the evidence for GiveDirectly's cost-effectiveness estimates provides a reason for preferring GiveDirectly to DMI. Because the evidence behind their program is better, we can be more confident that GiveDirectly's estimates are approximately accurate, whereas the ten-dollars-per-QALY figure provided by DMI may be optimistic.

How well is each program implemented?

Even if a charity has chosen an extremely cost-effective program with very robust evidence supporting it, it still might implement that program badly. For example, distribution of antimalarial bed nets is an extremely cost-effective program if implemented correctly, but if recipients of the bed nets don't believe they're necessary or don't believe they're effective, they may use them for other purposes. For example, a study of bed nets distributed by the Kenyan government found that recipients often used bed nets for catching and drying fish. That's why the Against Malaria Foundation, for example, educates the recipients of its nets about proper use and benefits and later conducts site visits, taking photographs to ensure that the nets are correctly installed after use.

An even more common problem than knowing that a charity implements a program badly is simply not knowing whether it implements it well. Most charities provide almost no information about the programs they run, making it difficult to assess their effectiveness.

Both GiveDirectly and DMI seem excellent in terms of the quality of their implementation. GiveDirectly is led by a leading development economist; DMI is led by someone with extensive experience and achievements in radio education, and has an advisory board that includes some of the world's best epidemiologists and development economists. Both charities are extremely transparent. GiveDirectly even goes so far as to provide information on how many cash transfer recipients reported having to pay bribes to the local agents who transferred them the money (at the time of writing, the figure is 0.4 percent). This sort of admission is very encouraging: showing that the charity cares about identifying and fixing mistakes, rather than brushing them under the rug.

Does the charity need additional funds?

Even if you've found a charity that works on an extremely cost-effective program with robust evidence behind it, we still need to ask whether your contribution will make a difference. Many effective programs are fully funded precisely because they are so effective. For example, developing-world governments usually fund the costs of the cheapest vaccination programs, such as those for tuberculosis, polio, diphtheria, tetanus, pertussis, and measles, providing these vaccines through existing health systems. These programs also receive substantial support from Gavi, the global vaccine alliance, which received $4.3 billion in funding for 2011 to 2015, exceeding its target of $3.7 billion. For these programs, the main obstacle for universal coverage is logistical rather than a lack of funds.

The same can be true on a smaller scale. Even if there's plenty of room for more funding for a program in general, it can be difficult for a specific charity to scale up rapidly. If a charity has recently received a windfall, it might not be able to use additional donations effectively. This may have been true of the Against Malaria Foundation in 2013: GiveWell had named it its top-recommended organization in 2012, and it received a surge in donations totaling $10 million. They struggled to spend that money quickly, which suggested that additional donations to them wouldn't have the same effectiveness as previous donations, so GiveWell didn't recommend them in 2013. (They successfully increased their capacity in 2013, however, so GiveWell recommended them again in 2014.)

Both GiveDirectly and DMI are in a good position to use more funding, but GiveDirectly could do more with additional funds than DMI could. GiveDirectly could productively use an additional $25 to $30 million of donations in 2015 and expects to receive about $10 million, whereas DMI could productively use $10 million in 2015 and expects to receive $2 to $4 million. Moreover, the limit for how much money could be spent on cash transfers is much higher than how much could be spent on mass media education. Conceivably, hundreds of billions of dollars could be spent on cash transfer programs; whereas providing mass media health education in every country in the world would cost much less.

At the moment, this difference doesn't pose a problem, but depending on how each charity's funding situation progresses, it might provide a reason against donating to DMI. If DMI will close their funding gap regardless of whether you donate to them, then your specific donation will do very little.

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This brings us to the end of the framework. What should we conclude about which charity will do more good with one hundred dollars? As you may have guessed, I deliberately chose these two charities because the answer is unclear. Of the considerations we've canvassed, the most important issues are estimated cost-effectiveness versus robustness of evidence. The estimated cost-effectiveness of DMI is higher than that of GiveDirectly, but the evidence behind that estimate is weaker than the evidence behind the estimate of GiveDirectly's cost-effectiveness. Which charity one chooses depends crucially on how skeptical one should be of explicit cost-effectiveness estimates, and that depends on your level of optimism or pessimism about this program. This is a common difficulty we face when trying to do good: When should you pursue an activity with more robust evidence of more limited impact, versus an activity with much weaker evidence of potentially much greater impact?

If I had to choose between giving to these two charities today, I'd donate the one hundred dollars to DMI rather than to GiveDirectly, with my reasoning based on the "expected value" considerations we discussed in chapter six. I think that DMI is probably considerably less cost-effective than the ten-dollars-per-QALY figure, but if that figure is even approximately accurate, then DMI is *much* more cost-effective than GiveDirectly. I therefore think the expected value of donating to DMI is higher than that of donating to GiveDirectly. However, I would not fault someone who thought that the robustness of evidence in favor of GiveDirectly was more important than their lower estimated cost-effectiveness, and chose to donate to them instead. Assessing charity effectiveness is difficult, and these are both excellent charities whose programs provide impressive benefit to the extremely poor.

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We've just looked at three specific charities and determined that two of them are cost-effective. But what about all of the other charities in the world? I don't have the time or the space in this book to apply my framework to every charity, so to end this chapter I'll provide a list of some highly cost-effective charities, based on GiveWell's research, and I'll provide a brief explanation of why each of them passes muster. Before then, I want to clarify a few things.

First, you won't find mega-charities like WorldVision or Oxfam or UNICEF

on these lists. These charities run a variety of different programs, and for that reason they are very difficult to evaluate. I also think it's unlikely that, even if we were able to evaluate them in depth, we would conclude that they are as effective as the charities I list here. If a charity implements a variety of programs, inevitably some of these programs will be more effective than others. In which case, we should simply focus on funding those very best programs. For example, we argued earlier that disaster relief is generally not the most effective use of funding, but many mega-charities spend a large portion of their energies on just that.

Second, you might be surprised by how few charities are on this list. Aren't there dozens of worthy charities in the world? Yes, there are, but that doesn't mean you should give to them all. As we saw in chapter three, the best charities are often far better even than merely very good charities. Given that we've got only limited money to spend, we should focus on the very best charities rather than merely very good charities.

Finally, you'll notice that most of the charities I discuss implement health-based programs in poor countries. I've already explained that we should focus on poor countries because it's easier and cheaper to save lives there than it is in developed countries. But what about education, or water provision, or economic empowerment? These are all promising areas, but global health stands out for a couple of reasons. First, it has a proven track record: smallpox eradication is the clearest example, but development aid has made significant contributions toward other areas of health, such as polio, measles, diarrheal disease, and guinea worm disease. In contrast, the link between aid and economic growth is less clear. Second, by its nature, the evidence behind health interventions is more robust. If we know that the drug albendazole kills the parasitic worm *Ascaris lumbricoides* in the United States, we can safely conclude that it will probably kill that same worm in Kenya or India because human bodies are pretty similar all round the world. In contrast, it's much harder to be confident that an educational program that works in India will also work in Kenya, where the culture and educational infrastructure are very different.

There can be exceptions to the "focus on global health" heuristic: GiveDirectly, for example, is an economic empowerment charity. We will also look at a much broader array of causes in chapter ten, though the arguments for focusing on these causes rather than global health will necessarily be more speculative.

With those caveats out of the way, let's look at some extremely cost-

effective charities, as judged by GiveWell (accurate as of January 2015), the leading effective altruism charity evaluator. I'll rate each charity along four dimensions: estimated cost-effectiveness, robustness of evidence, implementation, and room for more funding. These ratings should be used to compare to one another only the charities I list, because, for example, any charity that I assess as "fairly cost-effective" in the following list would look extremely cost-effective compared to charities not listed here. For completeness and for purposes of comparison, I'll include those we've already read about: GiveDirectly, Development Media International, Deworm the World Initiative, and Against Malaria Foundation.

TOP CHARITIES

GiveDirectly

What do they do? Provide direct unconditional cash transfers to poor households in Kenya and Uganda.

Estimated cost-effectiveness? Fairly cost-effective. One dollar in donations results in ninety cents delivered to the poorest households in Kenya and Uganda; this leads to increases in investment, consumption, education spending, and subjective well-being.

Robustness of evidence? Extremely robust. There have been a large number of studies of cash transfers showing their efficacy, and GiveDirectly has collaborated with independent evaluators to conduct a randomized controlled trial of its own program.

Implementation? Extremely well implemented. The charity is run in part by a leading development economist. They are very open, transparent, and self-skeptical.

Room for more funding? Extremely large. They estimate they could productively use an additional $20 to $30 million in 2015 and expect to receive about $10 million in that time. The potential for GiveDirectly to scale up beyond that in future years is very great.

Development Media International

What do they do? Produce and run radio shows to educate people in Burkina Faso on basic health matters, with plans to cover the Democratic Republic of the Congo, Mozambique, Cameroon, and the Ivory Coast.

Estimated cost-effectiveness? Extremely cost-effective. According to Development Media International's interpretation of previous studies, and their own mathematical model, their cost-effectiveness is on the order of ten dollars per QALY.

Robustness of evidence? Fairly robust. They are collaborating with external investigators to conduct a randomized controlled trial on their own program but have not yet gathered final mortality data; their current data is based on self-reporting.

Implementation? Very well implemented. Their CEO, Roy Head, has extensive experience in running radio programs in developing countries, and they work with leading epidemiologists to monitor their effectiveness. They have been open, transparent, and self-skeptical.

Room for more funding? Very large. In order to scale up to four more countries, they believe they could use about $10 million in 2015 and expect to receive about $2 to 4 million.

Deworm the World Initiative

What do they do? Provide technical assistance to governments in Kenya and India to help those governments run school-based deworming programs.

Estimated cost-effectiveness? Extremely cost-effective. Because they provide assistance to governments, rather than running deworming programs themselves, the cost to DtWI per child treated per year is extremely low, at about three cents.

Robustness of evidence? Fairly robust. Two major randomized controlled trials, one of which included long-term follow-up, suggest that deworming has significant education and economic benefits. However, insofar as DtWI provides technical assistance to governments, rather than running the deworming programs themselves, it's more difficult to know for certain that these programs wouldn't have happened were it not for DtWI's support.

Implementation? Very good. They have been highly transparent about their operations.

Room for more funding? Not very large. They could use an additional $2 million over both 2015 and 2016, and I expect that they will receive at least that amount.

Schistosomiasis Control Initiative

What do they do? Provide funding for governments to run school-based and community-based deworming programs in countries across sub-Saharan Africa, then provide advisory support and conduct monitoring and evaluation. (Schistosomiasis is one type of parasitic worm infection; initially SCI focused just on schistosomiasis, hence the name, but now they treat other parasitic worm infections, too.)

Estimated cost-effectiveness? Very cost-effective. The cost to SCI of deworming one child is less than one dollar per year.

Robustness of evidence? Very robust. Two major randomized controlled trials, one of which included long-term follow-up, suggest that deworming has significant educational and economic benefits.

Implementation? Fairly good. There have been some concerns from GiveWell regarding SCI's transparency and communication about its activities.

Room for more funding? Fairly large. SCI believes it could productively use about $8 million in additional donations in 2015; however, it's not clear how much revenue it will receive in 2015 and it may become fully funded.

Against Malaria Foundation

What do they do? Provide funding to buy and distribute long-lasting insecticide-treated bed nets to poor households across sub-Saharan Africa.

Estimated cost-effectiveness? Very cost-effective. It costs six dollars to provide one bed net that covers two children for two years, for an estimated one hundred dollars per QALY.

Robustness of evidence? Very robust. There have been multiple randomized controlled trials and two meta-analyses supporting the efficacy of bed nets.

Implementation? Extremely good. AMF has been extremely transparent and open in communication.

Room for more funding? Very large. AMF could productively use $20 million in 2015.

Living Goods

What do they do? Run a network of community health promoters in Uganda who go door-to-door selling affordable health products such as treatments for malaria, diarrhea, and pneumonia; soap; menstrual pads; contraception; solar lanterns; and high-efficiency cookstoves, and providing health-care advice.

Estimated cost-effectiveness? Very cost-effective. According to the estimates from the randomized controlled trial they're running on their project, $3,000 spent on their program would save a life and provide a number of other benefits; GiveWell estimates their cost per life saved at $11,000.

Robustness of evidence? Fairly robust. A high-quality study has been conducted by independent investigators on the very program that Living Goods is running. However, there is no evidence from multiple studies, as there are for other programs on this list.

Implementation? Fairly good. They have been open and fairly transparent with providing information. However, they have only limited ongoing monitoring and evaluation.

Room for more funding? Fairly large. Living Goods' budget will be about $10 million per year for the next three years; it is likely to have a shortfall of about $2 to $3 million per year.

The Iodine Global Network (IGN)

What do they do? Advocate for governments to fortify salt with iodine and thereafter monitor progress of implemented programs and provide country-specific guidance.

Estimated cost-effectiveness? Extremely cost-effective. Iodine deficiency is a major cause of physical and intellectual stunting in developing countries. Iodine fortification can alleviate these problems at a cost of pennies per person. One estimate put the economic benefits of these programs at twenty-seven dollars for every dollar spent.

Robustness of evidence? Fairly robust. Fortification of salt with iodine is a well-studied program and has been shown to lead to significant improvements to people's lives. However, IGN does not implement the iodine fortification programs themselves. It's therefore crucial to determine whether IGN is causing more people to receive iodized salt than they would otherwise have done. This is not yet clear.

Implementation? Very good. IGN is run by leading experts on micronutrient deficiencies and has been open and transparent.

Room for more funding? Not very large. Though their budget was only $500,000 in 2014, GiveWell believes they could productively use $1 million in 2015; they have so far only raised $400,000 toward that goal, giving $600,000 in room for more funding.

Here's a table showing all of my ratings in one place.

	Estimated Cost-Effectiveness	Robustness of Evidence	Quality of Implementation	Size of Room for More Funding
GiveDirectly	••	••••	••••	••••
Development Media International	••••	••	•••	•••
Deworm the World Initiative	•••	•••	•••	•
Schistosomiasis Control Initiative	•••	•••	••	••
Against Malaria Foundation	•••	•••	••••	••••
Living Goods	•••	••	••	••
Iodine Global Network	•••	••	•••	•

••••-extremely (best/most) and •-not very (least)

EIGHT

THE MORAL CASE FOR SWEATSHOP GOODS

How can consumers make the most difference?

The clothing retailer American Apparel, known for selling "fashionable basics" like solid-color T-shirts, proudly claims to be "sweatshop-free." On its "about us" webpage, it says:

OUR GARMENT WORKERS ARE PAID UP TO 50 TIMES MORE THAN THE COMPETITION

> A garment worker in Bangladesh earns an average of $600 a year. An experienced American Apparel garment worker can earn $30,000+ and receive benefits such as comprehensive health care. American Apparel garments are created by motivated and fairly paid employees who don't just have jobs—they have careers. Our culture recognizes outstanding performance and promotes from within. Most importantly, our workers have a voice and influence the direction of the company. At American Apparel we call it Sweatshop-Free, a term we coined in 2002.

The popularity of American Apparel is just one example of a trend toward "ethical consumerism," where people spend a little more money on goods that are produced by workers who are treated well, thereby using their purchasing power to, hopefully, make the world a better place.

This chapter will look at ethical consumerism through the lens of effective altruism, trying to figure out if it's an effective way of doing good. I'll talk about sweatshops, fair-trade, low-carbon living, and vegetarianism. Ultimately, I'm

going to conclude that ethical consumerism is not all it's cracked up to be—at least, not compared to other ways of making a difference. Let's start with sweatshops.

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Sweatshops are factories in poor countries, typically in Asia or South America, that produce goods like textiles, toys, or electronics for rich countries, under pretty horrific working conditions. Workers often face sixteen-hour days, six or seven days a week. Sometimes they're prohibited from taking breaks to eat or use the bathroom. Air-conditioning is rare, so factories can be very hot. Health and safety considerations are commonly neglected, and employers sometimes abuse their workers.

Because conditions in sweatshops are so bad, many people have pledged to boycott goods produced in them, and a number of organizations devoted to ending the use of sweatshop labor, such as United Students Against Sweatshops, National Mobilization Against Sweatshops, SweatFree Communities, and the ingeniously named No Sweat Apparel, have proliferated in service to the cause. For this reason, there's significant public animosity toward big companies like Nike, Apple, and Disney that rely on sweatshop labor to manufacture their products.

This movement has noble intentions: the people who campaign against sweatshops are justifiably horrified that people work in such awful conditions. However, those who protest sweatshops by refusing to buy goods produced in them are making the mistake, which we discussed in chapter five, of failing to consider what would happen otherwise. We assume that if people refused to buy goods from sweatshops, these factories would succumb to economic pressure and go out of business, in which case their employees would find better employment elsewhere.

But that's not true. In developing countries, sweatshop jobs are the good jobs. The alternatives are typically worse, such as backbreaking, low-paid farm labor, scavenging, or unemployment. *The New York Times* columnist Nicholas D. Kristof illustrated this well when he presented an interview with Pim Srey Rath, a Cambodian woman who scavenges plastic from dumps in order to sell it as recycling. "I'd love to get a job in a factory," she said. "At least that work is in the shade. Here is where it's hot."

A clear indicator that sweatshops provide comparatively good jobs is the

great demand for them among people in developing countries. Almost all workers in sweatshops chose to work there, and some go to great lengths to do so. In the early twenty-first century, nearly four million people from Laos, Cambodia, and Burma immigrated to Thailand to take sweatshop jobs, and many Bolivians risk deportation by illegally entering Brazil in order to work in sweatshops there. The average earnings of a sweatshop worker in Brazil are $2,000 per year—not very much, but six hundred dollars a year more than the average earnings in Bolivia, where people generally work in agriculture or mining. Similarly, the average earnings among sweatshop workers are: $2 a day in Bangladesh, $5.50 a day in Cambodia, $7 a day in Haiti, and $8 a day in India. These wages are tiny, of course, but when compared to the $1.25 a day many citizens of those countries live on, the demand for these jobs seems more understandable. Because conditions in sweatshops are so bad, it's difficult for us to imagine that people would risk deportation just to work in them. But that's because, as we discussed in chapter one, the extremity of global poverty is almost unimaginable.

In fact, among economists on both the left and the right, there is no question that sweatshops benefit those in poor countries. Nobel laureate and left-wing economist Paul Krugman has stated, "The overwhelming mainstream view among economists is that the growth of this kind of employment is tremendous good news for the world's poor." Jeffrey Sachs, Columbia University economist and one of the foremost proponents of increased efforts to help those in extreme poverty, has said, "My concern is not that there are too many sweatshops but that there are too few." The reason there's such widespread support among economists for sweatshops is that low-wage, labor-intensive manufacturing is a stepping-stone that helps an economy based around cash crops develop into an industrialized, richer society. During the Industrial Revolution, for example, Europe and America spent more than one hundred years using sweatshop labor, emerging with much higher living standards as a result. It took many decades to pass through this stage because the technology to industrialize was new, and the twentieth century has seen countries pass through this stage of development much more rapidly because the technology is already in place. The four East Asian "Tiger economies"—Hong Kong, Singapore, South Korea, and Taiwan—exemplify speedy development, having evolved from very poor, agrarian societies in the early twentieth century to manufacturing-oriented sweatshop countries mid-century, and finally emerging as industrialized economic powerhouses in recent decades.

Because sweatshops are good for poor countries, if we boycott them we make people in poor countries worse off. This isn't just a hypothetical argument. In 1993, the junior United States senator from Iowa Tom Harkin brought a child labor bill to Congress. The Child Labor Deterrence Act would have made it illegal for the United States to import goods from countries using child labor. Bangladesh had a large number of children employed in ready-to-wear garment sweatshops at the time. Out of fear that this act would pass, factories quickly laid off fifty thousand child workers. According to the US Department of Labor, rather than going to school or even finding better jobs, "it is widely thought that most of them have found employment in other garment factories, in smaller, unregistered, subcontracting garment workshops, or in other sectors." Considering that transnational corporations typically pay much higher wages than domestic sweatshops, the lives of these youths likely became worse. Indeed, an investigation by UNICEF found that many of these laid-off underage garment workers had resorted to even more desperate measures to survive, including street hustling and prostitution.

We should certainly feel outrage and horror at the conditions sweatshop laborers toil under. The correct response, however, is not to give up sweatshop-produced goods in favor of domestically produced goods. The correct response is to try to end the extreme poverty that makes sweatshops desirable places to work in the first place.

What about buying products from companies that employ people in poor countries (unlike American Apparel) but claim to have higher labor standards, like People Tree, Indigenous, and Kuyichi? By doing this, we would avoid the use of sweatshops, while at the same time providing even better job opportunities for the extreme poor.

If we really could effectively pass on benefits to the very poor through consumer pressure, then I would be all in favor of it. In practice, however, I'm not so sure that "ethical consumption" works as intended. To see this, let's look at the most widespread attempt to give the very poor better working conditions: fair-trade.

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Fairtrade certification is an attempt to give higher pay to workers in poor countries. It's commonly used for consumables grown in developing countries, such as bananas, chocolate, coffee, sugar, and tea. The Fairtrade license is only

given to producers who meet certain criteria, such as paying their workers a minimum wage and satisfying specified safety requirements. The Fairtrade license has two benefits. First, the producers are guaranteed a certain minimum price for the good. For example, coffee producers are guaranteed to receive $1.40 per pound of coffee, even if the market rate drops below $1.40. Second, producers are paid a "social premium" on top of the market rate. For coffee, if the market rate is above $1.40, the producers are paid an additional twenty cents per pound. This social premium is used to pay for democratically chosen community programs.

Demand for fair-trade products has grown rapidly. The Fairtrade label was launched only in 1988, and in 2014, $6.9 billion was spent on Fairtrade-certified products worldwide. The fact that so many people are willing to pay more to ensure that farmers in other countries are paid a fair wage is heartening. But if we're thinking about buying fair-trade ourselves, we need to ask how much we're actually benefitting people in poor countries by shelling out a few extra dollars for fair-trade versus regular coffee. The evidence suggests that the answer is "disappointingly little." This is for three reasons.

First, when you buy fair-trade, you usually aren't giving money to the poorest people in the world. Fairtrade standards are difficult to meet, which means that those in the poorest countries typically can't afford to get Fairtrade certification. For example, the majority of fair-trade coffee production comes from comparatively rich countries like Mexico and Costa Rica, which are ten times richer than the very poorest countries like Ethiopia. In chapter one we saw how fast money diminishes in value and how extreme global inequality is. That means that *even if* buying fair-trade was a good way of paying farmers more, you might make a bigger difference by buying non-fair-trade goods that are produced in the poorest countries rather than fair-trade goods that are produced in richer countries. Because Costa Rica is ten times richer than Ethiopia, one dollar is worth more to the average Ethiopian than several dollars is to the average Costa Rican.

Second, of the additional money that is spent on fair-trade, only a very small portion ends up in the hands of the farmers who earn that money. Middlemen take the rest. The Fairtrade Foundation does not provide figures on how much of the additional price reaches coffee produces, but independent researchers have provided some estimates. Dr. Peter Griffiths, an economic consultant for the World Bank, worked out that for one British café chain, less than 1 percent of the additional price of their fair-trade coffee reached coffee exporters in poor

countries. Finnish professors Joni Valkila, Pertti Haparanda, and Niina Niemi found out that, of fair-trade coffee sold in Finland, only 11 percent of the additional price reached the coffee-producing countries. Professor Bernard Kilian and colleagues from INCAE Business School found that, in the United States, while fair-trade coffee would sell for five dollars per pound more than conventional coffee, coffee producers would receive only forty cents per pound, or 8 percent of that increased price. In contrast, remember that, if you donate one dollar to GiveDirectly, ninety cents ultimately reaches the poor.

Finally, even the small fraction that ultimately reaches the producers does not necessarily translate into higher wages. It guarantees a higher price for goods from Fairtrade-certified *organizations*, but that higher price doesn't guarantee a higher price for the farmers who work for those organizations. Professor Christopher Cramer at the University of London School of Oriental and African Studies led a team of researchers who conducted a four-year study on earnings of Fairtrade workers in Ethiopia and Uganda. They found that those Fairtrade workers had systematically lower wages and worse working conditions than comparable non-Fairtrade workers, and that the poorest often had no access to the "community projects" that Fairtrade touted as major successes. Professor Cramer commented that "the British public has been led to believe that by paying extra for Fairtrade-certified coffee, tea, and flowers they will 'make a difference' to the lives of poor Africans. Careful fieldwork and analysis in this four-year project leads to the conclusion that in our research sites Fairtrade has not been an effective mechanism for improving the lives of wage workers, the poorest rural people."

Independent reviews of studies have found much the same thing. Though the evidence is limited (which is itself worrying), the consistent finding among the studies that have been performed is that Fairtrade certification does not improve the lives of agricultural workers. Even a review commissioned by the Fairtrade Foundation itself concluded that "there is limited evidence of the impact on workers of participation in Fairtrade."

Given this, there is little reason to buy Fairtrade products. In buying Fairtrade products, you're at best giving very small amounts of money to people in comparatively well-off countries. You'd do considerably more good by buying cheaper goods and donating the money you save to one of the cost-effective charities mentioned in the previous chapter.

• • •

Another major area of ethical consumerism is “green living.” On a per-person basis, US citizens emit more greenhouse gasses than any other large country, with the average American adult every year producing twenty-one metric tons of carbon dioxide equivalent. (Remember that carbon dioxide equivalent, or CO_{2eq}, is a way of measuring your carbon footprint that includes greenhouse gasses other than carbon dioxide, like methane and nitrous oxide.) As we’ve seen, climate change is a big deal. It’s therefore natural to want to do something about it, and the obvious way is to move to a lower-carbon lifestyle.

Sadly, many popular ways of reducing your greenhouse gas emissions are rather ineffective. One common recommendation is to turn off or shut down electronic devices when you’re not using them, rather than keeping them on standby. However, this achieves very little compared to other things you could do: one hot bath adds more to your carbon footprint than leaving your phone charger plugged in for a whole year; even leaving on your TV (one of the worst offenders in terms of standby energy use) for a whole year contributes less to your carbon footprint than driving a car for just two hours. Another common recommendation is to turn off lights when you leave a room, but lighting accounts for only 3 percent of household energy use, so even if you never used lighting in your house, you would save only a fraction of a metric ton of carbon emissions. Plastic bags have also been a major focus of concern, but even on very generous estimates, if you stopped using plastic bags entirely, you’d cut out one hundred kilograms CO_2 equivalent per year, which is only 0.4 percent of your total emissions. Similarly, the focus on buying locally produced goods is overhyped: only 10 percent of the carbon footprint of food comes from transportation, whereas 80 percent comes from production, so what type of food you buy is much more important than whether that food is produced locally or internationally. Cutting out red meat and dairy for one day a week achieves a greater reduction in your carbon footprint than buying entirely locally based food. In fact, exactly the same food can sometimes have a higher carbon footprint if it’s locally grown than if it’s imported: one study found that the carbon footprint from locally grown tomatoes in northern Europe was five times as great as the carbon footprint from tomatoes grown in Spain, because the emissions generated by heating and lighting greenhouses dwarfed the emissions generated by transportation.

The most effective ways to cut down your emissions are to reduce your intake of meat (especially beef, which can cut out about a metric ton of CO_{2eq} per year), to reduce the amount you travel (driving half as much would cut out two

metric tons of CO_{2eq} per year and one fewer round-trip flight from London to New York would eliminate a metric ton of CO_{2eq}), and to use less electricity and gas in the home (especially by installing loft insulation, which would save a metric ton of CO_{2eq} for a detached house).

However, there is an even more effective way to reduce your emissions. It's called offsetting: rather than reducing your own greenhouse gas emissions, you pay for projects that reduce or avoid greenhouse gas emissions elsewhere.

Environmentalists often criticize carbon offsetting. Here's the British journalist George Monbiot:

> While the carbon we release by flying or driving is certain and verifiable, the carbon absorbed by offset projects is less attestable. Many will succeed, and continue to function over the necessary period. Others will fail, especially the disastrous forays into tree planting that some companies have made. To claim a carbon saving, you also need to demonstrate that these projects would not have happened without you—that Mexico would not have decided to capture the methane from its pig farms, or that people in India would not have bought new stoves of their own accord. In other words, you must look into a counterfactual future. I have yet to meet someone from a carbon offset company who possesses supernatural powers.
>
> At the offices of Travelcare and the forecourts owned by BP, you can now buy complacency, political apathy, and self-satisfaction. But you cannot buy the survival of the planet.

Monbiot makes his point somewhat melodramatically, but his concern is reasonable. For many offsetting programs offered by companies, you might feel unsure about how effective they really are. For example, many airline companies give you the option when you purchase a flight to pay an extra fee to offset your contribution to the greenhouse gasses emitted during that flight. If you do this, you have to trust the airline to successfully offset those emissions. But you might worry that the airline won't do so effectively: perhaps they pay for a project that was going to happen anyway, or perhaps they overestimate the greenhouse gas reduction from the projects they undertake. If so, then the fee you pay to offset your emissions doesn't actually offset all of your emissions; it only offsets some of them.

However, Monbiot's concern doesn't provide a good argument against carbon offsetting *in general*. It just shows we've got to do some research in order to find a way of offsetting that's genuinely effective. That's what we did at my organization Giving What We Can. We considered more than one hundred organizations that claim to reduce greenhouse gas emissions with donations and tried to figure out which ones most cost-effectively prevent the release of one metric ton of carbon dioxide equivalent. The charity we ultimately decided was best is called Cool Earth.

Cool Earth was founded in 2007 in the United Kingdom by businessman Johan Eliasch and MP Frank Field, who were concerned with protecting the rain forest and the impact that deforestation might have on the environment. The charity aims to fight global warming by preventing deforestation, primarily in the Amazon. They use donated money to help develop rain-forest communities economically to a point where they do better by not selling their land to loggers. Cool Earth does not buy rain forest directly; instead, it provides economic assistance to local communities, helping the people who inhabit the rain forests establish more profitable ventures than selling trees. This involves working to secure property rights, improving community infrastructure, and connecting the inhabitants of the forests with markets where they can sell their produce at good prices, among other things. The work Cool Earth does therefore incidentally improves the lives of those living in the rain forest while working to prevent climate change.

Given this apparently indirect route (via something that looks more like development aid) you might wonder if this can really be an effective way of protecting forests. But if a community already wants to preserve the forest but can't afford not to sell it, helping that community find an alternative strategy seems like a promising way to make a big difference for comparatively low cost. The evidence suggests that Cool Earth's program has been effective, with far less deforestation in Cool Earth areas than the surroundings. Moreover, those at Cool Earth think strategically about which regions to provide assistance to. By protecting key areas of rain forest, they can create a "wall" of forest that blocks off a much wider landscape from illegal logging.

Cool Earth claims it costs them about a hundred dollars to prevent an acre of rain forest from being cut down, and that each acre locks in 260 metric tons of CO_2. This would mean that it costs just about thirty-eight cents to prevent one metric ton of CO_2 from being emitted.

When we assessed Cool Earth, however, we wanted to try to remain

conservative, so we created our own estimates rather than relying on their figures. After looking at their track record, we estimated it cost Cool Earth less than $154 to protect one acre of rain forest, which protected a further four acres by walling off other areas of forest. Thirty percent of similar areas of rain forest that were not protected by Cool Earth had been logged, suggesting that they were protecting an acre of rain forest for $103. However, we realized that, to some extent, it might be that by protecting a given area of rain forest, Cool Earth simply causes loggers to cut down a different area of rain forest. We took this consideration into account using economic data and estimated that every acre protected by Cool Earth would prevent 0.5 acres from being felled, giving a cost of $206 per acre protected.

The estimate of 260 metric tons of CO_2 per acre is already low, insofar as it doesn't take into account the carbon dioxide stored in the soil and doesn't take into account greenhouse gas emissions other than CO_2. But there is a risk that the forest will still be logged in the future, and for this reason we scaled down their estimate to 153 metric tons of CO_2 per acre.

Bringing these numbers together ($206 to prevent 153 metric tons of CO_2) gives our best-guess estimate at $1.34 per metric ton. Even after trying to be conservative in our calculations, this number may still be too optimistic. So, to play extra safe, we could assume a 300 percent margin of error and use a figure of five dollars per metric ton of CO_2 emissions prevented.

Using this figure, the average American adult would have to spend $105 per year in order to offset all their carbon emissions. This is significant, but to most people it's considerably less than it would cost to make large changes in lifestyle, such as not flying. This suggests that the easiest and most effective way to cut down your carbon footprint is simply to donate to Cool Earth.

People sometimes make other objections to carbon offsetting, but they're not very compelling. For example, in the article I quoted earlier, George Monbiot claimed that carbon offsetting is a way of "selling indulgences," in reference to the medieval practice in which Christians would pay the Church in exchange for forgiveness for their sins. On a similar theme, a satirical website, CheatNeutral.com, offers the following service: "When you cheat on your partner you add to the heartbreak, pain, and jealousy in the atmosphere. CheatNeutral offsets your cheating by funding someone else to be faithful and NOT cheat. This neutralizes the pain and unhappy emotion and leaves you with a clear conscience."

However, in both cases the analogies are flawed. In buying indulgences, you

don’t “undo” the harm you’ve caused others or the sins you’ve done. In contrast, through effective carbon offsetting, you’re preventing anyone from being harmed by your emissions in the first place: if you emit carbon dioxide throughout your life but effectively offset it at the same time, overall your life contributes nothing to climate change. Similarly, “offsetting” your adultery (even if you genuinely could) would still affect *who* is harmed, even if it keeps the total number of adulterous acts constant. In contrast, carbon offsetting prevents anyone from ever being harmed by your emissions; it’s the “equivalent” of never committing adultery in the first place.

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Another area where people try to change their purchasing habits in order to make a difference is meat eating and vegetarianism. As I mentioned earlier, cutting out meat (especially beef) is one effective way to reduce your carbon emissions. However, we’ve also seen that by donating to Cool Earth you can offset one metric ton of carbon emissions for about five dollars. If you’d rather pay five dollars than go vegetarian, then the environmental argument for vegetarianism is rather weak.

The animal welfare argument for vegetarianism is comparatively stronger. The vast majority of farmed animals are raised in factory farms, which inflict severe and unnecessary suffering on those animals merely for the sake of slightly cheaper produce. The living conditions of factory farm animals have been extensively documented in books, magazines, and documentaries, so I will spare you the grim details here. I personally believe it’s important to treat animals humanely, and for that reason I’ve been a vegetarian for many years.

However, the animal welfare argument is much stronger for some animals than for others, because some sorts of animal produce involve a lot more suffering on the part of the animals than others. In fact, eliminating chicken and eggs removes the large majority of animal suffering from your diet. This is because of the conditions those animals are kept in, and the number of animals needed to provide a given number of calories.

Of all the animals raised for food, broiler chickens, layer hens, and pigs are kept in the worst conditions by a considerable margin. The only quantitative estimates of farmed animal welfare I’ve been able to find come from Bailey Norwood, an economist and agricultural expert. He rated the welfare of different animals on a scale of –10 to 10, where negative numbers indicate that it would

be better, from the animal's perspective, to be dead rather than alive. He rates beef cattle at 6 and dairy cows at 4. In contrast his average rating for broiler chickens is –1, and for pigs and caged hens is –5. In other words, cows raised for food live better lives than chicken, hens, or pigs, which suffer terribly.

The second consideration is the number of animals it takes to make a meal. In a year, the average American will consume the following: 28.5 broiler chickens, 0.8 layer hens, 0.8 turkeys, 0.37 pigs, 0.1 beef cows, and 0.007 dairy cows. These numbers might suggest that cutting out chicken has a far bigger impact than any other dietary change. However, most broiler chickens only live for six weeks, so insofar as we care about how long the animal spends in unpleasant conditions on factory farms, it's more appropriate to think about animal years rather than animal lives. In a year, the number of animal years that go into the average American's diet are as follows: 3.3 from broiler chickens (28.5 chickens consumed, each of which lives 6 weeks = 3.3 animal years), 1 from layer hens, 0.3 from turkeys, 0.2 from pigs, 0.1 from beef cows, and 0.03 from dairy cows.

Combining these two considerations, we arrive at the conclusion that the most effective way to cut animal suffering out of your diet is to stop eating chicken, then eggs, then pork: by doing so, you're taking out the worst suffering for the most animals for the longest time.

This may have implications for animal-welfare advocates, who often tout vegetarianism's environmental benefits (which we've already discussed) and health benefits, pointing to research that those who don't eat meat are at reduced risk for cardiovascular diseases. These advocates often argue for eliminating beef from your diet, since raising cattle produces a lot of CO_{2eq} emissions and red meat is linked to health problems such as heart disease. However, if people hear the environmental or health arguments and then decrease their beef consumption but compensate even a little bit by eating more chicken, those animal-welfare advocates may have caused more animal suffering than they eliminated.

Earlier, I suggested that offsetting might be an easier and more effective way of reducing your carbon footprint than making large lifestyle changes. Could you apply the same argument here? Rather than personally cutting out meat, couldn't people "offset" their meat consumption by donating to an animal-advocacy charity, thereby causing some other person to become vegetarian who wouldn't otherwise have done so? I don't think so. There's a crucial difference between greenhouse gas emissions and meat consumption: if you offset your greenhouse

gas emissions, then you prevent anyone from ever being harmed by your emissions. In contrast, if you offset your meat consumption, you change which animals are harmed through factory farming. That makes eating meat and offsetting it less like offsetting greenhouse gas emissions and more like committing adultery and offsetting it, which we all agree it would be immoral to do.

If you care about animal suffering, you should certainly alter your diet, either by cutting out the most harmful products (at least eggs, chicken, and pork), or by becoming vegetarian or vegan. However, there's no reason to stop there. In terms of making a difference to the lives of animals, the impact you can have through your donations seems even greater than the impact you can have by changing your own behavior. According to Animal Charity Evaluators (a research charity I helped to set up), by donating to charities like Mercy For Animals or the Humane League, which distribute leaflets on vegetarianism, it costs about one hundred dollars to convince one person to stop eating meat for one year. If you can donate more than that to animal advocacy charities per year, then your decision about how much to donate to animal advocacy is even more important, in terms of impact, than the decision about whether to become vegetarian yourself.

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We've seen that, in general, changing your consumption habits is not a very effective way to make a difference compared to the alternatives (though, we've also seen how the law of expected value demonstrates that it's still a good idea to change your behavior in certain cases). Whether our concern is the global poor, climate change, or animal welfare, we've seen that the decision about how much and where to donate is much greater, in terms of impact, than the decision about what products to buy.

On reflection, we should expect it to be this way. By donating, you can ensure that your money is spent only on the most effective activities. Given the difference between the best activities and merely very good activities, this is a big deal. In contrast, spending more in order to buy more "ethical" produce is not a very targeted way of doing good.

Things may even be worse than that, however. There's some reason to think that the rise in ethical consumerism could even be harmful for the world, on balance. Psychologists have discovered a phenomenon that they call *moral*

licensing, which describes how people who perform one good action often compensate by doing fewer good actions in the future.

For example, in a recent experiment, participants were told to choose a product from either a selection of mostly "green" items (like an energy-efficient lightbulb) or from a selection of mostly conventional items (like a regular lightbulb). They were then told to perform a supposedly unrelated visual perception task: a square box with a diagonal line across it was displayed on a computer screen, and a pattern of twenty dots would flash up on the screen; the subjects had to press a key to indicate whether there were more dots on the left or right side of the line. It was always obvious which was the correct answer, and the experimenters emphasized the importance of being as accurate as possible, telling the subjects that the results of the test would be used in designing future experiments. However, the subjects were told that, whether or not their answers were correct, they'd be paid five cents every time they indicated there were more dots on the left-hand side of the line and five cents every time they indicated there were more dots on the right-hand side. They therefore had a financial incentive to lie, and they were alone, so they knew they wouldn't be caught if they did so. Moreover, they were invited to pay themselves out of an envelope, so they had an opportunity to steal as well.

What happened? People who had previously purchased a "green" product were significantly more likely to both lie and steal than those who had purchased the conventional product. Their demonstration of ethical behavior subconsciously gave them license to act unethically when the chance arose.

Amazingly, even just *saying* you'd do something good can cause the moral licensing effect. In another study, half the participants were asked to imagine helping a foreign student who had asked for assistance in understanding a lecture. They subsequently gave significantly less to charity when given the chance to do so than the other half of the participants, who had not been asked to imagine helping another student.

Moral licensing shows that people are often more concerned about looking good or feeling good rather than actually doing good. If you "do your bit" by buying an energy-efficient lightbulb, your status as a good human being is less likely to be called into question if you subsequently steal a small amount of money.

Often, the moral licensing effect isn't that decision-relevant. If we're encouraging people to engage in effective actions to make the world a better place, it's not that big a deal if that means they compensate, to some extent, by

doing less of other altruistic activities. If we encourage people to do a small action but frame the request as the first step toward a larger commitment, then the moral licensing effect may not occur. Where it becomes crucial, however, is when people are encouraged to do fairly ineffective acts of altruism and, as a result, are less likely to perform effective ones later. If, for example, encouraging someone to buy fair-trade causes that person to devote less time or money to other, more effective activities, then promoting fair-trade might on balance be harmful.

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In this chapter, we've seen that the benefits of ethical consumerism are often small compared to the good that well-targeted donations can do. In the next chapter, we'll look at an area where you really can make an astonishing difference: your career.

NINE

DON’T “FOLLOW YOUR PASSION”

Which careers make the most difference?

As Peter Hurford entered his final year at Denison University, he needed to figure out what he was going to do with his life. He was twenty-two, majoring in political science and psychology, and he knew he wanted a career that would both be personally satisfying and would make a big difference. Graduate school was the obvious choice for someone with his interests, but he didn’t know what his other options were, or how to choose among them.

How should young people like Peter who want to make a difference in their careers go about their decisions? What if you’re later in your career but are considering changing jobs so you can have a bigger impact? In chapter five, we saw that earning to give is one powerful way to make a difference, but it’s certainly not the only way. There are a dizzying number of career paths, each with their positives and negatives. At the same time, the decision is high-stakes. Your choice of career is a choice about how to spend more than eighty thousand hours over the course of your life, which means it makes sense to invest a considerable amount of time in the decision. If you were to spend just 1 percent of your working time thinking about how to spend the other 99 percent, that would mean you’d spend eight hundred hours, or twenty working weeks, on your career decision. I doubt many people spend this much time thinking about their careers, but it might be worth it.

Over the last few years at 80,000 Hours, we’ve coached hundreds of people like Peter, as well as people later in their careers, most of whom find the following framework useful in figuring out what their next career steps should be. There are many considerations relevant to choosing the right career for you, and this framework ensures that you give due weight to what’s most important. You should ask yourself:

How do I personally fit with this job? How satisfied will I be in this job? Am I excited by the job? Do I think I could stick with it for a significant period of time? How good am I, or could I become, at this type of work, compared to other people and compared to other careers I might choose?
What's my impact while I'm working at this job? How many resources can I influence, whether that's the labor I provide, the people or budget I manage, the money I earn, or a public platform I have access to? How effective are the causes to which I can direct those resources?
How does this job contribute to my impact later on in life? How well does this job build my skills, connections, and credentials? How well does this job keep my options open? How much will I learn in the course of this job about what I might want to do next?

Let's discuss each of these three key factors in turn.

Personal fit

Personal fit is about how good you'll be in a particular job. An important part of this is whether you'll be happy doing the work. People often want job satisfaction as an end in itself, but it's also a crucial factor when thinking about impact: if you're not happy at work, you'll be less productive and more likely to burn out, resulting in less impact in the long-term. However, we need to be careful when thinking about how to find a job you'll love. There's a lot of feel-good misinformation out there, and the real route to job satisfaction is somewhat counterintuitive.

On June 12, 2005, Steve Jobs stood in front of the graduating class at Stanford and gave them his advice on what they should do with their lives:

> You have to trust in something—your gut, destiny, life, karma, whatever—because believing that the dots will connect down the road will give you the confidence to follow your heart, even when it leads you off the well-worn path, and that will make all the difference.
>
> You've got to find what you love, and that is as true for work as it is for your lovers. Your work is going to fill a large part of your life, and the only way to be truly satisfied is to do what you believe is great work,

> and the only way to do great work is to love what you do. If you haven't found it yet, keep looking, and don't settle. As with all matters of the heart, you'll know when you find it, and like any great relationship it just gets better and better as the years roll on. So keep looking. Don't settle.

Jobs's message is emotionally resonant and appealing, and career advice is commonly built around slogans like "follow your heart" or "follow your passion." The first paragraph of the advice book *Career Ahead* ends, "You owe it to yourself to do work that you love. This book will show you how." A popular YouTube video, *What If Money Was No Object?* narrated by British writer Alan Watts, advises similarly. It suggests that, unless you ask yourself, "What makes you itch?" and pursue the answer, you will "spend your life completely wasting your time. You'll be doing things you don't like doing in order to go on living, that is to go on doing things you don't like doing, which is stupid." At its most extreme, the talk around career choice sounds similar to the talk around romance: when you find your perfect fit, you'll just know.

Taken literally, however, the idea of following your passion is *terrible* advice. Finding a career that's the right "fit" for you is crucial to finding a career, but believing you must find some preordained "passion" and then pursue jobs that match it is all wrong. Ask yourself, is following your passion a good way to achieve personal satisfaction in the job you love? Should you pick a career by identifying your greatest interest, finding jobs that "match" that interest and pursuing them no matter what? On the basis of the evidence, the answer seems to be no.

First, and most simply, most people don't have passions that fit the world of work. In one study of Canadian college students, it was found that 84 percent of students had passions, and 90 percent of these involved sports, music, and art. But by looking at census data, we can see that only 3 percent of jobs are in the sports, music, and art industries. Even if only half the students followed their passion, the majority would fail to secure a job. In these cases, "doing what you're passionate about" can be actively harmful.

Indeed, often the fact that you're passionate about something is a good reason why it will be difficult to find a job in that area, since you have to compete with all the other people who are passionate about the same thing. This is the situation in sports and music, where only extremely talented (or lucky) people can make a steady living. In the United States, fewer than one in one

thousand high school athletes will make it into professional sports. For the large majority of people who don't have work-related passions, the advice to "follow your passion" might merely prompt anxious soul-searching and send them into the wrong careers.

Second, your interests change. Psychologists Jordi Quoidbach, Daniel T. Gilbert, and Timothy Wilson have shown that they change much more than we anticipate, so we overrate their importance. Just think about what you were most interested in ten years ago. Chances are, it's completely different from what you're interested in today. If you focus only on what you're currently passionate about, then you risk committing to projects that you soon find you're no longer interested in.

This takes us to our third point against passion, which is that the best predictors of job satisfaction are features of the job itself, rather than facts about personal passion. Instead of trying to figure out which career to pursue based on whatever you happen to be most interested in today, you should start by looking for work with certain important features. If you find that, passion will follow.

Research shows that the most consistent predictor of job satisfaction is engaging work, which can be broken down into five factors (this is known in psychology as the job characteristics theory):

1. **Independence**—To what extent do you have control over how you go about your work?
2. **Sense of completion**—To what extent does the job involve completing a whole piece of work so that your contribution to the end product is easily visible, rather than being merely a small part of a much larger product?
3. **Variety**—To what extent does the job require you to perform a range of different activities, using different skills and talents?
4. **Feedback from the job**—How easy is it to know whether you're performing well or badly?
5. **Contribution**—To what extent does your work "make a difference," as defined by positive contributions to the well-being of other people?

As well as job satisfaction, each of these factors also correlates with motivation, productivity, and commitment to your employer. Moreover, these factors are similar to those required to develop flow, the pleasurable state of

being so immersed in an activity that you're completely free of distractions and lose track of time, which some psychologists have argued is the key to having genuinely satisfying experiences.

There are other factors that also matter to your job satisfaction, such as whether you get a sense of achievement from the work, how much support you get from your colleagues, and "hygiene" factors, such as not having unfair pay or a very long commute. But again, these factors have little to do with whether the work involves one of your "passions"—you find them in many different jobs.

The evidence therefore suggests that following your passion is a poor way to determine whether a given career path will make you happy. Rather, passion grows out of work that has the right features. This was even true of Steve Jobs. When he was young, he was passionate about Zen Buddhism. He traveled in India, took plenty of LSD, shaved his head, wore robes, and seriously considered moving to Japan to become a monk. He first got into electronics only reluctantly, as a way to earn cash on the side, helping his tech-savvy friend Steve Wozniak handle business deals while also spending time at the All-One Farm. Even Apple Computer's very existence was fortuitous: while Jobs and Wozniak were trying to sell circuit boards to hobbyists, the owner of one local computer store said he would buy fully assembled computers, and they jumped at the chance to make more money. It was only once they started to gain traction and success that Jobs's passion for Apple and computing really bloomed.

What about following your heart, your gut, or your itch to find work you love? The evidence suggests that won't work, either, since we're bad at predicting what will make us happy.

The way we predict how some event will affect us emotionally is by running a simulation in our heads: when I imagine feeling anxious while taking an exam, I'm imagining taking an exam, which makes me feel anxious. This anxiety is an indicator of the feeling I expect to have when I actually take the exam. From a psychological perspective, this ability to simulate the effects of an as-yet-unexperienced event is a remarkably powerful skill, one that humans possess to a degree far beyond other animals. But the simulations we run bias us in a number of predictable ways. For example, our tastes and preferences change considerably over time, in ways we don't accurately predict. You might plan your life believing you'll never want to have kids, but then find when you're thirty that your preferences change dramatically.

Our simulation-based predictions of the future are also often incomplete.

Simulating future events is hard to do, and we can't possibly focus on every minute aspect of the event, so our brains just include the most important details. However, this means we may miss out on some nonessential features that would make a big difference to our emotional responses. For example, it's been found that professors on average end up much less happy after getting tenure than they predicted they would be prior to getting tenure. One possible explanation for this is that they focused too much on the positive features of getting tenure—the sense of achievement and recognition—at the neglect of others, such as an increased number of dull departmental meetings. When deciding which career to pursue, therefore, we are likely to focus our attention on factors that come to mind easily, such as salary and working hours. This might lead us to ignore other factors that are actually crucial to predicting happiness. Simply "following your heart" without paying attention to what really predicts job satisfaction can easily lead you astray.

For all these reasons, 80,000 Hours prefers to talk about "personal fit" rather than "following your heart" or "following your passion." How can you work out where you have the best personal fit? As we've just seen, it's difficult to predict where you'll be most satisfied and where you'll perform the best just by thinking about it. Indeed, it's hard for anyone to know which job you'll be best at. Even corporate recruiters regularly make mistakes, and they have huge amounts of resources at their disposal to find the people who fit best.

This means it's best to take an empirical approach, trying out different types of work and using your track record to predict how well you'll perform in the future. At the start of your career, be open-minded about where you'll eventually be able to perform best.

Beyond track record, if you want to predict how well you'll perform, the first step is to learn as much about the work as you can. Go and speak to people in the job. Ask what traits they think are most important to success, and see how you measure up. Ask about the main reasons people end up leaving the job. Find out how people who are similar to you have performed in the past. Look at whether you think you'd find the work satisfying based on the factors mentioned earlier. The "follow your passion" slogan assumes it's as easy as looking inward to figure out what you ought to be doing. In contrast, identifying a job with what we call good "personal fit" involves finding out as much about a job as you can, because it's features of the job itself that are much more important in determining how well you succeed and enjoy your work than whether that job corresponds with your preexisting passions.

These considerations affected Peter Hurford's decision. While at college, he was most interested in political science and had enjoyed completing several research projects with a professor there. He had always presumed he'd go to grad school to study political science. However, after reading our research on personal fit, he widened his search considerably. Instead of trying to figure out what career path fitted his current passions best, he drew up a list of fifteen possible options across a range of areas and thought about each of them in turn, spoke to people who knew about them, and thought about which he might perform best in based on his skills and experience to date. He was able to rule out some of his options after just a little bit of investigation: consulting would involve a lot of travel, which he'd hate; medicine would require a lot of retraining, which didn't seem worth it. In the end, he was able to narrow his options down to five plausible candidates. Graduate school stayed on the list but was joined by options he hadn't thought as much about previously: law school, nonprofit work, computer programming, and market research.

He thought he would fit well within any of these five categories, so he tried to decide primarily on the basis of his long-run potential for impact. This takes us to the next two aspects of the career effectiveness framework.

Impact on the job

The second issue in our framework is how much impact you'll have within the job. Typical advice on making a difference through your career emphasizes this factor heavily. The most obvious way to do this is to work in the social sector: social-impact-focused-careers websites list job opportunities at charities, or in corporate social responsibility. However, like "following your passion," this advice can be misleading.

First, to make a difference in the social sector, the organization you work for must be effective. If your charity job was at PlayPumps International, then, no matter how enthusiastically or efficiently you worked, you'd have made very little positive impact. It's difficult to assess how effective an organization is, but the frameworks given in the chapters on effective charities and effective causes can help you, as can the key questions described in the first part of this book.

Second, you need to provide substantial value over the person who the charity would have hired instead. If you offer unusual skills or are particularly good at that job compared to others who would have worked there, then you can

offer significant additional value. If these conditions don't hold and you don't add more value than whoever would have been in your place, your impact might be small. In the most extreme case, if you are simply very good at interviewing and not that great an employee, you could even cause harm by displacing someone better who would have been in your shoes.

Third, there are many other ways of making a difference. Earlier, we saw the arguments in favor of earning to give, and helping others through your donations rather than through your direct labor. As we'll discuss later in this chapter, there are also very compelling ways of making a difference that aren't in the social sector, such as entrepreneurship, research, journalism, or politics.

In general, we recommend people think of three primary routes by which they can have impact on the job. The first is through the labor you provide. This can be the work you do if you are employed by an effective organization, or the research you do if you are a researcher. The second is the money you can give. The third is the influence you can have on other people. In order to work out the total impact you can have, you should look at all three of these; whereas advice that solely focuses on the charity sector only looks at the first.

Next, you need to assess how effective the causes or organizations to which you can direct these resources are. The more effective the cause or organization, the more good those resources will do. For your labor, that's the effectiveness of the organization you work for. For your donations, that's the effectiveness of the organization you donate to. What you're able to influence depends heavily on your situation: you might be able to influence the expenditure of the charity you work for, you might be able to influence the donations of your coworkers, or you might be able to influence the general public through a public platform. In each case, the more effective the causes you're able to support, the more impact you'll have.

The fourth and most important reason why "work in the social sector" might be bad advice is that if you're just starting out, it's much more important to build skills and credentials than it is to have an impact on the job. There are a few reasons for this. First, there are many ways of boosting your potential for influence later that have high return on investment, such as getting an advanced degree or an MBA, learning to program, or building your network. Whereas your first position might last a few years, your subsequent career will last decades. Spending a few years building your abilities now, therefore, can pay off with increased impact over a much longer period. In addition, the most senior people within a field generally have a disproportionate amount of influence and

impact within that field. Maximizing your chances of getting into more senior and influential positions is therefore a key part of maximizing your impact.

For these reasons, especially when starting out, you should focus on building up skills, network, and credentials, rather than trying to have an impact right away. This is how many of the most effective charities we've discussed have been founded. GiveDirectly, Schistosomiasis Control Initiative, Deworm the World Initiative, and Development Media International were all founded by academics who discovered innovative new ways to help the poor. Rob Mather, who founded Against Malaria Foundation, had spent many years building skills in strategy consulting before moving into the charity sector. This meant he had a good grasp of how to run an organization well, and that, once he came to set up AMF, he didn't need to take a salary.

Building career capital can be important later in your career as well if you're not sure which causes to support. Instead of trying to make an immediate impact, you can invest in yourself while continuing to learn about which causes are most important, preparing yourself to make a bigger difference in the future.

With these considerations in mind, Peter Hurford didn't place too much weight on the immediate impact he could have in the job he worked for. If he looked at immediate impact only, earning to give and nonprofit work were his best options, with graduate school and law school following. However, these options differed significantly in terms of the impact they'd enable him to have later on in his life, and that was more important to him. This takes us to the final section of the framework.

Impact later in life

There are a number of ways in which a given job can help you have a larger impact later on in life. Through your initial work, you develop "career capital"—skills, a network, and credentials—which will help you take a higher-impact job later on. If you develop organizational skills, then all other things being equal, you will be more effective in your next job. If you get to know a large number of people through your work, you are more likely to be connected with job opportunities. If you work at a high-prestige firm like Google or McKinsey & Company, that line on your résumé will make you more attractive to future potential employers.

In addition to career capital, there are two other ways in which your initial

job will affect the impact you have later on in your career. First is how well it keeps your options open. It's easier, for example, to transition from the for-profit sector to the nonprofit sector than vice versa. Similarly, it's easier to transition from academia into industry than it is the other way around; for people who are highly uncertain about whether they want to leave academia after their PhD, this asymmetry provides a reason for staying in academia until they have better information. Keeping your options open also provides a reason for building transferable skills—such as sales and marketing, leadership, project management, business knowledge, social skills, personal initiative, and work ethic—rather than highly specific skills, like piano tuning or knowledge of the shipping industry.

Second, there is "exploration value": How much do you learn about what careers you should choose in the future in the course of doing the job? Especially when you're just starting out, you won't know much about what opportunities are out there and which will fit you best. Your first few jobs will give you valuable information that will inform your later decisions. This can provide a reason in favor of trying things that are less well-known to you at first. Perhaps, after college, you have a good understanding of what pursuing a master's and a PhD would involve, but you have very little understanding of the for-profit world, both in terms of how much you'd enjoy it and in terms of how well you'd fit there. Exploration value provides a reason in favor of working in the for-profit sector for a year or two: you might discover that the opportunities there suit you well.

People embarking on their careers often neglect these considerations. People often tend to think of choosing a career as an all-or-nothing proposition: a one-off life decision that you make at age twenty-one and that you can't change later. A way to combat this mistake is to think of career decisions like an entrepreneur would think about starting a company. In both career choice and entrepreneurship, you start out with a tiny amount of relevant information, but you have to use that information to cope with a huge number of variables. Moreover, as things progress, these variables shift: you're constantly gaining new information; and new, often entirely unexpected, opportunities and problems arise. Because of this, armchair reasoning about what will and won't happen isn't very useful.

In the case of entrepreneurship, Eric Ries has argued forcefully for this idea and created the popular Lean Startup movement. The idea behind the Lean Startup is that many entrepreneurs make the mistake of getting excited about

some product or idea and then doing everything they can to push it onto the world even before they've tested it to see if there's a market for it. When companies do this, products often fail because they were reasoning from the armchair when they should have been experimenting. Ries argues that entrepreneurs should think of their ideas or products like hypotheses, and continually test, ultimately letting the potential customers determine what the product should be.

In the case of career choice, many people make an analogous mistake. They try to decide, early on, what their career should be and then they doggedly try to pursue that career, ignoring other possibilities that might arise and failing to consider that the job might not be right for them. (Sometimes this is caused by the idea of having a "calling" that you try to force onto the world, without testing to find out if it's something that the world actually needs.) Instead of trying to work out what your calling is and then forming a rigid plan on the basis of that calling, you should think like a scientist, testing hypotheses. This has three implications.

First, it means you should think of your career as a work in progress. Rather than having a fixed career plan, try to have a career "model"—a set of provisional goals and hypotheses that you're constantly revising as you get new evidence or opportunities. It's better to have a bad plan than no plan, but only if you're open to changing it.

Second, find out where you're uncertain, then reduce that uncertainty. Before making a decision, don't merely try to weigh all the pros and cons as you currently see them (though that is a good thing to do). Ask yourself: What is the single most important piece of information that would be most useful for my career decision? Now, what can I do in order to gain that information?

Third, test yourself in different paths. In science, you try to test hypotheses. Similarly, if you can, you should try to run "tests" of different career plans; this is important because it's often very difficult to predict in advance what careers will work out and which won't. For example, one person we coached started an internship at an asset management firm. Having no experience in it, she didn't know whether she'd like it, but she guessed she wouldn't. It turned out she hated the job. In a sense, she "failed." But that failure meant she could be much more confident in pursuing a different (academic) path instead. Her bad experience was very valuable.

Peter’s decision

For all these reasons, Peter regarded his potential impact later in life as the most important factor to consider when deciding which career to pursue. This made law school look considerably worse than he had previously thought: he’d be committed to one path, learning a very specific set of skills, ending up after three years with considerable debt.

Similar reasoning made software engineering or market research look more promising than nonprofit work. Both options would allow him to have a big immediate impact (via his donations if he was working as a software engineering or market researcher; via his labor if he was working at a nonprofit), and he also felt he would gain better long-term skills, and learn more, if he pursued options in software engineering or market research than he would if he worked for nonprofits straight out of college.

As a result, in his final year at school, he invested heavily in developing his computer programming skills, which enabled him to get a job as a software engineer at a start-up in Chicago that offers online loans to people with near-prime credit ratings. The organization he’s working for is certainly improving the world, but it isn’t the most effective organization he could work for. However, it allows him to build his skills in programming and statistics and will also allow him to gain business and financial experience, which will potentially open doors later on. Finally, the job gives him enough free time to focus on his nonprofit projects, which will further allow him to work out whether he should ultimately transition into full-time nonprofit work, or whether he should stick to his current path and focus on earning to give.

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By using this framework, you can assess the different career options available to you, but what are some of the best options? There are a huge number of possible paths, so I’ve used this framework to carve out some “career strategies” that I and the others at 80,000 Hours think are particularly promising. (As our research progresses, these are likely to change somewhat, so it’s worth also looking at the 80,000 Hours website for more information.) I’ll divide top career options into “solid bets,” where one is very likely to make a positive impact, and “high-potential long shots” where one has a smaller chance of making a very large impact.

Solid bets
Direct work for a highly effective organization

We don't often recommend that people go into nonprofit work straight out of college, because you will typically build fewer skills and credentials than you would in for-profit companies, which typically have greater resources to invest in training. However, there are still many situations where starting off working in a nonprofit can be a good bet. If you're considering working for a nonprofit, ask yourself the following questions:

- Is the organization particularly effective?
- Will I learn a lot working here?
- Is the organization money-rich but talent-poor?
- Am I sure I want to work within nonprofits long-term?

Given these conditions, GiveWell is an example of a nonprofit that could represent an excellent place to work. It's highly effective; it's very well run and gives excellent training to those who work there. It also needs talent much more than it needs money. One way in which you can assess whether an organization is money-constrained or talent-constrained is simply to ask the organization if they would prefer you donate to them or work for them. For example, in 2011, Alexander Berger had just graduated from Stanford with an MA in policy, organization, and leadership studies. He was unsure whether to earn to give or to take up a position at GiveWell. When he asked them how much they'd be willing to pay to have him as an employee, he found it was considerably beyond the amount he could donate if he earned to give.

There are also personal reasons why working for nonprofits might be a good option. For example, if you are fired up by a specific cause, it might feel important to you to be in the midst of the action. Alternatively, you might worry that your values will wane if you pursued something with either indirect benefits, like earning to give, or later benefits, like building skills. Perhaps you find it helpful and inspiring to surround yourself with like-minded people, and working for a place that shares these values will keep you committed to your ultimate goals. Personal considerations like these should be taken very seriously.

Finally, it's worth bearing in mind that nonprofits are not the only effective organizations you can work for. Most of the incredible progress that humanity

has made over the last few hundred years has not been due to the activities of nonprofits but through technology and innovation generally spurred by for-profit companies and governments. If you can find a company that is benefitting many people, or is correcting market failure in some way (such as by developing renewable alternatives to fossil fuels), this might be an effective means to have an impact. I'll discuss the potential of for-profits more under the Entrepreneurship section later in this chapter.

Earning to give

Earning to give enables you to start having a significant positive impact via the very most cost-effective organizations right from the beginning of your career. Often, it also allows you to build valuable skills and a valuable network that will prove useful later on in life.

If you're aiming to pursue earning to give over the long term, it's important to work out the long-run earning potential of different careers. On the Internet, you can often find the pay at a given level of experience within a field, but it's harder to find out how difficult it is to get to that level of experience, and how high-paying the alternatives are for those who move out of that career type. Moreover, within a given career path the earnings within specific subfields and from company to company can also vary dramatically.

We've researched this to help make the decision easier. Unsurprisingly, the very highest-paying careers are extremely competitive, such as finance in front-office positions, followed by consulting with somewhat lower earnings. Both of these careers also come with a high chance of dropping out, since at each stage, if you fail to be promoted, you'll probably have to switch into a different job with lower pay. Even taking this into account, however, they're still among the career paths with the highest expected earnings. Tech entrepreneurship and quantitative trading in hedge funds offer even higher expected earnings, though tech entrepreneurship comes with even higher risks (entrepreneurs have less than a 10 percent chance of ever selling their shares in the company at profit) and quantitative trading requires exceptionally strong mathematical skills.

Among less risky careers, medicine is probably the highest-earning option, especially in the United States, though earnings are probably less than in finance. Law is less appealing than one might think, because unless you can get into one of the very top law schools such as Harvard, you won't likely earn as much as

you would in consulting or finance. You also have to delay your earnings for several years while you complete law school, and graduate with substantial debt.

Outside of the hypercompetitive fields mentioned, there are still some very good options. Software engineering is a lucrative career with an unusually low barrier for entry, and many of the people we've coached chose to pursue that career. Chris Hallquist, for example, completed a philosophy degree at the University of Wisconsin–Madison. His degree didn't naturally lead to other career paths, so he looked at a wide range of options. He considered law but decided that the market for lawyers was too poor. Programming, in contrast, was highly promising. He was able to apply to App Academy, a three-month intensive programming school, and from there he got a job at a start-up in San Francisco with a six-figure salary.

Sales and marketing can also be good options. As well as being fairly high paying for a given level of competitiveness, they provide particularly useful skills if you want to move into the social sector later on in your career. Accountancy and actuarial work are also high paying for their level of competitiveness.

For those without a college degree, the highest-paying careers are usually trade professions, such as electricians, elevator installers and repairers, or the police force. Other options are pilots, or working in the energy sector, such as power plant operators. For those with an associate degree, the highest-paying careers are air traffic control, or within the medical profession, such as radiation therapists, nuclear medicine technologists, or dental hygienists.

One important issue to consider for all careers, but especially when earning to give through trade professions, is whether a job will be around in the future. A job might be outsourced (as IT support has been to some extent), or automated as a result of new technology. For example, before the advent of alarm clocks, people called knocker uppers were employed to knock on the windows of sleeping people in the morning, so they could get to work on time. Similarly, computers have decreased the need for jobs that involve basic number crunching; refrigerators have decreased the need for milkmen; robotic assemblers have decreased the need for assembly-line workers. The technology for self-driving cars is already here, so it may be unwise to become a taxi or a truck driver because there is a good chance that this industry will become automated over the next couple of decades. Improvements in technology are reducing demand for clerks and secretaries. In general, jobs that require social skills (like public relations), creativity (like fashion design), or precise

perception and manipulation (like boilermaking) are the least likely to become automated. Jobs that require physical proximity or high levels of training are also unlikely to be outsourced.

Another important consideration regarding earning to give is the risk of losing your values by working in an environment with people who aren't as altruistically inclined as you are. For example, David Brooks, writing in *The New York Times*, makes this objection in response to a story of Jason Trigg, who is earning to give by working in finance:

> You might start down this course seeing finance as a convenient means to realize your deepest commitment: fighting malaria. But the brain is a malleable organ. Every time you do an activity, or have a thought, you are changing a piece of yourself into something slightly different than it was before. Every hour you spend with others, you become more like the people around you.
>
> Gradually, you become a different person. If there is a large gap between your daily conduct and your core commitment, you will become more like your daily activities and less attached to your original commitment.

This is an important concern, and if you think that a particular career will destroy your altruistic motivation, then you certainly shouldn't pursue it. But there are reasons for thinking that this often isn't too great a problem. First, if you pursue earning to give but find your altruistic motivation is waning, you always have the option of leaving and working for an organization that does good directly. At worst, you've built up good work experience. Second, if you involve yourself in the effective altruism community, then you can mitigate this concern: if you have many friends who are pursuing a similar path to you, and you've publicly stated your intentions to donate, then you'll have strong support to ensure that you live up to your aims. Finally, there are many examples of people who have successfully pursued earning to give without losing their values. Bill Gates and the other members of the Giving Pledge (a group of billionaires who have pledged at least 50 percent of their earnings to charity) are the most obvious examples, but there are many more. When Jim Greenbaum graduated from the University of Virginia in the early 1980s, his primary aim was to make as much money as he could in order to use that money to make the world a better place. He founded a telecommunications company, Access Long

Distance, in 1985, selling it fourteen years later. Now age fifty-six, he's as committed to philanthropy as he ever was, donating more that 50 percent of his assets. There is certainly a risk of losing one's values by earning to give, which you should bear in mind when you're thinking about your career options, but there are risks of becoming disillusioned whatever you choose to do, and the experience of seeing what effective donations can achieve can be immensely rewarding.

Skill building

Skill building is a short-term strategy, which can be a very good option if you aren't sure about what you ultimately want to do. The idea behind this path is that you build up general-purpose career capital in order to keep your options open as much as possible, giving you time to figure out your long-run plans for having an impact and giving you skills that will be useful in what you choose to do.

Given this strategy, consultancy is a great first step. For example, Habiba Islam graduated from Oxford with a degree in politics, philosophy, and economics in 2011. She considered going into politics, and still thinks of that as a potential long-term aim, but she decided to work in consulting first. This makes sense: by working in consulting for a few years, you get a good all-round business education, you get to meet a wide variety of people, and you get clear evidence on your CV that you're capable of working hard and meeting deadlines. You're also able to earn to give, having an impact through your donations, in the meantime.

Other areas that are good for skill building are sales and marketing, because this training seems useful if you want to move into the social sector, where the ability to advertise particular messages persuasively is important. Another alternative is to get a PhD in a useful area. This is what Jess Whittlestone did: having studied math and philosophy previously, she pursued a PhD in behavioral science at Warwick Business School. This gives her the option of going into research, but if not she has still gained an important credential as well as knowledge of statistics and organizational decision making that will become useful later on. In addition, during a PhD your time is often more flexible than when employed full-time, which means you have more opportunity to start or pursue other projects on the side. Jess, for example, has used the opportunity to

write popular science articles in her spare time, giving her the option to become a full-time writer after her PhD if she chooses.

High-potential long shots

Working for an effective organization, earning to give, and skill building are all safe bets because if you pursue them, you can be confident that you will either have an impact immediately or that you're putting yourself in a good position to have an impact later on. However, as we saw in the chapter on expected value, we should also be interested in lower-probability higher-payoff activities, and there are some promising careers where your impact takes this form. Let's look at them.

Entrepreneurship

Entrepreneurship is an extremely promising option, giving you the potential to effect massive change, build valuable career capital, and, if pursuing for-profit entrepreneurship, make large profits that can be donated to effective causes. Entrepreneurship is also an area with lower barriers to entry than other careers, and many people without college degrees have become successful entrepreneurs. However, most start-up enterprises fail, and one has to be prepared to accept that fact. In addition, entrepreneurship usually comes with very long working hours and high levels of stress. Not everyone is cut out to start his or her own business.

To illustrate how valuable nonprofit entrepreneurship can be consider GiveDirectly, which we discussed in the chapter on effective charities. With an economics PhD from Harvard, the founder, Paul Niehaus, had very good earning-to-give options. However, he clearly made the right choice to set up GiveDirectly. Since its official launch in 2011, GiveDirectly has raised more than $20 million in donations—an amount that is growing rapidly. Even after taking into account the fact that most of those donations would have been donated anyway (albeit probably to less effective charities), Niehaus has done far more good by founding GiveDirectly than he would have if he'd earned to give.

If you're starting a nonprofit, one good strategy is to focus on a particularly important cause (which we'll discuss in the next chapter). Another important

question is to ask why the problem your new organization is addressing has not been solved already, or won't be solved in the future. Ask yourself:

- Why hasn't this problem been solved by markets?
- Why hasn't this problem been solved by the state?
- Why hasn't this problem already been solved by philanthropy?

In many cases, the answers to these questions will suggest that the problem is very difficult to solve, in which case it may not be the most effective problem to focus on. In other cases, the answers might suggest that you really can make good progress on the problem. If the beneficiaries of your action don't participate fully in markets and aren't governed by a well-functioning state, then there is a clear need for philanthropy. For example, we should expect the interests of future people to be systematically underrepresented because they don't participate in present-day markets or elections.

For-profit entrepreneurship can be even more compelling as an option than nonprofit entrepreneurship. Though it generally will be more difficult to focus your activities on the most important social problem within for-profit entrepreneurship, there is a much greater potential to grow quickly, and there is the additional benefit of larger earnings that can be used for good purposes later on in life. Economists also suggest that innovative entrepreneurship is undersupplied by the market. Professor William Nordhaus at Yale University has estimated that innovators only collect 2 percent of the value they generate; that is, for every dollar an innovative company makes in profit, society has benefitted by fifty dollars. By becoming an innovative entrepreneur, you are, on average, producing benefits to society that far exceed your paycheck.

The delightfully named Lincoln Quirk pursued this option, quitting graduate school in order to found a company called Wave, which makes it easier and cheaper for immigrants to send remittances to their home countries. Currently, if immigrants wish to send remittances, they have to use Western Union or MoneyGram. They have to go to a physical outlet to make the transfer and pay 10 percent in transfer costs. Lincoln Quirk and his cofounder, Drew Durbin, have built software that allows transfers from a mobile phone in the United States to a mobile phone in Kenya, and they take only 3 percent of the transfer costs. For now, they are just focused on Kenya, because that has particularly good infrastructure for this project, but they plan to significantly expand.

The potential positive impact of this idea is huge. Annual global remittances

are over $400 billion, several times the total global foreign-aid budget. The potential impact Lincoln's start-up could have, by making the costs of remittances a few percent cheaper, therefore amounts to tens of billions of dollars in increased financial flow from rich countries to poorer countries every year. Even just from Maryland to Kenya, annual remittances are more than $350 million; within one state Wave could therefore increase the amount going to Kenya every year by $24 million. After only a few months of operation, they already have thousands of users who have collectively transferred millions of dollars to Kenya.

Research

When Norman Borlaug was awarded the Nobel Peace Prize in 1970, the committee suggested that he'd saved one *billion* lives. Was he a politician? Or a military leader? Or a superhero? No, he was a fairly regular guy from Iowa who worked in agricultural research. He wasn't a typical academic: his credentials were limited, and he used techniques that had been available to the Victorians. Moreover, the innovation that made his name was rather boring—a new type of short-stem disease-resistant wheat. That wheat, however, was able to radically increase crop yield across poor countries. It helped to cause the "green revolution." Even after taking into account the fact that similar innovations may have happened even if he hadn't done his research, Borlaug's impact should be measured in the prevention of tens of millions of deaths.

In terms of researchers with impact, Borlaug isn't a lone example: in any list of the most influential people of all time, scientists and researchers make up a large percentage. Scientists who have clearly had a huge positive effect on the world include Fritz Haber and Carl Bosch, who invented synthetic fertilizer; Karl Landsteiner, who discovered blood groups, thus allowing blood transfusions to be possible; Grace Eldering and Pearl Kendrick, who developed the first whooping cough vaccine; and Françoise Barré-Sinoussi and Luc Montagnier**,** who discovered HIV.

In each of these cases, even after taking into account that these developments would have eventually happened anyway, the good each of these researchers did should be measured in the millions of lives saved. And clearly many other researchers, from Isaac Newton to Daniel Kahneman, have made a huge contribution to human progress even if it's not easy to quantify their impact

in terms of lives saved.

Like innovative entrepreneurship, research is an area that is drastically undersupplied by the market because the benefits are open to everyone, and because much of the benefit of research occurs decades into the future. Governments try to fix this problem to some extent through state-funded research, but academic research is very often not as high-impact as it could be—the incentive facing many academics is work on the most theoretically interesting questions rather than the most socially important questions. This means that, by deliberately pursuing research that has a large impact, one could make a significant difference that wouldn't have happened otherwise.

However, the distribution of achievements in research (as suggested by a number of publications, awards, and citations) is heavily fat-tailed: a large proportion of scientific achievement comes from a very small number of scientists. This suggests that research might be the best option only if it's an area you really excel in. But if you might be able to become such a person, it's an option you should take seriously.

If you're thinking about going into research, it's important to bear in mind the job prospects: fields vary dramatically in both the difficulty of getting an academic job post PhD, and in the difficulty of finding jobs outside of academia. Within philosophy, for example, there are about four times as many doctoral candidates as there are tenure-track positions; as a result, many aspiring academic philosophers end up unable to find a job in academia. In contrast, within economics the number of people who seek academic employment more closely matches the number of academic jobs. Another important consideration is the extent to which one can have an impact outside of academia. Again, an economics PhD is a good bet, being generally well respected in policy and business.

With these considerations in mind, 80,000 Hours suggests that the areas with the greatest potential to do high-impact research while simultaneously gaining career capital that keeps your options open are economics, statistics, computer science, and some areas of psychology. This, however, shouldn't deter you if you have some particular interest or expertise within an area of research that is relevant to a particularly high-priority cause area.

One good way to have impact within research is to combine fields. There are far more combinations of fields than there are individual fields, and research tends to be influenced by traditional disciplinary distinctions, so research at the intersection of two disciplines is often particularly neglected and can for that

reason be very high-impact. For example, Daniel Kahneman and Amos Tversky were psychologists who caused a revolution within economics: they applied methods developed in psychology to test assumptions about rational choice that were prevalent within economics, thereby leading to the new field of "behavioral economics." By giving us a better understanding of human behavior, this field has improved our ability to cause desirable behavior change, including in development. Similarly, effective altruism has made the progress it has by combining concepts from moral philosophy and economics.

Combining fields can be especially useful when one moves from a more theoretical area to an area with real-world applications. Within academia, the most prestigious research fields—which often therefore attract the best researchers—are often those that have the fewest practical applications. (A friend of mine has jokingly commented that a Fields Medal—the equivalent of a Nobel Prize in mathematics—indicates two things about the recipient: that they were capable of accomplishing something truly important, and that they didn't.) If you are a top researcher and are willing to sacrifice some amount of status within academia, you can have considerable impact by moving into more applied areas of research.

Politics and advocacy

Politics is another area where one has a small chance of extremely large influence. For someone entering party politics in the United Kingdom, most of his or her expected impact comes from the chance of ending up in the cabinet or as prime minister. Even though the chances of being that successful are small, your potential influence, if you do succeed, is very great indeed. As discussed in the chapter on expected value, this was the reason Laura Brown pursued a career in party politics. Though we only discussed British politics in that chapter, similar considerations apply everywhere.

Advocacy also has potentially high payoffs, as one could influence the behavior of many thousands of people and help to influence debates around particular policies, though this is particularly difficult to quantify. One could become an effective advocate through journalism, or by pursuing an early career in academia and then moving to become a "public intellectual." Someone from the effective altruism community who's pursued this path is Dylan Matthews. He studied moral and political philosophy at Harvard. He considered continuing

his studies at graduate school but instead pursued journalism in part because doing so gave him a platform from which to champion particularly important causes. He worked for *The Washington Post* and now works for Vox.com. In this position, he's been able to promote and discuss ideas he thinks are important, such as more liberal immigration policies, a universal basic income, and the idea of earning to give.

In advocacy, we would expect the distribution of impact to be highly fat-tailed: it's a winner-takes-all environment, where a small number of thought leaders command most of the attention. We don't have data on impact through advocacy in general, though the distribution of book sales, which one could use as a proxy, is highly fat-tailed, as is the distribution of Twitter follower counts. Again, therefore, this is an area you might only want to go into if you think you have an unusually good chance of being successful.

Volunteering

So far I've discussed how you can choose a career in order to make a difference. Similar considerations apply to volunteering, though there's an additional challenge. As a volunteer, you're often not trained in the area in which you're helping, which means the benefit you provide might be limited. At the same time, you're often using up valuable management capacity. For that reason, volunteering can in fact be harmful to the charity you're volunteering for. Anecdotally, we have heard from some nonprofits that the main reason they use volunteers is because those volunteers subsequently donate back to the charity.

This means you should try to volunteer only in ways that cost an organization relatively little. For example, by contributing high-quality work to *Wikipedia*, you can provide a significant benefit to many people at almost no cost to others. Some organizations also have opportunities that are designed to take on board volunteers with little cost. Mercy For Animals, for example, is a vegetarian advocacy organization. It has volunteers contact people who have commented on videos on factory farming on Facebook. These volunteers then discuss the option of going vegetarian with them. This provides a significant benefit while costing the charity very little in management time. An alternative route to having an impact without imposing a burden on charities is to work additional hours instead of volunteering and donate the money you make.

However, you don't need to limit yourself to this. Instead, I'd encourage you

to think about volunteering primarily in terms of the skills and experiences you'll gain, which will enable you to have a greater impact later in your life. Because the total time you spend volunteering will be only a tiny fraction of the total time you spend on your career, the impact volunteering has on other areas of your life will generally be much greater than the impact you have via the volunteering itself.

For example, as an undergraduate, I went to Ethiopia to teach at a school. The impact I had there was limited (as far as I can tell, I mainly just allowed the real teacher to take some time off—which is a benefit, but a small one compared to other things I could have done with my time and money, especially given the costly flights). However, the impact that seeing extreme poverty up close had on me was significant: it shaped the choices I have made in the years since then, and it helps motivate me when I'm doing activities that are more abstract than teaching at that school. The main impact of that trip to Ethiopia was its effect on me.

It might feel odd to volunteer simply because it benefits you, but I think that, as long as you think of volunteering as the first step toward generally moving your life in the direction of making a difference, there's nothing problematic about this. Like anything, benefitting others requires some training, and volunteering can be a good way to get experience.

Later career moves

What if you're later on in your career and want to make a difference?

Later in life, the same framework we introduced at the start applies, but career capital becomes a lot less important, and facts about your specific situation (the skills and experience that you've developed) become a lot more important. For people who didn't set out to build skills that are useful for making a difference, earning to give can be a particularly good option. Often, people move from high-paying jobs to something that directly makes a difference even though they have limited expertise in the area they move into, when they could have done much more good by keeping their high salary and earning to give.

For example, after graduating with a PhD in philosophy from Brown University in the seventies, Frederick Mulder left academia in order to become an art dealer. He became very successful but progressively wanted to use his career to make a difference. He thought being an art dealer was of neutral moral

value—or perhaps slightly negative, he told me, because of the amount of flying he has to do—but realized that moving out of art and into the nonprofit world wasn't the best way for him to use his talents. "There are many things that I'd like to see done in the world," he said, "but I can't do them myself because I don't have those skills. So what better than to use the resources I can generate by doing something I love in order to help someone else do something really important that needs to be done?" He continued in his career, donating every year between 10 and 80 percent of his earnings.

If you've built up useful skills, on the other hand, then it can be a good option to contribute those skills directly to an effective area. This is what Rob Mather of the Against Malaria Foundation did. He had extensive experience in business and sales, which meant he understood how to run an organization and pitch ideas, and had developed an incredible capacity to make things happen. (His first foray into altruism was organizing a swimming-based fund-raiser and he managed to get one hundred thousand swimmers to participate.) His background also meant that he doesn't need to take a salary, something that impressed donors in the early stages. His sales skills allowed him to get a huge amount of pro bono support from a variety of companies. As a result, he has built a charity that is among the top recommended at GiveWell, has raised more than $30 million, and has distributed more than ten million long-lasting insecticide-treated bed nets, saving thousands of lives.

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Choosing a career is one of the most important decisions you'll ever make, and I hope that the framework I've presented in this chapter will help you to think through this decision.

In order to use your career to make a difference, one rule of thumb that I mentioned is to work on a particularly important cause. However, so far I've primarily discussed only the cause of fighting global poverty. What about all the other problems in the world? How can we decide which are most important to focus on? In the next chapter, I tackle this question.

TEN

POVERTY VERSUS CLIMATE CHANGE VERSUS . . .

Which causes are most important?

In the summer of 2013, President Barack Obama referred to climate change as "the global threat of our time." He's not alone in this opinion. The US secretary of state, John Kerry, called climate change "the greatest challenge of our generation"; former Senate majority leader Harry Reid has said that "climate change is the worst problem facing the world today," and the cochair of the Intergovernmental Panel on Climate Change Thomas F. Stocker called climate change "the greatest challenge of our time."

Are Obama and these other commentators correct? Is climate change the most important cause in the world today—a greater global priority than extreme poverty? How could we decide?

A lot of people have asked these questions. Though foundations and social entrepreneurs often talk about trying to maximize their impact, they normally just focus on maximizing their impact within the cause or causes that they're personally passionate about (like poverty, or education, or climate change), rather than thinking strategically about which causes they should focus on. If we're really trying to do the most good we can, however, then we need to think carefully about cause selection. We'll be able to help more people to a greater degree within some cause areas than we will in others, which means that, in order to have the biggest impact we can, we have to think carefully about what causes we choose to focus on.

So far the organizations I've recommended as extremely cost-effective have all been focused on global poverty. We can have a high degree of confidence that these charities do a substantial amount of good. However, you might

reasonably think that the very best way of helping others isn't to fight global poverty, or that the best way of fighting global poverty is through activities whose benefits are more difficult to quantify than those of the charities I've mentioned. Moreover, you might want to do good with your time (whether through volunteering or your work) rather than your money. In which case, your own particular skills, experiences, and opportunities become a lot more important and these might not fit as well with global poverty as they do with other areas. This means we need to think about cause selection.

In this chapter, I'm not going to attempt to definitively answer the question of what cause is most important to focus on, which would be impossible to do in a whole book let alone a single chapter. Instead, I'm going to introduce a framework for thinking about the question and then use that framework to suggest some causes that, on the basis of research at GiveWell and the Centre for Effective Altruism, I think should be given high priority. Again, bear in mind that decisions about cause selection involve value judgments to an even greater degree than some of the other issues I've covered in this book, so the conclusions you reach might be quite different from the ones I reach. Though effective altruism aims to take a scientific approach to doing good, it's not exactly physics: there is plenty of room for differences of opinion. This doesn't, however, make thinking rigorously about which cause one chooses to focus on any less important.

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On the framework I propose, you can compare causes by assessing them on how well they do on each of the following three dimensions:

First, *scale*. What's the magnitude of this problem? How much does it affect lives in the short run and long run?

Second, *neglectedness*. How many resources are already being dedicated to tackling this problem? How well allocated are the resources that are currently being dedicated to the problem? Is there reason to expect this problem can't be solved by markets or governments?

Third, *tractability*. How easy is it to make progress on this problem, and how easy is it to tell if you're making progress? Do interventions within this cause exist, and how strong is the evidence behind those interventions? Do you expect to be able to discover new promising interventions within this cause?

If we're thinking about contributions of time rather than just money, then

there is a fourth important dimension:

Personal fit. Given your skills, resources, knowledge, connections, and passions, how likely are you to make a large difference in this area?

We discussed personal fit in the previous chapter on career choice, and most of that discussion applies equally well to the choice of causes. This chapter will therefore focus on the first three criteria, but you should keep in mind that, if you're thinking about working or volunteering in an area, the considerations I give need to be mediated by your personal fit with the cause.

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Scale refers to the size of the problem, which should usually be measured in terms of total actual or potential impact on others' well-being. For example, cancer, as I noted earlier, is a bigger problem than malaria because it is responsible for 7.6 percent of all ill health (measured in QALYs lost) worldwide, whereas malaria is responsible for 3.3 percent of ill health worldwide.

All other things being equal, the larger the problem, the higher priority the cause should be. This is for a couple of reasons. First, many activities make a proportional impact on a problem. If you can develop a new cheap treatment for either cancer or malaria, you should probably develop the cheap treatment for cancer. Cancer causes more ill health and death than malaria, so, when that treatment is rolled out, it will have a larger total benefit. Political change is another area where you can have a proportional effect on a problem: if you can improve the health-care policy of either New Jersey or the whole of the United States, the fact that the US policy would affect a far larger number of people is clearly relevant.

Second, the scale of a problem also determines how long we should expect the problem to persist. There's no point in investing significant time and resources into learning about a cause if that problem will be resolved a few years later. Similarly, if the problem is very big, then it will take a large amount of resources before the most effective opportunities are used up.

The second aspect of the framework is *tractability*, which means the long-run average of people's ability to turn resources into progress toward solving the problem. Even if a problem is hugely important and highly neglected, that doesn't mean it's an important cause to focus on. There might simply be very little we can do about it. For example, aging is a problem that is huge in scale: almost two-thirds of global ill health is a result of aging. It's a problem that's

highly neglected: there are only a tiny number of research institutes focused on trying to prevent the causes of aging (rather than to treat its symptoms, like cancer, stroke, Alzheimer's, and so on). However, the reason it's neglected is because many scientists believe it to be highly intractable. Preventing the aging process is just a very difficult problem to solve.

In previous chapters, I discussed explicit cost-effectiveness estimates (e.g., one QALY costs one hundred dollars). This is useful when thinking in the short term. If we have good evidence on specific interventions, we can compare cost-effectiveness directly. But these estimates only apply to specific interventions, and the estimated cost-effectiveness of specific programs within a cause area will change over time. This means that, when thinking about investing time and effort into a cause, it's important not just to look at our current best-guess estimates but to make estimates about the long-run tractability of the cause as well.

The third aspect of the framework is *neglectedness*, which refers to how many resources are being invested into this cause, relative to its scale. Because of diminishing returns, all other things being equal, the more resources that have been invested in a specific cause, the more difficult it will be to make progress within that cause with a given amount of resources, because typically many of the most cost-effective opportunities will have already been taken.

This is an easy consideration to forget. If something seems like a huge problem—perhaps the biggest problem in the world—it is natural to think one should focus on it. But if that problem already has a large amount of resources invested in it, then additional resources might be better spent elsewhere. For example, HIV/AIDS, tuberculosis, and malaria traditionally received much more attention than conditions like intestinal worms. One reason for this, I think, is that these other conditions cause a much greater amount of ill health (measured in number of deaths, or QALYs lost) than intestinal worms do, and thereby attracted a disproportionate share of attention. However, precisely because intestinal worms had much less attention, the cheapest and most effective ways of treating them were still available. In fact, it wasn't until Alan Fenwick, executive director of the Schistosomiasis Control Initiative, coined the term *neglected tropical diseases* that these conditions became more prominent in the discussion of global health. The term had two benefits: first, it classified a wide range of conditions under one heading. This meant that, even though the burden of disease from schistosomiasis, for example, was small compared to the global burden from HIV/AIDS, the global burden of disease from all neglected tropical

diseases was comparable to the global burden from HIV/AIDS. Second, the name highlighted the fact that these diseases were neglected.

When you read the rest of this chapter, you might be surprised that you haven't heard much about many of the causes I suggest are high-priority. That just shows the importance of considering neglectedness when choosing a cause. The causes we hear the most about are precisely those where it will be harder to make a big difference; the causes that get less attention are those where we may be able to have a massive impact.

To illustrate the framework of scale, tractability, and neglectedness, let's see how it provides a strong case for focusing on global poverty rather than domestic poverty. The scale of global poverty is much larger, in both numbers and extremity, than domestic poverty within first world countries. There are 46.5 million Americans living in relative poverty, defined as living on less than $11,000 per year, but there are 1.22 billion people worldwide living in extreme poverty, defined as living on less than $550 per year. Second, global poverty is much more neglected than domestic poverty. In 2014, $500 billion was spent on welfare in the United States, whereas total aid and philanthropy to poor countries was only $250 billion in that year. Most important, as we've seen, extreme poverty is far more tractable than domestic poverty, with current cost-effectiveness estimates suggesting that you can do one hundred times as much to provide the same size of benefit to someone in extreme poverty as you can to someone domestically.

It is relatively easy to compare global and domestic poverty, because the former seems more promising on all three dimensions. In other cases, it's not so easy: one cause might be better on one dimension but worse on another. In what follows I'll give a number of examples of what seem like highly promising causes. As with the chapter where I discussed charities, I'll score each cause from "not very" to "extremely," on each dimension of scale, neglectedness, and tractability. We'll see that it's difficult to find causes that score very highly on each dimension. Criminal justice reform seems unusually tractable, but comparatively low on scale. Increasing international labor mobility scores highly on scale but does very poorly on tractability. Which cause to focus on is therefore something that involves difficult judgment calls: it's not clear how to weigh these different criteria against one another, and people may reasonably disagree on how to do so.

A couple of caveats before we begin. First, in each case, I'm going to be able to describe only the cause and why, in short, I think it's promising (I

provide further reading in the endnotes for those who are interested). This chapter should therefore be taken as an invitation to explore causes other than extreme poverty, with recommendations rather than a definitive argument for why these are the most important causes. Second, for some of these causes, it's not as easy for casual donors to make a big difference in the same way they can if they donate to the charities listed in chapter seven; some of these causes are more in need of good people than more money.

With those caveats in mind, let's look at some high-priority causes.

US criminal justice reform

What's the problem? At any one time, 2.2 million people are incarcerated in the United States. That's 0.7 percent of its population, giving it one of the highest incarceration rates in the world. As a comparison, the United Kingdom has an incarceration rate of 0.14 percent; Canada's is 0.1 percent; Japan's is 0.05 percent. At the same time, the United States has the highest level of intentional homicide (which is a good proxy for rates of criminality in general) in the developed world, at 4.7 per 100,000 people per year (in comparison, the United Kingdom's homicide rate is 1 per 100,000 people per year; Canada's is 1.6; Japan's is 0.3). This suggests that the high incarceration rate is not deterring crime, and may even be increasing it. Since the 1990s, incarceration rates in the United States have increased dramatically despite a fall in violent crime in that period. According to expert criminologists, incarceration rates could be reduced (especially for low-risk offenders) by 10 percent or more while keeping levels of criminality the same, or even reducing them.

In this book, I've argued that the highest-impact ways of doing good typically won't be aimed at providing benefits to people in rich countries, so it might seem surprising to see criminal justice reform on this list. But though opportunities to make a truly massive difference domestically are rarer than opportunities to make a massive difference abroad, that doesn't mean they're nonexistent. What distinguishes criminal justice reform from other sorts of domestic issues is that, while being fairly great in scale, it is an issue that is both neglected and unusually tractable at this point in time.

Scale: Fairly large. A reduction of the prison population by 10 percent would have a variety of benefits. It would of course greatly benefit all the people (more than two hundred thousand each year) who would not have to spend time in

prison (often for crimes such as drug possession, which does not pose as great a threat to society as violent crime). As well as the misery of life in prison itself, costs of a prison sentence include: forgone earnings, the reduction of future income, and the costs borne by families, especially children, of having a family member in prison.

Incarceration also costs the government about $25,000 per person per year, whereas parole costs only about $2,000 per year, meaning the government could save billions annually. If larger reductions in incarceration rates were feasible (remembering that even a 50 percent reduction in incarceration rate would still mean that the US incarceration rate is 3.5 times as high as Canada's), then the scale would be considerably greater again.

Neglectedness: Fairly neglected. GiveWell estimates that only about $20 million is spent per year by organizations other than governments on prison reform to substantially reduce incarceration rates. (A further $40 million per year is spent on other sorts of prison reform, such as campaigning to abolish the death penalty.)

Tractability: Extremely tractable. Due to a combination of declining rates of criminality and a particularly poor economy following the recession, there appears to be an unusual level of bipartisan support for prison reform. The Pew Charitable Trusts provides on example of progress in this area: as of summer 2014, it had assisted with twenty-nine reform packages in twenty-seven states since 2007, at a cost of just $25 million, with a forecasted reduction in prison population in those states of 11 percent. On the assumption that these forecasts are accurate and that these reforms would not have happened without Pew's intervention, the cost per year of life in prison averted would be as low as twenty-nine dollars.

What promising organizations are working on it? The Pew Charitable Trusts Public Safety Performance Project aims to make criminal justice policy more effective and evidence-based by providing technical assistance to states, doing policy evaluations, providing information on what works, and fostering broad political support for specific policies.

BetaGov (accepts donations via GiveWell), led by Professor Angela Hawken of Pepperdine University, is a start-up center that provides tools to help practitioners conduct experimental trials of policies.

The University of Chicago Crime Lab (accepts donations) runs randomized

controlled trials to provide evidence-based criminal justice policy advice to governments.

International labor mobility

What's the problem? Increased levels of migration from poor to rich countries would provide substantial benefits for the poorest people in the world, as well as substantial increases in global economic output. However, almost all developed countries pose heavy restrictions on who can enter the country to work.

Scale: Very large. Eighty-five percent of the global variation in earnings is due to location rather than other factors: the extremely poor are poor simply because they don't live in an environment that enables them to be productive. Economists Michael Clemens, Claudio Montenegro, and Lant Pritchett have estimated what they call the place premium—the wage gain for foreign workers who move to the United States. For an average person in Haiti, relocation to the United States would increase income by about 680 percent; for a Nigerian, it would increase income by 1,000 percent. Some other developing countries have comparatively lower place premiums, but they are still high enough to dramatically benefit migrants. Most migrants would also earn enough to send remittances to family members, thus helping many of those who do not migrate. An estimated six hundred million people worldwide would migrate if they were able to.

Several economists have estimated that the total economic gains from free mobility of labor across borders would be greater than a 50 percent increase in world GDP. Even if these estimates were extremely optimistic, the economic gains from substantially increased immigration would be measured in trillions of dollars per year. (I discuss some objections to increased levels of immigration in the endnotes.)

Neglectedness: Very neglected. Though a number of organizations work on immigration issues, very few focus on the benefits to future migrants of relaxing migration policy, instead focusing on migrants who are currently living in the United States.

Tractability: Not very tractable. Increased levels of immigration are incredibly unpopular in developed countries, with the majority of people in Germany, Italy, the Netherlands, Norway, Sweden, and the United Kingdom favoring reduced

immigration. Among developed countries, Canada is most sympathetic to increased levels of immigration; but even there only 20 percent of people favor increasing immigration, while 42 percent favor reducing it. This makes political change on this issue in the near term seem unlikely.

What promising organizations are working on it? ImmigrationWorks (accepts donations) organizes, represents, and advocates on behalf of small-business owners who would benefit from being able to hire lower-skill migrant workers more easily, with the aim of "bringing America's annual legal intake of foreign workers more realistically into line with the country's labor needs."

The Center for Global Development (accepts donations) conducts policy-relevant research and policy analysis on topics relevant to improving the lives of the global poor, including on immigration reform, then makes recommendations to policy makers.

Factory farming

What's the problem? Fifty billion animals are raised and slaughtered in factory farms every year. Relatively small changes to farming practices could substantially improve these animals' welfare. Raising animals for consumption also produces substantial greenhouse gas emissions.

Scale: Up to very large, depending on value judgments. The scale of the problem depends on how much weight you put on the interests of nonhuman animals. Many people regard the suffering of non-human animals as morally important. Given these values, the scale of the problem of factory farming would seem very great. The meat industry is also one of the largest contributors to climate change, amounting to 14.5 percent of global greenhouse gas emissions.

Neglectedness: Extremely neglected. Total expenditure from nonprofits on factory farming practices is less than $20 million per year.

Tractability: Fairly tractable. Rates of meat consumption are decreasing, and there appear to be reliable ways to persuade people toward a vegetarian diet. In Europe there have been moves to improve conditions in factory farms, such as a ban on battery hen cages. In the United States, however, there is a strong farming lobby that opposes political change on the issue.

What promising organizations are working on it? Mercy For Animals (accepts

donations) conducts investigations to expose animal cruelty in farming and engages in education and outreach such as online videos and advertisements related to animal welfare.

The Humane League (accepts donations) engages in education and outreach, primarily through online videos and advertisements, on-the-ground leafleting, and Meatless Mondays campaigns.

The Humane Society of the United States Farm Animal Protection Campaign (accepts donations) aims to end the most extreme confinement practices in factory farming by working with farmers to improve the treatment of animals in food production, and lobbying for better laws and against antiwhistleblower legislation.

Mercy For Animals and the Humane League are top recommended by the independent evaluator Animal Charity Evaluators; the Humane Society of the United States Farm Animal Protection Campaign is rated as a standout organization.

2 to 4ºC climate change

What's the problem? Greenhouse gas emissions will probably lead to a 2 to 4ºC rise in average global temperatures. This will cause trillions of dollars of economic damage, the loss of hundreds of thousands or millions of lives, and significant reductions in biodiversity.

Scale: Fairly large. Economists typically estimate that a 2 to 4ºC rise in temperature would cause a reduction in GDP by about 2 percent. However, most of the damage from climate change will occur in the future, when people even in poor countries are considerably richer than they are now. For example, according to the second-most-pessimistic model in the *Stern Review* (a particularly grave assessment of climate change published in 2006), by 2100 the economic costs from climate change will amount to $400 per person, reducing the average GDP per person in developing countries from $11,000 to $10,600.

Economic assessments of the costs of climate change typically only look at human costs. If you also value preservation of the natural environment, you should regard climate change as considerably worse than the economists' models suggest. For example, climate change may potentially lead to the extinction of 20 to 30 percent of species.

Neglectedness: Not very neglected. Climate change is well-known as a major social issue. The US government spends about $8 billion per year on climate change efforts, and several hundred million dollars are spent each year by foundations.

Tractability: Fairly tractable. There are reliable ways in which individuals can reduce the amount of global greenhouse gas emissions. The opportunity to effect political change is unclear, however, as political progress has been slow. For example, the 2009 United Nations Climate Change Conference in Copenhagen was the largest meeting of the heads of state in history, but it achieved very little.

What promising organizations are working on it? Cool Earth (accepts donations) helps indigenous peoples in Peru and the Democratic Republic of the Congo protect the rain forest in which they live from illegal logging.

ClimateWorks (accepts donations) campaigns for public policies that will decrease the output of greenhouse gas emissions.

Catastrophic climate change

What's the problem? Given current climate models, we are unable to rule out the possibility that greenhouse gas emissions will lead to what I call catastrophic climate change, with temperature rises of 10ºC or more. Though the chance of this occurring is very small, the outcome would be very grave, which means the expected value of preventing this possibility may be very high.

Scale: From fairly large to extremely large, depending on value judgments. How one evaluates risks of global catastrophe depends crucially on how much value one places on maintaining a flourishing civilization long into the future. The chances of such bad outcomes are very small, but if you regard civilizational collapse as *extremely* bad, then it could be very important to prevent these worst-case scenarios.

Neglectedness: Fairly neglected. Most focus on climate change is on reducing emissions. This is a good thing whether or not the best-guess climate-change predictions are correct. However, there has been comparatively little research done into the likelihood of catastrophic climate change, or into mitigation and adaption strategies in extreme warming scenarios. About $11 million per year is spent on research into geoengineering (see below).

Tractability: Fairly tractable. The most pressing need is to fund further research into both assessing the likelihood of worst-case scenarios and to developing strategies to reduce the chance of the worst-case scenarios. One potential opportunity is research into geoengineering, which could be used as a measure of last resort. Geoengineering is the attempt to deliberately cool the planet, for example by pumping sulfates (which are gasses that reflect sunlight and in turn cool the planet) into the stratosphere. Geoengineering itself may pose significant risks such as depletion of the ozone layer, but if it turns out we're facing very large temperature increases, then the risks may be justified. Moreover, geoengineering is cheap enough that in the future, individual countries could unilaterally undertake risky geoengineering projects. It would therefore be desirable to have a good understanding of the impacts and risks of geoengineering ahead of time. However, it may be that increased research into this area could detract from other mitigation and adaptions strategies.

What promising organizations are working on this? The University of Oxford Geoengineering Programme advocates to conduct transparent and socially informed research into the social, ethical, and technical aspects of geoengineering.

The Solar Radiation Management Governance Initiative provides advice on the regulation of geoengineering, seeking to ensure that research into solar radiation management (one form of geoengineering) is conducted in a responsible manner.

General mitigation of climate change is also a way to reduce catastrophic risk, so Cool Earth and ClimateWorks, previously mentioned, are also promising charities in this area.

Other global catastrophic risks

What's the problem? There are a number of low-probability risks that could have disastrous outcomes. These include risks of nuclear war, pandemics, and bioterrorism.

Scale: From fairly large to extremely large, depending on value judgments. As with catastrophic risk from climate change, how one evaluates risks of global catastrophe depends crucially on how much value one places on maintaining a flourishing civilization long into the future.

Neglectedness: Fairly neglected. Because global catastrophes are unprecedented and unlikely, they may not receive the attention they deserve. The amount of philanthropic funding on these issues is comparatively small: about $30 million per year on nuclear security, and only a few million per year on biosecurity. However, there is considerable funding and involvement from governments. Only a very small amount of funding ($1 to $2 million) is spent on global catastrophic risks in general, such as research to identify currently overlooked risks of global catastrophe.

Tractability: Fairly tractable. There are opportunities for funding academic research into catastrophic risks in general, and there are some opportunities for increasing policy influence. However, none of these activities have as clear a path to impact as, for example, donating to fight extreme poverty.

What promising organizations work in this area? The Nuclear Threat Initiative (accepts donations) works on a variety of projects to reduce the spread of nuclear, biological, and chemical weapons.

The Future of Humanity Institute and the Centre for the Study of Existential Risk (both accept donations) are interdisciplinary research institutes at the Universities of Oxford and Cambridge that assess the magnitudes of global catastrophic risks and try to develop risk-mitigation strategies.

In summary, here's a table of top causes:

	Scale	Neglectedness	Tractability
Extreme Poverty	●●●	●●	●●●●
US Criminal Justice Reform	●	●●	●●●●
International Labor Mobility	●●●	●●●	●
Factory Farming	**Up to ●●● depending on value judgments**	●●●●	●●●
2-4ºC Climate Change	●●	●	●●
Catastrophic Climate Change	**●● to ●●●● depending on value judgments**	●●	●●
Other Global Catastrophic Risks	**●● to ●●●● depending on value judgments**	●●●	●●

●●●●-extremely (best/most) and ●-not very (least)

CONCLUSION

BECOMING AN EFFECTIVE ALTRUIST

What should you do right now?

This book has presented effective altruism's approach to making a difference. By outlining the five key questions of effective altruism and the frameworks for choosing a charity, a career, and a cause (all restated in the appendix), I hope that I have given the tools to help you increase your impact in all areas of your life. The next time you reach for your wallet after seeing a charity fund-raiser, think about signing up to volunteer, or go shopping and wonder whether to buy ethically produced goods, I hope you will bear this perspective in mind.

We've seen that, by employing effective altruism's way of thinking, we each have the power to do a tremendous amount of good. A donation of $3,400 can provide bed nets that will save someone's life, deworm seven thousand children, or double the income of fifteen people for a year. Those charities with less concretely measurable benefits, like those working on criminal justice reform, or more relaxed immigration policy, or catastrophic climate change, may, in terms of expected value, do even more good again.

The film *Schindler's List* tells the story of the war hero Oskar Schindler, a Polish entrepreneur who ran munitions factories for the Nazis. Initially, he was an opportunist, happy to take advantage of the war for his own gain. But as he saw the horrors that the Nazis inflicted on the Jews, he realized he couldn't simply stand by and watch. So he bribed officials to spare his Jewish workers, ultimately saving more than a thousand of them.

Though Schindler's story is inspiring, you might think that war is a particularly unusual time, and therefore stories like Schindler's aren't really that relevant to our lives. What we've seen in this book is that this isn't true. Every one of us has the power to save dozens or hundreds of lives, or to significantly improve the welfare of thousands of people. We might not get books or films

written about us, but we can each do an astonishing amount of good, just as Schindler did.

If you feel empowered by this, by far the most important thing for you to do is ensure that this feeling doesn't dissipate over the coming weeks or months. Here are some ideas about how best to do that.

1: Establish a habit of regular giving.
Go onto the website of a highly effective charity and sign up to make a regular donation, even if it's just ten dollars per month. This is the easiest and most tangible way of having a massive immediate positive impact. Even if you think that the main way you'll help others in life won't be through your donations, starting to give is a good way of solidifying your intentions, and of proving to yourself that you mean business.

Some of the top charities I've mentioned in this book are Against Malaria Foundation, Cool Earth, Development Media International, Deworm the World Initiative, GiveDirectly, and the Schistosomiasis Control Initiative. Pick whichever you believe to be best and begin a habit of effective donations. Even a relatively small monthly donation to these charities will have a big impact.

2: Write down a plan for how you're going to incorporate effective altruism into your life.
Get a pen and paper, or open up a document, and make some notes about the changes you plan to make. Make the plan specific and concrete. If you think you're going to start giving, write down what proportion you intend to start giving and when. If you're going to change what you buy, write down what changes you plan to make and by when. If you're going to pursue a career that makes a difference, write down which dates you're going to set aside in order to find out more information relevant to your next steps.

3: Join the effective altruism community.
Go onto efffectivealtruism.org and sign up to the effective altruism mailing list. That way you can learn more about effective altruism and about how to get involved in the community, and read stories of people putting effective altruism into practice. You can also talk with others in the Effective Altruism Forum, and there you can find out more about issues that I haven't been able to cover in this book, like whether to give now or invest and give later, or the impact of giving on your personal happiness.

4: Tell others about effective altruism.

Go on Facebook, Twitter, Instagram, or your blog, and write some of your thoughts about what you've read. If you found the arguments in this book convincing, then your friends, family, and colleagues might do so, too. If you can get one person to make the same changes you make, you've doubled your impact.

It can be awkward to raise the idea of effective altruism—you don't want to come across as holier-than-thou, or critical of projects that are less effective—but there are ways to do so naturally. For your birthday, instead of presents, you could ask for donations to a highly effective charity, creating a webpage on Causevox.com; Charity Science, a fund-raising website set up by two people in the effective altruism community, helps you to do this on their Take Action page. If it's the holiday giving season, you could offer to match any donations made by your colleagues up to a certain amount. You could organize discussion groups on career choice, or cause selection, or ethical consumerism.

If you want to go further than these actions, you might wish to take Giving What We Can's pledge to donate 10 percent of your income. You could read the career advice on 80,000 Hours, or apply for one-on-one career coaching there. Or you might wish to set up a local meet-up group, starting discussions about effective altruism in your area, with your friends or through your church or university. Further information on all these things is available at effectivealtruism.org.

Whatever you choose to do, think of today as a pivotal step on your journey to making the world a better place. Each of us has the potential to have an enormous positive impact. I hope this book has both inspired you to do so and given you the tools you need to get there.

APPENDIX

THINKING LIKE AN EFFECTIVE ALTRUIST

The five key questions of effective altruism.

1. How many people benefit, and by how much?
Like James Orbinski, the doctor who engaged in triage during the Rwandan genocide, we need to make hard decisions about who we help and who we don't; that means thinking about how much benefit is provided by different activities. The quality-adjusted life year, or QALY, allows us to compare the impact of different sorts of health programs.

2. Is this the most effective thing you can do?
The very best health and education programs are hundreds of times better than "merely" very good programs. Smallpox eradication did so much good that it alone shows development aid to be highly cost-effective on average.

3. Is this area neglected?
Natural disasters get far more funding than ongoing causes of death and suffering such as disease; for that reason, disaster relief usually isn't the most effective use of funds. Diseases, like malaria, that affect people in the developing world get far less funding than conditions like cancer; for that reason you have a much bigger impact treating people with malaria than with cancer.

4. What would have happened otherwise?
After going through Scared Straight, juveniles were more likely to commit crimes than they would have been otherwise, so the program did harm overall. In careers like medicine, you're sometimes simply doing good work that would have happened anyway; if you earn to give, however, you make a difference that wouldn't otherwise have occurred.

5. What are the chances of success, and how good would success be? Some activities—such as voting, entering politics, campaigning for systemic change, or mitigating risks of global catastrophe—are effective not because they're likely to make a difference but because their impact is so great if they do make a difference.

Which charity should you donate to?

What does this charity do? How many different types of programs does it run? For each of these programs, what exactly is it that this charity does? If it runs more than one program, why is that?

How cost-effective is each program area? Is the charity focused on one of the most important causes? How cost-effective does the evidence suggest the program to be?

How robust is the evidence behind each program? What is the evidence behind the programs that the charity runs? Are there trials showing that the program is effective? Does the charity rigorously monitor and evaluate the success of its programs?

How well is each program implemented? Do the leaders of the charity have demonstrated success in other areas? Is the charity highly transparent? Does it acknowledge mistakes that it's made in the past? What are the alternative charities you could give to? Are there good reasons for supposing that this charity is better than others?

Does the charity need additional funds? What would additional funding be used to do? Why haven't other donors already funded the charity to the point it can't use extra money?

Which career should you pursue?

How do I personally fit with this job? How satisfied will I be in this job? Am I excited by the job? Do I think I could stick with it for a significant period of time? How good am I, or could I become, at this type of work, compared to other people and compared to other careers I might choose?

What's my impact while I'm working at this job? How many resources can I

influence, whether that's the labor I provide, the people or budget I manage, the money I earn, or a public platform I have access to? How effective are the causes to which I can direct those resources?

How does this job contribute to my impact later on in life? How well does this job build my skills, connections, and credentials? How well does this job keep my options open? How much will I learn in the course of this job about what I might want to do next?

Which cause should you focus on?

Scale. What's the magnitude of this problem? How much does it affect lives in the short run and long run?

Tractability. How easy is it to make progress on this problem, and how easy is it to tell if you're making progress? Do interventions to make progress within this cause exist, and how strong is the evidence behind those interventions? Do you expect to be able to discover new promising interventions within this cause?

Neglectedness. How many resources are already being dedicated to tackling this problem? How well allocated are the resources that are currently being dedicated to the problem? Is there reason to expect that markets or governments can't solve this problem?

Personal fit. Given your skills, resources, knowledge, connections, and passions, how likely are you to make a large difference in this area?

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NOTES

INTRODUCTION

"African [kids] have almost nothing": Private conversation with Trevor Field, September 2014.
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women of the village ended up pushing the merry-go-round themselves: "When children are not available, adults (especially women) have no choice but to operate the PlayPump. While some women in South Africa and Mozambique reported that they did not mind rotating the 'merry-go-round,' in Mozambique they also reported that they got embarrassed where the people watching them did not know the linkage between the 'merry-go-round' and the water pumping (e.g., where the pump is near a public road). All women interviewed in Zambia reported that they did not like operating the pump." UNICEF, *An Evaluation*, 10.
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"I got sent down to big projects that had failed": Private conversation with Rachel Glennerster, May 2014.

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he found no discernible improvement from decreasing class sizes: Abhijit Banerjee and Michael Kremer, "Teacher-Student Ratios and School Performance in Udaipur, India: A Prospective Evaluation" (Washington, DC: Brookings Institution, 2002).

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"We didn't expect deworming": Private conversation with Michael Kremer, November 2014.

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compared to those who had not been dewormed: Sarah Baird, Joan Hamory Hicks, and Edward Miguel, "Worms at Work: Long-run Impacts of Child Health Gains," working paper, 2011.

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an extreme example of a much more general trend: David Anderson of the Coalition for Evidence-Based Policy comments that "1) the vast majority of social programs and services have not yet been rigorously evaluated, and 2) of those that have been rigorously evaluated, most (perhaps 75 percent or more), including those backed by expert opinion and less-rigorous studies, turn out to produce small or no effects, and, in some cases negative effects." "Guest Post: Proven Programs Are the Exception, Not the Rule," *GiveWell Blog*, December 18, 2008, http://blog.givewell.org/2008/12/18/guest-post-proven-programs-are-the-exception-not-the-rule/.

If it weren't for the independent investigations: In fact, the feedback that the still-operating Roundabout Water Solutions gets from the schools they work with is very positive. The headmaster of one school says: "I hereby use the opportunity to heartily thank you for the round-about which you donate[d] to

our school. You have enable[d] us to get access to water for our learners as well as our community. . . . Allow me to also say may the Good Lord bless you for services rendered by you. WATER IS LIFE !!!!!!!!!!" Why is the feedback that they receive so positive, even though the PlayPump itself is of dubious value? A second letter provides a clue: "We are so grateful for the clean running water. We used to drink with beasts in the river. We are children of a struggling school / government having abandoned her work of providing schools with basic services. We the learners of the aforesaid school can be very glad for any other project like painting our buildings and/or our grade R to grade 6 are still learning in mud structures, if we could get another sponsor, even if you could refer or recommend us to other sponsors in this regard." That is, it would be in the interest of schools to provide extensive gratitude to Roundabout Water Solutions even if the PlayPump did no good at all. There's little cost to writing a thank-you letter, and if the recipients of PlayPumps do so, they might later receive other gifts that are more useful.

This just shows the sheer difficulty of ensuring that you're having an impact. Another example comes from Canadian engineer Owen Scott, who wrote: "Each time I've visited a PlayPump, I've always found the same scene: a group of women and children struggling to spin it by hand so they can draw water. I've never found anyone playing on it. But, as soon as the foreigner with a camera comes out (aka me), kids get excited. And when they get excited, they start playing. Within five minutes, the thing looks like a crazy success." ("The Playpump III—'The challenge of good inquiry,'" *Owen in Malawi* (blog), November 3, 2009, http://thoughtsfrommalawi.blogspot.co.uk/2009/11/playpump-iii-challenge-of-taking-photos.html). That is, you'll get positive feedback from installing PlayPumps almost no matter how good or bad it is.

"deworming is probably the least sexy development program there is": Private conversation with Grace Hollister, June 2014.

I helped to develop the idea of effective altruism: Toby and I were both heavily influenced by Peter Singer's arguments for the moral importance of giving to fight poverty, made in "Famine, Affluence, and Morality," *Philosophy and Public Affairs* 1, no. 1 (Spring 1972): 229–43 and *The Life You Can Save: Acting Now to End World Poverty* (New York: Random House, 2009). On the basis of his arguments, we both made commitments to donate everything we earn above £20,000 per year—about £1 million pounds each over our careers, or 50 percent of our lifetime earnings. Because we were putting so much of our own money on the line, the importance of spending that money as effectively as possible seemed imperative. Peter Singer has since become a powerful advocate for effective altruism: see *The Most Good You Can Do: How Effective Altruism Is Changing Ideas About Living Ethically* (New Haven, CT: Yale University, 2015).

(the number of hours you typically work in your life): If you work forty hours per week, fifty weeks a year for forty years that's exactly 80,000 hours. For many careers, the real number of hours worked will be quite a bit more than that.

ONE

The term came from a popular statistic: Dawn Turner Trice, "How the 1 Percent Live, and Give," *Chicago Tribune*, December 29, 2011; Social Security Administration, "Measures of Central Tendency for Wage Data," http://www.ssa.gov/oact/cola/central.html. In an effort to avoid technical vocabulary whenever possible, throughout this book I use "typical" to refer to "median," and "average" to refer to "mean."

while typical household income: Congressional Budget Office, *Trends in the Distribution of Household Income Between 1979 and 2007*, October 2011, http://www.cbo.gov/sites/default/files/10-25-HouseholdIncome_0.pdf.

"probably higher than in any other society": Thomas Piketty, *Capital in the Twenty-First Century* (Cambridge, MA: Harvard University Press, 2014), 265.

Consider this graph of global income distribution: The data on world income distribution is drawn from several sources. The figures for between the richest 1 percent and the richest 21 percent are based on microdata from national household surveys carried out in 2008, kindly provided by Branko Milanovic. The figures for the poorest 73 percent are based on the 2008 data from PovcalNet (http://iresearch.worldbank.org/PovcalNet/index.htm?1), adjusted based on the approximation that the surveys covered unbiased samples of the poorest 80 percent of the world's population. The figure of $70,000 for the top 0.1 percent is from Milanovic's book *The Haves and the Have-Nots: A Brief and Idiosyncratic History of Global Inequality* (New York: Basic Books, 2011). All figures have been adjusted for the Consumer Price Index measure of inflation. To find out how rich you are, you can use the Giving What We Can calculator at: http://www.givingwhatwecan.org/get-involved/how-rich-am-i.

That spike goes off the chart: According to Equilar, the data provider for *The New York Times*, the highest-paid CEO in 2014 was Charif Souki, president and CEO of Cheniere Energy, who took home $141,949,280 ("Equilar 200 Highest-Paid CEO Pay Ranking Released," http://www.equilar.com/nytimes/the-new-york-times-200-highest-paid-ceos). Assuming that the distance on the page between $0 and $100,000 is 2 inches (the next graph has the *y* axis labeled), the spike would be 2,839 inches, or 237 feet. At ten feet per story, that's more than twenty-three stories high. That's only looking at income, however. If we included changes in net wealth on this graph, the spike would be considerably longer again. In 2014, the person with the greatest gain in net wealth was Zhang Changhong, whose wealth increased by $982.5 million ("The World's Billionaires," *Forbes*, http://www.forbes.com/billionaires/list/#tab:overall; "#864 Zhang Changhong," *Forbes*, http://www.forbes.com/profile/zhang-changhong/; true as of December 9, 2014). If we put him on this graph, the spike would be 1,638 feet, a full four hundred feet taller than the Empire State Building.

the typical income for working individuals: Social Security Administration, "Measures of central tendency for wage data."

that's 1.22 billion people who earn less than $1.50 per day: "Poverty Overview," World Bank, http://www.worldbank.org/en/topic/poverty/overview. This figure is true as of 2010. The extreme poverty line is usually expressed as $1.25 per day. However, that's $1.25 per day in 2005 prices. In order to make the figure more easily understandable, I've updated the figure in line with inflation: $1.50 in 2014 prices is approximately the same as $1.25 in 2005 prices.

they live on an amount of money equivalent to what $1.50 could buy in the United States: Martin Ravallion, Shaohua Chen, and Prem Sangraula, "Dollar a Day Revisited," Policy research working paper 4620 (World Bank, May 2008). Extreme poverty is generally understood as referring to earnings below $1.25 per day in 2005 prices, or equivalently, as earnings below a dollar per day in 1996 prices.

According to the way these figures are calculated: "Millennium Development Goal Indicators," United Nations Statistics Division, Department of Economic and Social Affairs, United Nations, http://mdgs.un.org/unsd/mdg/Metadata.aspx?IndicatorId=0&SeriesId=580.

in poor countries in sub-Saharan Africa it is only fifty-six years: "Life Expectancy at Birth, Total (Years)," World Bank, http://data.worldbank.org/indicator/SP.DYN.LE00.IN/countries/LS-ZF-XN?display=graph&hootPostID=cc8d300b9308f8acab94418eff2132ac.

Professors Abhijit Banerjee and Esther Duflo: "The Economic Lives of the Poor," *Journal of Economic Perspectives* 21, no. 1 (Winter 2007): 141–67.

I'll discuss just one method: The reliability of this method is discussed in Alan B. Krueger and David A. Schkade, "The Reliability of Subjective Well-being Measures," *Journal of Public Economics* 92, no. 8–9 (August 2008): 1,833–45.

Estimates via other methods: For an overview of these methods, see Ben Groom and David Maddison, "Non-identical Quadruplets: Four New Estimates of the Elasticity of Marginal Utility for the UK," London School of Economics and Political Science, Grantham Research Institute on Climate Change for the Envornment, working paper no. 121, August 2013.

Their results are given in this graph: Betsey Stevenson and Justin Wolfers, "Subjective Well-Being and Income: Is There Any Evidence of Satiation?" *American Economic Review* 103, no. 3 (May 2013):

598–604. In this graph, each line represents how levels of subjective well-being within a country change with changes in income. For example, a Brazilian earning $3,000 per year will, on average, report a score of 6.5 in life satisfaction, whereas a Brazilian earning $8,000 will on average report a score of 7. Note that the same income level is associated to different levels of reported life satisfaction in different countries. Nonetheless, it remains true for all countries that, as incomes rise, so does life satisfaction. The graph is plotted on a logarithmic scale: each increment on the horizontal axis represents a doubling of income. The graph thus shows that it takes increasingly more income to attain an increase in subjective well-being.

Subjective well-being is measured by asking subjects to rate how satisfied they are with their lives as a whole. Although this is one accepted measure of happiness, it is not the only one. An alternative way of measuring how happy people are, known as the experience sampling method, involves asking subjects to rate how well they are feeling at the present moment. See Reed Larson and Mihaly Csikszentmihalyi, "The Experience Sampling Method," *New Directions for Methodology of Social and Behavioral Science* 15 (March 1983): 41–56. An advantage of the experience sampling method over reports of subjective well-being is that it does not ask subjects to recollect and aggregate past experience—tasks which humans are not very good at. See Barbara L. Fredrickson and Daniel Kahneman, "Duration Neglect in Retrospective Evaluations of Affective Episodes," *Journal of Personality and Social Psychology* 65, no. 1 (July 1993): 45–55. Using this method, it's been found that with more than a household income of $75,000—which, given the average US household size of 2.5 people, equals a personal income of $30,000—additional income makes people no happier. This method would therefore considerably strengthen my conclusion: for many people, giving up additional money is of no loss, in terms of happiness, at all.

the 100x Multiplier: One point to clarify—the 100x Multiplier refers to the discrepancy in the benefit of one dollar to you as a member of an affluent country versus the benefit of one dollar to someone in extreme poverty. That doesn't yet entail, however, that we should prefer to give one dollar to someone in extreme poverty rather than ninety-nine dollars to the typical US citizen. This is because increases in income have effects on the wider economy. If I give ninety-nine dollars to Jo Bloggs in the United States, doing so benefits her, but it also benefits others, such as the people who Jo Bloggs buys goods from using that money (and these others may live in other, much poorer, countries). The same is true of the dollar given to the person in extreme poverty. But, importantly, nothing I've said so far shows that the ratio of the benefits from those wider effects is also 100:1. That's why we can't yet conclude that it does a hundred times as much good in general to give one dollar to the extreme poor than to give one dollar to someone in an affluent country. We can just conclude that, when thinking about me versus the person in extreme poverty (considered in isolation from the rest of the world), giving the extremely poor person one dollar provides one hundred times as large a benefit as giving me one dollar.

For those of us living in rich countries: Note that the figure of one hundred is a baseline. I believe that if we try hard, we should be able to do even more good for even less personal cost. This is for two reasons. First, we've only looked at one problem: global poverty. As discussed in chapter ten, there may be even better opportunities for helping others, in which case the 100x Multiplier is an underestimate. Second, while I just described giving in terms of choosing between a benefit to yourself or others, that's not a good way of thinking about it because giving benefits the giver as well as the receiver. If anything, my life has become happier since I've started donating some of my income. That's the upside of the "warm glow" effect. Indeed, academic studies suggest I'm not alone. In one case, experimental subjects ended up more satisfied when they were given money and told to use that money to benefit others, than when they were told to use that money to benefit themselves. (See Elizabeth Dunn, Lara Aknin, and Michael Norton, "Spending Money on Others Promotes Happiness," *Science* 319, no. 5,870 [March 21, 2008]: 1,687–8.) So in fact we should expect that, if we focus on the most effective activities, the benefits to others will be greater and the costs to ourselves will be less than the 100x Multiplier would suggest. (For an overview, see Andreas Mogensen, "Giving without Sacrifice?

The Relationship between Income, Happiness, and Giving," unpublished paper, http://www.givingwhatwecan.org/sites/givingwhatwecan.org/files/attachments/giving-without-sacrifice.pdf.)

In a mere two hundred years: Louis Johnston and Samuel H. Williamson, "What Was the U.S. GDP Then?" MeasuringWorth, 2014, http://www.measuringworth.com/usgdp/, accessed July 2014.

this graph of gross domestic product per person: Angus Maddison, "Statistics on world population, GDP and per capita GDP, 1–2008 AD," University of Groningen, http://www.ggdc.net/maddison/content.shtml.

For almost all of human history: See also Maddison's *Contours of the World Economy 1–2030 AD* (Oxford, UK: Oxford University, 2007).

more than half of the world: Using the data from the Global Income Distribution graph, given on page 16.

transform the lives of thousands of people: For further discussion of global inequality and economic progress over time, see Milanovic, *The Haves and the Have-Nots*, and Gregory Clark, *A Farewell to Alms: A Brief Economic History of the World* (Princeton, NJ: Princeton University, 2007).

TWO

The problems in Rwanda began to build up decades before: For a summary of the main events of the Rwandan genocide, see Jonathan Glover, *Humanity: A Moral History of the Twentieth Century* (New Haven, CT: Yale University, 1995), 119–22. A comprehensive discussion may be found in Gérard Prunier, *The Rwanda Crisis: History of a Genocide* (New York: Columbia University, 1995).

"There were so many, and they kept coming": James Orbinski, *An Imperfect Offering: Humanitarian Action for the Twenty-first Century* (New York: Walker, 2008), 226.

How many people benefit . . .: Is this just utilitarianism? No. Utilitarianism is the view, roughly speaking, that one is always required to do whatever will maximize the sum total of well-being, no matter what. The similarity between effective altruism and utilitarianism is that they both focus on improving people's lives, but this is a part of any reasonable moral view. In other respects, effective altruism can depart significantly from utilitarianism. Effective altruism doesn't claim that you are morally required to do as much good as you can, only that you should use at least a significant proportion of your time or money to help others. Effective altruism doesn't say that you may violate people's rights for the greater good. Effective altruism can recognize sources of value other than happiness, like freedom and equality. In general, effective altruism is a much broader and more ecumenical philosophy than utilitarianism.

the "fifty dollars for five books" figure is accurate: Charities' claims about what your donation will buy are often highly misleading, representing a best-case figure, or a figure that doesn't take into account "hidden" costs. I talk about this more in chapter 7. Moreover, even if it does cost fifty dollars to buy and provide five books, it's not really true that if you donate fifty dollars you will cause five additional books to be bought. United Way of New York City spends its money on a wide variety of different programs, spending $55 million in total in 2013. Your fifty dollars will be added to its total income and, in effect, will be distributed across all its program areas. For the purpose of this discussion, however, I assume that an additional fifty dollars donated to United Way of New York City will provide an extra five books.

provide a larger benefit by saving five lives: Some philosophers have argued that, in cases of this sort, we should decide by tossing a coin, or by using a lottery that gives more weight to the greater number. See, for example, John M. Taurek, "Should the Numbers Count?" *Philosophy and Public Affairs* 6, no. 4 (Summer 1977), 293–316, and F. M. Kamm, *Morality, Mortality, Volume I: Death and Whom to Save from It* (Oxford: Oxford University Press, 1993). I am not persuaded by these arguments. If we think that all people matter equally, we should save more people rather than fewer. As the Oxford philosopher Derek Parfit writes: "Why do we save the larger number? Because we *do* give equal

weight to saving each. Each counts for one. That is why more count for more." ("Innumerate Ethics," *Philosophy and Public Affairs* 7, no. 4 [Summer 1978], 301.)

For health benefits, economists: In this discussion I've left out the most common metric used by economists to measure harms and benefits, which is called *willingness to pay*. According to this metric, the size of a benefit of something to a person is measured by how much that person is willing to pay for it. If Jones is willing to pay a dollar for an apple, but Smith is willing to pay ten dollars for an apple, then, if we used this metric, we would conclude that giving Smith an apple provides ten times as great a benefit as providing Jones with an apple. The reason I don't rely on this metric is that it treats an additional dollar as being of equal worth no matter who has it. But this is clearly wrong. If Smith is a multimillionaire, whereas Jones is poor, then one dollar will be much less valuable to Smith than a dollar is to Jones. This problem becomes particularly severe if we try to compare activities that benefit people in rich countries with activities that benefit people in poor countries. For example, Americans are on average willing to pay fifteen times as much as Bangladeshis to reduce their risk of dying by 1 percent (John Broome, *Weighing Lives*, [Oxford, UK: Oxford University, 2004], 263). If we used the willingness-to-pay metric, we'd have to conclude that American lives are worth fifteen times as much as Bangladeshi lives. But that's clearly wrong. The real reason Americans are willing to pay much more to avoid risks of death than Bangladeshis is simply that they have much more money that they can spend.

people on average rate a life with untreated AIDS: "Global Burden of Disease 2004 Update: Disability Weights for Diseases and Conditions," World Health Organization, http://www.who.int/healthinfo/global_burden_disease/GBD2004_DisabilityWeights.pdf.

official lists of quality-of-life estimates: Estimates are provided in Joshua A. Salomon et al., "Common Values in Assessing Health Outcomes from Disease and Injury: Disability Weights Measurement Study for the Global Burden of Disease Study 2010," *Lancet* 380 (2012) table 2, 2,135–7; available at http://www.jefftk.com/gbdweights2010.pdf; the 2004 estimates (created using a slightly different methodology) are available at http://www.who.int/healthinfo/global_burden_disease/GBD2004_DisabilityWeights.pdf. In both articles, the authors talk about disability-adjusted life years, or "DALYs": very approximately, one DALY is just the negative of one QALY. (So it's good to gain QALYs and bad to gain DALYs.) QALYs tend to be used in domestic settings, whereas DALYs are used in global health. Note that the disability weight estimates should be taken as exactly that: estimates, which may be too high or too low in a given context. For that reason, we shouldn't slavishly follow explicit estimates of cost per QALY that rely on these numbers; instead we should think of them as one important tool in our attempt to do the most good.

quality of life with AIDS: In this example I use the disability weights from the 2004 WHO Global Burden of Disease, available at http://www.who.int/healthinfo/global_burden_disease/GBD2004_DisabilityWeights.pdf.

academics continue to debate: An overview of the QALY, and issues in defining it and measuring how bad different conditions are, is given in Milton C. Weinstein, George Torrance, and Alistair McGuire, "QALYs: The Basics," *Value in Health* 12, supplement 1 (2009): S5–S9. Note that these problems should not cause us to reject entirely the attempt to think in terms of quantitative harms and benefits. David Spiegelhalter, Winton Professor of the Public Understanding of Risk at the University of Cambridge, puts the point nicely: "Of course the QALY approach is not perfect, but some mechanism is needed to provide consistent comparisons across different medical interventions, based on aggregate benefit and cost. Otherwise the money could go to those with the most appealing emotional argument." ("Experts Dismiss Claims NHS Drug Decisions Are 'Flawed,'" National Institute for Health and Care Excellence, January 25, 2013, https://www.nice.org.uk/news/article/experts-dismiss-claims-nhs-drug-decisions-are-flawed.)

We could use these methods: This has been suggested by both philosophers and economists: see, for example, Broome, *Weighing Lives*, 261.

$50,000 to train and provide: See "FAQ," Guide Dogs of America, http://www.guidedogsofamerica.org/1/mission/#cost.

a critical piece on effective altruism: Ken Berger and Robert M. Penna, "The Elitist Philanthropy of So-Called Effective Altruism," *Stanford Social Innovation Review (blog)*, November 25, 2013, available at http://www.ssireview.org/blog/entry/the_elitist_philanthropy_of_so_called_effective_altruism.

They objected that: They clarified this as their position in personal communication, November 2013.

I came across the Fistula Foundation: More information about the Fistula Foundation and about obstetric fistula can be found at www.fistulafoundation.org.

THREE

"aid is malignant": Dambisa Moyo, *Dead Aid: Why Aid Is Not Working and How There Is a Better Way for Africa* (New York: Farrar, Straus and Giroux, 2009), 47.

"So there we have it": Ibid.

wrote a book: William Easterly, *The White Man's Burden*: *Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good* (New York: Penguin, 2006).

"The other tragedy": Easterly, *The White Man's Burden*, 4.

The total annual economic output of the world is $87 trillion: World GDP for 2013 was $87.25 trillion in terms of purchasing power parity, and $74.31 trillion in nominal terms. Central Intelligence Agency, *The World Factbook, 2014*, https://www.cia.gov/library/publications/the-world-factbook/geos/xx.html.

the United States spends about $800 billion on social security: Congressional Budget Office, "Monthly Budget Review—Summary for Fiscal Year 2013," November 7, 2013, https://www.cbo.gov/sites/default/files/44716-%20MBR_FY2013_0.pdf.

a decade of cosmetics sales amounts to $1.7 trillion: Perry Romanowsky, "A Cosmetic Industry Overview for Cosmetic Chemists," *Cosmetics Corner*, April 14, 2014, http://chemistscorner.com/a-cosmetic-market-overview-for-cosmetic-chemists/.

lost track *of $2.3 trillion*: Aleen Sirgany, "The War On Waste," *CBS Evening News*, January 29, 2002.

the average population of sub-Saharan Africa during that time period: The total population of sub-Saharan African was 177 million in 1950 and 815 million in 2010. (Dominique Tabutin and Bruno Schoumaker, "The Demography of Sub-Saharan Africa from the 1950s to the 2000s: A Survey of Changes and a Statistical Assessment," *Population* 59, no. 3–4 [2004], 525.) Assuming a constant growth rate of 2.58 percent, we sum the estimated population for every year in this period and take the mean.

Now it's 56 years: Tabutin and Schoumaker, "Sub-Saharan Africa," 538; "Sub-Saharan Africa," World Bank, http://data.worldbank.org/region/sub-saharan-africa.

the eradication of smallpox: For details, see David Koplow, *Smallpox: The Fight to Eradicate a Global Scourge* (Berkeley: University of California, 2003), and D. A. Henderson, *Smallpox: The Death of a Disease—The Inside Story of Eradicating a Worldwide Killer* (New York: Prometheus, 2009).

smallpox killed 1.5 to 3 million people every year: Koplow, *Smallpox*, 1; Henderson, *Smallpox*, 13.

The eradication of smallpox is one success story from aid: I owe this argument for the cost-effectiveness of aid on average to Toby Ord. Critics may question whether the smallpox campaign was a form of overseas development aid (ODA) understood as the flow of funds from donor to recipient nations. But note that smallpox is usually included as a success story in studies about the effectiveness of aid, even those written by aid skeptics. See, e.g., Roger C. Riddell, *Does Foreign Aid Really Work?* (Oxford: Oxford University, 2007), 184. More to the point, the WHO Smallpox Eradication Unit was partially funded by official aid funds: international donors supplied one-third of the funding for the program. And it appears that these contributions played a key role in the success of the program, which would probably not have succeeded without international funding (Ruth Levine, *Case Studies in Global Health: Millions Saved* [Sudbury, MA: Jones and Bartlett, 2007], 1–8). But even if we credit foreign

aid with only one-third of the benefits that resulted from the smallpox eradication program, that would still leave us with at least a life saved per $225,000 spent in aid.

The total aid spending of all countries: Easterly, *The White Man's Burden*, 4.

if doing so costs less than about $7 million per life saved: Binyamin Appelbaum, "As U.S. Agencies Put More Value on a Life, Businesses Fret," *The New York Times*, February 17, 2011.

it still would have prevented a death: The die-hard aid critic might suggest that non–health aid hasn't just been *ineffective*, but that it's been downright *harmful*. However, if so, we need to ask just how harmful it's been. Even just given the example of smallpox, in order for foreign aid to have been harmful overall, the costs must have exceeded 122 million lives lost—which, as we noted, is greater than that of all war. No respectable development economist would suggest that foreign aid has been this harmful. For example, Professor Adrian Wood at the University of Oxford's Department for International Development comments that: "The one thing we can be clear about is the evidence that almost certainly aid does not reduce growth." (Select Committee of Economic Affairs, House of Lords, *The Economic Impact and Effectiveness of Development Aid: 6th report of session 2010–12* (London: Stationery Office, 2012) 23, n. 45.

Moreover, studies published since Moyo's and Easterly's books indicate a positive relationship between aid and economic growth. (See Arndt Channing, Sam Jones, and Finn Tarp, "Aid, Growth, and Development: Have We Come Full Circle?" *Journal of Globalization and Development*, vol. 1, no. 2 [2010], and Camelia Minoiu and Sanjay G. Reddy, "Development Aid and Economic Growth: A Positive Long-Run Relation," *Quarterly Review of Economics and Finance*, vol. 50, no. 2 [2010].) This is so even, though, given the small size of aid that flows to poor countries, one would not expect a detectable impact on economic growth. As Owen Barder of the Center for Global Development comments: "Given the modest volumes of aid, we should not expect an impact on growth [that] is bright enough to shine through the statistical fog." (Select Committee of Economic Affairs, *The Economic Impact and Effectiveness of Development Aid*, 23, n. 42.)

"there are well-known and striking donor success stories": William Easterly, "Can the West Save Africa?" *Journal of Economic Literature* 47, no. 2 (June 2009): 406–7.

"even those of us labeled as 'aid critics'": William Easterly, "Some Cite Good News on Aid," *Aid Watch*, February 18, 2009, http://aidwatchers.com/2009/02/some-cite-good-news-on-aid/.

Look at the following graph: This graph uses the same data as for the one in chapter one.

most people live in a small number of cities: For this and the other examples mentioned, see Mark E. J. Newman, "Power Laws, Pareto Distributions and Zipf's Law," *Contemporary Physics* 46, no. 5 (2005), 323–51.

The effectiveness of different aid activities forms a fat-tailed distribution: Ramanan Laxminarayan, Jeffrey Chow, and Sonbol A. Shahid-Salles, "Intervention Cost-Effectiveness: Overview of Main Messages," in Dean Jamison et al. (eds.), *Disease Control Priorities in Developing Countries*, 2nd edition (Oxford: Oxford University, 2006), 41–42.

the estimated cost-effectiveness of different health programs: Laxminarayan, Chow, and Shahid-Salles, "Intervention Cost-Effectiveness," 62 (for Kaposi's sarcoma); Omar Galárraga et al., "HIV Prevention Cost-Effectiveness: A Systematic Review" (for condom promotion and antiretroviral therapy); "Against Malaria Foundation (AMF)," GiveWell, November 2014, http://www.givewell.org/files/DWDA%202009/Interventions/Nets/GiveWell%20cost-effectiveness%20analysis%20of%20LLIN%20distribution%202014.xls (for distribution of bed nets). To be conservative, I've rounded down GiveWell's estimate of QALYs per $1,000 provided by bed-net distribution through donations to the Against Malaria Foundation (they give six estimates of the cost per QALY, the harmonic mean of which is $68.90). Their estimate is also only an estimate of the QALYs per dollar as a result of prevention of death of children under the age of five. It doesn't take into account prevention of death of children over the age of five, or prevention of illness. GiveWell are keen to emphasize, however, that these are only estimates. The real number may be higher or lower than their model suggests.

costing less than the governments of the United States or the United Kingdom are willing to spend to provide one QALY: Laxminarayan, Chow, and Shahid-Salles, "Intervention Cost-Effectiveness," 62.
the cost to save a life in the developing world: "Against Malaria Foundation," GiveWell.

FOUR

"I want to study medicine": Private conversation with Greg Lewis, April 2014.
According to one study: Oliver Robinson, "Planning for a Fairer Future," *The Guardian*, July 14, 2006.
"jobs that make a difference": "5 More Do-Good Jobs You've Never Considered," *Oprah*, April 19, 2012, http://www.oprah.com/money/Jobs-That-Make-a-Difference-in-the-World.
Which is more valuable: water or diamonds?: The apparent difficulty in answering this question is known as the paradox of value. It was discussed by Nicolaus Copernicus and John Locke, among others, but the most influential presentation appears in a passage of Adam Smith's *The Wealth of Nations*:

> The things which have the greatest value in use have frequently little or no value in exchange; and, on the contrary, those which have the greatest value in exchange have frequently little or no value in use. Nothing is more useful than water: but it will purchase scarce anything; scarce anything can be had in exchange for it. A diamond, on the contrary, has scarce any value in use; but a very great quantity of other goods may frequently be had in exchange for it." (book I, chapter 4)

why the cost of a gallon of water from the tap in New York City: "Residential Water Use," New York City Department of Environmental Protection, http://www.nyc.gov/html/dep/html/residents/wateruse.shtml.
they are therefore scarce in the way that water isn't: Diamonds aren't, in fact, as rare as you might think. They are so expensive because for most of the twentieth century the supply of them has been artificially restricted by the De Beers monopoly, in order to keep prices high. For more information, see Eric Goldschein, "The Incredible Story of How De Beers Created and Lost the Most Powerful Monopoly Ever," *Business Insider*, December 19, 2011, http://www.businessinsider.com/history-of-de-beers-2011-12?op=1&IR=T.
the Tohoku region of Japan was hit: Richard Hindmarsh, ed., *Nuclear Disaster at Fukushima Daiichi: Social, Political and Environmental Issues* (New York: Routledge, 2013); Kevin Voigt, "Quake Moved Japan Coast 8 Feet, Shifted Earth's Axis," CNN, April 20, 2011.
an earthquake hit Haiti: Clarens Renois, "Haitians Angry Over Slow Aid," *The Age* (Australia), February 5, 2010; "Haiti Quake Death Toll Rises to 230,000," BBC News, February 11, 2010.
the total international aid: Report of the United Nations in Haiti 2011, Chapter 7, www.onu-haiti.org/Report2011/Chapter7.html; "Disaster donations top ¥520 billion," *Japan Times*, March 8, 2012, p. 1. Haiti has continued to receive aid support; now the total pledged is $13.3 billion (Office of the Special Envoy to Haiti, "International assistance to Haiti key facts as of December 2012").
"The Japanese Red Cross Society": "Japan and Pacific: Earthquake and Tsunami," International Federation of Red Cross and Red Crescent Societies, information bulletin no. 2, March 12, 2011, p. 1.
it only raised $500 million in international aid: The various sources of international aid are listed at "Reactions to the 2008 Sichuan earthquake," *Wikipedia*, https://en.wikipedia.org/wiki/Reactions_to_the_2008_Sichuan_earthquake.
For every death the Japanese earthquake caused: As mentioned in the text, $5 billion was raised for the Japanese earthquake, which caused fifteen thousand deaths. $5 billion ÷ 15,000 ≈ $330,000.
for every person who dies from poverty-related causes worldwide: According to the Organisation for Economic Co-operation and Development, globally, $135 billion was spent on foreign aid in 2013 (Claire Provost, "Foreign Aid Reaches Record High," *The Guardian*, April 8, 2014). US private philanthropy is $37.3 billion (Carol Adelman, Jeremiah Norris, and Kacie Marano, *The Index of Global Philanthropy and Remittances: 2010* [Washington, DC: Hudson Institute, 2010], 12). Non-US private philanthropy is $15.3 billion (Ibid., 41). This brings total overseas aid and philanthropy to approximately $188 billion per year. A rough conservative estimate of total poverty-related deaths can

be determined by adding up from leading poverty-related causes of death, giving 12.7 million poverty-related deaths (giving $15,000 of overseas aid and philanthropy per poverty-related death). The causes of death I used in this calculation (with corresponding number of deaths in parentheses) are: malnutrition (3.1 million); lower respiratory infections (3.1 million); tuberculosis (1.5 million); HIV/AIDS (1.5 million); diarrheal diseases (1.5 million); preterm birth complications (1.1 million); malaria (600,000); maternal conditions (280,000). See World Health Organization, fact sheets nos. 94, 104, 310, 330, 348 360 (2014), and "Hunger Statistics," World Food Programme, 2014, http://www.wfp.org/hunger/stats.

"emergency health interventions": Claude de Ville de Goyet, Ricardo Zapata Marti, and Claudio Osorio, "Natural Disaster Mitigation and Relief," in Jamison, *Disease Control Priorities*, 1,153.

it costs about $50,000: See http://www.guidedogsofamerica.org/1/mission/#cost.

Not only is $50,000 enough: Matthew J. Burton and David C. W. Mabey, "The Global Burden of Trachoma: A Review," *PLoS Neglected Tropical Diseases* 3, no. 10 (October 27, 2009), e460. The authors give estimates of cost per surgery ranging from $6.13 to $41; to be conservative, I've rounded this up to $100. Even still, we should bear in mind that both of the cost for a guide dog and the cost for a trachoma surgery are only estimates, and that the estimates for cost-effectiveness of a given program in published articles or on charity's websites may be more optimistic than the real cost-effectiveness of programs when actually implemented. Correcting for this won't, however, change the central point.

Per year, $217 billion is spent on cancer treatment: "Breakaway: The Global Burden of Cancer—Challenges and Opportunities," *The Economist*, 2009, 25, http://graphics.eiu.com/upload/eb/EIU_LIVESTRONG_Global_Cancer_Burden.pdf. This figure does not include costs due to loss of productivity, which amount to $69 billion per year.

Malaria is responsible for 3.3 percent of QALYs lost worldwide: These figures come from the Global Burden of Disease website, http://vizhub.healthdata.org/gbd-compare/. The Global Burden of Disease measures the health cost of different illnesses in "DALYs": very approximately, these are the same as QALYs, but where one QALY = one negative DALY. So it's a good thing to gain QALYS; it's a bad thing to gain DALYs.

(It was eliminated from the United States in 1951): Margaret Humphreys, *Malaria: Poverty, Race, and Public Health in the United States* (Baltimore: Johns Hopkins University, 2001), 140–54.

any program that provides one QALY for less than $50,000: Milton C. Weinstein, "How Much Are Americans Willing to Pay for a Quality-Adjusted Life Year?" *Medical Care* 46, no. 4 (April 2008), 343–5; Scott D. Grosse, "Assessing Cost-Effectiveness in Healthcare: History of the $50,000 per QALY Threshold," *Expert Review of Pharmacoeconomics & Outcomes Research* 8, no. 2 (April 2008), 165–78; Chris P. Lee, Glenn M. Chertow, and Stefanos A. Zenios, "An Empiric Estimate of the Value of Life: Updating the Renal Dialysis Cost Effectiveness Standard," *Value in Health* 12, no. 1 (January/February 2009), 80–87.

providing the same benefit in poor countries: "Combination Deworming (Mass Drug Administration Targeting Both Schistosomiasis and Soil-transmitted Helminths)," GiveWell, December 2014.

This is what Greg did initially: Gregory Lewis, "How Much Good Does a Doctor Do?" unpublished paper.

878,194 doctors in the United States: Aaron Young, Humayun J. Chaudhry, Jon V. Thomas, and Michael Dugan, "A Census of Actively Licensed Physicians in the United States, 2012," *Journal of Medical Regulation* 99, no. 2 (2013), 11–24.

work by an epidemiologist named John Bunker: "The Role of Medical Care in Contributing to Health Improvements within Societies," *International Journal of Epidemiology* 30, no. 6 (December 2001), 1,260–3.

FIVE

"The 75 Best People in the World": "The 75 Best People in the World," *Esquire*, January 2012,

http://www.esquire.com/features/best-people-1009#slide-1.

Ohio-born doctor D. A. Henderson: For background, see D. A. Henderson and Petra Klepac, "Lessons from the Eradication of Smallpox: An Interview with D. A. Henderson," *Philosophical Transactions of the Royal Society B* 368, no. 1,623 (August 5, 2013), 1–7.

Viktor Zhdanov, a Ukrainian virologist: For biographical details about Zdhanov, see Alice Bukrinskaya, "In Memory of Victor Zhdanov," *Archives of Virology* 121, no. 1–4 (1991), 237–40.

"I avail myself of this occasion": Cited in Frank Fenner, "Development of the Global Smallpox Eradication Program," in *Smallpox and Its Eradication* (Geneva: World Health Organization, 1988), 366–418.

the paramedic would have been able to do the same thing: Unlike in TV shows, in the real world, paramedics are almost never able to restart someone's heart using defibrillation. However, for the purposes of the thought experiment, let's assume that the defibrillation was certain to work.

an episode of Beyond Scared Straight: "Oakland County, Michigan," *Beyond Scared Straight*, Season 2, Episode 2, A&E, 2011.

Nine high-quality studies: Anthony Petrosino, Carolyn Turpin-Petrosino, Meghan E. Hollis-Peel, and Julia G. Lavenberg, "Scared Straight and Other Juvenile Awareness Programs for Preventing Juvenile Delinquency: A Systematic Review," *Campbell Systematic Reviews* 9, no. 5 (2013).

"The analyses show": Ibid., 7.

In a separate study: Steve Aos, Roxanne Lieb, Jim Mayfield, Marna Miller, and Annie Pennucci, "Benefits and Costs of Prevention and Early Intervention Programs for Youth," Washington State Institute for Public Policy, September 17, 2004, http://www.wsipp.wa.gov/ReportFile/881/Wsipp_Benefits-and-Costs-of-Prevention-and-Early-Intervention-Programs-for-Youth_Summary-Report.pdf.

(only one-third of kids): Petrosino, Turpin-Petrosino et al., "Scared Straight," 31.

I suspect the apparent effectiveness of Scared Straight: Another part of the explanation may be that juveniles are simply less likely to offend as they get older; this effect is well documented. See Thomas P. Locke, Glenn M. Johnson, Kathryn Kirigin-Ramp, Jay D. Atwater, and Meg Gerrard, "An Evaluation of a Juvenile Education Program in a State Penitentiary," *Evaluation Review* 10, no. 3 (June 1986), 282.

one hypothesis is that the inmates: Thomas Dishion, Joan McCord, François Poulin, "When Interventions Harm. Peer Groups and Problem Behavior," *American Psychologist* 54, no. 9 (September 1999), 755–64.

The average salary of a doctor in the UK: "Annual Survey of Hours and Earnings, 2013 Provisional Results," Office of National Statistics, http://www.ons.gov.uk/ons/publications/re-reference-tables.html?edition=tcm%3A77328216. Average salaries are considerable higher in the United States, at about $250,000 per year. See "Occupational Outlook Handbook: Physicians and Surgeons," Bureau of Labor Statistics, http://www.bls.gov/ooh/healthcare/physicians-and-surgeons.htm#tab-5.

By pursuing a particularly lucrative specialty: For discussion, see Ryan Carey, "Increasing Your Earnings as a Doctor," *80,000 Hours*, July 17, 2014, https://80000hours.org/2014/06/increasing-your-earnings-as-a-doctor/#fn:3.

(the average is about 2 percent): "Charitable Giving in America: Some Facts and Figures," National Center for Charitable Statistics, http://nccs.urban.org/nccs/statistics/Charitable-Giving-in-America-Some-Facts-and-Figures.cfm.

SIX

"The possibility of a severe accident occurring": Phred Dvorak and Peter Landers, "Japanese Plant Had Barebones Risk Plan," *Wall Street Journal*, March 31, 2011.

"the root cause of the Fukushima crisis": Mari Yamaguchi, "Gov't Panel: Nuke Plant Operator Still Stumbling," Associated Press, July 23, 2012.

one ecstasy session: Advisory Council on the Misuse of Drugs, *MDMA ("Ecstasy"): A Review of Its Harms*

and Classification Under the Misuse of Drugs Act 1971 (London: Home Office, 2009).

going scuba diving: Richard D. Vann and Michael A. Lang (eds.), *Recreational Diving Fatalities Workshops Proceedings, April 8–10, 2010* (Durham: Divers Alert Network, 2011), http://www.diversalertnetwork.org/files/Fatalities_Proceedings.pdf.

going skydiving: "Skydiving safety," United States Parachute Association, http://www.uspa.org/AboutSkydiving/SkydivingSafety/tabid/526/Default.aspx.

Flying in a space shuttle: Out of 833 crew members on space shuttle flights (with some flying multiple times), fourteen people have died (Tariq Malik, "NASA's Space Shuttle by the Numbers: 30 Years of a Spaceflight Icon," Space.com, July 21, 2011, http://www.space.com/12376-nasa-space-shuttle-program-facts-statistics.html).

climb Mount Everest beyond base camp: Firth, PG, et al., "Mortality on Mount Everest, 1921–2006: descriptive study," BMJ, 337 (2008), 1,430.

The same concept: Richard Wilson, "Analyzing the Daily Risks of Life," *Technology Review* 81, no. 4 (February 1979), 45.

Whereas an hour on a train: Many of these facts also appear in the excellent book *The Norm Chronicles: Stories and Numbers About Danger and Death* by Michael Blastland and David Spiegelhalter (New York: Basic Books, 2014).

Psychologists have found: See George F. Loewenstein, Eike U. Weber, Christopher K. Hsee, and Ned Welch, "Risk as Feelings," *Psychological Bulletin* 127, no. 2 (March 2001), 267–86.

"Nobody in their right mind": "Your FREAK-quently asked questions, answered," *Freakonomics* (blog), January 20, 2011, http://freakonomics.com/2011/01/20/freakonomics-radio-your-freak-quently-asked-questions-answered/.

the odds of an individual vote swaying the outcome of the 2008 presidential election: Andrew Gelman, Nate Silver, and Aaron Edlin, "What Is the Probability Your Vote Will Make a Difference?" *Economic Inquiry* 50, no. 2 (April 2012), 321–6. The authors reached this estimate by using election forecasts to calculate the probability that (1) a given state is necessary for an electoral college to win, and that (2) the election for that state is exactly tied—the two quantities that jointly determine whether a single vote is decisive in a US presidential election.

Although this $1,000-per-citizen figure is hypothetical: This is also the hypothetical figure used in Aaron S. Edlin, Andrew Gelman, and Noah Kaplan, "Vote for Charity's Sake," *Economists' Voice* 5, no. 6 (October 2008).

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influence by half: Our belief that these assumptions are conservative is based on surveys of the British politics literature and discussions with Westminster sources. Qualitative discussions of the roles of different aspects of the political system (such as Parliament, civil service, and international bodies) in determining policy, on which these estimates are partially based, can be found in: Dennis Kavanagh, et

al., *British Politics: Continuities and Change*, 5th edition, (Oxford: Oxford University, 2006); Martin Smith, *The Core Executive in Britain* (London: Palgrave MacMillan, 1999); Gillian Peele, *Governing the UK: British Politics in the 21st century*, 4th edition (Cambridge, MA: Wiley-Blackwell, 2004).

in part on the basis: As discussed in chapter nine, considerations like "personal fit" are also very important when considering choice of a career.

"it is extremely likely that human influence": IPCC, "Summary for policymakers," in *Climate Change 2014: Impacts, Adaptation, and Vulnerability* (Cambridge, UK: Cambridge University, 2014), 3.

"97.1 percent endorsed the consensus position that humans are causing global warming": John Cook et al., "Quantifying the Consensus on Anthropogenic Global Warming in the Scientific Literature," *Environmental Research Letters* 8, no. 2 (April–June 2013). Even people whom the media label as "climate skeptics" agree. "It is quite odd that so much of the argument on global warming had been on whether or not a human influence can be seen," wrote Bjørn Lomborg, perhaps the most famous "climate skeptic," in the chapter on global warming of his controversial book *The Skeptical Environmentalist*: "Even with lots of countervailing (negative feedback) climate effects, it would seem unlikely that there would not be some form of warming coming from increased CO2." (*The Skeptical Environmentalist: Measuring the Real State of the World* [Cambridge, UK: Cambridge University, 2001], 265–6.) Later, he made his position even clearer, stating that climate change is "undoubtedly one of the chief concerns facing the world today." (Quoted in Matthew Moore, "Climate 'Skeptic' Bjørn Lomborg Now Believes Global Warming Is One of World's Greatest Threats," *Telegraph*, August 31, 2010.) What I'm saying here in this secant is not that the jury is out on climate change, but that *even if it were, we should still be taking action.*

millions of lives will be lost: The World Health Organization estimates that without significant action to reduce carbon emissions, climate change will be causing approximately 250,000 additional deaths due to climate change *per year* between 2030 and 2050. These deaths are primarily caused through climate change's impacts on heat exposure in elderly people, diarrhea, malaria, and childhood undernutrition. See Simon Hales, Sari Kovats, Simon Lloyd, and Diarmid Campbell-Lendrum, eds., *Quantitative Risk Assessment of the Effects of Climate Change on Selected Causes of Death, 2030s and 2050s*, World Health Organization, 2014, http://apps.who.int/iris/bitstream/10665/134014/1/9789241507691_eng.pdf?ua=1.

the world economy will lose trillions of dollars: Estimates of the economic costs of climate change range from a benefit to the global economy of a couple percent of GDP, to a decrease of 20 percent of global GDP or more. Most estimates suggest climate change will damage the global economy by a few percent of global GDP. (Richard S. J. Tol and Gary W. Yohe, "A Review of the Stern Review," *World Economics* 7, no. 4 [December 2006].) As global GDP is currently approximately $80 trillion and growing (CIA World Factbook, 2014, https://www.cia.gov/library/publications/the-world-factbook/geos/xx.html), the damage from climate change to the global economy is expected to be trillions of dollars.

the costs are much lower: Nicolas Stern et al., *The Economics of Climate Change: The Stern Review* (Cambridge, UK: Cambridge University, 2007).

low-carbon technology: Some of the money invested in low-carbon technology would have been beneficial, such as the investments into solar photovoltaics, which are a useful technology for producing electricity in remote regions, and in the future potentially for generating electricity at low cost. Other investments, such as investments into carbon capture and storage technologies to bury carbon dioxide emissions under the ground, would probably have been less beneficial in the absence of climate change (though these investments would probably still have had some positive impact in improving our ability to do activities such as enhanced oil recovery).

slowed economic progress a bit: The Stern Review of the Economics of Climate Change estimates that mitigating climate change would cost approximately 1 percent of GDP every year, which is money that we would otherwise be spending on other things, which currently would be approximately one trillion dollars per year. See Stern et al., *Economics of Climate Change*.

half a billionth of a degree Celsius: Current estimates of the warming caused per trillion metric tons of carbon emitted are between 0.8 and 2.5ºC (IPCC, *Climate Change 2013: The Physical Science Basis*, [Cambridge: Cambridge University Press, 2013], 1,033). The average emissions per capita for someone living in the United States are approximately five metric tons of carbon (Carbon Dioxide Information and Analysis Center, http://cdiac.ornl.gov/trends/emis/top2009.cap). Current life expectancy in the United States is about eighty years (World Health Organization, *World Health Statistics 2013* [Geneva: World Health Organization, 2013]). If we assume lifetime emissions at this average rate, then an average US citizen would contribute approximately half a billionth of degree of warming.

that increase of half a billionth of a degree: Scientists are unable to say whether a given extreme weather event was caused by climate change, but they can say whether a given event was *made more likely by climate change*. So we should expect a small increase in carbon emissions will slightly increase the chances of certain types of extreme weather events occurring. See IPCC, *Climate Change 2013*, 867–952.

the expected *harm of raising global temperatures*: The amount by which your individual emissions would increase the chance of a given extreme event occurring is very small. However, there are many possible extreme events that could occur as a result of increased climate change, and so the chances of your individual emissions causing an extreme event at some point are nonnegligible.

Most estimate that climate change will cost only a couple percent of global gross domestic product (GDP): Estimates of the economic costs of climate change range from benefiting the global economy by a couple percent of GDP to decreasing it by 20 percent of global GDP or more. Typical estimates suggest climate change will damage the global economy by a couple percent of global GDP (Tol and Yohe, "A Review of the Stern Review").

Economic growth per person over the last decade: Gross world product has grown at rate of 3.5 percent over the past nine years ("World Economic Outlook Databases," International Monetary Fund, 2014, http://www.imf.org/external/ns/cs.aspx?id=28), while the global population growth rate over this period has been 1.2 percent (United Nations Department of Economic and Social Affairs Population Division, "World Population Prospects: The 2012 Revision," 2012, http://esa.un.org/unpd/wpp/Excel-Data/EXCEL_FILES/1_Population/WPP2012_POP_F01_1_TOTAL_POPULATION_BOTH_SEXES. Dividing gross world product growth by global population growth yields an average economic growth rate per person of 2.2 percent.

one metric ton of methane produces as much warming as twenty-one metric tons of carbon dioxide: "Overview of Greenhouse Gases," United States Environmental Protection Agency, http://epa.gov/climatechange/ghgemissions/gases/ch4.html.

the social cost of one metric ton of CO_{2eq} is about thirty-two dollars: The median of 311 published estimates of the social cost of carbon is $116 per metric ton of carbon (Richard Tol, "An Updated Analysis of Carbon Dioxide Emission Abatement as a Response to Climate Change," 2012, http://www.copenhagenconsensus.com/sites/default/files/climateemissionsabatement.pdf). Note that this is measured in metric tons of carbon, and so to convert to metric tons of carbon dioxide we need to multiply this figure by the ratio of their relative molecular masses, 12/44, giving a social cost of carbon of thirty-two dollars per metric ton of carbon dioxide. If we were to use the mean rather than the median estimate, we would get a social cost of carbon of forty-eight dollars per metric ton of CO_2.

The average American emits about twenty-one metric tons of CO_{2eq} every year: This figure does not include emissions from land-use change or forestry. (World Resources Institute, 2014, http://cait2.wri.org/wri/Country%20GHG%20Emissions?indicator[]=Total%20GHG%20Emissions%20Excluding%20Land-Use%20Change%20and%20Forestry%20Per%20Capita&year[]=2011&sortDir=desc&chartType=geo)

people in the United Kingdom: Department of Energy and Climate Change, "2013 UK greenhouse gas emissions, final figures."

even acknowledges a small risk of catastrophic *climate change*: I'm using the term *catastrophic climate change* to refer to warming significantly beyond the usual 2 to 4ºC range. Most scenarios in which this occurs involve one or more tipping points in the climate system that may increase global warming more than otherwise anticipated. We should distinguish this from *runaway climate change*, which astronomers refer to as a situation where a planet such as Earth heats so much that the water on the planet boils off. That's considerably more extreme (and very considerably less likely) than what I'm referring to by *catastrophic climate change*.

the consequences would be disastrous: The chances of climate change bringing an end to the human race are extremely small, but the consequences of such an extinction event would be so severe as to warrant further thinking. In order for runaway climate change to happen, a number of feedbacks in the climate system would need to push temperatures considerably higher than we expect. The sorts of feedbacks that could contribute include: frozen methane trapped in melting arctic ground ice; extensive forest fires in rain forests; frozen methane on deep-sea beds; and ice melting and revealing darker ground, which absorbs more sunlight. To cause human extinction, this extreme climate change would probably need to give rise to societal collapse, perhaps triggered by water or crop shortages. The situation could be exacerbated if geoengineering, previously used to cool the planet, was discontinued during the societal collapse, which could cause even more warming. Even in a situation of this sort it is unlikely that the human race would end, however.

(the death tolls from disasters form a fat-tailed distribution): A comprehensive overview is given by Anders Sandberg, "Power Laws in Global Catastrophic and Existential Risks," unpublished paper, 2014.

(Nassim Taleb describes these as Black Swans): Nassim Nicholas Taleb, *The Black Swan: The Impact of the Highly Improbable* (New York: Random House, 2007).

most people who've died in war have died in the very worst wars: Steven Pinker, *The Better Angels of Our Nature: Why Violence Has Declined* (New York: Viking, 2011).

This is what the Skoll Global Threats Fund focuses on: "About Us/Mission & Strategy," Skoll Global Threats Fund, http://www.skollglobalthreats.org/about-us/mission-and-approach/.

GiveWell is currently investigating these sorts of activities: Alexander Berger, "Potential Global Catastrophic Risk Focus Areas," *GiveWell Blog*, June 26, 2014, http://blog.givewell.org/2014/06/26/potential-global-catastrophic-risk-focus-areas/.

SEVEN

"Most African children who attend school": "Why books?," Books For Africa, https://www.booksforafrica.org/why-books.html.

"Books For Africa is a simple idea": "Home," Books For Africa, http://www.booksforafrica.org/.

"6.3 million children worldwide die under the age of five every year": "Priority Issues," Development Media International, http://www.developmentmedia.net/priority-issues, and "Demand creation," http://www.developmentmedia.net/demand-creation.

"Recipients use transfers for whatever is most important to them": "Operating Model," GiveDirectly, https://www.givedirectly.org/howitworks.php.

"Savvy donors know": "Top 10 Best Practices of Savvy Donors," Charity Navigator, http://www.charitynavigator.org/index.cfm?bay=content.view&cpid=419&print=1.

Development Media International's overhead amount to 44 percent of its total budget: Conversation with Will Snell, of Development Media International, July 2014. As came up in conversation, what counts as "overhead" is fairly arbitrary. He comments:

> The overhead calculation all depends on definitions:
>
> Defined at the minimum level (London office rent and associated expenditure, non-project staff, non-project-related travel, IT/comms), our overhead is around 16 percent of total expenditure.

> Defined at the mid-level (all HQ expenses, including staff who work directly on project delivery from London, such as our research manager), our overhead is around 44 percent of total expenditure.
>
> Defined at the maximum level (the [randomized controlled trial]), our overhead is 100 percent of total expenditure because we do not currently have any other live projects!

In 2012, their site received a total of 6.2 million visits: "Where We Are Headed (2013 and Beyond)," Charity Navigator, http://www.charitynavigator.org/index.cfm?bay=content.view&cpid=1193#.VJddHsAA.

this approach to evaluating a charity's effectiveness is seriously misguided: There are many other problems with focusing on overhead; some of these are described in Holden Karnofsky, "The Worst Way to Pick a Charity," *GiveWell Blog*, December 1, 2009, http://blog.givewell.org/2009/12/01/the-worst-way-to-pick-a-charity/; Dean Karlan, "Why Ranking Charities by Administrative Expenses Is a Bad Idea," *Freakonomics* (blog), June 9, 2011, http://freakonomics.com/2011/06/09/why-ranking-charities-by-administrative-expenses-is-a-bad-idea/; and Dan Pallotta, *Uncharitable: How Restraints on Nonprofits Undermine Their Potential* (Medford, MA: Tufts University, 2008), 128–176. One crucial problem is that it's often highly indeterminate what counts as overhead and what doesn't. There is one aspect of the "overhead" metric that it's potentially important to maintain, however, which is the focus on keeping fund-raising costs low. The reason that low fund-raising costs should be encouraged in general involves the "what would have happened otherwise" consideration. When one charity raises money, in part it's creating new philanthropic money, and in part it's merely causing donors to donate to them rather than to other charities. This is the same as all other marketing: when Coca-Cola runs a marketing campaign, in part it will get people to buy more fizzy drinks than they otherwise would have, but in part it will just get people to buy Coke rather than Pepsi. Adrian Sargeant, professor of fund-raising at the University of Plymouth, commented to me: "On the issue of charities and whether expenditure grows the pie, I think the answer there is no—it doesn't" (private communication, August 2014). This creates a risk: if charities keep spending more and more on fund-raising, battling against each other, then they'll reduce the overall size of the charitable pie, because a larger amount of money will have been squandered on negative-sum fund-raising. If we have a social norm against charities spending too much of their budget on fund-raising, we can avoid this bad outcome to some extent.

providing textbooks has either no discernible effect on children's school performance: Glewwe, Kremer, and Moulin, "Many Children Left Behind?"; Maria Kuecken and Anne-Marie Valfort, "When Do Textbooks Matter for Achievement?" *Economic Letters*, 2013; Glewwe and Kremer, "Schools, Teachers, and Education Outcomes in Developing Countries," in Eric A. Hanushek and F. Welch (eds.), *Handbook of the Economics of Education*, vol. 2, (New York: Elsevier, 2006), 945–1017; Patrick McEwan, "Improving Learning in Primary Schools of Developing Countries: A Meta-Analysis of Randomised Experiments," unpublished paper.

The American Cancer Society spends: American Cancer Society, "Stewardship Report," 2013, 44, http://www.cancer.org/acs/groups/content/@corporatecommunications/documents/document/acspc-041227.pdf.

The ALS Association (of ice-bucket-challenge fame) spends: ALS Association, "Annual Report," 2014, 3, http://www.alsa.org/assets/pdfs/annual_report_fy2014.pdf. The figures have been recalculated to exclude administration and fund-raising costs.

it costs DMI between forty and eighty cents per listener per year: Private conversation with Will Snell, December 2014. For example, their program in the Democratic Republic of the Congo is $1 million per year, and through that they will reach 2.5 million people.

What do the recipients of these cash transfers do with the money?: Private conversation with Paul Niehaus, August 2014; Johannes Haushofer and Jeremy Shapiro, "Policy Brief: Impacts of Unconditional Cash Transfers," unpublished paper, October 24, 2013, 16–17, http://www.princeton.edu/~joha/publications/Haushofer_Shapiro_Policy_Brief_2013.pdf.

Diarrhea is a major problem: "Diarrhoeal Disease," WHO Fact Sheets, http://www.who.int/mediacentre/factsheets/fs330/en/. Diarrhea is also a leading cause of malnutrition.

A significant number: "The pooled relative risk of diarrhoeal disease associated with not washing hands from the intervention trials was 1.88 [i.e., not washing hands increases risk of diarrhea by 88%] . . . implying that hand washing could reduce diarrhoea risk by 47%." Val Curtis and Sandy Cairncross, "Effect of Washing Hands with Soap on Diarrhoea Risk in the Community: A Systematic Review," *Lancet Infectious Diseases* 3, no.5 (May 2003), 275–81.

The ads that DMI runs are terribly corny: "Audio: Burkina Faso breastfeeding spot A," Development Media International, 2013, http://www.developmentmedia.net/audio-burkina-faso-breastfeeding-spot-2013.

"one $10 bed net can mean the difference between life and death": "Saving Lives," Nothing But Nets, http://www.nothingbutnets.net/new/saving-lives/.

But when high-quality studies were conducted: See, for example, David Roodman, *Due Diligence: An Impertinent Inquiry into Microfinance* (Washington, DC: Center for Global Development, 2012). Roodman comments: "On current evidence, the best estimate of the average impact of microcredit on poverty is zero. . . . The commonsense idea that credit is a useful tool that sometimes helps and sometimes hurts appears close to the truth." (http://www.cgdev.org/doc/full_text/DueDiligence/Roodman_Due_Diligence.html). Initially, there were academic studies conducted on microfinance that did seem to show strong impact, summarized in Nathanael Goldberg, *Measuring the Impact of Microfinance: Taking Stock of What We Know*, Grameen Foundation, December 2005, http://files.givewell.org/files/Cause1-2/Independent%20research%20on%20microfinance/GFUSA-MicrofinanceImpactWhitepaper-1.pdf. However, these studies weren't randomized; when randomized controlled trials came out, they changed the picture dramatically. This example illustrates just how important getting high-quality evidence is: low-quality evidence can include highly cited academic studies, not just anecdotes. For an overview of the "hierarchy of evidence," see Trisha Greenhalgh, "How to Read a Paper: Getting Your Bearings (Deciding What the Paper Is About)," *BMJ* 315, no. 7,102 (July 26, 1997), 243–6. Note that microcredit is only one form of microfinance; other forms of microfinance, such as microsavings (providing secure places for the very poor to save money), have shown promise.

Rather than starting new companies: In an interview with *Time*, David Roodman from the Center for Global Development commented that "there are a fair number of stories where women cannot pay back their loans but they're in [community borrowing groups]. So people come and take their roofs, flashlights, everything." For this and the other problems listed, see "Why We Don't Recommend Microfinance," *Giving What We Can* (blog), November 29, 2012, https://www.givingwhatwecan.org/blog/2012-11-29/why-we-don%E2%80%99t-recommend-microfinance; Sam Donald, "Why We (Still) Don't Recommend Microfinance," *Giving What We Can* (blog), March 12, 2014, http://www.givewell.org/international/economic-empowerment/microfinance; Holden Karnofsky, "6 Myths About Microfinance Charity That Donors Can Do Without," *GiveWell Blog*, October 23, 2009, http://blog.givewell.org/2009/10/23/6-myths-about-microfinance-charity-that-donors-can-do-without/.

The latest evidence suggests: Abhijit Banerjee, Dean Karlan, and Jonathan Zinman, "Six Randomized Evaluations of Microcredit: Introduction and Further Steps," September 11, 2014, http://karlan.yale.edu/p/AEJ%20Intro.pdf.

Cash transfers are one of the most well-studied development programs: See the list of studies in "Cash Transfers in the Developing World: Program Track Record," GiveWell, November 2014, http://www.givewell.org/international/technical/programs/cash-transfers#ProgramTrackRecord.

Innovations for Poverty Action has run a randomized controlled trial on GiveDirectly: Johannes Haushofer and Jeremy Shapiro, "Household Response to Income Changes: Evidence from an Unconditional Cash Transfer Program in Kenya," November 15, 2013, http://www.princeton.edu/~joha/publications/Haushofer_Shapiro_UCT_2013.pdf.

may be optimistic: It's very difficult to know how to adjust one's estimates of cost-effectiveness in order to take this into account. GiveWell has tried to make adjustments to the estimate in order to take into account the facts that the program may not work as well in countries other than Burkina Faso, and that self-reporting may overstate the benefits of the program. According to their revised estimate, the cost per child life saved is $5,200 (this figure doesn't include non-lifesaving health benefits). DMI, however, thinks these adjustments are inappropriate and stand by an estimate of approximately $1,100 per life saved (again not including non-lifesaving health benefits, which the ten-dollars-per-QALY figure does include). "Development Media International (DMI): What Do You Get for Your Dollar?" GiveWell, December 2014, http://www.givewell.org/international/top-charities/DMI#cea.

a study of bed nets distributed by the Kenyan government: Noboru Minakawa, Gabriel O Dida, Gorge O Sonye, Kyoko Futami, and Satoshi Kaneko, "Unforeseen Misuses of Bed Nets in Fishing Villages Along Lake Victoria," *Malaria Journal* 7, no. 165 (2008).

(figure is 0.4 percent): "Quality of Service," GiveWell, https://www.givedirectly.org/quality-of-service.html, accessed January 2015.

These programs also receive substantial support from Gavi: "Gavi Pledging Conference June 2011," Gavi: The Vaccine Alliance, http://www.gavi.org/funding/resource-mobilization/process/gavi-pledging-conference-june-2011/.

GiveDirectly could productively use an additional $25 to $30 million: "GiveDirectly," GiveWell, December 2014, http://www.givewell.org/international/top-charities/give-directly.

DMI could productively use $10 million: "Development Media International," GiveWell, December 2014, http://www.givewell.org/international/top-charities/DMI.

such as polio, measles, diarrheal disease, and guinea worm disease: See Levine, *Case Studies*, 33–40, 57–64, 81–88, 127–34.

the link between aid and economic growth is less clear: For a nontechnical overview, see David Roodman, "Macro Aid Effectiveness Research: A Guide for the Perplexed," Center for Global Development, working paper 135, December 2007, http://www.cgdev.org/publication/macro-aid-effectiveness-research-guide-perplexed-working-paper-135.

GiveDirectly: https://www.givedirectly.org/; "GiveDirectly," GiveWell, December 2014, http://www.givewell.org/international/top-charities/give-directly.

Development Media International: http://developmentmedia.net/; "Development Media International," GiveWell, December 2014, http://www.givewell.org/international/top-charities/DMI.

Deworm the World Initiative: http://www.evidenceaction.org/; "Deworm the World Initiative (DtWI), Led by Evidence Action," GiveWell, December 2014, http://www.givewell.org/international/top-charities/deworm-world-initiative.

Schistosomiasis Control Initiative: http://www3.imperial.ac.uk/schisto; "Schistosomiasis Control Initiative (SCI)," GiveWell, November 2014, http://www.givewell.org/international/top-charities/schistosomiasis-control-initiative.

Against Malaria Foundation: https://www.againstmalaria.com/Default.aspx; "Against Malaria Foundation (AMF)," GiveWell, November 2014, http://www.givewell.org/international/top-charities/AMF.

Living Goods: http://livinggoods.org/; "Living Goods," GiveWell, November 2014, http://www.givewell.org/international/top-charities/living-goods.

The Iodine Global Network (IGN): http://www.ign.org; another "standout" charity, as listed by GiveWell in December 2014, is Global Alliance for Improved Nutrition: Universal Salt Iodization (GAIN-USI) program (http://www.givewell.org/international/top-charities/GAIN). Like IGN, GAIN-USI focuses on increasing the coverage of salt iodization. I didn't include them in the body text because of their similar focus to IGN and because, at the time of writing, details of how GAIN-USI would use additional funding were not available.

Another charity worthy of note is Project Healthy Children (PHC), http://projecthealthychildren.org. PHC works with developed world governments to fortify foods like flour, sugar, rice, and oil with micronutrients such as folic acid, iodine, iron, vitamin A, and zinc, at a

cost of between five and ten cents per person. This is a top-recommended charity by Giving What We Can; however, PHC declined to participate in GiveWell's review process. For consistency, I list only GiveWell recommendations in this chapter.

One estimate put the economic benefits of these programs at twenty-seven dollars for every dollar spent: For an overview of the relevant estimates, see "Micronutrients," Giving What We Can, 2013, https://www.givingwhatwecan.org/research/charities-area/micronutrients.

EIGHT

"Our Garment Workers Are Paid Up to 50 Times More Than the Competition": "About Us," American Apparel, http://www.americanapparel.net/aboutus/. I use American Apparel as an example of a supposedly "ethical" company because of their emphasis on "sweatshop-free." Independently of this, however, other aspects of the company are problematic. Dov Charney, the founder and former CEO of the company, has been the subject of numerous allegations of misconduct and sexual harassment, which in part led to his termination in early 2014 (see http://www.buzzfeed.com/sapna/exclusive-read-ousted-american-apparel-ceo-dov-charneys-term#.lgZarE7Wz). American Apparel's advertising campaigns have also come under criticism for sexualizing minors (see http://www.independent.co.uk/life-style/fashion/features/american-apparels-most-controversial-moments-following-ban-on-back-to-school-ad-9712735.html).

under pretty horrific working conditions: Lucy Ash, "Inside China's Sweatshops," BBC News, July 20, 2002.

columnist Nicholas D. Kristof illustrated this well: Nicholas D. Kristof, "Where Sweatshops Are a Dream," *The New York Times*, January 14, 2009.

nearly four million people from Laos, Cambodia, and Burma immigrated to Thailand: Marc Margolis, "Roads to Nowhere: More and More Migrants from Poor Countries Are Heading to Other Former Backwaters for Work," *Newsweek*, September 11, 2006.

Bolivians risk deportation by illegally entering Brazil: Jack Chang, "Bolivians Fail to Find Better Life in Brazil," *Miami Herald*, December 28, 2007. Cited in Benjamin Powell, *Out of Poverty: Sweatshops in the Global Economy* (New York: Cambridge University, 2014), 60.

The average earnings of a sweatshop worker in Brazil are $2,000 per year: Powell, *Out of Poverty*, 60–61.

the average earnings among sweatshop workers are: Ibid.

"The overwhelming mainstream view among economists": Quoted in Allen R. Myerson, "In Principle, a Case for More 'Sweatshops,'" *The New York Times*, June 22, 1997.

"My concern is not that there are too many sweatshops but that there are too few": Ibid.

low-wage, labor-intensive manufacturing is a stepping-stone: Powell, *Out of Poverty*, 120–121.

"it is widely thought that most of them": Bureau of International Labor Affairs, *By the Sweat & Toil of Children: The Use of Child Labor in American Imports* (Washington, DC: US Deptartment of Labor, 1994), 30.

an investigation by UNICEF: UNICEF, *The State of the World's Children* (Oxford, UK: Oxford University, 1997), 60.

We should certainly feel outrage and horror at the conditions sweatshop laborers toil under: Note that none of the arguments I've given apply to the use of forced labor: there is no argument that involuntary work benefits the workers. (Though chattel slavery, where people are bought and sold as if they were commodities, is thankfully now very rare, debt bondage, where a person pledges their labor in return for a loan, and where that debt can be passed down through generations, still exists.) If a company contracts with factories that use involuntary workers, we should utterly condemn this and demand that the situation is rectified.

in favor of domestically produced goods: Nor is the solution to buy secondhand clothes, as is advised by, for example, the Institute for Humane Education (http://humaneeducation.org/blog/2013/04/03/5-tips-

keeping-sweatshop-free-closet/) and Labor Behind the Label (http://www.labourbehindthelabel.org/jobs/item/980). This would have the same effect of reducing employment opportunities for the very badly off in poor countries.

$6.9 billion was spent on Fairtrade-certified products worldwide: Rebecca Smithers, "Global Fairtrade Sales Reach £4.4bn Following 15% Growth During 2013," *The Guardian*, September 3, 2014.

fair-trade coffee production comes from comparatively rich countries: Paul Griffiths, "Ethical Objections to Fairtrade," *Journal of Business Ethics* 105, no. 3 (February 2012): 364.

less than 1 percent of the additional price of their fair-trade coffee reached coffee exporters: Griffiths, "Ethical Objections," 359–60.

only 11 percent of the additional price reached the coffee-producing countries: Joni Valkila, Pertti Haaparanta, and Niina Niemi, "Empowering Coffee Traders? The Coffee Value Chain from Nicaraguan Fair Trade Farmers to Finnish Consumers," *Journal of Business Ethics* 97, no. 2 (December 2010): 257–70.

coffee producers would receive only forty cents per pound: Bernard Kilian, Connie Jones, Lawrence Pratt, and Andrés Villalobos, "Is Sustainable Agriculture a Viable Strategy to Improve Farm Income in Central America? A Case Study on Coffee," *Journal of Business Research* 59, no. 3 (March 2006): 322–30. For further discussion of these estimates, see Griffiths, "Ethical Objections," 359–60.

a four-year study on earnings of Fairtrade workers in Ethiopia and Uganda: Fairtrade, Employment and Poverty Reduction Project, *Fair Trade, Employment and Poverty Reduction in Ethiopia and Uganda*, April 2014, http://ftepr.org/wp-content/uploads/FTEPR-Final-Report-19-May-2014-FINAL.pdf.

"the British public has been led to believe": Carl Mortished, "Fairtrade Coffee Fails to Help the Poor, British Report Finds," *Globe and Mail* (Toronto), May 26, 2014.

Fairtrade certification does not improve the lives of agricultural workers: Fairtrade, Employment and Poverty Reduction Project, "Response to Fairtrade Statement on FTEPR Final Report 31st May 2014," http://ftepr.org/wp-content/uploads/Response-to-Fairtrade-Statement-on-FTEPR-Final-Report-Posted.pdf.

"there is limited evidence of the impact on workers of participation in Fairtrade": Valerie Nelson and Barry Pound, *The Last Ten Years: A Comprehensive Review of the Literature on the Impact of Fairtrade*, September 2009, 35, http://www.fairtrade.net/fileadmin/user_upload/content/2009/about_us/2010_03_NRI_Full_Literature_

many popular ways of reducing your greenhouse gas emissions: These facts come from David JC MacKay, *Sustainable Energy—without the Hot Air* (Cambridge, UK: UIT, 2009), 68–72.

you would save only a fraction of a metric ton of carbon emissions: US domestic energy use has been estimated at thirteen metric tons of CO_{2eq} (Christopher M. Jones and Daniel M. Kammen, "Quantifying Carbon Footprint Reduction Opportunities for U.S. Households and Communities," *Environmental Science & Technology* 45, no. 9 [2011], 4,088–95). UK domestic energy use is approximately five metric tons per year per household. (Department for Energy and Climate Change, Great Britain's Housing Energy Fact File, 2011, https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/48195/3224-great-britains-housing-energy-fact-file-2011.pdf.)

if you stopped using plastic bags entirely, you'd cut out one hundred kilograms CO_2 equivalent per year: "Plastic Bags and Plastic Bottles—CO_2 Emissions During Their Lifetime," Time for Change, April 2009, http://timeforchange.org/plastic-bags-and-plastic-bottles-CO2-emissions; "Facts About the Plastic Bag Pandemic," Reuseit, http://www.reuseit.com/facts-and-myths/facts-about-the-plastic-bag-pandemic.htm. What about other reasons to reduce the use of plastic bags, such as cutting down on landfill? In reality, plastic bags only account for a tiny amount of the trash that we produce (Joseph Stromberg, "Why Our Environmental Obsession with Plastic Bags Makes No Sense," *Vox*, October 4, 2014, http://www.vox.com/2014/10/4/6901299/plastic-bags-environment).

only 10 percent of the carbon footprint of food comes from transportation, whereas 80 percent comes from

production: Sarah DeWeerdt, "Is Local Food Better?" *World Watch* 22, no. 3 (May/June 2009), 6–10; "The Tricky Truth About Food Miles," Shrink that Footprint, http://shrinkthatfootprint.com/food-miles.

Cutting out red meat: Christopher Weber, and Scott Matthews, "Food-Miles and the Relative Climate Impacts of Food Choices in the United States," *Environmental Science & Technology* 42, no. 10 (2008), 3,508–13.

locally grown tomatoes: Annika Carlsson-Kanyama, "Food Consumption Patterns and Their Influence on Climate Change," *Ambio* 27, no. 7 (November 1998), 528.

(beef, which can cut out about a metric ton of CO_{2eq} per year): Gidon Eshel and Pamela A. Martin, "Diet, Energy, and Global Warming," *Earth Interactions* 10, no. 9 (April 2006).

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You might have some concerns about this idea. Won't mass immigration be politically disruptive? Won't it cause a "brain drain," resulting in all the best talent from poor countries leaving, making those left behind worse off than before? Won't it harm the native workers of the rich country,

depressing wages and increasing unemployment?

There are good responses to each of these worries. Let's take the objections in turn. Regarding political disruption, it would improve politics in poor countries: dictators and corrupt governments would have far less power over their people, because those people would have a much easier opportunity to leave the country. For the rich countries, the evidence is ambiguous. For example, most social scientists detect little effect of immigration on the size of government, even though immigrants are more in favor of the welfare state: there is a delay before they are eligible to vote, and even when they do have the vote, their turnout at elections is very low.

Regarding the "brain drain," immigration from poor countries to rich countries would significantly benefit those who choose to remain in the poorer country. Immigrants send substantial remittances back to their home countries—where total global remittances are several times larger than total foreign-aid spending and can be as much as a third of a poor country's GDP. A larger diaspora increases trade between the home country and the country to which immigrants move. Immigrants often return to their home countries, and when they do, they bring back valuable skills. Puerto Rico provides a good illustration of this. More than half of Puerto Ricans live abroad, but the very fact that Puerto Ricans have been able to emigrate to the United States means that the standard of living of those who still live in Puerto Rico has increased sixfold since 1980, and is now comparable with countries like the United Kingdom and Italy.

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