

Despite these concerns, I found parts of the book to be engaging and illuminating. I particularly liked and learned from the discussions on Swift, Defoe, and Harriet Martineau. The weakest parts of the book were the chapter on pre-1600 ideas, the discussion of Malthus, and the chapter on neoclassical economics. The authors' attempt to explain the changes in economic thinking in the 1870s as some sort of conspiracy against revolutionary agitation (175) strains credulity. As does their other suggestion that neoclassical economics was simply a reflection of the new consumerist spirit. I remain puzzled why there is so much attention to Jevons, Walras, and Menger rather than Lionel Robbins, who was responsible for the definition of scarcity that features in so many contemporary textbooks of economics. It is not as if there is a shortage of literature on Robbins's definition, from the work of Roger Backhouse and Steve Medema, to the recent PhD thesis of Nathan McLellan who attends carefully to the religious dimensions of different ideas of scarcity.

I applaud the authors' stated "goal of universal flourishing within planetary constraints" (246). But is neoclassical economics really the bogeyman? Is the authors' story that locates the fall in the 1870s with Jevons, Walras, and Menger eating the forbidden fruit, and locates redemption in certain late twentieth-century scientists and philosophers really credible?

In my view a better story and solution is found in the biblical account of divine creation, providence, and the eschaton. The partial nature of human understanding and unpredictable results of human action are in my view better accounted for in the biblical account of our fallen state. Here perhaps is an opportunity for *Journal of Markets & Morality* writers who have expertise in environmental science, economics, and theology to offer a better account.

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## What We Owe the Future

**William MacAskill**

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At its best, *What We Owe the Future* is an exercise in not knowing. We do not know what the future holds or what future people will want or value. This uncertainty should condition the way we make decisions which affect future people.

MacAskill argues we ought not assert our own evaluative judgments on future generations who may disagree and will likely have greater insight than we do, just as our insight and judgment differs from past generations. We should keep future possibilities open for those generations (keep the molten glass fluid, to borrow MacAskill's central metaphor) and avoid doing things that "lock-in" our values for

those generations (92–104). Openness and flexibility are themselves values, and often controversial ones, which future generations may regret; perhaps future generations will regret the open society MacAskill advocates, but it is the most prudent option to respect the autonomy of future people.

MacAskill's book is built on a crucial observation: The most pressing moral question of our age is not philosophical, but motivational. Regardless of one's background moral theory, the status quo of severe poverty, lack of access to healthcare, education, and economic opportunity throughout the world is not morally defensible. Regardless of the scientific details, the current trajectory of our planet is not sustainable. The central lesson of *Effective Altruism* and longtermist varieties is how they address the motivational problem. Despite identifying clear moral problems such as poverty, malaria, and access to education, there is a lack of political will and financial investment from countries and people who have the resources to make a difference.

*Effective Altruism* has been extremely effective at raising money from a sector that has historically not been especially philanthropically involved. MacAskill has been an effective motivator, especially for projects that are morally uncontroversial, such as direct cash transfers (combatting poverty) and insecticidal nets (combatting malaria).

The book departs from uncertainty by making confident, often ill-informed assertions about some policies it seeks to motivate. The book surveys and prognosticates on technical issues in climate change mitigation (134–38, 230), artificial intelligence (80–91), global public health (107–14), nuclear catastrophe (127–31, 228–30), and so on. These sections simplify aggressively, as one might expect; but more troubling is that many claims overstate epistemic authority and miss critical underlying facts.

Global public health is my bailiwick, so I will use that to illustrate.

MacAskill claims the longtermist community was ahead of the game on global pandemics (113–14), noting he raised the issue of a possible global pandemic in 2017. “One of the main longtermist funders, Open Philanthropy, was one of the few pre-COVID funders of pandemic preparedness in the world. It made its first grant in the area in 2015 and has since give out more than \$100 million in the area” (113).

To those outside of global public health, this may look impressive. But it is wrong on several levels. The year 2015 was far behind the ball on pandemic preparedness in global public health, many organizations funded pandemic preparedness, and \$100 million is not a large amount of funding in that area.

Scholars of global public health raised the issue of a severe upper-respiratory pandemic as a major global threat in the mid-1990s. Much of this work fell under “biodefense” and “biosecurity,” which MacAskill (correctly) considers pandemic preparedness (228). In 1998, President Clinton oversaw substantial expansion of Department of Defense preparedness concerned with biological weapons (including Project Bacchus), projects that escalated following the 2001 anthrax attacks, but under the Bush administration these projects expanded to nondefense preparedness

more broadly. President Bush laid out \$7 billion for pandemic preparedness in a speech at the National Institute of Health (NIH) in November 2005. President Obama continued this throughout his administration, including a December 2014 address at the NIH focused on influenza.

MacAskill treats the longtermist predictions of a global pandemic as evidence of epistemic credibility. But by 2015 (his dating of their prescient predictions) this prospect was common knowledge to global public health scholars and major political leaders. Further, the \$100 million number is far less than global infrastructure organized through the WHO (annual budget of \$8 billion) or even private philanthropies such as the Bill and Melinda Gates Foundation. By contrast, the Center for Epidemic Preparedness Innovations (CEPI) was founded in 2017, the same year MacAskill reports speaking on the issue, with an investment of \$460 million. The design for CEPI was laid in 2015 by Jeremy Farrar, Stanley Plotkin, and Adel Mahmoud, all of whom had been working on pandemic preparedness for years.

I cannot survey all the problems with MacAskill's book here. There are similar problems with MacAskill's claims on climate change, artificial intelligence, and so on. Those passages make similar, severe mistakes, both overstating the epistemic credibility of the Effective Altruist movement and misstating the history and state of play in these issues.

MacAskill's book is ambitious, but this ambition is not circumscribed by appropriate epistemic humility. These areas require subject matter expertise; I caught the problems with public health, biosecurity, and pandemic preparedness because that is an area in which I had post-graduate training. Approaching subject matter experts with his claims on climate mitigation measures and nuclear nonproliferation, those subject matter experts found his analysis similarly deficient.

Subject matter expertise matters, and MacAskill is not merely exaggerating his subject matter expertise regarding public health in that passage; he's attributing a prediction to shore up credibility for "longtermists" despite (in the case of pandemic predictions) being three decades behind subject matter experts. This raises concerns about MacAskill's judgment.

I started writing this review when longtermism's lessons for business ethics were unclear. MacAskill is an enormously influential philosopher, one whose views are central to a growing, valuable philanthropic culture. MacAskill has raised an enormous amount of money; he has done an enormous amount of good. That makes mounting a critique (especially an admittedly harsh one above) morally difficult. I want to encourage people to read MacAskill, to find motivation in what he writes and do good.

The collapse of the FTX exchange and the steady deterioration of cryptocurrency markets have clarified things. MacAskill was a close associate of Sam Bankman-Fried, the now disgraced founder of FTX, and MacAskill was active in promoting Bankman-Fried as a figure in Effective Altruism generally and longtermism specifically.

Epistemic hubris is a feature of many recent scandals in venture capital, frauds that would have been prevented had investors and customers turned to subject matter experts to evaluate claims made by fraudsters such as Bankman-Fried (and Elizabeth Holmes, inter alia). The FTX case is the one that tangled up the Effective Altruist movement, but it is hardly the only case of fraud brought on by epistemic hubris. For this reason, while criticism of MacAskill is appropriate, we should not make those criticisms too harshly.

“Move fast and break things” started as a Zuckerberg-ism but has become a mantra in technology companies. The focus is on growth and technological investment to establish future benefit, while addressing harms with other technological solutions as they come up. Petrol causes problems in air quality and climate change, so we will address that not with fuel efficiency standards and carbon capture; the problem is not the technology, it is the absence of a new technological solution. This is a symptom of the epistemic hubris characterized by many of the proposals in MacAskill’s book. We should avoid bringing such hubris to bear and take more seriously the criticisms of subject matter experts within these domains.

—Joshua Stein

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