CESifo WORKING PAPERS

10160
2022

December 2022

The Short-Termism of 'Hard' Economics

Ilan Noy, Shakked Noy

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 10160*

The Short-Termism of 'Hard' Economics

Abstract

"Longtermism" is the view that the impacts of our actions on the very long-term future deserve prominent consideration in decision-making. We discuss the primary barrier that prevents academic economists from contributing to longtermist research: an overly rigid preference for methodological "hardness" (Akerlof, 2020). Hardness bias prevents economists from engaging in methodologically pluralistic, interdisciplinary, qualitative, and other kinds of research, including most potential longtermist research. We unpack hardness bias, discuss its roots, illustrate how it prevents economists from engaging in longtermist research, and try to present a positive vision of the kinds of longtermist research economists could engage in if hardness norms were relaxed.

JEL-Codes: B400.

Keywords: economic methodology, longtermism, academic economics, methodological hardness.

Ilan Noy
Victoria University of Wellington
Wellington / New Zealand
ilan.noy@vuw.ac.nz

Shakked Noy
Massachusetts Institute of Technology
Cambridge / MA / USA
snoy@mit.edu

This essay has been prepared for a collected volume of essays on Longtermism.

1. Introduction

The academic economics profession has contributed, and seems poised to contribute, very little to longtermist research. This is despite the following facts: Effective altruist organizations have long recommended an economics PhD as one of the best ways to acquire skills for global priorities research, including longtermist research; many foundational concepts and tools in economics lend themselves naturally to longtermist research, including ideas about constrained optimization, strategic interactions, and intertemporal decision-making; and many individual economists are enthusiastically interested in longtermism.

In this paper, we attempt to explain this surprising juxtaposition. Our main claim is that the academic economics profession is stuck in a methodological straitjacket that prevents it from accepting more methodologically pluralistic kinds of research — including, as a side effect, potential longtermist research. Our criticism of the profession's methodological narrow-mindedness is not new: we echo George Akerlof's (2020) indictment of "hardness bias" in economics and point to survey evidence showing that a majority of economists dislike the profession's current methodological norms (Andre and Falk, 2021). Our aim with this paper is threefold. First, for readers of this volume, who are by now familiar with longtermism but may be unfamiliar with academic economics, we give an overview of the state of the profession. Second, we aim to raise the salience and importance of the profession's narrow-mindedness by illustrating how it prevents economists from contributing to longtermist research. Third, we try to articulate a more positive vision for how economists could contribute to longtermist research if current methodological norms were relaxed.

Throughout, we take "longtermism" to mean the view that the impacts of our actions on the very long-term future deserve prominent consideration in decision-making. Areas of interest to longtermists that we mention in this chapter include catastrophic or existential risks from artificial intelligence, climate change, or nuclear war; long-run rates of scientific innovation and economic growth; and global governance institutions.

The rest of this chapter proceeds as follows. In Section 2, we describe the methodological constraints and norms that shape the way economists currently do research. In Section 3, we consider *why* these methodological norms have arisen. In Section 4, we use three case studies to illustrate how these norms prevent economists from producing research relevant to longtermism. In Section 5, we argue that academic economists could contribute substantially to longtermist research if given the opportunity. In Section 6, we consider alternative explanations for economists' failure to engage with longtermist research. Finally, in Section 7 we offer some speculative recommendations to institutions focused on longtermist research.

2. What Does Academic Economics Research Look Like?

2.1 Norms of Typology, Exclusion, and Omission

The production of academic economics research is tightly disciplined by a set of norms, which we can divide into norms of typology, exclusion, and omission.

Norms of *typology* establish a small set of "kinds" of economic research and insist that any new piece of research must fall neatly into one kind. Each kind is associated with its own methodology and its own standards for what counts as a valuable (and hence publishable) contribution to economic knowledge. Norms of typology, by forcing new research to adhere rigidly to one predetermined kind, discourage projects that draw on a more varied set of methodologies and combine them in different ways, or venture into entirely new methodological terrains. As we will later suggest, longtermist projects disproportionately share these latter characteristics.

Norms of *exclusion* say that certain types of evidence or research practices are not scientifically acceptable. Often, these are types of evidence or research practices that are widely used in other fields of social science (e.g., focus groups or Delphic surveys). By precluding research that draws heavily on these types of evidence or research practices, norms of exclusion narrow the field of acceptable economics research. Again, this makes longtermist economic research —

which is often methodologically pluralistic and draws on insights from multiple fields of social science — difficult.

Norms of *omission* say that certain types of evidence or research practices, while acceptable, are not significantly professionally rewarded. Since economists, like any other researchers, face time and resource constraints, and since career promotion paths force them to concentrate on professionally rewarded activities, norms of omission inevitably cause certain types of research to fall by the wayside — including, as we will describe, policy-relevant research, interdisciplinary research, book-length research, and highly speculative research.

In the following subsections, we describe the key norms of typology, exclusion, and omission that shape the current landscape of academic economics research. We then turn, in Section 3, to the underlying fundamental values of the economics profession that give rise to these norms.

Two important caveats apply to our discussion in this section. First, we are not claiming that these norms are wholly unjustified or even harmful on average. As we will see in this section and Section 3, there are many good reasons behind these norms, and these norms have many positive effects. Rather, our argument is that the profession currently adheres to these norms too rigidly, and that relaxation of these norms on the margins could beneficially broaden the scope of economics research, including by increasing the profession's receptiveness to longtermist research. Second, we are making broad generalizations about the economics profession; obviously these generalizations are not *universally* true, and there are exceptions to each claim we are making. Nevertheless, we believe that identifying these very common features is still important in clarifying the potential role of economics in longtermist research.

2.1. Norms of Typology

Almost all academic economics research can be classified into one of the following three categories: theory, empirical causal inference, and structural modelling and estimation.

A typical *economic theory* paper does one of two things. One approach is to introduce a novel economic mechanism, describe the conditions under which it arises, and illustrate its consequences. For example, the mechanism might be that, when sellers have more information about the quality of a product than potential buyers, a recursive series of buyer inferences about expected product quality and seller decisions can cause a market to 'unravel' and prevent mutually beneficial trades from occurring (Akerlof, 1970). Alternatively, an economic theory paper might introduce a conceptual framework for thinking about a particular problem or context and show what can be proven about that problem or context. For example, a paper might provide a framework for thinking about optimal savings rates across generations (Ramsey, 1928).

Economic theory papers share two key features. First, there is a focus on precision through mathematical formalization, which avoids the ambiguity that often clouds ordinary language but also results in heavy abstraction. Second, there is a focus on generality: identifying the broadest possible conditions under which a mechanism arises, or keeping a conceptual framework as expansive as possible. Naturally, a focus on generality encourages abstracting away from contextual particulars, even when there is a relatively limited set of contexts for which this focus is meaningful. It also encourages a search for mechanisms or problems that apply very generally (even if they are not that important), and discourages work on mechanisms or problems that may be very important but operate only in narrow particular contexts (i.e., in the context of the European Union's scientific funding model, or China's regulation of artificial intelligence research). This discourages theoretical economists from modelling practical situations or applying general results to concrete contexts.

Empirical economics research generally attempts to estimate the causal effect of a particular shift, change, or intervention (a policy, an event, or a circumstance) on a given outcome. For example, a paper might try to estimate the effects of class size on educational attainment and future earnings, the effects of a neighbourhood's characteristics on the well-being of its residents, or the effects of fiscal and monetary policy tools on inflation. Clear causal inference is the gold standard of most empirical research and it is frequently the strength of this causal identification (rather than the importance of the question being studied or its policy implications) that is the main yardstick by which journal publication decisions are being made.

A focus on causal effects sounds innocuous, but it narrows remarkably the scope of acceptable (i.e. publishable) empirical research. First, it results in bias against *descriptive*, *explanatory*, or *predictive* empirical research. It is difficult to publish *descriptive* research illustrating an important pattern or trend, or providing suggestive evidence for a causal mechanism that is hard to identify more credibly (think, for example, about work on "deaths of despair"). Economists almost never formulate general *explanations* of what drives variation in the outcome they study (Rodrik, 2021). For any outcome Y, there will be hundreds of papers asking whether X_i has a causal effect on Y for different choices of i, but very few papers ask which variables explain most of the variations in Y. Finally, generally speaking, economists are not interested in attempts to use empirical evidence to *forecast* important future trends (contrary to the lay perception that economists spend all day trying to predict recessions). All of this is bad news for longtermist research, since it precludes study of questions like “how has aggregate global welfare changed over time?” “what are the main drivers of scientific innovation?” or “what are the most plausible trajectories for economic growth over the rest of this century?”

Moreover, the constraints of causal inference restrict the questions, contexts, and time periods that can be studied. Clean identification of causality is much more likely for narrow policy interventions that can be neatly isolated. This prevents study of larger, more important interventions that are harder to disentangle from other factors and from each other. Economists only rarely try to answer big questions like whether democracy contributes to economic growth (Acemoglu et al., 2019). Moreover, empirical causal inference usually requires very large existing datasets (especially since most research designs require zooming in on a small subset of the data). Most large datasets began their coverage only in the 1980s or 1990s, mostly in high-income countries. Besides, the assembly of new datasets is not adequately rewarded professionally.

Structural modelling and estimation, the third type of paper, occupies a kind of middle ground between the first two types. A typical ‘structural’ paper sets up a model to describe a particular economic system or context, usually drawing on the economic theory literature on that subject. The model will feature various ‘parameters’ that could have a variety of values — for example, the price-demand elasticity of a certain class of products, or the discount rate. Typically, the paper will “plug in” estimates of these parameters from the existing empirical literature on the subject, or produce its own estimates based on some set of assumptions. The model will then be used for any of the following purposes: connecting estimates of behavioural changes to fundamental parameters like time preferences; making statements about counterfactuals (what would have happened if we changed this policy parameter by X); or making statements about welfare (how does this policy affect aggregate welfare?).

Of the three kinds of research, structural modelling is the most methodologically pluralistic and perhaps the closest to what ideal longtermist research could resemble. Papers of this type combine theoretical modelling with estimates from specific contexts; standards for the empirical component of these methodologies tend to be more permissive than those of pure causal inference papers, and the theory is also typically less abstract, sometimes incorporating institutional details. That said, there are still important limitations to the structural modelling approach. Even in very complex modelling (for example, in agent-based models — e.g. Lamperti et al., 2018) the approach requires many structural assumptions about functional forms and parameters, because of the formalistic methodology, and these make those models of questionable use in identifying solution to real-world problems.

In recent years, there has been a growing expectation that papers should include both theoretical and empirical components. A paper is then viewed either as a primarily theoretical paper with supporting empirical evidence, or a primarily empirical paper grounded in a mathematized conceptual framework. While this trend, an attempt to unify these disparate methodological approaches, has many advantages, it has not fundamentally transformed the character of economics research. The scope of publishable “primarily empirical” research remains constrained by the norms we outlined above, and the requirements imposed on “primarily theoretical” papers also remain the same as above.

2.2 Norms of Exclusion and Omission

Implicit in this typology of papers is that specific kinds of empirical evidence are excluded from consideration in mainstream economics. First, qualitative research as conducted by sociologists or anthropologists — e.g. interviews, case studies, focus groups — is not considered an acceptable source of evidence. Second, non-statistical empirical work of the kind that historians conduct is also dismissed. Pieces of qualitative and non-statistical evidence can be (and often are) invoked by economists to *motivate* theoretical models or statistical research or to supplement discussion of possible mechanisms, but these kinds of evidence can never be the centrepiece of a published research project. This is regrettable even if we agree that quantitative causal inference is superior to other kinds of empirical evidence, because there are *many* questions or contexts that cannot be studied with quantitative causal inference. Although they cannot be studied because of data limitations or lack of easily-justified exogenous variation, they may be amenable to alternative forms of empirical investigation. These, again, may include questions of interest to longtermists.

Economics is the least interdisciplinary of the social sciences, by a wide margin. Compared to sociologists, political scientists, and anthropologists, economists are much less likely to cite papers from other disciplines, as they view interdisciplinarity as less important (Fourcade, Ollion, and Algan 2015). Our anecdotal impression is also that economists are much less likely to coauthor with scholars from other disciplines and to publish papers in other disciplines' journals. Since, as we will argue further on, promising longtermist projects often cuts across disciplinary boundaries, the insularity of economists and their hostility to interdisciplinary work is a barrier to longtermist research.

In addition, or maybe as a reflection of this disciplinary insularity, economics research published in leading venues rarely cites research written in any language other than English. This is true even for research papers that focus on a non-English-language country.

The standard in economics is to adopt a conservative and buck-passing approach to normative analysis. Economists are usually unwilling to impose substantive and explicit normative assumptions, and typically restrict themselves to "recovering" normative valuations from the behaviours of market participants. For example, the social discount rate, a parameter central to any longtermist discussion, is calculated from the time preferences implicit in the relative returns of financial instruments; another example is the practice in standard welfare analysis in public economics to uses people’s willingness to pay for an intervention as a measure of that intervention’s benefits (Greaves, 2017; Finkelstein and Hendren, 2020). These approaches are *conservative* in the sense that they take the prevailing and observable normative values (such as people’s time preferences) as given and do not question them, and they are *buck-passing* in the sense that they recognize the need to make normative judgements, but are reluctant to do so, arguing that these should be made by non-economist others. Obviously, this approach to normative analysis makes economics reflexively resistant to longtermist projects that are underpinned by unconventional normative assumptions or attempt to engage in welfare analysis without any access to observable data (like willingness-to-pay surveys) that can help pin down the prevailing normative view of these issues.

Another peculiar aspect of academic economics, which leads to equally perverse exclusionary practices, is the dramatically disproportionate career payoffs associated with publishing papers in the ‘top 5’ economics journals (the *American Economic Review*, *Quarterly Journal of Economics*, *Econometrica*, *Journal of Political Economy*, and *Review of Economic Studies*). Successfully publishing in the top-5 journals increases an economist’s probability of getting tenure, prospects for promotion, and status in the profession by an amount that is significantly greater than any other kind of professional achievement (Heckman and Moktan, 2020). This results in a strong norm of omission: any work not ultimately oriented towards a publication in the top-5 has much smaller professional payoff. Consequently, a laundry list of activities are implicitly, and often explicitly, discouraged. These include publishing in other disciplines’ top journals (e.g. *American Political Science Review*), or even top interdisciplinary outlets such as *Nature* or *Science*. These practices end up producing a high volume of important knowledge that aligns perfectly with the prevailing top-5 norms, but is nevertheless not pathbreaking. Moreover, these top-5 journals are mostly controlled by editors from very few US academic departments, resulting in extreme centralization of power and hence of decisions about which kinds of research are interesting or valuable. This narrows the scope of economics research even further.

The fact that economists rarely write books deserves particular emphasis. Unlike other social sciences, where book-writing is encouraged and often a de facto requirement for tenure or promotion, academic economists very rarely write books and produce most of their intellectual output in the form of refereed journal articles. This further contributes to the aforementioned culture of focusing on the identification of particular causal effects or explication of particular mechanisms rather than the production of broad general explanations of important phenomena. A causal effect can be estimated in one paper, but a persuasive explanation or overview of a new and complex problem often demands a book-length exposition.

3. Why Does Academic Economics Research Look Like That?

The norms we just outlined are relatively recent phenomena, having arisen in the past 30–50 years. (Economists prior to this period were much more methodologically pluralistic, interdisciplinary, interested in general explanations, and so on). Why has the academic economics profession acquired all the norms we outlined above?

George Akerlof's (2020) diagnosis, which we find persuasive, is that the underlying cause of all the aforementioned norms is economists' increasingly strong preference for methodological *hardness* in research. A glib way of defining "hardness" would be to say that modern economists like to see themselves as sitting near the *hard* end of a spectrum ranging from "hard sciences" like physics to "soft sciences" like sociology. More descriptively, a preference for "hardness" means a preference for precise mathematical formalization over verbal exposition; for "hard, objective" numerical quantitative evidence over "soft, subjective" qualitative evidence; for causal over correlational/descriptive quantitative evidence; and so on.

Economists' preference for hardness provides a unified explanation of everything we described in Section 2. Papers are divided into three discrete types because each type exemplifies the "hardest" possible realization of theory or applied work. Economists are insular and disdain interdisciplinary engagement because other social scientific disciplines are less 'hard' so that engaging with them would pollute the quality of economics research. Certain types of evidence are excluded because they are 'soft.' The same goes for substantive normative assumptions, which are avoided because they are perceived as allowing subjective assumptions to slip in. Economists focus on narrow identification of causal effects rather than general explanation of phenomena because the former is more amenable to 'hard' methods. Even the disproportionate professional rewards associated with the top-5 journals arguably reflect a preference for hardness; there is a perception that these journals are uniquely demanding in the rigor they expect.

Of course, there is much to like about a focus on hardness. For example, economists tend to have higher standards than other social scientists for drawing inferences about causal effects from statistical evidence, and are much less likely to over-infer causality from correlational evidence. Economists are also relentlessly precise about isolating and stating theoretical mechanisms in a way that other social sciences are not.

But even initially beneficial preferences can go too far, and we agree with Akerlof that that the economics profession currently places an *excessive* premium on hardness. Many economists concur; Andre and Falk (2021) report that a majority of economists would prefer to see more interdisciplinary research, more applied research, more risky and disruptive research, and more research on important questions even at the expense of the quality of causal identification — all of which entails a partial sacrifice of hardness.

What, exactly, is the problem with too much hardness? As Akerlof describes it, the problem is that an excessive focus on hardness means the economics profession fails to balance the 'hardness' and 'importance' of potential research projects. A balanced research portfolio would include projects that are amenable to hard methods, meaning ones that can produce very precise and confident answers to those questions, even if the questions are ultimately not very important; *and* it would include projects on important questions, even if those questions are not very amenable to hard methods, so that we must make do with the evidence that can feasibly be mustered. Unduly strict demands for hardness *rule out* the latter kind of projects and mean that the economics profession neglects many important questions simply because they are unsuitable for study using the "hard" methods the profession prizes above all else.

Obviously, *many* longtermist projects fall straight into the "important but difficult to study with hard methods" bucket. Questions about the long-run drivers of scientific innovation, or ways of structuring global governance to prevent catastrophic risks, are incredibly important but difficult to study with hard methods (AI governance being a prime example of such a topic). They are difficult to study with hard *theoretical* methods because making progress on these questions presumably involves analysis of very concrete particular facts about existing or historical institutions and events, rather than the exposition of new abstract and general mechanisms. And they are difficult to study with hard *empirical* methods: they ask prospective rather than retrospective questions, and require answers to a combination of descriptive, explanatory, predictive, and causal sub-questions.

There is another way of stating the problem — one that ties neatly into the *epistemic* culture of longtermism. The hardness paradigm imposes a kind of dichotomy onto pieces of empirical evidence: either this piece of evidence meets the commonly accepted standards for causal inference (i.e. having a recognizable identification strategy that depends on plausible identifying assumptions), or it does not. In the latter case, it is thrown out completely and disregarded (or published in 'inferior' journals). But this approach is incompatible with the subjective Bayesian approach popular among longtermists, which acknowledges that *any* kind of observation can be informative about a hypothesis. In particular, subjective Bayesians recognize that for many very important questions, (a) we have very little relevant evidence to begin with, (b) credibly identified causal evidence is probably unattainable for now, and (c) as a consequence, new pieces of correlational, descriptive, or qualitative evidence can be *very* informative and valuable. Many, if not most, economic questions relevant to longtermism fall into this category; yet because they cannot be studied using standard methodologies and get published in economics journals, they are rarely pursued.

This is not to say that *no* research relevant to longtermism can fit within the confines of the economics profession's existing norms. In the next section, we will consider what is possible within the current confines of economic research, and what is not. We do so by considering three fields of inquiry closely related to longtermist considerations: frameworks for thinking about long-term decision making; climate change; and artificial intelligence.

4. Academic Economics and Longtermist Research Topics

We now survey some areas of research relevant to longtermism and discuss economists' limited contribution to them. The first topic---conceptual frameworks for thinking about the long-term---is intrinsically amenable to hard methods and therefore economists have contributed significantly to it. By contrast, for the second and third topics---climate change and artificial intelligence---much less has been done and what has been done is narrowly focused on topics that are amenable to the methodological preference for hardness. The two topics are different. Climate change, as a focus of research, has generated voluminous work in many disciplinary fields for several decades, while artificial intelligence is a much newer topic in general, and for the social sciences in particular. Still, in both cases, the contribution of economics has been quite limited.

4.1 Conceptual Frameworks

At least since the pioneering work of Frank Ramsey in the 1920s, economists have been at the forefront of producing rigorous frameworks that permit analysis of questions relating to long-term resource allocation, welfare analysis, and existential risk. These are areas where intuitions and qualitative arguments break down very quickly, and where economic theory really shines by providing elegant mathematical models with simple foundations that flesh out, systematize, and extrapolate from basic intuitions and assumptions. Economic theorists have analysed questions about optimal intergenerational savings (Ramsey, 1928), evaluation of social welfare across time and generations (Dasgupta, 2019), the long-run drivers of economic growth, and the importance of catastrophic and existential risks (Heal and Millner, 2022), among others.

Often, despite making minimal assumptions, these frameworks produce substantive recommendations, in addition to helping us think more clearly about specific questions. For example, they might tell us that catastrophic risks (with non-trivial probabilities) make standard cost-benefit analysis break down (Weitzman, 2009; Millner, 2013), that the primary levers to affect long-run economic growth are related to the rate of innovation (Romer, 1990), or that economic growth cannot be sustained in the long run with a declining population (Jones, 2022).

In this area, we think academic economics has done and continues to do well. The tools of economic theory are extremely well suited for tackling these sorts of questions, and the profession appropriately rewards the development of elegant theories illustrating important abstract mechanisms and hypotheses. As long as economists are aware of the importance of questions about humanity's long-term future — which we think they increasingly are — there should be few barriers to conducting this kind of conceptual longtermist research.

However, these conceptual frameworks and simple recommendations only get us so far. They may tell us which parameters likely govern the optimal savings rate or identify the welfare trade-offs between the present and the distant future, but they do not let us straightforwardly estimate those parameters. They tell us that the innovation rate is important, but offer little guidance on how to increase it. They impress on us the importance of reducing catastrophic risks but offer little guidance on the sources of risk, or on how to reduce risks. When we come to those more practical policy questions, hardness bias causes economics to do even less well. Climate change and artificial intelligence belong exactly to this group of policy-relevant research topics.

4.2 Climate Change

Climate change research is of interest to longtermists for a number of reasons. First, while there is a majority view among longtermists that climate change is unlikely to directly cause human extinction, there is still substantial uncertainty surrounding that assessment. Second, climate change might be a contributing risk factor for other genuine sources of existential risk such as a nuclear war between the Great Powers. Third, climate change research is the largest and most mature research field dedicated to a forward-looking, long-term topic; it therefore offers a convenient window into what longtermist research that is not focused exclusively on existential risks might look like.

Economists are naturally not expected to contribute to research on the physical science of climate change. But two areas of climate research cry out for economic expertise: research on the economic and socio-economic consequences of anthropogenic climatic changes, and research on ways to reduce greenhouse gas emissions or adapt to the consequences of any residual climatic changes. These areas of research correspond roughly to the Intergovernmental Panel on Climate Change's (IPCC) Working Group II, on vulnerability, impacts and adaptation, and Working Group III, on mitigation (henceforth, WG-II and WG-III).

Below, we argue that the hardness norms we identified above are responsible for economists' lack of engagement with the issues arising because of anthropogenic climate change. These norms have led economists to make narrow contributions that have alienated researchers from other disciplines and led to the side-lining of the profession from the literature (Noy, 2023). Moreover, we argue that hardness biases have led economists to neglect considerations that are of special importance to longtermists, specifically extreme tail risks from climate change (such as those that can arise from tipping points and cascading impacts).

4.2.1 Working Group II

The official mandate of WG-II is to "assess the vulnerability of socio-economic and natural systems to climate change, negative and positive consequences of climate change and options for adapting to it." The engagement of economists with WG-II has mostly focused on Integrated Assessment Models (IAMs), the workhorses of climate change economics since the pioneering work of William Nordhaus (1975, 1992) and Nicholas Stern (2007). These models have been widely criticized for underestimating the risks of climate change (Howard and Sterner 2017; Stern and Stiglitz, 2022). This underestimation is a direct consequence of hardness bias. In Nordhaus's pioneering IAM (the DICE model), the link between climate and the economy is modelled by the equation: $D = \alpha \Delta T^2$ — with D defined as the damage sustained by the global economy from climate change and ΔT denoting the difference between global average temperature today and in preindustrial times. The parameter α is an aggregator that summarizes the various channels through which climate change has an impact on economic activity (it is mysteriously parameterised as $\alpha = 0.00236$). This setup incorporates no uncertainty around the magnitude of α and does not allow for extreme tail risks, resulting in "profoundly misleading" underassessment of climatic risks (Stern, 2013, p.839).

The α shortcut exemplifies the methodological straitjacket economists operate under. The shortcut is motivated by the demand for mathematical tractability and simplicity. The lack of attention paid to the assumed magnitude of α is a consequence of economists' lack of engagement with other disciplines (like the physical sciences, or the sociology of disasters), as well as their reluctance to engage with messy empirical questions that cannot be tackled with 'neat' causal methods. Meanwhile, the failure to accommodate tail risks stems from the constraints of structural modelling and estimation. IAMs cannot, as a rule, accommodate extreme trail risks, nor can they estimate the key parameters involved in modelling phenomena that have not happened repeatedly and whose likelihood cannot be estimated from retrospective data. Economists' refusal to engage with subjective data (like aggregations of expert forecasts of these risks) means that these risks and phenomena are left out altogether.

Of course, economic questions relating to climate change are very difficult to answer. It is challenging enough to model climatic systems featuring nontrivial likelihood of abrupt changes and tipping points; additionally modelling society-climate interactions, with their own tipping points, irreversibilities, and multiple equilibria, is doubly complex. But the self-imposed constraints of economists' research practices mean they do not seriously engage with the complexity and contextual particulars of these problems and appear to prefer abstractions that render their conclusions unconvincing to those working outside of economists' disciplinary boundaries.

The other approach adopted by economists to examine the impacts of climate change is based on empirical causal inference from, typically, country-level macroeconomic data, collected across both different geographies and different times. These types of investigations can provide identification of the causal impact of the climate by, basically, assuming that weather measurements are exogenous to the economic system (e.g. Dell et al., 2012; Hsiang et al., 2017; and Kalkuhl and Wenz, 2020). This same approach, of 'looking back to see better ahead,' is also used in the few papers that have looked at climate change adaptation practices (e.g. Burke and Emerick, 2016; and Chen and Gong, 2021).

These papers use economists' standard methods of empirical causal inference. Again, the inherent limitations of these methods mean that these papers neglect important considerations that have little trace in recent historical data, including catastrophic tail risks. These backward-looking papers also rely on reduced-form partial-equilibrium approaches, again limiting their scope. Specifically, this literature is largely unable to consider a crucial scenario — the low likelihood that current impacts will make a major difference to very long-term economic dynamics. This typological straitjacket has thus largely prevented economists from working on extreme risks, but it also prevents them from asking other questions, such as the impact of social cascades following weather extremes (such as was hypothesized for the recent Syrian Civil War (Ide, 2018)).

4.2.2 Working Group III

Economic research is equally constrained on the topic of mitigation (WG-III), as it mostly focuses on two issues: quantifying the social cost of carbon, and questions around the (mechanism) design of emission-permit markets and carbon taxes on various GHG emissions. The calculation of the social cost of carbon, a central quantity, requires an assessment of the impact of climate change (the remit of WG-II), so the description of constraints described above applies here as well. Within the context of WG-III's discussion of mitigation, the widely perceived underestimates of the social cost of carbon have angered researchers from other disciplines, and have recently led to the marginalization of economics from the mitigation literature within the IPCC (Chan et al., 2016).

Reliance on formalised economic theory has led most economists to view the emissions problem as a classic negative externality (i.e., when a consequence of an action is not part of the consideration set of the actor). Hence the near-unanimous conclusion has been that the preferred policy tool to prevent and mitigate climate should be a carbon tax (Timilsina 2022). Economists have been especially blind to the near-total political infeasibility of adequately high carbon taxes. In a paper summarising economists' views on climate policy, Hassler et al. (2016) claim, in spite of the overwhelming evidence to the contrary, that "at this moment in time, we judge a carbon tax to be politically feasible. One often hears that carbon taxes are politically infeasible; we argue that they are likely not." (p. 506).

It would be unfair to give an entirely negative view of economists' work on these topics. A small number of researchers do, for example, engage better with the issue of catastrophic risks — issues that relate to our discussion in the previous section on conceptual frameworks (economists typically model catastrophic climate change as an irreversible but non-existential risk; e.g. Cropper, 1976, Ulph and Ulph, 1997, Tsur and Zemel, 2006, Besley and Dixit, 2019). This literature, including recent attempts to insert catastrophic risks (half-heartedly) into the IAMs, is reviewed in Tsur and Zemel (2021). Many of these papers are rigorously theoretical, and their conclusions are almost always limited. They typically conclude, rather uselessly, that models incorporating catastrophic risks suggest more investment in mitigation than models ignoring them.

Possibly the best-known exception to the lack of relevance of conventional economic research for the longtermist agenda, specifically as it relates to catastrophic risk, is Weitzman (2009). In his article, Weitzman sets out the 'dismal theorem' (DT) which posits that with catastrophic risks, standard utility-maximizing frameworks cannot be satisfactorily used (see also Nordhaus, 2011). In a modest statement atypical for an economics paper, Weitzman says: "I simply do not know the full answers to the extraordinarily wide range of legitimate questions that DT raises. I don't think anyone does. But I also don't think that such questions can be allowed in good conscience to be simply brushed aside by arguing, in effect, that when probabilities are small and imprecise, then they should be set precisely to 0." (Weitzman, 2009, p. 13).

4.3 Artificial Intelligence

Like climate change, economists obviously cannot be expected to contribute that much to computer-science research on artificial intelligence (AI). But economists obviously ought to be interested in the social consequences of AI, in the design of institutions and incentives that shape AI research, and in how policy can be adjusted to make sure outcomes are beneficial or at the very least benign, even in the long-term.

Economists have written many papers discussing AI, but most of this research has a short-term focus, consisting of backwards-looking or short-term forwards-looking analyses of automation, technological unemployment, and inequality (e.g. Autor, 2015; Mokyr et al., 2015). What of the key possibility of core interest to longtermists: an ‘intelligence explosion’ generated by recursively self-improving AI leading to a transformation of economic growth, likely sometime in the next century (Karnofsky, 2022)?

The short-term focus of the existing literature on artificial intelligence is a natural consequence of the hardness biases we have outlined. To satisfy prevailing methodological standards, empirical work must rely on historical data (or historically informed parameterization), usually covering only a few decades; it is therefore difficult to draw out implications for future developments in AI that may look very different from the preceding waves of automation. The profession is averse to more forward-looking empirical work that, for example, would draw on expert predictions or use existing evidence to generate forecasts about future trajectories of AI research and their impacts.

Theoretical work on AI and automation, which coevolves with the empirical literature as models are developed to explain patterns observed in historical data (such as changes in employment or the wage structure), consequently has a short-term focus. There is a strong appetite for models that explain results observed in the empirical literature, and little appetite for models that rely on controversial assumptions about what future developments in artificial intelligence might look like.

Economics research that takes seriously the possibility of transformative AI therefore faces steep barriers to publication. Consider research projects on any of the following longtermist topics: analysing which institutional setups best protect social welfare in the event of a non-catastrophic intelligence explosion; solving mechanism design problems for pre-intelligence-explosion AI governance; or improving the quality of AI timeline forecasts by incorporating factors like strategic interactions between different AI developers and governments. Projects in this vein would be highly speculative and not very mathematically tractable (hence not sufficiently ‘hard’) despite being very important. Moreover, they would likely rely on a mix of recent empirical evidence, subjective forecasts, and theoretical arguments, meaning they would not fit neatly into the standard typology of research. An economist interested in AI would benefit more, professionally, from writing papers that use the standard toolbox of causal inference or structural estimation from historical data, or pure theory, to speak about phenomena like technological unemployment from incremental automation.

As in the climate change literature, the news on this front is not all bad. Over the past few years, economists have become increasingly uneasy about the potential consequences of AI, and abandoned some of their previous complacency (e.g. Acemoglu and Restrepo, 2018; Autor, 2022). This bodes well for their receptiveness to longtermist perspectives on AI. Moreover, economists have recently started to write speculative papers about artificial intelligence that take longtermist assumptions seriously (e.g. Aghion et al., 2019; Agrawal et al., 2019; Cockburn et al., 2019; Korinek, 2019; Korinek and Stiglitz, 2019, 2020, 2021; Trammell and Korinek, 2021). Many members of the community are hence *aware* of longtermist perspectives on AI and seem to think they should be taken seriously.

Unfortunately, this work has not yet penetrated the leading mainstream economics journals, and does not seem poised to, for the reasons listed above. Many of the aforementioned articles are by some of the most prominent and respected economists within the profession, yet at the time of writing all are either unpublished working papers or book chapters (the latter widely viewed as a publishing venue for papers that are otherwise unpublishable by reputable refereed journals). While individual *economists* have worked on artificial intelligence from a longtermist perspective, the economics *profession* is not close to doing so due to its systematic hardness bias. Economists must become much more pluralistic before the profession can start to substantively contribute to longtermist AI research.

5. Why Economists? Why *Academic* Economists?

If the kind of research that would be useful for longtermists is radically different from the kinds of research that academic economists have chosen to specialize in, why put the burden of conducting this research on academic economics? Why not suggest that the mantle of conducting useful longtermist research be taken up by other social scientists more used to dealing with qualitative or historical information, or even by economists at research think tanks and other organisations, who are free of the publishing pressures for promotion in academia and the consequent 'tyranny of the top-5'?

This question really has two parts. First, why economists rather than other social scientists? Second, why should *academic* economists participate rather than leaving this to economists working outside of academia?

The answer to the first question is simple. Producing credible and useful longtermist research will require a cooperative effort across many social sciences. Economics, in particular, can offer a number of important concepts, methods, and stylized facts that are relatively neglected by other disciplines. These include:

- Concepts of equilibrium and methods of systematically analysing the disorganized behaviour of many actors.
- A typology of the ways in which the decentralization of choices (i.e., without an optimal, mythical social planner) can lead to perverse consequences — including negative and positive externalities, under-provision of public goods, and negative consequences from information asymmetries or from monopolistic market power.
- Practical tools developed by some specialised fields of economics (e.g. mechanism design or non-linear econometrics).
- Robust frameworks for thinking about quantitative causal inference.
- Ideological blind spots that may be *different* from blind spots in other disciplines, meaning that economists could contribute beneficially to a diversity of thought (for example, economics is the least left-wing social science, and the social science most friendly to markets).

These concepts and methods can contribute, for example, to explanations for why nations underinvest in existential risk reductions, and the corollary of what policies may increase that investment. They can explain why privately funded artificial intelligence research will produce, possibly inevitably, a fast race towards artificial general intelligence which may be harmful to society's longer-term interests. They can also tell us how scientific grant-making institutions or prediction markets should be designed to incentivise the most useful (and the least dangerous) AI research; they can help us design better long-term mitigation and adaptation strategies for climate change, and so forth.

Importantly, a full longtermist research agenda will demand qualitative, historical, descriptive, predictive, and explanatory work *infused* with these conceptual frameworks and ideas. So a naïve division of labour where, for example, the qualitative work is fully delegated to sociologists and historians, and economists are off in their own corner writing theoretical models of externalities or analysing retrospective data, would not produce the necessary research portfolio. Instead, economists must get their hands dirty with 'softer' kinds of research and collaborate much more closely with researchers from other disciplines.

Second, why should *academic* economists be involved in these efforts, rather than just economists in governments, international organisations, or think tanks? Those working outside of academia are relatively unconstrained by academic norms and closer to practical decision-makers; wouldn't they be a better fit for this kind of research? Three reasons lead us to argue that both academic and non-academic economists should be involved.

First, academic economists form a large pool of talented individuals motivated (at least in part) by the intrinsic reward of finding convincing answers to important questions. With better incentives, they would provide an additional, and much larger, arsenal to draw on in addition to the efforts of non-academic economists. Second, academia has a unique culture of transparency and intellectual rigor that — while imperfect in many ways — still offers advantages relative to the culture often prevalent in these other institutions. We believe that economics stands out in academia along these dimensions; consider economists' famously intellectually combative and critical (and sometimes, unfortunately cruel) seminars, as well as recent moves towards reproducibility and research transparency in the discipline (moves that are still far from complete, but seem to be leading other disciplines).

And finally, academics are also teachers, and thus exercise large influence over future generations of talent. Getting academic economists interested and active in longtermist research will therefore create large trickle-down effects to their students, which can percolate for many years (or indeed generations). Anton Korinek, one of the most prolific economists working on this topic, developed a Coursera MOOC (Massive Open Online Course) on AI, with a small section devoted to longtermist issues. This kind of investment in generating interest in the questions posed by longtermist research agendas will not be possible if longtermist economic research stays mostly outside of academia.

6. Alternative Explanations

As one reviewer of this chapter noted, there may be alternative explanations for the reluctance of economists to engage with a longer-termist research and policy agendas. One explanation is that economists, like many other people (and many other academics) have not been exposed to or actively reject the basic tenets of longtermism, and therefore do not view it as a plausible focus of research. Another is that economists' research is responsive to the interests of major policy institutions (governments and international institutions like the IMF or World Bank) which are not currently interested in longtermist policymaking.

With respect to the first explanation, we think the main problem is that most economists have not heard of longtermism, not that they actively reject it. As the general public and intellectual prominence of longtermism increases, we are optimistic that economists will be receptive. It is true that, as we noted earlier, economists traditionally adopt a conservative approach to the estimation of normative parameters. On the other hand, and partially due to the demands of mathematical tractability, impartialist utilitarian welfare frameworks are practically the only way economists know how to think about normative questions, which bodes well for their receptiveness to longtermism.

Still, we think exposure to and acceptance of longtermist ideas is necessary but not sufficient to compel academic economists to engage in longtermist research. There are many important questions that economists *currently* ignore because of overly strict methodological standards. The addition of new and important longtermist questions to that list will not necessarily change methodological standards by itself. An active push for changes in methodological norms and standards is needed.

With respect to the second explanation, it is true that interest (and funding) from the policy arena – for example from the World Bank or the European Union – plays a role in determining the research focus of economists. However, there is reason to be optimistic about the future direction of these institutions. Economists' rapidly growing interest in climate change in the past couple of years, and the 2020-2021 surge of interest in pandemic economic research, arose in part because of interest from policymakers and funding bodies. Even so, we still view economists' methodological standards as a crucial bottleneck on the ability of funding bodies to entice economists into longtermist research. Most economists would easily choose a top-5 publication over a large research grant, and are hence more responsive to the expectations of their peers than to grantmaking institutions.

7. Practical Recommendations

What does our discussion in this chapter imply for what institutions and economic researchers interested in longtermism ought to do? As economists, we are professionally reluctant to offer recommendations that are not backed by cleanly identified causal estimates and contingent on acceptance of a clearly-specified welfare framework. But in the spirit of practicing what we preach, we will offer one (informal and speculative) recommendation.

We think longtermists should be clear-eyed about the difficulty of attracting career-concerned economists to longtermist research. For example, the Global Priorities Institute runs scholarships and conference programs for economics PhD students that encourage them to engage in longtermist research. But most PhD programs implicitly or explicitly hold up the acquisition of a tenure-track academic job as the ultimate goal for PhD students, and the unfortunate fact is that---given current methodological standards---doing longtermist research is a bad career move for PhD students who have this goal.

We are hence pessimistic about initiatives that target early-career researchers. The problem is that tractable alternatives are not obviously available. One strategy that might be worth considering is instead targeting initiatives at the profession's gatekeepers---the senior economists who edit journals, influence hiring, and thereby shape the direction of research. The economics profession is very centralized and is thus more amenable to "top-down" than "bottom-up" change. Senior economists with secure careers have the freedom to pursue research that might not be currently professionally rewarded. In this case, at least, change might be best pursued from above.

References:

Acemoglu, D., and Restrepo, P. (2018). The Race Between Man and Machine: Implications of Technology for Growth, Factor Shares, and Employment. *American Economic Review* 108(6): 1488-1542

Acemoglu, D., Naidu, S., Restrepo, P., and Robinson, J.A. (2019). Democracy Does Cause Growth. *Journal of Political Economy* 127(1), 47-100.

Aghion, P., Jones, B.F., and Jones, C.I. (2019). Artificial Intelligence and Economic Growth. In A. Agrawal, J. Gans, and A. Goldfarb (eds), *The Economics of Artificial Intelligence: An Agenda* (University of Chicago Press), 282-289

Agrawal, A., McHale, J., and Oettl, A. (2019). Finding Needles in Haystacks: Artificial Intelligence and Recombinant Growth. In A. Agrawal, J. Gans, and A. Goldfarb (eds), *The Economics of Artificial Intelligence: An Agenda* (University of Chicago Press), 282-289

Akerlof, G. (1978). The Market for ‘Lemons’: Quality Uncertainty and the Market Mechanism. *Quarterly Journal of Economics* 84(3): 488-500

Akerlof, G. (2020). Sins of Omission and the Practice of Economics. *Journal of Economic Literature* 58 (2): 405-418.

Andre, P. and Falk, A. (2021). What’s Worth Knowing? Economists’ Opinions About Economics. *IZA Discussion Paper No. 14527.*

Anthoff, D., Tol, R.S.J. (2014), Climate policy under fat-tailed risk: an application of FUND. *Annals of Operations Research* 220: 223–237.

Autor, D. (2015). Why Are There Still So Many Jobs? The History and Future of Workplace Automation. *Journal of Economic Perspectives* 29(3): 3-30

Autor, D. (2022). The Labor Market Impacts of Technological Change: From Unbridled Enthusiasm to Qualified Optimism to Vast Uncertainty. *NBER Working Paper*

Besley, T. and Dixit, A (2019), Environmental catastrophes and mitigation policies in a multiregion world. *Proceedings of the National Academy of Sciences* 116(12), 5270-5276.

Bjurström, A. and Polk, M. (2011), Physical and economic bias in climate change research: a scientometric study of IPCC Third Assessment Report. *Climatic Change*, 108(1), 1-22.

Chan, G., Carraro, C., Edenhofer, O., , Kolstad, C., and Stavins, R. (2016). Reforming The IPCC’s Assessment of Climate Change Economics. *Climate Change Economics* 07(1).

Cockburn, I.M., Henderson, R., and Stern, S. (2019). The Impact of Artificial Intelligence on Innovation: An Exploratory Analysis. In A. Agrawal, J. Gans, and A. Goldfarb (eds), *The Economics of Artificial Intelligence: An Agenda* (University of Chicago Press), 115-146

Corbera, E., Calvet-Mir, L., Hughes, H., and Paterson, M. (2016), Patterns of authorship in the IPCC Working Group III report. *Nature Climate Change*, *6*(1), 94-99.

Cromar, K., Howard, P., Vásquez, V. N., and Anthoff, D. (2021), Health impacts of climate change as contained in economic models estimating the social cost of carbon dioxide. *GeoHealth*, 5, e2021GH000405.

Cropper, M.L, (1976), Regulating activities with catastrophic environmental effects. *Journal of Environmental Economics and Management*, 3(1), 1-15.

Dasgupta, Partha (2019). *Time and the Generations: Population Ethics for a Diminishing Planet*. (Columbia University Press, NYC).

Dell, M.,Jones, B.F, and Olken, B.A. (2012), Temperature shocks and economic growth: evidence from the last half century. *American Economic Journal: Macroeconomics*, 4(3), 66-95.

Finkelstein, A., and Hendren, N. (2020). Welfare Analysis Meets Causal Inference. *Journal of Economic Perspectives* 34(4): 146-167

Fourcade, M., Ollion, E., and Algan, Y. (2015). The Superiority of Economists. *Journal of Economic Perspectives* 29(1): 89-114

Greaves, H. (2017). Discounting for Public Policy: A Survey. *Economics and Philosophy* 33(3): 391-439

Hassler, J., Krusell, P., and Nycander, J. (2016). Climate policy. *Economic Policy*, 31(87), 503–558.

Heal, G. and Kriström, B, (2002). Uncertainty and Climate Change. *Environmental and Resource Economics* 22, 3–39.

Heckman, J., and Moktan, S. (2020). Publishing and Promotion in Economics: The Tyranny of the Top 5. *Journal of Economic Literature* 58(2), 419-470

Howard, P.H., and Sterner, T. (2017). Few and not so far between: a meta-analysis of climate damage estimates. *Environmental and Resource Economics* 68(1), 197-225.

Hsiang, S., Kopp, R., Jina, A., et al., (2017). Estimating economic damage from climate change in the United States. *Science* 356(6345), 1362-1369.

Ide, T. (2018). Climate War in the Middle East? Drought, the Syrian Civil War and the State of Climate-Conflict Research. *Current Climate Change Report* 4, 347–354.

Jones. C.I. (2022). The End of Economic Growth? Unintended Consequences of a Declining Population. *American Economic Review*, forthcoming

Kalkuhl, M., and Wenz, L. (2020). The impact of climate conditions on economic production. Evidence from a global panel of regions. *Journal of Environmental Economics and Management*, 103, 102360.

Karnofsky, 2022. The 'Most Important Century' Blog Post Series. URL: https://www.cold-takes.com/most-important-century/

Korinek, A. (2019). The Rise of Artificially Intelligent Agents. *Working Paper*

Korinek, A., and Stiglitz, J. (2019). Artificial Intelligence and Its Implications for Income Distribution and Unemployment. In A. Agrawal, J. Gans, and A. Goldfarb (eds), *The Economics of Artificial Intelligence: An Agenda* (University of Chicago Press), 349-390

Korinek, A., and Stiglitz, J. (2020). Steering Technological Progress. *Working Paper*

Korinek, A., and Stiglitz, J. (2021). Artificial Intelligence, Globalization, and Strategies for Economic Development. *Working Paper*

Lamperti, F., Dosi, G., Napoletano, M., Roventini, A., and Sapio, A. (2018). Faraway, So Close: Coupled Climate and Economic Dynamics in an Agent-based Integrated Assessment Model. *Ecological Economics*, 150, 315-339.

Millner, A. (2013). On Welfare Frameworks and Catastrophic Climate Risks. *Journal of Environmental Economics and Management* 65(2): 310-325

Millner, A., and Heal, G. (2022). Choosing the Future: Markets, Ethics, and Rapprochement in Social Discounting. *Journal of Economic Literature*, forthcoming

Mokyr, J., Vickers, C., and Ziebarth, N.L. (2015). The History of Technological Anxiety and the Future of Economic Growth: Is This Time Different? *Journal of Economic Perspectives* 29(3): 31-50

Nordhaus W.D. (2011). The Economics of Tail Events with an Application to Climate Change. *Review of Environmental Economics and Policy*, 5(2), 240–257

Nordhaus, W.D. (1992). An Optimal Transition Path for Controlling Greenhouse Gases. *Science*, 258(5086), 1315-1319.

Nordhaus, W.D., (1975). Can We Control Carbon Dioxide? *IIASA Working Paper* 75-63.

Noy, Ilan and Tomáš Uher (2022). Four New Horsemen of an Apocalypse? Solar Flares, Super-volcanoes, Pandemics, and Artificial Intelligence. *Economics of Disasters and Climate Change*, 6(2), 393-416.

Ramsey, F. (1928). A Mathematical Theory of Saving. *Economic Journal* 38(152): 543-559

Rodrik, D. (2021). How Economics and Non-Economists Can Get Along. *Project Syndicate Column*, URL: https://www.project-syndicate.org/commentary/economists-other-social-scientists-and-historians-can-get-along-by-dani-rodrik-2021-03

Romer, P.M. (1990). Endogenous Technological Change. *Journal of Political Economy* 98(5), S71-S102.

Stern, N. (2013). "The Structure of Economic Modeling of the Potential Impacts of Climate Change: Grafting Gross Underestimation of Risk onto Already Narrow Science Models." *Journal of Economic Literature*, 51(3), 838-59.

Stern, N., (2007). *The Economics of Climate Change: the Stern Review*. Cambridge University Press.

Stern, N., and Stiglitz, J.E. 2022. The economics of immense risk, urgent action and radical change: Towards new approaches to the economics of climate change. *The Journal of Economic Methodology,* forthcoming.

Timilsina, Govinda (2022). Carbon Taxes. *Journal of Economic Literature*, 60(4), 1456–1502.

Trammell, P., and Korinek, A. (2021). Economic Growth Under Transformative AI: A Guide to the Vast Range of Possibilities for Output Growth, Wages, and the Labor Share. *Global Priorities Institute Working Paper*

Tsur, Y., and Zemel, A. (2006). Welfare measurement under threats of environmental catastrophes. *Journal of Environmental Economics and Management*, 52(1), 421-429.

Tsur, Y., and Zemel, A. (2021). Resource Management Under Catastrophic Threats. *Annual Review of Resource Economics* 13(1), 403-425.

Ulph, A., and Ulph, D. (1997). Global Warming, Irreversibility and Learning, *The Economic Journal*, 107(442), 636–650.

Weitzman, M.L. (2009). On modeling and interpreting the economics of catastrophic climate change. *Review of Economics and Statistics* 91(1), 1–19.