Minor word-smithing was done throughout the document for style and typos.

Response to Referee 1

We thank once again Peter Haff for his insightful, constructive comments. We place comments in bold font and our response in normal font.

The variable E is defined (line 32) as the instantaneous rate of world energy consumption. E is power, but looks like it should be energy. So I would suggest that variables that are rates rather than stocks/quantities should be designated by an overdot or other indicator...

This is difficult. At one level, we entirely agree. At another, we have decided with some misgivings to adopt the traditional notation used in energy economics where E and Y are both rates. For example, if we used $\dot{Y}$, which is common in economics, it would be almost certainly misinterpreted as a term expressing economic growth, precisely what we want to avoid. What we have done to address the concern is clarify that E_i and Y_i remain rates, effectively yearly averages of the instantaneous quantities E and Y.

Eq. (3) is in my view a key summary of the results of the present paper. As such, its implications might be emphasized a little more fully...

We have added the following paragraph:

In the hypothetical limiting case of $dE/dt = 0$, the world attains a sort of metabolic steady-state characterized by a balance between energetic and material inputs and outputs. Energy consumption maintains a fixed rate, but also there is no real *economic production. Nominal production may remain, but it is completely eroded by inflation. The case of economic collapse may not be survivable. If so, the point at which $dE/dt = 0$ may only represent a temporary marker on a pathway to more complete thermodynamic collapse with the steady-state condition of $E = 0$. A distinction must be made with the quite different steady-state condition where $dY/dt = 0$, namely one of constant inflation-adjusted economic* production *Y, or zero GDP growth, as this would imply continued expansion of energy demands at rate Y/w. In the constant GDP growth case with fixed $d\ln Y/dt$ energy consumption accelerates.*

Response to Referee 2

We place comments in bold font and our response in normal font.

Garrett's current paper appears to use roughly the same methods as his 2009 Climatic Change article, which Steve Schneider asked Danny Cullenward and Lee Schipper to review. Garrett doesn't cite this critique or otherwise discuss its concerns. Nor does he address the critical differences between exergy and the primary energy consumption accounting paradigm reported in the BP statistics. He just runs some new data through the same basic calculations. The Cullenward et al response lays out the problems with Garrett's work very clearly, and it applies just as well to Garrett's latest article. The abstract summarizes things well:

"Uncertainty in the trajectories of the global energy and economic systems vexes the climate science community. While it is tempting to reduce uncertainty by searching for deterministic rules governing the link between energy consumption and economic output, this article discusses some of the problems that follow from such an approach. We argue that the theoretical and empirical evidence supports the view that energy and economic systems are dynamic, and unlikely to be predictable via the application of simple rules. Encouraging more research seeking to reduce uncertainty in forecasting would likely be valuable, but any results should reflect the tentative and exploratory nature of the subject matter."

According to the editor Michael Oppenheimer handling the Cullenward article, it was not peer-reviewed. The article included personal attacks by way of reference to a cartoon and no quantitative analysis to support the contention in the abstract repeated above. Further, the article claimed that the scaling between energy and cumulative production we claimed in Climatic Change, and through more extensive analysis in the ESDD article here, was incorrect by making a straw man argument: there does not exist a fixed ratio between energy consumption and economic output. Specifically, Cullenward et al state " Indeed, the trend we observe in the data—a declining energy/GDP ratio—would seem to be the counterargument to Garrett's model formulation, per inflation-adjusted 1990 USD.". This changing E/Y ratio, which is true, was explicitly noted both in the Climatic Change paper and in the article here. Figure 1 of this paper show a declining energy/GDP ratio, precisely what the reviewer and Cullenward claim invalidates our argument. It appears that neither Cullenward et al., nor the reviewer, understand the primary thrust of the arguments we make, a basic relationship taught in introductory calculus, or even arrived from casual observation, that cumulative and instantaneous quantities do not evolve at the same rates.

In terms of specific issues with Garrett's analysis, there are a few key points:

1) Primary energy is calculated in different ways, and it's important to correct for differences between different sources. Garrett's averaging of primary energy statistics between sources that use inconsistent conventions for estimating primary energy of non-combustion resources (eg EIA and BP) shows that he doesn't appear to understand these subtleties.

Taking the period from 1980 to 2018 for which both EIA and BP values are published, the difference between the two datasets is small relative to the growth, that is the difference is $8.5 \pm 1.5\%$ compared to a near energy consumption tripling. There is no trend in the difference either. So, whatever the subtleties in the choices made by the EIA and BP, the difference does not affect the constant scaling claimed in the article. We have added to the appendix:

The difference between the values in the BP and EIA data sets is significant, $8.5 \pm 1.5\%$*, but it is steady, and small relative to the 180% increase in energy consumption over the time period considered here.*

2) Primary energy is the wrong metric. Exergy accounts for both primary energy and ENERGY QUALITY (which reflects different sources' ability to do work),and that's what any analysis of long-term trends should be using. Electricity has the highest exergy of all energy forms, and the substantial shift in the percentage contributions of electricity vs. other fuels over time shows that this effect can't be ignored.

There appears to be some confusion in this remark about the term exergy and primary energy. Exergy is jargon for the amount of energy that is available to do work. Primary energy is also a form of exergy, as it too represents the potential energy for work by civilization as a whole. Now, in energy economics, a more restrictive definition of exergy is often employed wherein, as the reviewer points out, electricity has higher exergy than, say, combustion. Electricity in a toaster makes toast and heats the home, whereas natural gas only heats. This distinction may be useful from the perspective of a home, but doesn't account for the energy that must have been made available to produce electricity. Ultimately, all potential energy that is available to enable civilization activities of whatever kind must be traced back to primary energy. Or, all primary energy consumed by civilization is used to sustain civilization activities, however inefficiently, ending its lifetime as waste heat, which is eventually radiated to space. Now, there is a slight subtlety, which is that a small fraction of this energy is available to contribute to the growth of civilization. The characteristic timescale for sustenance of civilization activities is most probably one day. The characteristic time scale for growth, taking civilization's energy consumption growth rate to be 2.2% per year, is about 30 years. Thus, the two basic processes of sustenance and growth through consumption of civilization "exergy" can be readily separated. We claim that the energy/exergy required to sustain civilization's daily activities, the primary energy, is tied to historically cumulative real economic production. That is what is supported by the data.

The text beginning Section 2 now reads

To avoid complications associated with the details of trade, interactions between economic sectors, or distinctions between energy types, this study is focused only on global quantities, as described in the Materials and Methods below. Annual primary energy sources, those that are available to drive civilization activities of whatever type, are consumed and ultimately dissipated as waste heat at a rate that can be expressed as an instantaneous quantity E *(e.g., Terawatts) or a yearly-averaged quantity* E_i *with units of power (e.g., either Terawatts or Exajoules per year) (Garrett et al., 2020).*

3) GDP is a terrible metric of human activity and well being. In addition, changes in the structure of the economy over time over decades are so large as to make the expectation of structural constancy unreasonable.

The paper is not focused on discussions of metrics of human activity or well-being, which we would agree are not well characterized by the GDP. GDP is purely a financial quantity that we show can be linked to a thermodynamic quantity. Civilization activities encompass far more than human concerns, including flows along networks that include machines, mines, roads, shipping routes and much more. We are unsure what structural constancy is being referred to or, how our study neglects it as the article explicitly remarks on how much has changed in 50 years at the beginning of Section 3. We only assume that thermodynamic laws are constant over the time period being considered.

4) Treating energy and economic statistics like they are as reliable as measurements of physical systems is a mistake that many physicists make, but it's a category error. Physical determinacy for economic systems is a logical fallacy to which some in the degrowth community (among others) appear to be susceptible, but it's not a justifiable intellectual position.

All physical measurements are uncertain, and to widely varying degrees. Witness for example the burgeoning study of exoplanetary properties, where almost nothing is known. Unfortunately economic statistics are often not reported with error bars. For example, world GDP is reported by the World Bank and United Nations to either the dime or the penny. While, presumably, these statistics are far more uncertain than such precision, no readily available basis is provided for estimating the magnitude of the uncertainty. That said, the results presented here are relatively insensitive to relative uncertainty in yearly economic statistics because a 50-year time series is considered over which GDP increased by a factor of 4.5.

The text in the Appendix now reads:

Uncertainties in UN, WB and PWT economic values are not published. They are assumed here, as with the energy estimates, to be small compared to the many factor increase in their sizes.

Response to Referee 3

We thank the reviewer for constructive comments and place the reviewer comments in bold font and our response in normal font.

I have read the paper and the other reviews. I have a couple of small criticisms that can be fixed quite easily and a couple of others that might need more thought. The first criticism (p 4, is that their comment about production functions incorporating E explicitly is incomplete and inaccurate. The LINEX function used by Ayres & Warr, where E is a third factor and the variant where E is replaced by useful work U are explained in detail in chapters 5 and 6 with numerical results in Chapter 7 of "The Economic Growth Engine" by Ayres and Warr (Elgar, 2009). That reference should be cited in addition to the 2003 paper. Garrett's comment that exponents are "physically and dimensionally non-sensible" is unjustified and unnecessary. If the comment is important it should be explained better. My second comment is that the discussion of Equation 3 is vary opaque. I've reread it several times and I don't see the point. What I do see is that it could make make sense to express a possible proportionality between capital stock K and total stock W. If not, why not? But that possibility wasn't considered. Wouldn't it make sense? Beyond that, the purpose of the paper is unclear. The discussion of Lotka's wheel is interesting but where does it take us? I am not persuaded that we now have a better production function, especially given the excellent results of the LINEX function and recent developments in that domain.

The possible proportionality between K and W is shown in Figure 1. They do not scale. Otherwise, the text after Equation 3. has been written to read

Eq. 3 expressing economic production as proportional to an increase in energy demands, that is its derivative with respect to time, differs from prior approaches that tend to ignore any explicit mention of the role of energy. Where energetic demands are considered, the production functions are complex, and the dimensions of the problem are not considered. Rather than starting with the constraint that the factors of economic production, of whatever combination, must tally dimensionally to units of currency per time, quantities such as dimensionless capital, labor, and useful work are set to non-integer exponents, or are themselves placed in exponents (Ayres et al., 2003; Ayres and Warr, 2009; Lindenberger, 2011; Keen et al., 2019). While the functions can be shown to reproduce past behaviors for specific nations, it is only by way of specifying coefficients, or "output elasticities", that are themselves determined from past economic conditions, and that vary according to the time period considered. The production functions become moving targets, and therefore cannot be presumed to express something fundamental about the economic system. As attributed by E. Fermi to J. von Neumman "with four parameters I can fit an elephant, and with five I can make him wiggle his trunk." Here, by contrast, Eq. 3 is simple, dimensionally reasoned, and assumes that w is a constant, so it can be readily refuted (or supported) with data.

Personally, I think more could be done with this material, and it would be interesting to see how it looks just for the US (which used to be quite self-sufficient) back to 1860 or so.

We appreciate the comment. The remark about self-sufficiency is important however as it would need to be accounted for. For the moment, since the intent is to address such global scale problems as carbon dioxide emissions, we are limiting our studies to global scale quantities.