This is a short and interesting paper. It is well written. It is a decent, but perhaps not perfect, fit for the journal.

We think it's a perfect fit for the following reasons:

Humans are most definitely part of the Earth system

The analysis is fundamentally about dynamics

It complements earlier publications in ESD by for example Garrett and Raupach on related issues

Are the harmonics fully related to the data or a consequence of the methodology?

Observations of long waves have been filled under "a curious finding with no consequence" by economists for many decades. This is hard to understand given how much has been invested in attempting to understand higher frequency business cycle events and that systemic traits like this potentially offer some predictability to global scale behaviour over the medium term. That said, you are right to query the robustness of the frequency analysis given the results can often be a bi-product of the analysis. We cannot offer definitive reassurance on this. However, we have used two independent methods to try and make sure the results are not simply a bi-product of the methodology. The first is the one reported in the paper, namely to fit an auto-regressive (AR) model to the time series and then look at the frequency characteristics of that. To make sure we didn't cherry pick the AR model to give us a particular result, we investigated all AR models from 5th to 50th order. These are what are shown in Figure 2 and, as you can see, most AR models support the ~60 years plus harmonics story. The second is the more familiar Fourier Transform (FT) of the data into the frequency domain. Figure R1 attached shows how this compares to the first method. As can be seen the peaks line up showing the methods agree that there are frequency components and approximately on what the frequency components are. Not surprisingly, the AR method provides a clearer picture given the estimation of the AR model filters out noise that is otherwise partially obscuring the FT result. These FT results will be added to Figure 2 in the manuscript.

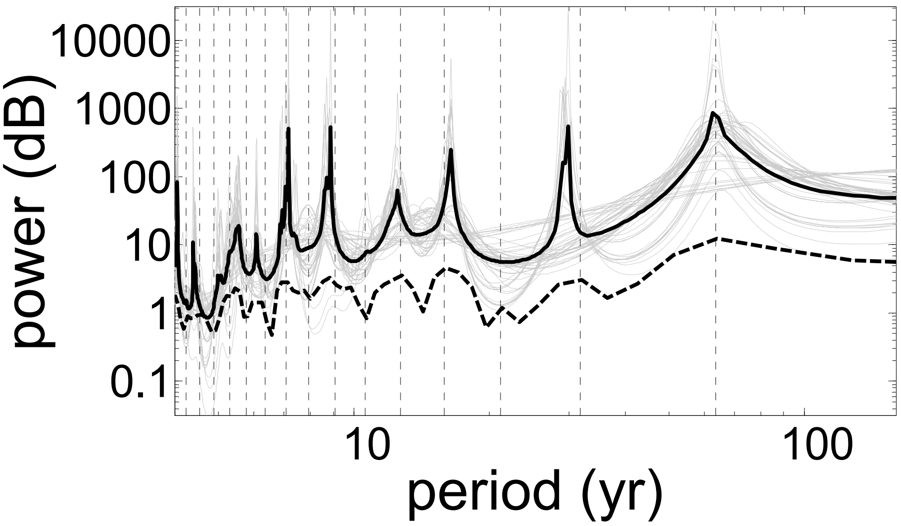

Figure R1. The period (frequency^{-1}) power relationship for the RGR estimates for global primary energy, 1850-2010. The grey lines are the spectra of all individual autoregressive models (5th to 50th order) fitted to these relative growth rate series of which the black line is the mean spectra. The vertical lines mark a 62 year cycle and its harmonics. The black dashed line is the spectra derived from the Fourier transform of the same data.

The authors write "These harmonics are unlikely to occur if a and b did not possess ~60 year periodicity", but is this opinion or something that is well known in statistics (if so, reference)?

It is physics rather than statistics. If a periodic signal is observed, then one would expect to observe its harmonics too unless something more complicated is going on.

If the fundamental frequency f, then the harmonics would be 2f, 3f, etc. Likewise, the time periods T, T/2, T/3, etc. If T=60 corresponds to the time scale, there would be harmonics at 30, 20, 15, etc? Why are you missing some of the harmonics here (3rd,6th, 8th)?

We don't know specifically why those periodicities are missing but superficially it is because either the system is behaving non-ideally (nonlinearly) or it is down to the quality of the data, or both. Neither of these explanations should be surprising given the system in question and the nature of the observations of it. Any attempt to go beyond these explanations at this stage would be giving a very false impression of our understanding of this system, although a full description of global energy use should account for this in future.

I would suspect the periodicity in CO2 is related to the periodicity in energy, as the y/x is rather stable (does it have a 60 year cycle?).

It does appear to, as shown in Figure 2. The text also reads:

“Figure 2 shows the mean response of an ensemble estimate of the frequency spectrum of the perturbations in both a(t) and b(t). From this we see that both a(t) and b(t) appear to exhibit ~60 year periodicity”

Interestingly, Figure 2 suggests the spectra of primary energy and CO_2 emissions differs around the 8 year mark suggesting some dynamics operating between the two around this timescale.

Following on, I would expect the energy to be related to GDP or population. If you could piece together a GDP and population time-series over the same period, it would be worthwhile to repeat the analysis for those variables. Do all these datasets have the same periodicity?

We agree GDP, population and many other factors co-evolve with energy use. However, as highlighted by e.g. David Stern’s work, proving causality between these metrics is a problematic enterprise. The work we present is not trying to identify what causally determines the evolution of global energy trends. Instead, we observe that in the last 160 years it has demonstrated relatively stationary endogenous systemic tendencies (exponential growth with oscillations) and we simply use this to forecast over the near term.

A frequency analysis of long run global GDP has already been done and we point the reader to this (Korotayev and Tirisal, 2010). They used a different method to us but found ~55 year wave plus harmonics in global GDP. Their results were somewhat more uncertain because of the method they were using and the fact that the GDP data are poorer quality than primary energy use. We have the Madison dataset on which this work was based but not the homogenised long run global GDP time series derived from it by the papers authors and so have not been able to compare results directly (Maddison global GDP only goes back to 1950 as an annual time series). We also struggle to see what value there would be in repeating Korotayev and Tirisal (2010) analysis and deem it to be beyond the scope of our study. The available, annually sampled global population data also only extends back to 1950 so again will not say anything about long wave dynamics. That said, it is clear simply from visual inspection of the population data that they do not express the oscillatory dynamics seen in either GDP or energy use.

Korotayev, A. V. and Tsirel, S. V.: A Spectral Analysis of World GDP Dynamics: Kondratieff 15 Waves, Kuznets Swings, Juglar and Kitchin Cycles in Global Economic Development, and the 2008–2009 Economic Crisis, Struct. Dynam., 4, 1, 2010.

If so, where does the periodicity originate, in population, in GDP, in energy? This would really strengthen the analysis.

Again, this paper is not about identifying causal relationships between e.g. GDP and energy use, there are plenty of other papers on this complex issue. Even if we could analyse population and GDP in the same framework we have applied for 160 year energy data, it is not at all clear how you could use the different spectra to assign causality as is being requested here. In fact, there are better approaches for this than to investigate the data in the frequency domain.

What about if you repeat the analysis on the temperature or co2 concentration record over the same time period? I am not suggesting there is a direct link (either way), but curious what the method might say about it. Suppose it came up with a 60 yr periodicity in temperature or co2? What would that mean about the energy and emissions periodicity? Would this suggest the periodicity is method based?

Why would it suggest the finding is method-based? Given we can link emissions and atmospheric CO2 mechanistically we would expect them to share common periodic components other than the global carbon cycle/climate system should act to filter those components and the climate system appears to have the potential to introduce its own long-wave dynamics. Again, we believe this is outside the scope of the current paper.

The authors argue this is a superior method than the alternative scenarios, but this can be tested. What about running the analysis to 1990 and project to 2010, likewise run to 2000 and project to 2010. China slows explosive growth since 2000 and this last 10 years may have an impact on the periodicity.

Interestingly there is an IIASA paper published pre-1990 (which I am struggling to locate right now) which did this for 1990 to 2010 forecasts based on long-wave dynamics in energy use prior to this. They correctly predicted the upswing post 1990. When I find it I will most definitely cite it. Beyond this, these suggested in sample evaluations are valuable and will be added to the manuscript.

How do the results change if the analysis starts in 1900 and not 1850?

Removing the more uncertain 1850 – 1900 data does not affect the results substantially (see Figure R2 attached). However, in analysing long term trends it is advantageous to have as long a data series as possible and there is no justification for not using the 1850-1900 data given it is available.

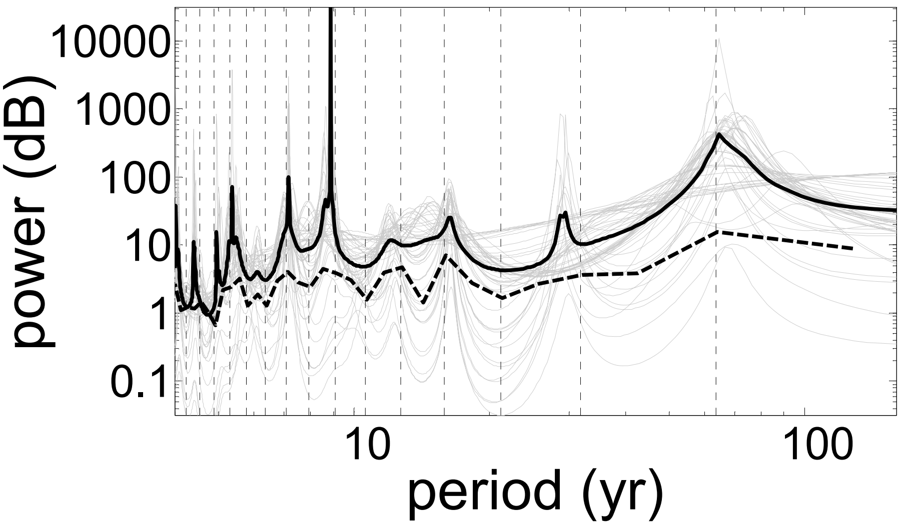

Figure R2. As in Figure R1 but only using the 1900-2010 data

I agree with the arguments to stick to 2020 forecasts, but surely the method is more ambitious! This is what the authors argue in the conclusion? You have a 150 year record, why not project out to 2100? It is a baseline after all. It would be interesting to see what you may learn from that.

It's a 160 year record. We qualitatively speculate on possible directions of travel immediately after 2020, but it is not sensible to stretch the past = future framework quantitatively beyond this point because: Firstly, the uncertainty bounds grow so rapidly as to embrace a very broad range of possible scenarios. Secondly, systemic change is possible (even if it hasn't happened thus far). Thirdly, 2020 has such political significance it provides a meaningful objective for the forecast. Forecasting to 2100 is foolhardy in our view.

A part of the reason the IPCC baselines level out in 2100 is due to a stabilisation in population. Would your method still work if a key driver changes? (this relates also to the point on using population and GDP in the analysis).

As discussed in the paper, if there was significant systemic change affecting patterns of global energy use then we would observe a deviation from the observed historic BAU trajectory of exponential growth with oscillations. This may not be observable in levels of global energy use, but would be observable in its relative growth rate. Interestingly, the growth in global population appears to have gone sub-exponential back in the 1960's whereas global primary energy use did not suggesting they became systemically decoupled around then.

Minor comments:

Abstract: Put the unit on 14Gt/yr. I presume you mean 14GtC/yr.

Correct

"emissions must follow a Business-As-Usual (BAU) trajectory". I think "will" instead of "must", but I guess by construction this is really how you have defined BAU?

It is, but we are happy to re-word to "will".

Instead of BAU, I think more common usage would be to say "baseline" or "reference".

We dislike both reference and baseline. These are terms suitable for comparative simulation exercises. We specifically mean BAU as an absolute forecast. Whether climate modellers (for example) want to then assume it as a reference/baseline to explore 'what if' scenarios is up to them.

Second paragraph. This may be a bit misleading. Up until AR5, the IPCC only considered "baseline" scenarios. The "IPCC scenarios" did not include climate policies. The fact that emissions sit at the "top end" of scenarios is nothing to do with mitigation. I think it is worth stating this explicitly. See for example the SRES http://en.wikipedia.org/wiki/Special_Report_on_Emissions_Scenarios

We appreciate the methods used to construct scenarios needs to be faithfully reported, and that explicit mitigation policy may not have featured in their derivation. However, their practical use certainly implicitly recognised the role of policy to, for example, give rise to emissions reduction trajectories. However, this has no bearing on our statement that, the fact that observed emissions sit at the top end of the portfolio of scenarios developed by the IPCC is everything to do with the lack of effective mitigation actions over the last 25 years.

As an aside, the scenarios sitting at the top end is perhaps not necessarily due to the different world views either, one would have to go back and compare the underlying drivers in SRES with what happened in reality.

Sorry, but we don't understand this point.

As an update to Le Quere et al 2009, perhaps http://www.nature.com/nclimate/journal/v3/n1/full/nclimate1783.html

Noted

Section 2.1. The IPCC uncertainty on LUC is larger than 20%. You can get updated fossil and LU emissions from here, but you don't need to update for the analysis (an optional extra) http://www.globalcarbonproject.org/carbonbudget/

Corrine pointed us to this source for the data and the 20% figure which we acknowledge in the paper.

Section 2.3. Why not include M=1 to 4 as well? M=1 would be no correlations? M=2 would be the simplest correlation?

Pre-analysis rejected these very low order autocorrelations, which is not surprising given the relative growth rates are clearly quite dynamic. M=1 means the growth rate this year is correlated to that last year only. M=2 means the growth rate this year is correlated to that last two years only, etc.

Figure 1: You should include RCP8.5, or the baselines from the AR5 https://secure.iiasa.ac.at/web-apps/ene/AR5DB/dsd?Action=htmlpage&page=about

RCP8.5 is included in Figure 1 in the form of the IIASA A2r scenario.

Figure 1: Interestingly, y/x seems good in the scenarios you compare with. It seems co2 is underestimated as energy is underestimated?

Correct, as stated in the text.

"These are approximately twice the current energy industry growth forecasts and one and a half times greater than the IPCC's worst case A1f scenario. In contrast, the 2020 carbon intensity forecasts are all similar (see Fig. 1d)."

Friedlingstein et al 2014 did projections to 2020, and it would be interesting to compare with these.
http://www.nature.com/ngeo/journal/vaop/ncurrent/full/ngeo2248.html

We are happy to include these forecasts for comparison. Clearly there are some important overlaps between this and our paper.