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Earth System Dynamics
Discussions

Interactive comment on "The "Business-As-Usual" growth of global primary energy use and carbon dioxide emissions – historical trends and near-term forecasts" *by* A. Jarvis and C. N. Hewitt

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This is a short and interesting paper. It is well written. It is a decent, but perhaps not perfect, fit for the journal. I recommend the article for publication after adequate changes.

The authors argue that global energy supply and CO2 emissions exhibit periodicity with a fundamental timescale of 60 years, and this relationship is a robust means of estimating "business as usual" emissions. The results are intriguing, but I need convincing!

Is this just a curious finding with no consequence, or is there something deeper to the results?

I suggest the authors perform a few variations on their analysis to tease that out the robustness of the relationship further. I have a few comments more directed towards methods or alternative analysis, and a few minor comments.

Methodological comments:

I am not a statistical expert, and so I cannot evaluate the robustness of the particular methodology used. However, I have a few simple variations of the analysis that would make me feel more comfortable with the findings and their implications.

* Are the harmonics fully related to the data or a consequence of the methodology? The authors write "These harmonics are unlikely to occur if a and b did not possess âĹij 60 year periodicity", but is this opinion or something that is well known in statistics (if so, reference)?

* 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)?

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

* 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?

* 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.

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

* 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.

* 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).

Minor comments:

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

* "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?

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

* 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

* 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.

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

* 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/

* 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?

* 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

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

* 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

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