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Earth System Dynamics
Discussions

Interactive comment on "Can Limits to Growth in the Renewable Energy Sector be Inferred by Curve Fitting to Historical Data?" *by* Kristoffer Rypdal

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Major scientific points:

1. Why are only 4 of the 19 points independent?

Response: This question was also asked by another referee. In the revision I will present an additional figure explaining this. It arises from the observation that when considering the residual after subtracting a fitted curve it looks like a smooth curve on time scales shorter than five years, i.e. the autocorrelation time is of this order (observe the slow, smooth oscillation of the data points around the fit curves in Fig. 1). With such a small sample (19 points) this is of course a very crude estimate, but it invalidates the underlying assumption that this is a sample of 19 randomly distributed data points.

2. How would random exclusion of data points inform the error estimation and associated model selection?

Response: I am not sure I understand the point here. I demonstrate theoretically and by example in the manuscript (Fig. 1) that the result depends strongly on the last data points in the time series if the fit is made to the installed capacity itself. However, the result is insensitive to removal of the last data points if the fit is made to the log-data, and this is therefore the preferred method. It should be quite apparent from Fig. 1b that the fits of both models to the log-data are so good that random removal of a few data points would not change the fit parameters by much. Rather than exploring uncertainty by random removal of data points, I have produced Monte Carlo ensembles of realizations of the fitted model stochastic processes that reflect the variance of the residual log-data set after subtracting the fitted model from the log-data (Fig. 3a). Then I have explored model uncertainty by fitting the models to these realizations (Fig. 3b). This is a much more meaningful method of estimating model uncertainty (uncertainty of the fitted model parameters) than random removal of data points. After all, we do have those points, and we don't obtain more information by throwing away meaningful data.

3. What would be inferred if only the first or second 10 year data series of data of Fig. 1 was utilized - see p. 10 line 9?

Response: What would be inferred from using only the first part of the data series was indicated in Fig. 4 for the slightly longer consumption data series. Here, dropping the 10 last data points had almost no effect on the estimated model parameters. Again the fitted models both represent so good fit to the log-data over the entire period that the fit is insensitive to whether using the entire data set or only the first 17 points. What this essentially implies is that exponential growth is an extremely good model for the observed data, while the modeled point of saturation is either very uncertian (Fig.1) or too far into the future to be realistic (there must be physical limits to growth that haven't yet had an observable impact). Since a linear fit through the last few points in Fig. 1b

and Fig. 4 would yield a slightly smaller slope than a fit to the entire data set, a logistic fit would yield a lower growth limit, but the uncertainty would be huge since this local slope could be a fluctuation and not a trend.

Scientific minor issues:

4. Consumption preferable to installed capacity.

Response: Maybe I wasn't sufficiently clear on this point. Actually, the main reason I have used consumption as an additional data set is that this data set is almost a decade longer and thereby demonstrates even more clearly the exponential nature of the growth. But electricity consumption is also a more relevant measure than installed capacity, since it incorporates the effects of advances other than those that are expressed through the nameplate generation capacity. Renewables do not have to constitute a large fraction of the world's electricity production for such advances to take place. A generator's output may vary according to changing conditions at the power plant, in the power grid, or in the electricity market. Nevertheless, it turns out that the exponential growth rates for the two data sets are equal to the second digit, which indicates that the two types of data are roughly equivalent. The reason why the logistic fit to consumption time series stays close to the exponential for a longer period of time into the future is that the series extends further into the past, and hence the result becomes less sensitive to the lower slope of the last few data points.

5-9. These technical issues have also been raised by other referees and will be addressed in the revision.

Writing major issues:

10. Inflammatory remarks?

Response: I would be more than happy to remove inflammatory style, but I have a hard time seeing that the phrases mentioned by the reviewer are inflammatory:

"This type of scientific controversy is rooted in intellectual bias and/or lack of knowl-

edge." This could have been inflammatory if I accuse particular persons or a particular side in the controversy for being biased or unknowledgeable. However, what I am stating is basically that if contradicting results appear in the literature, at least some must be wrong.

"...have led some authors to search for sign of stagnating growth in historical data.." I can't see anything inflammatory in stating this fact. There is of course nothing wrong in searching for such signs.

"... since nobody believes that solar+wind will grow more than two orders of magnitude during the next two decades." This is not an inflammatory statement, but it could be formulated more carefully. My point is that the two models yield the same growth up to a consumption level more than two orders of magnitude higher than today. Such a consumption level is beyond reasonable physical limits, and implies that the limits to growth cannot be found from these historical data.

"... will embrace "results" like those presented by Hansen et al. and accept them as proven scientific facts." I may want to rewrite this entire paragraph, but I still can't see why it is inflammatory to state that some readers may take results published in the peer-reviewed scientific literature as proven scientific facts.

11. Focus on attacking one study, why not focus on how to improve forthcoming projections and estimates? Hansen et al. could be right for the wrong reasons.

Response: Although hypothesis testing and falsification is mainstream in the philosphy of science, it is not in very high esteem in scientific journals. The subject of the present paper is a hypothesis raised in the paper by Hansen et al. (2017). The hypothesis is not that there are specific limits to growth in the renewable energy sector (of course there are), but that these limits can be inferred simply from historical data. I do not agree that I am "continually attacking Hansen et al. (2017)," although it would be improper not to point out where they go wrong. My focus is on pointing at the correct way of making curve-fitting on more or less exponentially growing data and on how to make model

selection. The result of doing this correctly is that we cannot select rationally between logistic and exponential growth based on historical data, and that we cannot conclude anything about the limits to growth from such data. Somehow, the reviewer's view is that this "negative" conclusion implies that the "scientific knowledge it contributes is very limited in scope as the projections by Hansen et al. could be right for the wrong reasons." I find this conclusion very problematic. Would the scientific knowledge contributed have been more valuable if the conclusion were that we CAN conclude from historical data, i.e. are affirmative conclusions more valuable than negative ones? Are the projections of Hansen et al. more right, and my paper less relevant, if their projected limits to growth happen to be quantitatively correct by accident?

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