Reply to Hendry and Pretis

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Abstract

Hendry and Pretis make six methodological criticisms of our paper in which we questioned the anthropological interpretation of global warming (AGW) since 1880. We reject their criticisms and point out that they make the same methodological errors which they claim we made.

Two Misunderstandings

Hendry and Pretis (HP) misunderstand our paper in two important respects. The first misunderstanding is methodological. They write, "Beenstock et al (2012) purport to show that because temperature and greenhouse gases have different orders of integration they cannot be related." This misunderstands what we were trying to do. Our conclusion that temperature is stationary in first differences whereas anthropogenic forcings are stationary in second differences is not original, and is standard. The same conclusion had been reached more than a decade ago, and in the extensive literature we cited, by leading time series experts including James Stock (Kaufmann, Kauppi and Stock, 2006, 2010). We shall argue that HP's Tables 1 and 2 cannot be considered as a serious challenge to the standard model.

We never purported to show, as HP suggest, that just because temperature is difference stationary but anthropogenic forcings are stationary in second differences, temperature and anthropogenic forcing cannot be related. We clearly stated to the contrary in section 2.3 of our paper; differences in orders of integration do not necessarily refute AGW because temperature and anthropogenic forcings might be polynomially cointegrated. Our original contribution was to show that temperature, solar irradiance and anthropogenic forcings are not polynomially cointegrated.

The second misunderstanding is about greenhouse gas theory. Our clearly stated motivation was not to refute this well-established theory (see sections 1 and 4). Below we reiterate why the time series data that we and others use are inappropriate for these purposes. We emphasized that our paper is not about physics but about data

interpretation. Specifically, do data since 1880 suggest that temperature was influenced by anthropogenic phenomena? Just because greenhouse gas theory explains, for example, why Earth is warmer than it would have been without an atmosphere, does not automatically imply that rising greenhouse gas concentrations must have caused the increase in temperature during the 20th century. This misunderstanding makes HP's analogy between fatalities and motoring invidious and irrelevant.

Points of Agreement

First, we agree with HP (section 2.6) that rejection of a null hypothesis, such as AGW, does not necessarily mean that AGW is false due to type I error. This matter is discussed in section 4 our paper. However, our results mean that proponents of AGW are probably wrong. Second, HP's point (HP section 2.4 and 3.1) about linear v nonlinear representations of AGW is well taken. We make the same point ourselves in section 3.6 of our paper. This is why we included an extensive discussion on nonlinear cointegration tests of AGW, which do not however change our results. Third, we also agree with HP on the possible effects of data errors (HP section 2.5 and 3.2), which is why we carried out robustness checks in section 3.9 of our paper using alternative measurements of temperature and forcings. Fourth, we entirely agree with HP about the dangers of omitted variables (HP section 2.1), which is why we specify a broader range of anthropogenic phenomena than is common in the literature. HP need to be specific regarding which variables we have omitted.

HP's Criticism of the Standard Model

The most serious of HP's criticisms concerns their claim that a structural break occurred in the time series properties of the radiative forcing of carbon dioxide, or $rfCO_2$ (HP section 3.1). We discussed this matter in section 3.1 of our paper, where we conclude that $rfCO_2$ is nevertheless stationary in second differences. HP challenge this standard result. They relate this claim to the fact that direct measurements of CO_2 concentrations started in 1958. Just because the method of measurement changed does not in itself mean that the time series properties of $rfCO_2$ had to change. Also, structural breaks might have occurred even if the method of measurement had not changed. HP claim that during 1850-1957 $rfCO_2$ was stationary in first differences. The ADF statistics reported in their Table 1 are marginal in 3 out of the 6 versions reported. Therefore, this claim is weak at best.

The methodological literature on testing for structural breaks distinguishes two main cases; when the timing of the break is known (Perron 1989), and when it is not known (Zivot and Andrews 1992). In either case the conventional critical values for ADF are no longer valid. They become stricter when the timing of the break is known, and become stricter still when the timing of the break is not known. HP use neither of these stricter critical values for ADF. Instead, they use conventional critical values, which because they are insufficiently strict, risks falsely confirming the presence of a structural break.

It is usual to carry out sensitivity tests around the break-point. HP do not do this, but we have done so. We find that it makes almost no difference to ADF if the break-point is earlier than 1957. On the other hand, if the break point is after 1957 it makes a substantial difference to ADF, which becomes less negative, and therefore less statistically significant. If the break-point is 1960 instead of 1957 ADF (4 augmentations) increases from -3.1 to -2.48. If the break-point in Table 1 has been "cherry-picked", the critical values used by HP are incorrect in which case perhaps none of their ADF statistics is statistically significant. Their results in Table 1 hardly constitute a case for over-turning the standard result that $rfCO_2$ is stationary in second differences during 1850-1957.

HP also claim that since 1958 the first difference of $rfCO_2$ is trend stationary. This model means that $rfCO_2$ has a deterministic polynomial time trend, and deviations from this trend are stationary. Here too they arrive at this conclusion using the standard critical values for testing trend stationarity; they do not use the stricter critical values that are required when a break-point is involved. According to the standard model which HP challenge, $rfCO_2$ is stationary in second differences, which means that $rfCO_2$ has a stochastic polynomial time trend rather than a deterministic polynomial time trend as claimed by HP. However, HP do not test their model against its standard rival. This would have involved a comparison of the standard second difference stationary model with HP's alternative (that $rfCO_2$ was difference stationary until 1957 and its first difference was trend stationary subsequently). This comparison requires constructing appropriate critical values which are not currently available.

In summary, the results in Table 1 are insufficient to overturn the standard model after 1958 as well as before 1958. Similar criticisms apply to the results in Table 2 regarding rfN_2O. In our paper we pointed out that it is most probably no

coincidence that all of the six anthropogenic forcings in our study happen to be stationary in second differences. This stems from the fact that they share a common anthropogenic factor. An original result in our paper was to establish this claim empirically by showing that these forcings are cointegrated.

Whereas we have a unified model of anthropogenic forcings, HP have idiosyncratic models for each forcing. They treat each forcing separately, when for anthropological and perhaps physical reasons too they are probably related. They have one model for $rfCO_2$ and quite a different model for rfN_2O. They do not explain the physical and anthropological reasons for this. Although they do not discuss the other four anthropogenic forcings in our study as well as solar irradiance, we think that their piecemeal statistical analysis is conceptually wrong. The same conceptual error appears in Estrada and Perron (2012) who introduce arbitrary break-points into their cointegration tests, and who like HP reject the standard model without testing it.

Stationarity Tests

	d	**$rfCO_2$**		**Temperature**	
		ADF	**KPSS**	**ADF**	**KPSS**
1850-2007	**1**	-1.378	2.66		
	2	-6.806*	0.035*		
1880=2007	**0**			0.135	2.46
	1	-1.217	2.26	-8.228*	0.139*
	2	-6.329*	0.083*		
1910-2007	**0**			-0.005	1.82
	1	-1.134	2.02	-7.295*	0.090*
	2	-5.644*	0.055*		
1940-2007	**0**			0.230	1.37
	1	-1.310	1.51	-5.204*	0.168*
	2	-4.677*	0.058*		
1960-2007	**0**			0.047	1.69
	1	-2.111	0.924	-4.100*	0.087*
	2	-4.185*	0.030*		

Notes: d order of differencing. ADF includes 4 augmentations. KPSS bandwidth = 4.
* significant at p = 0.05.

We report ADF and KPSS statistics (see table) calculated over different observation periods for the first and second differences of $rfCO_2$ and the levels and first differences of temperature. These statistics clearly show that $rfCO_2$ is stationary in second differences regardless of the observation period. We add to this the previously mentioned results that during 1850 – 1957 ADF increases after 1957. Also temperature is stationary in first differences regardless of the observation period. We

see no reason to change the standard result in the literature regarding the time series properties of temperature and rfCO_2. Certainly HP's results do not constitute a serious challenge to the standard model.

The Physics of Greenhouse Gases

Although various aspects of the physics of the greenhouse effect have been studied by many great scientists for close to 200 years, the application of the ideas developed over this period to Earth's radiation budget is still a perplexing issue. Starting with well known facts we note that during the 30 years 1910 - 1940 Earth's average annual temperature, T_{av}, has increased by about 0.5°K (i.e. 0.5 degrees centigrade) while the CO_2 concentration in the atmosphere remained nearly unchanged. During the 35 years 1940 – 1975 T_{av} actually dropped by about 0.05°K (and in 1972 concerned scientists warned US president of that time, Richard Nixon, that humanity is on the brink of an upcoming Ice Age) despite the contemporaneous increase in CO_2 concentration by close to 40 PPM. Between 1975 and 1998 both CO_2 concentration and T_{av} have increased (the latter by about 0.3°K). Finally from about 1998 to the end of 2012, T_{av} remained unchanged despite an increase (of nearly 25 PPM) in the concentration of atmospheric CO_2. Therefore, there is more to T_{av} then the simple minded greenhouse effect by CO_2 molecules on Earth's radiation budget.

Given that CO_2 is a greenhouse gas, how can its concentration increase without affecting T_{av} which is what was observed during parts of the last century? Briefly, radiation is not the most significant contributor to the transfer of heat from the ocean to the atmosphere. Instead, most of the energy transfer across the ocean-atmosphere interface takes place in the form of latent heat release in which the evaporation of water cools the ocean (and adds H2O greenhouse gas to the atmosphere) and warms the atmosphere when the H2O gas condenses to precipitate. This latent heat transfer is not affected directly by the increase in greenhouse gas concentration, instead it is affected mainly by the relative humidity in the atmosphere, by the intensity of the winds that blow over the ocean, and by the difference between the sea-surface temperature and air temperature.

We should also mention that CO_2 does not have absorption bands in the atmospheric window, i.e. the range of wavelengths between 8 and 11 microns where the atmosphere absorbs only a small fraction of the outgoing IR radiation. In most of the other wavelengths the atmosphere absorbs a much larger fraction of the outgoing

radiation. In order to significantly enhance the absorption of the outgoing IR radiation (i.e. increase the greenhouse effect) one has to absorb the radiation in the band of wavelengths where the current absorption is poor and much of the outgoing radiation passes the atmosphere into space. CO_2 does not absorb in this range of wavelengths, and adding CO_2 molecules that absorb radiation at wavelengths where the previous absorption by the atmosphere is nearly complete, is not expected to have a significant effect on earth's radiation budget. Indeed, most of the forecast increase in T_{av} results from various hypothesized feedbacks and not from the direct increased absorption by CO_2 itself.

The physical principles in our paper are clearly stated in terms of the influential Stochastic Energy Balance Model. Moreover, we discussed the methodological threats that arise from absence of sufficient data on dependent forcings such as water vapor and ocean heat. HP compare our results to a simple model between fatalities and motoring. Their comparison is incorrect.

Alchemy not Science

In reference to their Figure 4 HP write, "A simple bivariate plot of temperature and log(CO_2ML) over the second period, matched by means and ranges, suggests the obvious; they are closely related." This is precisely an example of the sort of spurious correlation result that we sought to expose in our paper. According to HP admit the first difference of $rfCO_2$ is trend stationary since 1958, whereas the first difference of temperature is stationary (without the need to add a deterministic time trend). This means that the trends in the two series must eventually diverge, and that there can be no long-run relationship between temperature and $rfCO_2$. At most, deviations from the trends for temperature and $rfCO_2$ might be related, but HP do not test this. If they happened to be related, it would mean that the carbon footprint is merely temporary rather than permanent, as in fact we suggested in our paper.

Another way of putting this is as follows. During 1958 – 1980 and 1996 - 2011 temperature did not rise despite the fact that $rfCO_2$ increased throughout the whole period. During 1980 – 1996 temperature increased. This handful of observations was apparently enough to persuade HP and many others about the "obvious". Indeed, the "obvious" was not so obvious during the 1970s when a similar handful of observations persuaded many climate scientists that a new Ice Age had started. Temperature has been stable during the last 16 years despite the fact that

greenhouse gas forcings have continued to increase. One wonders how much longer this must continue before belief in the "obvious" will be shaken.

HP's interpretation of Figure 4 makes the methodological errors which they claim we made. No account is taken of omitted variables such as solar irradiance and other forcings, the possibility of regime changes is not considered, appropriate statistical tests are not carried out, and the possibility that the "obvious" might be wrong is not entertained. Also, their criticisms apply to the existing literature which we reviewed in our paper, including Kaufmann, Kauppi and Stock (2006, 2010). The main difference between this literature and our paper is that we do not think that greenhouse gas emissions have a long term effect on Earth's climate. Perhaps this is why HP waited until 2013 to voice their criticisms rather than 1997 when this literature was pioneered by Stern and Kaufmann.

References

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