Comments on

"SOLAR ACTIVITY: A DOMINANT FACTOR IN CLIMATE DYNAMICS by Dr Theodor Landscheidt"

by

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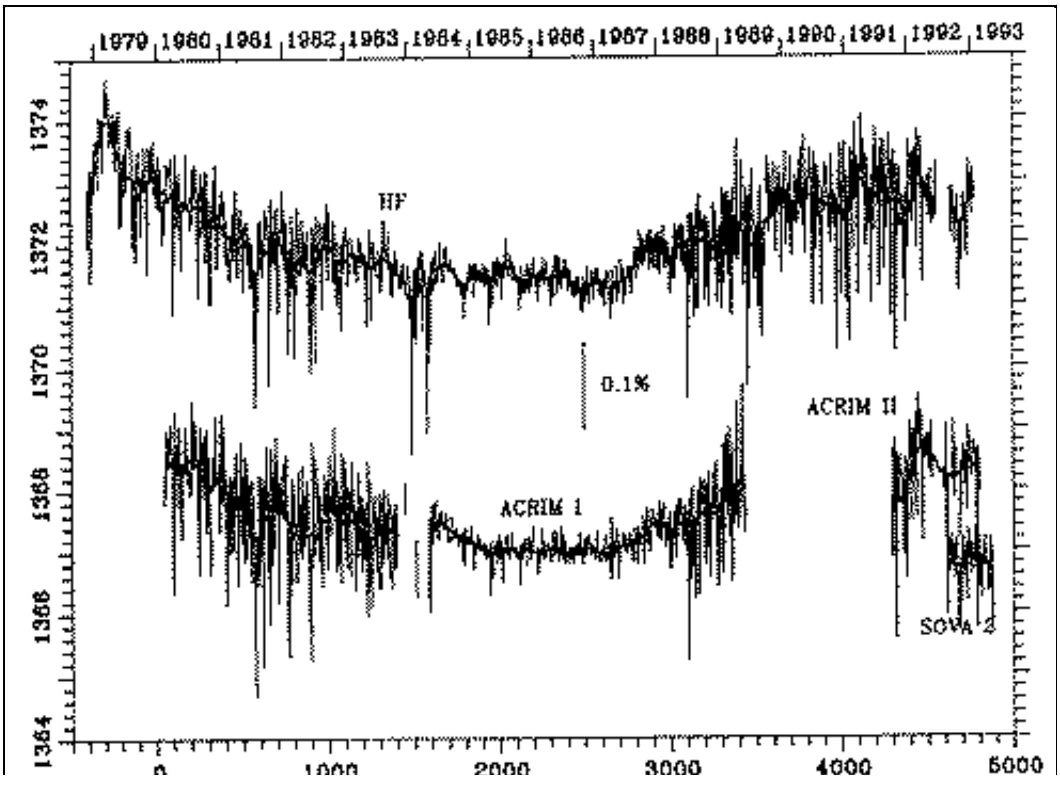
In General

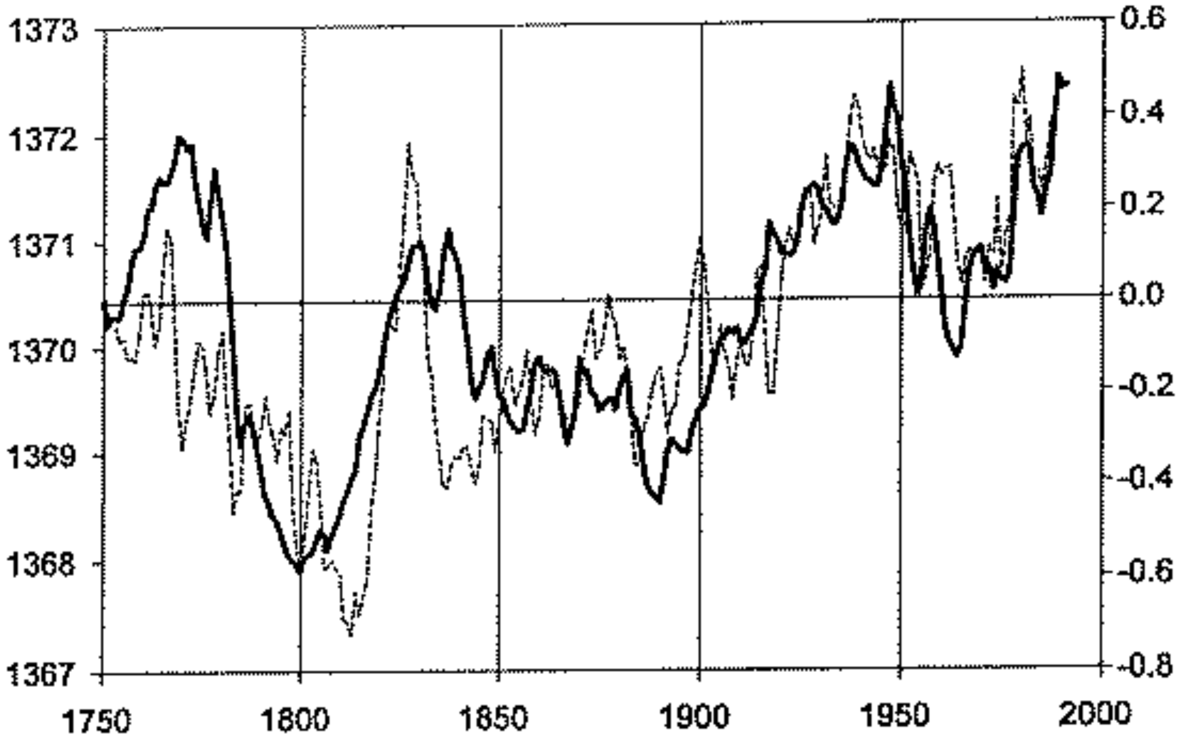
This is a lengthy article that covers, not only the stated theme, but other objections to aspects of global warming (many of which have been explained in the past). As such its main points are partially obscured. Also the presentation of the initial arguments for solar forcing are not clear, and there appear to be several inconsistencies discussed below. Finally the article lacks references to many recent and critical publications which leaves an unbalanced picture in the reader's mind.

"Solar Constant" Variations in the 11-Year Sunspot Cycle and Climatic Effects

My first objection is that Dr. Landscheidt uses values that seem to me much too large. This is from my own looking at the data, and from findings of other researchers in the refereed literature. Also from comparison with his own figure 5 from Hoyt and Schatten.



When I look at Figure 1 (left), I don't get 0.22% for the irradiance difference over that solar cycle. Taking one year averages I get that the variation is about 0.1 %. between minimum 1986 to maximum 1991, and a bit more for the maximum 1979 to 1986. So at the outset it's hard to agree with the posited 0.22%. Accepting the following arguments, this means that the variation in radiative forcing over this solar cycle was about 1-1.5 w/m2 at the top of the atmosphere and much less when converted to correspond to Greenhouse Gas (GHG) forcing within the troposphere. Further, one needs to recognize that due to the nature of the temporal variation, the sun spends most of its time, 7 years out of 11, near the lowest forcing (less than 1372 w/m2) and only 3 years near its maximum (about 1373 w/m2). Thus the average forcing over the entire time is more nearly that of the minimum. Finally, for comparison, Figure 5 below, shows that the entire solar variation 1750-present is about 0.3% and 1920 to present is 0.15%.



The conversion from radiative forcing to temperature change is discussed. Using the author's value of 0.85°C/1 w/m2, I compare the 20th century variations in solar forcing with the measured global temperature change.

Time	delta T (°C)	% delta I	fraction of delta T from solar
1920-40	0.37	0.10	.23
1940-70	-0.15	-0.04	.23
1970-90	0.37	0.07	.16

I note that this result -- that solar is significant but less than a quarter of the warming and cooling till 1970, and much less after 1970, is the conclusion of several other teams who did the study much more thoroughly. Thus, even if the temperature sensitivity to irradiance change were doubled, one would have to conclude that there is some other strong source of 20th century warming.

Further, looking at Figure 5 of this article (Hoyt and Schatten's proxy irradiance graph--although I wonder at using a temperature reconstruction from before 1979 (which is incorrect) when much better ones are now available) one can see that the entire % change in solar irradiance from 1750 to present has been only about 0.3% with the change since 1920 (when global temperatures began rising) about half that, 0.15%. One wonders why the author doesn't describe this apparant discrepancy with his conclusions from figure 1.

Also, I note the following from the IPCC **(1996)** global mean radiative forcing from 1861 to 1990 in watts per meter squared (including solar):

greenhouse gases	2.0-2.8
aerosols	-0.2 to - 2.3
solar variability	0.1 to 0.5

which looks pretty generous for solar.

> **Conclusion: multi-decadal solar variations are small, but apparantly do account for a significant part of the observed 20th century warming in the first half of the century. But they cannot account for much of the recent warming.**

In fact, most of the recent work done on this by various methods and different teams come up with similar results--in the first half of the 20th century, 1/3 to 2/3 of the warming may have been due to increases in solar activity. In the second half, only about 10-20%.--apparantly not the "dominant factor" suggested by the author. As to the fact that some solar-type stars seem to be more active than the sun, we must turn to paleodata. Although highly uncertain, the second half of the Holocene seems to show little evidence for a sun more active than at present. Perhaps our star is at a different place in its evolution than those observed to be more active?

Gleissberg Cycle of Solar Activity and Climate Change

Consider a recent paper by Damon and Peristykh **(Univ. of Arizona), GRL 26, pp. 2469-2472, 1999.** Here the authors reexamine the work of Friis-Christensen and Lassen, 1991 which showed correlations between length of solar cycle and temperature variations. (northern hemisphere (NH) only -- the correlations are less if SH is included). They discuss this in terms of the Gleissberg cycle. They conclude that t here is indeed something to what is being found, but their estimate of solar affect on temperature change is 25% to 1980 and 15% to 1997. Rind, Lean, and Healy, **JGR 104, pp 1973-1990. 1999** ("Solar forcing by itself was not sufficient to produce the rapid warming of the last few decades... The solar-induced warming during the 20th Century is about 30% that associated with trace gas warming in this model.") and others are getting about the same thing. Bottom line--yes there's a significant solar effect in the 20th century, but no it can't explain the last 25 year record.

Temperature records over the past 1,000 years

The point is made that people knew about the correlations between solar activity and temperature since 1982. The problem here (and often a problem in such discussions) is that there has been considerable improvement in our understanding of both global temperatures and solar activity since 1982. Earlier work concentrated on Europe and the NH. Gradually we have increased (still with large error bars) our understanding of truly global temperatures. The most recent studies are showing less and less global variation and less truly global response to solar variations.

One final point. In an interesting paper in **JGR, 103, pp 21,55-21,366 1998**, White, Cayan, and Lean use global upper ocean heat storage response to radiative forcing from changing solar irradiance to quantify the solar effect. Although it's a noisy record, you can clearly see the solar cycle in the temperature variations in the 42 year record they investigate. But it is clear that solar alone is unable to cause the secular temperature rise over that time period.

What this paper seems to be saying is: yes, you can see the solar cycle in the temperature changes observed in the global oceans, and yes, they can be explained by the observed solar irradiance variation. But no, the recent warming can't be explained by solar and **perhaps most importantly**, there is no evidence of any Svensmark-like added indirect forcing because the observed temperature change can be modeled with the direct effect alone.

Thus, I suggest a careful reading of papers from several teams, in addition to Hoyt and Schatten. I also note with some distress that this web site comment section has no comments from any of these teams. In fact it is a weakness in the paper not to have referenced that body of work. Is the author familiar with it?

Finally, as a clarification on all the ways one considers the effects of solar irradiance variability, I include here a note from Mike MacCracken where he tries to tutor me in the labyrithian ways people use these numbers. If you know all this stuff (I didn't) just pass it over and move on.

> It is not surprising there is confusion the way people throw things around -- Some people incorrectly compare the 2 W/m^2 out of 1370 to the 2.5 at the top of the troposphere.
>
> What the Sun puts out, measured at the Earth's distance from the Sun is usually said to be the solar irradiance and is the 1370 W/m^2. A 1% change in this would be the 13.7
>
> To get the average incoming solar flux over the Earth, divide by 4 for sperical geometry, so 342.5 W/m^2.
>
> For climate discussions, however, what is important is the net flux change at the top of the troposphere in that this is what drives the surface-troposphere climate system and in that the stratosphere is basically thermodynamically separate -- well, this is not quite right, of course, so the 4 (or 3.5) W/m^2 that has been quoted so much for a CO_2 doubling was originally calculated for after the stratosphere readjusted to the increased CO_2 (and since energy balance climate models do not have a stratosphere, this was the value to use). But GCMs do have a stratosphere, so what is now calculated is, I believe, the flux at the tropopause before stratospheric adjustment and let the GCM stratosphere adjust (and EBMs just have to somehow account for this term) -- it is this, I believe, that has helped us come down to 3.5 for a CO2 doubling (you can check all of this with Jeff Kiehl).
>
> So, what do I do about solar. Well, the albedo of the Earth is about 30 per cent, and this energy is just radiated away, so what I have to do for solar is to adjust what happens at the real top of the atmosphere to get to net value at the tropopause that I want to compare to the IR

change (which has no reduction due to reflection). So, take the 342 W/m^2 and multiply by 0.7 and you get about 240 W/m^2. Take 1 per cent of that and you have 2.4 W/m^2, which is less than 2.5, and so explains the value Tom Wigley is talking about.

Pretty confusing...

Variations in the Sun's Ultraviolet Radiation and Climate Models

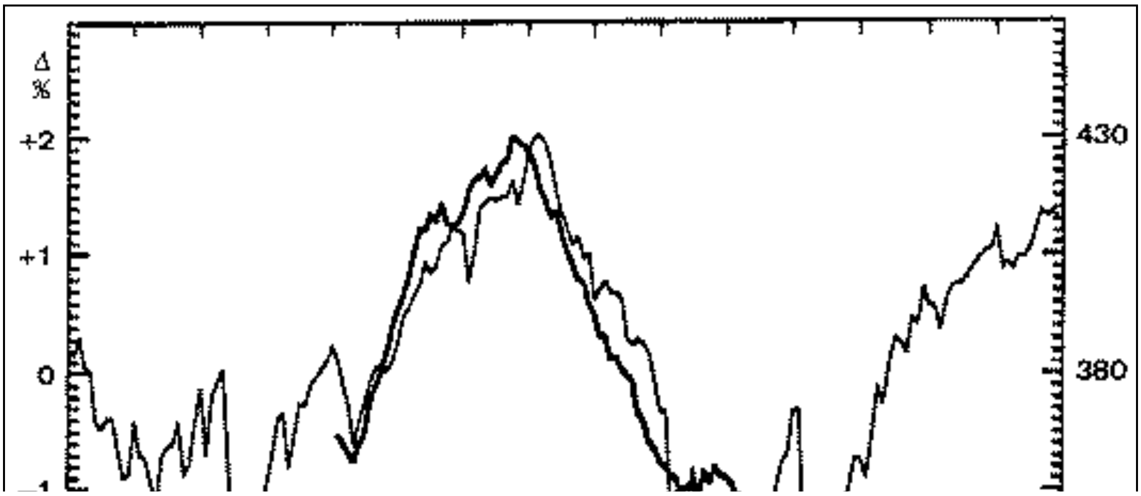
There has been increasing attention to this part of the solar spectrum especially to its effect on stratospheric ozone. The reasoning goes that since ozone is a GHG, more solar UV will make more ozone and this will warm the troposphere. This is a good qualitative argument, but to date, no one has been able to model it quantitatively. It just doesn't do much. This is not too surprising since we have an interesting analogue of such an effect.

After both the El Chichon and Pinatubo eruptions the stratosphere (as observed by the MSU 4 satellite detector) warmed dramatically. One might have expected that such warming would have had some effect on the upper troposphere. Now, of course, while the stratosphere was warming, it had "robbed Peter (the troposphere) to pay Paul (stratosphere)" of solar radiation and thus the troposphere was cooling rapidly. Still one might have expected the upper troposphere to have cooled less than the lower tropsphere and the surface. This isn't what happened. So we're not surprised that additional warming of the stratosphere by UV-caused increases in ozone didn't warm the troposphere much either. Further, the satellites (MSU4) and others give us a really good picture of stratospheric temperatures and of ozone concentrations. I don't see the solar cycle in either record.

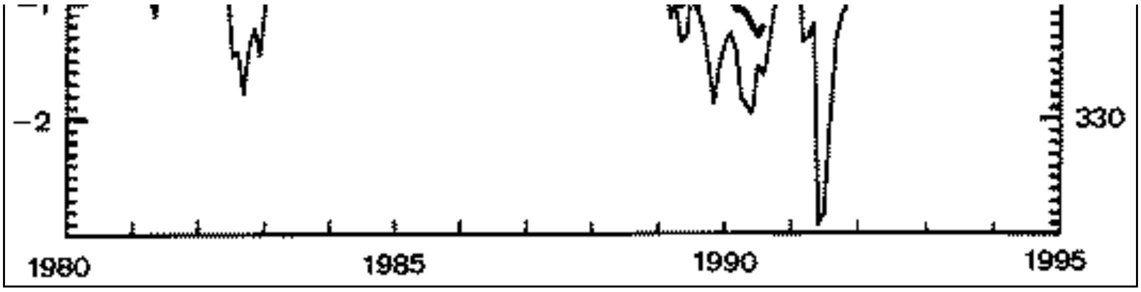
As to Brian Tinsley's ideas, Brian is a good friend who spent a year at my institute working on them. He does excellent work and is putting together quite a data set. His big problem is that the lower atmosphere doesn't seem to respond to his mechanism. The weak link seems to be cloud ice physics. We're still working on this.

Cosmic Radiation, Solar Wind, and Global Cloud Coverage

Here we come to a more specific version of Tinsley's cosmic ray/solar wind modulation affecting cloudiness. The Svensmark correlation of cloudiness with cosmic ray flux is very impressive (see Fig 6 below).



The problem is that the cloudiness part is incorrect. At IUGG last summer in England a paper was given that explained what happened. Everyone's



been looking for true global cloudiness changes, Tom Karl, among them. It would be so interesting if it could be shown convincingly that cloudiness has changed. But the data just don't support it. We still don't really have a good handle on cloudiness changes and certainly not at the year-to-year level Svensmark quotes. This is one reason his indirect forcing factor of 4 over the direct effect is not widely accepted. Also there's that paper I mentioned above by Miller Cayan and Lean that seems to show too little temperature variation over the solar cycle if such a factor were at work. The problem is that for solar to be strong enough to explain the secular rise in global temperature, it becomes too strong for the observed decadal temperature variation. You can't have it both ways.

This is not to say that we won't find some solar indirect effect. Just that it will not replace the stronger GHG forcing.

Now I also suggested to someone that there is a final logical problem here. If indeed the Svensmark indirect forcing is 4 times greater than direct forcing, it would then explain nearly all the warming. But then you have to say GHGs had no effect, which violates all the physics we know. Thus, you now would have to suggest that aerosol cooling and ozone depletion are even stronger negative forcings than currently thought. His answer was astonishing. He wrote that recent spectral absorption studies have shown that all the research of the past fifty years on infrared absorption and reradiation by carbon dioxide is wrong and it actually doesn't do much. Well, if that's your level of acceptance of a result, then what can anyone say?

Failure of Climate Predictions by IPCC Scientists

We always remember predictions selectively. Although there were some alarmists, what most were saying in the late 1980's was, that, given the large uncertainties, upper bounds could not rule out a rather large temperature rise, which, if it occurred would cause the things suggested. More recent work, as expected, has reduced the uncertainties. The most probable temperature rise is still about the same, perhaps a bit less, but the dire consequences are no longer being cited as much because we no longer think temperatures can rise that much due to GHGs alone. This, however, is modified by the ever-present fear that the climate could switch into some other mode which could indeed present great challanges to society. Still, the Holocene seems to tell us that is unlikely unless the rapidity of change is important. (The only very rapid changes seen in the Holocene were regional in nature, not global as is 20th century warming.) Thus, the change in predictions over the past 10 years or so reflects the better understanding we now have. Regardless, the IPCC 1996 error bars included a rise of some 4.5°C in the next century, and so we cannot yet discount the possiblity that things could be very unpleasant in certain areas of the world.

(satellites and sondes vs surface)

The next paragraph goes on to misinterpret the satellite data. Let me be entirely clear. Any careful study of the MSUd 2LT satellite record will show that it substantiates the surface record (thus, incidentally, telling us there is not substantial urban heat island contamination in the record (the same result as has been found by the most recent meticulous study by NOAA's National Climate Data Center **(Peterson, T. C. et al, Global rural temperature trends, Geophys. Res. Lett. 26, pp 329-332, 1999)**). Here I must note that the Daly web site also misrepresents the comparison of surface satellite and sonde temperature anomalies because it has shifted the satellite and sonde values arbitrarily to cooler temperatures by nearly 0.2°C. This is easily seen in the fact that all three records have been made to agree at the beginning of the satellite observations in 1978 when in fact satellites were showing temperatures nearly 0.2°C higher than the surface (this shift was made avowedly to make the 20 year trend difference show up). I would hope the unshifted records could be shown first. When the unshifted records are compared, it is seen that until the eruption of Mt. Pinatubo, there are only two years in which the surface was warmer than the middle troposphere!! Then an interesting thing happens. During the dramatic tropospheric cooling immediately following the Pinatubo eruption, the middle troposphere cools in a few months to nearly 0.2°C more than the surface. And it remains that much cooler constantly for the next 5-6 years! Even during that time the two records track each other -- just with the middle troposphere 0.2°C cooler. In fact were you to determine the global warming trend by satellite, sondes, and surface from 1992 to the present, you would find them to be nearly identical! (For more details on this, see my new piece at the IGPP web site below in the signature--" Can Volcanically Induced Ozone Loss Resolve Discrepancies between Surface and Tropopsheric Temperature Records?")

Clearly something happened after the Pinatubo eruption to render the middle troposphere cooler than the surface until the great 1998 El Niño (at which time the middle troposphere warmed far more than the surface--satellites recorded that it warmed nearly 0.35°C more).

All this has been to put into perspective the statements made that the earth hasn't warmed since 1940. It definitely has, but in some interesting ways that are telling us more about what's most important in climate dyanamics. For example, we're finding out that both the mid and upper troposphere warm more during El Niño and cool more during La Niña, than the surface does. And what's encouraging is that the best coupled ocean/atmosphere/land surface climate models show the same behaviour.

(other warming indicators)

As to other indicators of global warming, I'm not going to argue. I agree sea level rise is worth monitoring, but is probably not a good indicator of warming because of the complicated precipitation response the climate change. I also agree that excepting for the small effect Tom Karl and his folks are finding, extreme weather events are too infrequent and general over the entire century to be used as indicators. But it is clear that, taken as a whole, all indicators are showing the warming (the entire first part of this paper has assumed warming since "the little ice age"). The question is whether or not humans are responsible for any of it. Again, as with the physics of carbon dioxide, if you don't agree that there is global warming, then it's difficult to hold a conversation -- kind of like asking for an umbrella when the other person says its not raining.

(the climate codes)

Finally there appears an assessment on the ability/inability of the climate codes to simulate climate. The problem posed by Ed Lorenz is mentioned, but this is largely irrelevant because it deals with making exact temporal and spatial predictions of weather -- it will rain on Columbus Ohio on next Monday in the afternoon. No one suggests the codes can do that. But what we have shown is that we can predict that January in Columbus will be colder than June. The codes do pretty well in predicting seasonal climate. Their coarse resolution and over-simplified parametrizations, however, make regional predictions nearly impossible.

What the codes do rather well is in handling the gross energy balance of the planet. The codes do respond to different forcings. When the sun is brighter, it's warmer on average. In fact, the codes that were just run for inclusion in future assessments all post-dict the 20th century pretty well. And those that don't include solar forcing don't do as well as those that do. We seem to need to include GHGs, aerosols, solar, and stratospheric ozone depletion.

As to dependence on starting conditions (the butterfly effect) which cause daily weather prediction to be inaccurate, it is common to run climate simulations many times and to look at the ensemble results as well as the variations due to starting with different starting conditions. Even 8 years ago, when the codes attempted to predict the global cooling and recovery after the Pinatubo eruption, they were able to actually **predict** global temperatures that were subsequently observed. We hope we can do even better now nearly a decade later.

Now we come to how well the codes predict details of the warming such as latitudinal dependence, NH vs SH, oceans vs land. Here things are not as good as they need to be to give us the confidence we need. This is, I think, why the last IPCC assessment only made the more modest statement about our ability to see a human influence. There are several physical processes here that are not well understood. Perhaps the two most important are aerosols and water vapor feedback/clouds. From what I can see, our confidence is low, but our accuracy may be getting pretty high--that is, we can see lots of problems with how the codes handle clouds and watervapor, but the overall result seems to be about right.

Now are we ready to predict? Only with great caution. This for two reasons, the state of the codes, and our inability to foresee what forcings will be like decades from now. So we make what-if runs using reasonable forcings -- with and without aerosols (will we clean up the atmosphere?), with and without the present level of solar forcing, with different NH and SH scenarios, etc. The final cautious message from the codes is that increasing GHGs are slowly warming the planet.

The quote from Peixoto and Oort's book is not as relevant now as it was nearly ten years ago when they wrote it. For example computers are between 100 and 1,000 times faster than when they wrote this. We are indeed becoming able to integrate all this complexity into the codes.

So, I believe that the codes are getting it mostly together and are increasingly believeable. Still they are probably still missing things. I too have wondered if the polar regions shouldn't be warming more -- simple GHG physics says so, but then they are hard places to treat since they are geometrical singularities -- haze drifting north gets trapped, atmospheric flows over flat ice are bizarre, etc. I do note that Alaska has warmed many degrees in the past 25 years, as have large parts of Siberia. In the end, the codes are our only way to make sense of all of this, and at Los Alamos we're working hard to improve them. Wish us luck!

In closing this section I note that early in the next section the author will correctly point out that climate can be predicted if we know the forcings. Why didn't he say that here? After all, the only way to quantify and understand solar effects is to introduce them into these imperfect codes. It might be better if we wouldn't say, "the codes are not useful" when the IPCC uses them, but then say "these codes support our contention that solar forcing is important". I'd prefer cautious treatment of codes for all uses.

SUMMARY

The paper now goes into some vastly different effects involving actual solar motion and so this is perhaps a good place to stop for now and to continue comments in another contribution.

The bottom line, however, is that an increasing number of climate researchers think that solar variability is important to global climate change and are working to quantify it. The most are finding that, while definitely important, it's not enough to explain the observed warming, and as GHGs continue to grow, solar effects will most probably become relatively even less important. (We have good enough records over the past few thousand years to suggest that the sun's activity is not likely to exceed what it's doing at present.)

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