Attempt at a joint summary of the discussion between Fred Singer and KNMI

E-mail exchange between Gerbrand Komen and Fred Singer

Background

On 31 August 2011 Fred Singer gave a lecture at the Royal Netherlands Meteorological Institute (KNMI). This lecture was followed by a discussion on two propositions, which had been proposed to Dr Singer beforehand (on 25 July 2011):

    A. Climate scientists must communicate uncertainties and their consequences.
    B. None of current climate models overcome chaotic uncertainty

The discussion was moderated by myself. On behalf of KNMI proposition A was defended by Prof. Bart van den Hurk (KNMI, UU) and proposition B was discussed by Dr. Sybren Drijfhout.

Proposition B was one of the conclusions of Dr. Singer in his lecture. In essence, Sybren Drijfhout argued that this proposition was incorrect, because:

1. It was based on a case study which did not allow generalization.
2. KNMI had made runs with a 'current climate model' which actually did overcome 'chaotic uncertainty' (i.e. noise due to variability).

In his reaction on 31 August Dr. Singer ignored proposition A, and he did not comment on proposition B, saying that he first wanted to study the arguments of Dr. Drijfhout.

On 17 October I initiated an e-mail exchange, hoping to arrive at a joint statement. Initially there was some encouraging convergence. However, the final mails in this exchange, in December 2011, made further convergence unlikely.

I believe it is important that I present my conclusions:

- ***Both Singer and van den Hurk endorse proposition A.***
- ***Drijfhout refuted Singer's conclusion (proposition B). Singer's reaction is inadequate.***

The relevant e-mails are copied below. For the record.

Gerbrand Komen 7 December 2011

KOMEN to SINGER

	'S. Fred Singer'	RE: visit KNMI	ma 31-10-2011 8:14	93 kB
Datum: Drie weken geleden				
	'S. Fred Singer'	RE: visit KNMI	do 27-10-2011 9:29	84 kB
	'singer@sepp.org'	RE: visit KNMI	di 25-10-2011 9:28	526 kB
Datum: Vorige maand				
	'singer@sepp.org'	RE: visit KNMI	za 22-10-2011 16:24	92 kB
	'singer@sepp.org'	RE: visit KNMI	vr 21-10-2011 14:08	83 kB
	'singer@sepp.org'	RE: visit KNMI	do 20-10-2011 13:14	308 kB
	'singer@sepp.org'	RE: visit KNMI	wo 19-10-2011 14:46	39 kB
	'S. Fred Singer'	RE: visit KNMI	di 18-10-2011 12:26	27 kB
	'S. Fred Singer'	visit KNMI	ma 17-10-2011 17:58	23 kB

17, 18, 19, 20, 21, 22, 25, 27, 31
+ 4, 7 December 2011

SINGER to KOMEN

	S. Fred Singer	RE: visit KNMI	zo 30-10-2011 1:17	76 kB
	S. Fred Singer	RE: visit KNMI	di 25-10-2011 15:10	63 kB
	S. Fred Singer	RE: visit KNMI	di 25-10-2011 2:22	102 kB
Datum: Vorige maand				
	S. Fred Singer	RE: visit KNMI	zo 23-10-2011 19:26	92 kB
	S. Fred Singer	RE: visit KNMI	za 22-10-2011 2:29	67 kB
	S. Fred Singer	RE: visit KNMI	do 20-10-2011 13:52	68 kB
	S. Fred Singer	RE: visit KNMI	wo 19-10-2011 15:34	45 kB
	S. Fred Singer	RE: visit KNMI	di 18-10-2011 21:05	28 kB
	S. Fred Singer	RE: visit KNMI	di 18-10-2011 18:59	30 kB
	S. Fred Singer	Re: visit KNMI	ma 17-10-2011 22:53	2 MB

17, 18, [18 to Wilco Hazeleger], 19, 20, 22, 23, 25, [25 to Wilco Hazeleger], 30
+ 4, 7 December 2011

Gerbrand Komen to Fred Singer on 17 October 2011

Dear Fred,

I hope all is well.

Your visit here in Holland had quite some impact. I believe the discussions at KNMI were useful. Thanks again for this.

In the meantime I took note of the outcome of your discussion in the UK and in particular of one sentence in a mail by Brian Hoskins (a very respectable scientist) to you:

"The complete misrepresentation of what actually occurred given in your summary of the meeting addressed to your colleagues makes it clear that it is not useful for us to respond to you any further on these matters."

This seems regrettable to me.

Maybe we should react by making a short, joint summary of the discussion at KNMI. I have seen a number of accounts of your presentation on various blogs and websites, but I realize that there was no common statement. So therefore I now propose to prepare such a summary. What about that?

If you like the idea, could you please give a short written reaction to the propositions that were discussed?

A. Climate scientists must communicate uncertainties and their consequences.
B. None of current climate models overcome chaotic uncertainty

At KNMI your reaction to A was, there is no uncertainty, we know that the effects are small. (Hans Labohm later added that other risks are much larger than the risks of AGW.)

Your reaction to B was that you first wanted to study the details. So we are interested in what your conclusions are. I realize KNMI has been slow in reacting to your request for more information, but I believe they have done so now. In any case please find the relevant references below.

If you accept my proposal, I will also ask Drijfhout and van den Hurk to give their views in writing, and I would compile a short document, which I would first circulate between you and KNMI, and then, if all agree, I would attach it to the short note on my website.

Thanks,

Gerbrand Komen

Stone, D.A., M.R. Allen, F.M. Selten, M. Kliphuis and P.A. Stott, *The detection and attribution of climate change using an ensemble of opportunity* J. Climate, 2007, 20, 504-516. (See e.g. http://web.csag.uct.ac.za/~daithi/papers/StoneDA_AllenMR_etalii_2007.pdf)

Sterl, A., C. Severijns, H. Dijkstra, W. Hazeleger, G.J. van Oldenborgh, M. van den Broeke, G. Burgers, B. van den Hurk, P.J. van Leeuwen and P. van Velthoven, *When can we expect extremely high surface temperatures?* Geophys. Res. Lett., 2008, 35, 14, L14703, doi:10.1029/2008GL034071.

Oldenborgh, G.J. van, S.S. Drijfhout, A. van Ulden, R. Haarsma, A. Sterl, C. Severijns, W. Hazeleger and H. Dijkstra, *Western Europe is warming much faster than expected* Climate of the Past, 2009, 5, 1-12. (See http://www.clim-past.net/5/1/2009/cp-5-1-2009.pdf)

* * *

Dear Gerbrand

I like yr suggestion and am happy to cooperate.
It may require several e-mail exchanges but I agree that the result would be useful

Too bad about Hoskins. But he seems heavily committed to GW alarmism
See Mcintyre's climateaudit blog of Sept 22
http://climateaudit.org/2011/09/22/brian-hoskins-and-the-times-atlas/

My KNMI presentation is in 3 parts:(see attached EIKE report),
1. Our fingerprint controversy with Santer (IJC 2008) seems to be settled in our favor --
thanks to recent papers by Fu, Manabe, and Johanson (GRL 2011) and Thorne et al (JGR 2011)

2. Chaotic nature of climate models impedes their validation

3. The reported surface warming trend (1979-1997) is not real
(see also attached abstract for Santa Fe Conference)

Here are my comments on the references you kindly sent me

1. Stone, JClim: Rather complicated *"... the temperature
difference of the 1996-2005 decade relative to the 1940-49 decade can be attributed to greenhouse
gas emissions"*
Unfortunately, the period chosen includes the 1998 Super-El-Nino.. Could this affect their result?

2. Sterl et al.   Really interesting. Did they really do 17 runs with an identical model?
I will need more detailed info.

Best   Fred

PS Would like to start discussion on Sea level   Does KNMI have further info or studies besides the
graph on KNMI Climate Explorer?

* * *

Dear Fred,

Yes they made 17 runs with an identical model.

What about proposition A?

Please note that I am not KNMI.

Best,
Gerbrand Komen

* * *

A As a general proposition, I cannot disagree

B I was referring to the 22 '20CEN' models of the IPCC, run from 1979 to 1999
I showed the published example of the 5-run MRI (Japan) model

S. Fred Singer, PhD

* * *

Dear Fred,

See my reaction below.

A As a general proposition, I cannot disagree

Great, so this is settled. Maybe I'll ask Bart to expand on his motivation, and you might want to comment.

B I was referring to the 22 '20CEN' models of the IPCC, run from 1979 to 1999
I showed the published example of the 5-run MRI (Japan) model
Yes, I know, but you selected one specific time series which had no trend. Sybren argued that it is correct that you cannot detect a trend if there is none. In his example (17 runs) there was a trend, and he showed that 17 runs are more than enough to determine the trend in a statistical significant way. It is as simple as that, but is does refute the conclusion of your lecture (None of current climate models overcome chaotic uncertainty). I note that you now have adjusted the wording somewhat into" Chaotic nature of climate models impedes their validation". The word impede has many meaning in Dutch (belemmeren, verhinderen, hinderen, bemoeilijken). If you mean that the chaotic nature makes it difficult (but not impossible), I suppose we can agree on that.

Summarizing
1. You agree with proposition A
2. You modified your formulation of proposition B after taking note of KNMI's work with the 17 member ensemble.

Shall I work out a short note along these lines?

Gerbrand

* * *

Fred Singer to Gerbrand Komen on 19 October 2011

Dear Gerbrand

bemoeilijken is the right word, I think.

But I showed that >10 runs would be sufficient to overcome chaotic uncertainty
To remind you, I attach my report (see Fig 8)

So 17 runs of the KNMI model might be enough
This does not invalidate my assertion -- since none of the IPCC '20CEN' models had more than 5 runs.

Still -- I would like to see the results of these 17 individual runs --
specifically the 17 temp trends for 2010 to 2050

Best   Fred

* * *

Gerbrand Komen to Fred Singer on 20 October 2011

Dear Fred,

Proposition A
Great that you agree. How does this apply to the attached docs? I believe these letters hardly contradict each other. The skeptics stress uncertainty, and the alarmists stress risk. Socio-psychologically quite interesting, but scientifically boring.

Proposition B
It's all about wording. Nearly all.

I picked up "None of current models have a sufficient number of runs to overcome chaotic uncertainty" from your lecture (http://www.sepp.org/science_papers/ICCC_Booklet_2011_FINAL.pdf, now no longer available on SEPP), and this was in essence the proposition discussed at KNMI.

Sybren Drijfhout showed results of a 17 member simulation, refuting the above proposition.

I now see that you really meant "IPCC-4 climate models use an insufficient number of runs to overcome chaotic uncertainty". I noted that you formulated this carefully at the end of your lecture, in your summarizing statement. Thanks for helping me better understand your point.

You also claim that you need at least 10 runs.

Maybe the devil is in the details.

Climate modellers might argue that IPCC-4 provided a multimodel ensemble.

And Sybren Drijfhout challenged your figure of 10 (I quote him): "When the trend is zero; n must be very large; 10-20 in Singer's example. But when the trend is large n can be smaller. Singer's example is not generic. To reproduce the observed 1980-2000 temperature rise n=5-6 is sufficient; to reproduce the 1880-2010 temperature rise n=1-2. " Drijfhout based this on elementary statistical theory.

Conclusion: it very much depends on the trend. I suppose you would argue that there is no trend. Right? But the models do find a trend, which IS statistically significant.

I look forward to your reaction on both issues (propositions A and B).

Best,
Gerbrand Komen

* * *

Dear Gerbrand

Prop A:

I reread the two letters you had attached
Seems to me that they disagree

What they really demonstrate is lack of scientific consensus

But we have always known that

Prop B

Before I can answer, I need to see the 17 individual trend values, from 2010 to 2050, for the 17 runs of the model.
My previous letters asked for this info

Best   Fred

PS You raise an interesting point: Is a multi-model compilation (say 20 models with 1 run each) equivalent to running a single model 20 times?   I suppose so -- if the models are identical

S. Fred Singer, PhD

* * *

Dear Fred,

Thanks for your reply.

My own 'scientific consensus' is that there is uncertainty.

Why do you need the 21th century trends for a discussion of proposition B? You discussed simulations of the past in your lecture ('Five runs and "ensemble-mean" of the Japanese MRI climate model').

I'm not too familiar with the way in which KNMI has organized their data, so it's good that you are in direct contact with Hazeleger, but I searched a little bit to satisfy my own curiosity, and I found this plot in which different runs are superimposed.
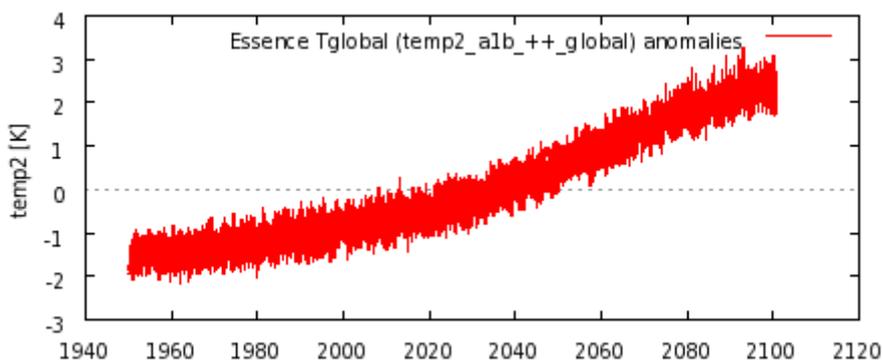
Source: http://climexp.knmi.nl/essence_indices.cgi?someone@somewhere

Best regards,
Gerbrand

* * *

Fred Singer to Gerbrand Komen on 22 October 2011

Dear Gerbrand

What you sent is interesting but hard to analyze.

What I really need are the INDIVIDUAL 27 runs from 2010 to 2050, with their resapective OLS trend values.

I am sure this info is archived -- but likely not published
I'd be happy to share coauthorship with whoever develops this info

Thank you

S. Fred Singer, PhD

* * *

Gerbrand Komen to Fred Singer on 22 October 2011

Dear Fred,

We made a lot of progress in the past week but now I feel a little stuck.

You ask for KNMI-Essence data, but I can't see why, and you don't react to my attempts to clarify.

KNMI has indicated how you can retrieve these data, but you need someone to help with this, and so far no one has volunteered.

I don't see how we can proceed much further. Would you agree with the following summary of the actual status?

<<
Proposition A
Singer en van den Hurk agree.

Proposition B.
Proposition as written was refuted by Drijfhout. Agreement was reached on the following formulation:
" The chaotic nature of climate models impedes (Dutch: bemoeilijkt) their validation".
Subsequent e-mail discussion diverged.
Singer argues that you need more than 10 members to determine a trend.
Drijfhout argues on the basis of elementary statistical theory that less than 10 members are adequate.
>>

One should agree to disagree, is not it?

If you don't like my formulation, how would you summarize?

Best regards,
Gerbrand Komen

* * *

Fred Singer to Gerbrand Komen on 23 October 2011

Dear Gerbrand

I think we have settled Prop A but need a little more work on Prop B

First, it is essential that I be quoted correctly, don't you agree?

I said and showed "you need more than 10 members to determine a trend -- <u>if the length of each model run is at least 40 years</u>"

I don't recall and need to understand Drijfhout's argument based "elementary statistical theory"

To do this, I need the detailed info from Essence (see my last 3 letters); it is not on the Internet but in the archives of the Project. Mind you, I don't question the correctness of Essence; I think that 17 runs are more than enough to give a reliable trend.

I hope this answers yr question.

Best   Fred

* * *

Dear Fred,

Sure, I agree. I know that you formulate your ideas very carefully. And I will try to do justice to that.

I attach Drijfhout's power point presentation. This contains the essence of his arguments. It also contains graphs with the individual Essence runs.
I hope this is helpful.

I look forward to your reaction, especially regarding his second conclusion bullet:

The amount of runs needed to detect trends and validate the model with observations depends on the strength of the forcing and the noise, and the length of the run, but it is generally enough in IPCC evaluations.

It would be nice if you could reject or clarify this.

Best regards,
Gerbrand

PS To access the Essence data I believe Wilco Hazeleger's guidance should be adequate.

* * *

 Dear Gerbrand

I really appreciate yr patience. But I think we are finally getting somewhere

Thanks for sending Drijfhout's slides. It clears up a possible misunderstanding.
In my talk I was referring to the 22 models of the IPCC , called "20CEN" -- and not to ALL climate models.

These IPCC models have GH forcing but use only runs of length 20 yrs -- unlike ESSENCE
I showed as an example the Japan MRI model with its 5 runs.[see also slide #2 of Drijfhout]

You can see clearly that the 5 [OLS] trends MRI are all very different

So I find that if you use longer runs, then you get 'convergence' of the cumulative trend (ensemble-mean) with fewer runs

Does KNMI agree?

I am not sure whether n depends on the strength of the forcing, i.e. whether "larger trend means smaller n" [see slide #2]
Interesting supposition -- but needs to be tested

Best   Fred

* * *

Gerbrand Komen to Fred Singer on 27 October 2011

Dear Fred,

I admire YOUR patience. Anyway, thanks for the compliments.

I am patient, but also insistent. We were trying to write a joint Singer/KNMI statement on the proposition discussed at KNMI.

I'm not sure how to proceed now. Do you have a suggestion? Should we still aim for a joint public statement?

Let me summarize where we are.

We were discussing a general proposition (B), and we found that this general formulation is not correct. The Essence runs leave no doubt here. And, in fact you agree with KNMI on this point: The chaotic nature of climate models *impedes* [Dutch: bemoeilijkt] their verification.
[By the way: in my own personal language I would not refer to the chaotic nature of *climate models*, but to natural climate variability *in nature*. Models can simulate this variability. Amazingly!] In your lecture you were referring to a specific simulation (MRI, IPCC 20CEN), and you were not aware of ESSENCE. Fair enough, is not it?

But then there was a second issue, the number of climate runs needed to detect a trend in the noise. This is more technical.

Drijfhout wrote: 'The amount of runs needed to detect trends and validate the model with observations depends on the strength of the forcing and the noise, and the length of the run, but it is generally enough in IPCC evaluations.'

I'm not quite sure whether I understand your reaction to this. So here are a few requests for clarification.

In my talk I was referring to the 22 models of the IPCC , called "20CEN" -- and not to ALL climate models.

I suppose you refer to the models listed in IPCC/AR4/wg1 Table 8.1. Correct? The model used in Essence is ECHAM5/MPI-OM, a coupled climate model developed at the Max-Planck-Institute for Meteorology (MPI-Met) in Hamburg, and listed as number 8 in this table.

These IPCC models have GH forcing but use only runs of length 20 yrs -- unlike ESSENCE

I do not understand. 20CEN stands for 20th century. These runs were 100-year runs (see e.g. IPCC/AR4, fig 9.5). Have I missed something?

I showed as an example the Japan MRI model with its 5 runs.[see also slide #2 of Drijfhout]
You can see clearly that the 5 [OLS] trends MRI are all very different

Yes, yes. But your time series is not typical. It is for a tropical belt, and it has NO trend. So you cannot detect it. The trends you showed are not significant.

So I find that if you use longer runs, then you get 'convergence' of the cumulative trend (ensemble-mean) with fewer runs
Does KNMI agree?

Yes they agree. Drijfhout is rather clear on this point in his ppt. The amount of runs needed depends on various factors: a larger trend means smaller n; larger noise means larger n; a longer run means smaller n

I am not sure whether n depends on the strength of the forcing, i.e. whether "larger trend means smaller n" [see slide #2]. Interesting supposition -- but needs to be tested.

I agree, the trend will be determined by the forcings and the dynamics of the system..

Best regards,
Gerbrand Komen

* * *

We are almost there

Drijfhout and I agree that fewer runs are required to obtain convergence -- if the runs are longer.
I have verified this in numerical experiments for run lengths of 20, 40, 80, and 100 years

Drijfhout also claims that if the forcing is strong.
I need to see proof before I can agree.

One way to check is to compare my trends from an unforced run with runs from Essence (which uses an increasing forcing)

That's why I keep asking for the 17 OLS trends for the interval 2010 to 2050
[This is the fourth time that I have asked for this info]

Best   Fred
* * *

Gerbrand Komen to Fred Singer on 31 October 2011

Dear Fred,

I see your point.

The Challenge graphs (which I sent to you) show that a small number of runs is adequate when the trend is large, but I can understand that you want to do a quantitative analysis yourself.

So, take your time. I will just wait until you have retrieved the Challenge runs, and studied their OLS trends.

Enjoy Santa Fe.

Kind regards, Gerbrand

* * *

Gerbrand Komen to Fred Singer on 4 December 2011

Dear Fred,

I hope you are well.

I wonder whether you have made any progress on the OLS trend analysis you had in mind.

I would hope that we could round off our attempt to summarize the 'KNMI discussion' before the end of the year. Do you think this is feasible?

Best regards,

Gerbrand Komen

* * *

Fred Singer to Gerbrand Komen on 4 December 2011

Dear Gerbrand

I never agreed to do such analyses and was counting on Drijfhout to back up his claim that the 'spread' in trend values depended on the level of forcing. It does not -- so we disagree:

I have shown that an (unforced) control run -- i e, a run with no increase in GH gas forcing -- shows a spread that increases as the length of each run decreases. I have tested this for runs of length 20 yrs, 40 yrs, and 80 yrs, and am quite confident of my results

If you send me one of yr control runs, I will be glad to repeat my analysis.

On the other hand, the Japan MRI graph shows a considerable spread in trend values for their 5 runs (of 20-yr length) -- almost a factor 10.

These MRI runs are forced, i e, with increasing GH gases.

QED
Best   Fred

* * *

Gerbrand Komen to Fred Singer on 7 December 2011

Dear Fred,

You are impossible!

You start all over again, just repeating yourself (consistently!), but ignoring our discussion in October.

I give up.

Our discussion was clarifying though. I can now summarize it in three lines:
- You have shown that it is hard to detect a trend if there is none
- KNMI has shown that you can detect trends (overcome the noise) if the trend is there

15

- Your generalization to "current climate models" is invalid.

I hope you don't mind if I put our e-mail exchange on my website, including a short summary giving my personal understanding (see attached).

If you would want to give a final reaction I would be happy to include it as well.

Thanks for your time and patience.

Best regards,
Gerbrand Komen

* * *

Fred Singer to Gerbrand Komen on 7 December 2011

Dear Gerbrand

We seem to have a problem in communication.

Why don't we just stop and agree to disagree
But pls, do send me a control run (with no increase in forcing)

With best wishes   Fred