Comments on 'Why have trust in climate change scenarios?'

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Summary
I now believe that the question addressed in my paper *Why have trust in climate change scenarios?* is not as clear as I thought, and should be reformulated as *Why do people have different belief in the relevance of (model)-generated climate change scenarios?* In this comment I explain why, and I discuss some of its implications. I conclude that 1. There is (room for reasonable) disagreement among experts; 2. There is no unique way to establish confidence in the usefulness of climate change scenarios; and 3. People (can and do) choose which futures they want to give serious consideration.

Introduction
I wrote my paper *Why have trust in climate change scenarios?* when I took part in a workshop on 'The roles of climate models: epistemic, ethical and socio-political perspectives' at Eindhoven University of Technology in November 2013, partly out of frustration with diverging discussions on the usefulness of climate models for the generation of climate change scenarios to be used for adaptation and mitigation policy.

IPCC and KNMI (in its 2006 climate change scenarios, which I co-authored) consider climate models useful for exploring possible futures. Others (Roger Pielke sr, Alexander Bakker) stress the limited skill of these models which make them poor tools for exploring futures. A dialogue did not resolve this controversy, which let me to consider the question *Why have trust in climate change scenarios?* However, after further study I now realize that this is not the right question. Here, I will explain why, and what this means for my original ideas.

My current reference frame
The real issue at stake is how to explore possible futures in order to take appropriate actions. This is a topic that has its own field of research, known as ***futures studies*** see e.g.

- http://en.wikipedia.org/wiki/Futures_studies
- World Future Studies Federation (www.wfsf.org)
- World Future Society (www.wfs.org)
- http://www.toekomstverkenning.nl/dynamic/index.asp

I particularly like the Dutch word *toekomstverkenning* (exploration of the future), which does not seem to have an English equivalent. Actually it has a double meaning because it means *the act of exploring the future* as well *a particular imagined future or future development*. For this latter concept I will also use the word *scenario*.

There are different ways of exploring the future. Sometimes one simply extrapolates trends, but this can be very unsuccessful. Therefore, in another approach groups of experts speculate about possible developments, taking more or less care of complex dependencies. In yet another approach process or agent-based dynamical numerical models are used. Which approach is to be preferred depends on the type of problem that is considered, and is also a matter of personal choice. Personally, I

believe that process-based dynamical models can be useful, provided they are based on sound science and, in particular, when they have shown some skill in reproducing observed variations. One may wonder what determines whether (and which) action is taken, once a particular set of possible future has been identified. This question is the object of study of other fields of research, such as ***decision theory*** and ***risk analysis***, see e.g.

- http://en.wikipedia.org/wiki/Decision_theory
- http://www.sra.org/frasg
- Foundations of risk and reliability assessment and management

The Society for Risk Analysis is currently preparing a useful glossary, see:

- http://www.uis.no/getfile.php/SEROS/SRA-glossary-draft-January8-2014.pdf

Different factors seem to be relevant:

1. The perceived likelihood[1] of occurrence,

and in addition

2. The expected impact as compared to a concept of what is desirable and what not,
3. The effectivity and cost of possible actions,
4. The possibility of collateral damage,
5. Individual and/or collective attitude towards risk,

and perhaps still others. Note that each of the factors 1-4 usually come with significant uncertainty. Some studies also consider the possibility of unspecified 'surprise' scenarios.

One sometimes attempts to order possible futures according to their likelihood of occurrence. This is not impossible, but appears to be highly subjective. The above mentioned glossary has the following definition:

> Probability is a measure for representing or expressing uncertainty, variation or beliefs, following the rules of probability calculus.

There are different methods: 1. Classical (finite number of outcomes which are equally likely to occur); 2. Frequentist; 3. Bayesian; and 4. Subjective. According to Jaynes probabilities are not so much a property of nature as well a way of dealing with uncertainty. I believe that in all cases, i.e. whatever method one uses, assumptions are being be made. This is most explicit through the choice of priors in the Bayesian approach. Jaynes and others have been searching for an objective prior, but I suspect no such thing exists.

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[1] [Wikipedia:] "In statistics, a likelihood function (often simply the likelihood) is a function of the parameters of a statistical model. Likelihood functions play a key role in statistical inference, especially methods of estimating a parameter from a set of statistics. In informal contexts, *likelihood* is often used as a synonym for *probability*. But in statistical usage, a distinction is made depending on the roles of the outcome or parameter. Probability is used when describing a function of the outcome given a fixed parameter value. For example, if a coin is flipped 10 times and it is a fair coin, what is the probability of it landing heads-up every time? Likelihood is used when describing a function of a parameter given an outcome. For example, if a coin is flipped 10 times and it has landed heads-up 10 times, what is the likelihood that the coin is fair?" When applying this to climate change one the following wording seems correct:

- *The probability that the mean global temperature in the last decade of this century is more than 2 degrees larger than in the first decade.*
- *The likelihood that the observed temperature increase is due to the burning of fossil fuel.*

I will not make this distinction here and use likelihood in the non-technical sense as capturing the idea that something is likely to happen or to have happened.

Interestingly, in futures studies and in risk analysis a distinction is often made between *probable scenarios* and *wildcard events* (also called black swans and perfect storms) which are low probability but high impact events (positive or negative).

A question that keeps puzzling me is why some scenarios are ignored by some people and not by others. I suppose factors 1-5 play a role, but perhaps also the way in which the scenarios have been generated. In their book **Risk Management and Governance: Concepts, Guidelines and Applications** Terje Aven and Ortwin Renn discuss the concepts trust, confidence and credibility and give extensive references to the relevant literature. I speculate that trust and confidence build up on the basis of experiences, not unlike classical or Pavlovian conditioning. If a hypothesis is often confirmed one starts to believe it is correct. This extends to trust in information sources. If an information source proves itself reliable, confidence in its authority grows. On the other hand, if a source spreads untrue or incomplete information, or information that conflicts with other knowledge, trust decreases[2].

This takes us into the ***socio-psychological*** domain. There actually is a huge body of literature on this, too much to summarize, and often of dubious quality, see e.g., the special issue of the European Journal of Social Psychology on *The social psychology of climate change.* Another useful link is http://www.culturalcognition.net. But there is much, much more. Social factors seem to get most attention, but there are also hints at psychological factors:

> The issue of trust also appears in the discussion of climate change. If more knowledge does not persuade the public to accept the expert consensus, why is this so? What drives the divergent views? Multiple factors come into play, including, as noted by Bolsen, Druckman, and Cook (this volume), local weather events, gender, ethnicity, party identification, and ideological frameworks (such as those articulated by cultural cognition theory). Trust in science and trust in government are also factors. But with all these factors on the table, we need to look more closely at what we are talking about, particularly as trust in government and trust in science seem conceptually related to the "egalitarian communitarian" and "hierarchical individualist" worldviews. Someone who believes that it is better to leave individuals to their own devices in the free market (hierarchical individualist) is clearly going to be less trusting of communal institutions such as governments. They are probably also going to be less trusting of science, based as it is on an epistemic community that values recognizing that each scientist "stands on the shoulders of giants" (i.e., all the other scientists who came before) and for which community criticism is part of the assurance of reliable knowledge (see below). [Heather Douglas, 2015]

Implications for the 'Why have trust in climate change scenarios?' paper [WHT].

The question I am raising now (*How can one explore possible futures in order to take appropriate actions?*) differs from my original question (*Why have trust in climate change scenarios?*) in a number of ways. To answer the original question I viewed science as a curiosity driven endeavour, in which confirmation theory investigates how one obtains knowledge as justified belief. This in essence is an

[2] For me personally, this means that I have more confidence in science than in the Enkhuizer almanac or (non-ethical parts of) the Bible, because of my positive experiences with science and the scientific method, and because it fits better in with my overall-worldview. For others this can be different. This raises two questions:

- Do I trust all of science, in particular, how much confidence do I have that model-generated futures are relevant?
- How do I deal with other views?

ongoing process, where justification is cumulative and takes time. The new question emphasises the need to make up one's mind at a particular moment, when 'confirmation' is incomplete.

In WHT I described current climate modelling and forecasting practice, including a discussion of the sources of uncertainty. I tried to clarify the epistemological meaning of forecasts, by giving a precise definition of the word *prediction*. I now believe this is not very useful as long as the concepts prediction, projection and scenario are widely used by others in various imprecise meanings.

In WHT I also discussed the way in which IPCC assigns probabilities to attribution statements of the type **It is *extremely likely* that human influence on climate caused more than half of the observed increase in global average surface temperature from 1951-2010 (IPCC 2013),** and to statements about the future: **Surface temperature is projected to rise over the 21st century under all assessed emission scenarios. It is *very likely* that heat waves will occur more often and last longer, and that extreme precipitation events will become more intense and frequent in many regions.**

I still believe that my discussion of probabilities in WHT is useful, although it has been criticized for not doing justice to the extensive literature on this subject. The bottom line, however, is my suspicion that an assignment of probabilities always involves (implicit) assumptions and/or values. Since different people or different groups of people have different values and make different assumptions, probabilities are subjective (see WHT for a discussion of *subjectivity* as a concept).

WHT was about forecasts, and not about attribution. Nevertheless, much of what I say about forecasting with the help of dynamical models is also applicable to attribution, which also leans upon the reliability of models. In addition, there is a psychological link. If one can prove that a particular phenomenon, such as burning fossil fuel, leads to a particular effect, such as a rise of the mean global temperature, people tend to assume that this will also be the case in the future.

So let me summarize. I have noted that one explores possible futures, with the help of dynamical models and with other methods, and that one tries to assess their likelihood of occurrence.

In this context the question why have trust in climate change scenarios needs some clarification. In WHT I wrote "I use the word 'trust' rather than belief or confidence, although I do not make a sharp distinction between these words, which I interpret as a measure of someone's willingness to act on the available information. This action would not need to imply specific policy decisions, but it would call for serious debate, where an 'accept the risks' outcome would be a reasonable alternative to more drastic action." As I discussed above I now realize that there are many factors that affect someone's willingness to act. My focus is on one of these factors, namely the perceived likelihood of occurrence. More in particular I am interested in a comparison of model[3]-generated scenarios and other scenarios, or specifically in the following question:

Why do people have different belief in the relevance of (model)-generated climate change scenarios?

[3] More specifically: process-based models as opposed to simpler conceptual and statistical models.

My answer in WHT was I don't know, but I speculated about psychosocial factors, and recommended further research along those lines. At the same time I suggested that there is room for a reasonable disagreement among experts. Here I can now reconsider this suggestion from a broader perspective, in three steps:

1. Is there room for disagreement?

The answer is a plain yes. I quote Matt Ridley[4]:

> Another thing that gave me pause was that I went back and looked at the history of past predictions of ecological apocalypse from my youth – population explosion, oil exhaustion, elephant extinction, rainforest loss, acid rain, the ozone layer, desertification, nuclear winter, the running out of resources, pandemics, falling sperm counts, cancerous pesticide pollution and so forth. There was a consistent pattern of exaggeration, followed by damp squibs: in not a single case was the problem as bad as had been widely predicted by leading scientists. That does not make every new prediction of apocalypse necessarily wrong, of course, but it should encourage scepticism.

Other evidence comes from a recent discussion on nrc.klimaat with 658 reactions. Some people have a deep distrust of IPCC and climate modellers, and believe that they exaggerate. Others find IPCC too conservative and too cautious about extreme scenarios that are not represented by the models.

2. Is there room for reasonable disagreement?

The answer depends on how one defines reasonable. The only thing I can think of is: a disagreement is reasonable if it is supported by arguments on either side. A stronger form would require that those arguments are not in obvious contradiction with well-established and/or observed facts. Some of the disagreements noted under point 1 are underpinned with varying arguments, such as lack of trust in IPCC and/or climate models, which are not in obvious contradiction with well-established and/or observed facts. I can only conclude that there is reasonable disagreement in these cases.

3. Is there room for reasonable disagreement among experts?

This then also depends on the definition of the word *expert* which is a tricky one, as it is hard to give. In fact there are many options:

a. Authors of published peer reviewed papers containing certain keywords[5].
b. People who have a MSc of PhD in one of the following fields: atmospheric sciences, physical oceanography, atmospheric chemistry . . .????
c. People with a PhD in a science?
d. IPCC authors and reviewers?
e. People who have run a computer model of the climate system?
f. Well-educated and well-informed journalists or blog writers?

If you suspect groupthink the definition should not be too narrow, but then, when you use a wide definition, the disagreement seems obvious to me. So, yes there is room for reasonable disagreement among experts. It is quite possible that this disagreement will be reduced in the future, but irrelevant for the present discussion.

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[4] Matt Ridley: My life as a climate change lukewarmer. The Times, 19 january 2015. See also Dana Nuccitelli, Matt Ridley wants to gamble the Earth's future because he won't learn from the past, for a critique. Theguardian, 21 January 2015.

[5] I was involved with a bibliometric study which attempted to delineate the field of climate research. It led to a kind of uproar because people were not able to agree on the keywords, nor could they accept the outcome of the study.

In WHT I suggested systematic quality assessment as a basis for discussion, but also concluded: "It seems unavoidable that different groups and different people will differ in their judgement, and in the trust they have in specific predictions. " I now add a few examples:

- Personally I give weight to the extensive tests and successes of general circulation models in weather prediction, and in the ability of climate models to reproduce key features of the climate system, in a way that is consistent with our qualitative understanding of climate processes. I am also aware of the shortcomings, but believe that the shortcomings of other methods are greater.
- Freeman Dyson and Lennart Bengtsson believe that models cannot do justice to the enormous complexity of the climate system.
- Elisabeth A. Lloyd considers model robustness a confirmatory virtue in climate science, but Alexander Bakker argued that current models are not independent because of tuning.
- Matt Ridley and others have no trust in environmental models because of many false alarms.

The question has been raised whether one can conclude that there ***generally*** is real room for reasonable disagreement among experts. The answer is *I do not know,* but perhaps philosophy could help.

- Inductive statements cannot be proven, i.e. they do not follow logically from the premises.
- A prediction is an inductive statement.
- Therefore a prediction cannot be proven and therefore there is always reason for doubting its validity.

This reasoning could perhaps be used for a more formal proof that there is generally room for reasonable disagreement among experts.

In summary:

- There is (room for reasonable) disagreement among experts (which perhaps can be reduced in future with the help of systematic quality assessment, and when new observations and new methods become available)
- There is no unique way to establish confidence in the usefulness of climate change scenarios.
- People (can) choose which futures they want to give serious consideration.

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