Why have trust in climate change scenarios?

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Abstract
Although climate change scenarios are widely used for the development of adaption and mitigation policy, their epistemic meaning remains a source of confusion. In this note we address this confusion from different perspectives: physical, philosophical and probabilistic, and sociopsychological. Climate change scenarios are interpreted as explorations of possible future states of the climate system, expressing what one *expects* that might happen. Expectations are always subjective, also when they are based on scientific models. So there is no unique way to establish confidence in the usefulness of climate change scenarios, and people (can) choose whether they have trust or not. From a review of existing literature and a critical discussion of some of the key concepts a number of conclusions and recommendations are formulated, stressing the multidisciplinarity of the subject, and suggesting careful knowledge quality assessment as a powerful alternative for other (probabilistic and non-probabilistic) methods, to serve as a basis for a dialogue between producers of climate change scenarios and their users. Quality assessment can then also serve as a procedure for individuals and groups to develop their own level of trust.

1. Introduction

In discussions with different actors over the last 25 years I have encountered fascinating differences in their appreciation of model generated climate change scenarios, with respect to meaning, confidence and usefulness. This often stands in the way of constructive discussion. I quote, as examples:

- IPCC, 2013: Climate and Earth System models are based on physical principles, and they reproduce many important aspects of observed climate. Both aspects contribute to our confidence in the models' suitability for their application in detection and attribution studies (. .) and for quantitative future predictions and projections.
- Freeman Dyson: I am saying that all predictions concerning climate are highly uncertain [statement on The Independent].
- Roger Pielke sr: If predictions (projections) on multi-decadal climate predictions are going to be given to the policymakers and impacts communities and claimed to be robust, they must show skill at predicting CHANGES on multi-decadal time scales in global and regional climate statistics in hindcast runs [statement on climatedialogue and Die Klimazwiebel].
- Lennart Bengtsson: There is no alternative to such computer simulations if one wants to predict future developments. However, since there is no way to validate them, the forecasts are more a matter of faith than a fact [Lennart Bengtsson, Neue Zürcher Zeitung, 15/04/14].
- Vincent R. Gray (private communication): The only reason you should believe in future scenarios is if they actually happen. Otherwise you have no reason to believe them.

From these citations it is clear that there are different views. On the one (extreme) hand Vincent Gray argues that there is no reason to have trust[2] in scenarios. However, it is a fact that climate change scenarios are widely used for the development of adaption and mitigation policy. The United Nations Framework Convention on

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[2] 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. For a more extensive discussion of the concept see e.g., McKnight et al (1996).

Climate Change (UNFCCC) calls for mitigation of climate change by the reduction emission of greenhouse gases. Originally, in 1992, the objective was to “stabilize greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system". In 2010 in Cancún agreement was reached that future global warming should be limited to below 2.0 °C relative to the pre-industrial level. Model predictions were an essential element in the decision making process. Achim Steiner, Executive Director United Nations Environment Programme (UNEP), recently said (http://www.unep.org/newscentre/Default.aspx?DocumentID=2764&ArticleID=10773): “Climate change is a long term challenge but one that requires urgent action today, given the risks of a more that 2 degrees C temperature rise. For those who want to focus on the scientific question marks, that is their right to do so. But today, we need to focus on the fundamentals and on actions. Otherwise the risks we run will get higher with every passing day". Steiner justifies his call for urgent action by referring to the IPCC predictions.

Another domain in which climate change predictions play an important role is climate change adaptation. Developments in the Netherlands may serve as an example, but similar developments have occurred elsewhere notably in the United Kingdom (see e.g., Hulme and Dessai 2008). In 2006 The Royal Netherlands Meteorological Institute (KNMI) developed a set of climate change scenarios (KNMI’06, Hurk et al. 2007) in response to a strong request from multiple stakeholders in the Dutch society, particularly in the water management sector (see e.g. Kabat et al. 2005). The publication of the KNMI’06 climate change scenarios resulted in many requests for additional information and guidance of these scenarios and their implications for society stakeholders (Bessembinder et al. 2011). Information need appeared to be very diverse, which resulted in additional efforts in tailoring climate information for various user groups and societal sectors. Given the changes in scientific insights and enhanced detail in the user requests a new set of climate change scenarios, KNMI’14, was published in 2014 (Hurk et al. 2014).

So predictions play an important role, but the earlier quotations clearly show that people differ in the trust they have in these predictions. The question *Why this difference in confidence?* forms the central theme in this note. In the first part it will be considered from different perspectives: physical (§ 2), philosophical and probabilistic (§ 3), and (social)psychological (§ 4). This will lead me to conclude that

- Climate change scenarios are explorations of possible future states of the climate system, expressing what we expect that might happen.
- Expectations are always subjective, also when they are based on scientific models.

In (§ 5) I discuss ways of dealing with this subjectivity. My conclusions are given in § 6.

My use of the adjective ‘subjective’ requires some clarification, as this concept has been and is being used in many different meanings. For the purpose of this paper I define subjective as ‘depending on the qualified judgement of individuals or groups’, such as when there is reasonable disagreement between experts in the field. This may be intersubjective, but I also leave room for individual dissidence, as is already clear from the quotations in the opening of this paper. I will discuss this in some more detail in section 4, where I will also address the questions: ‘which groups and which individuals’? and ‘how do judgements qualify’?

Sources of uncertainty are well-known and have been addressed in many papers (see e.g. Sluijs 1997, 2008; Komen 2001; Stainforth et al. 2005; Mastrandrea et al. 2010; Curry and Webster 2011; Katzav et al. 2012; Petersen 2012; and Parker 2013). In the physical domain uncertainty originates from chaotic behaviour and our inability to describe turbulent geophysical flow from first principles. This requires ad hoc parametrisations of processes like friction, mixing, convection, air/sea fluxes of momentum, heat and moisture, radiation transfer, cloud microphysics, etc, etc. The example of clouds may further exemplify this. Models cannot do justice in detail to the evolution of individual clouds. Therefore, their overall effect needs to be approximated in the coarser global models. Other sources of uncertainty are the choice of grid size and numerical schemes, and the selection of observations that are used for tuning/validation. Last but not least there are uncertainties arising from the inclusion of biogeochemical and economic processes (including ecology).

2. Physical perspective

It is important to be clear about the concepts that we are using. Therefore, I will briefly elaborate on some semantic aspects, before discussing the role of models and observations in the development of predictions.

2.1 What is a prediction (semantics)?

The word prediction is used in different meanings. In a loose but frequently used form it simply refers to

A. A model result for a particular time or period in the future.

In a stronger form it is

B. The statement that the future is expected to be in a particular state with some probability < 1. Often no attempt is made to explicitly specify the probability.

In the strongest form this probability is 1. In this case prediction is

C. the statement that the future will be in a particular state.

One often fails to distinguish between these definitions. Concepts like prediction and forecast (*voorspelling* and *verwachting* [=expectation] in Dutch), introduced to emphasize different shades of (un)certainty (cf Gulik, 1910) are used indiscriminately in practice. For lack of better, I will use model forecast to describe A, and (probabilistic) prediction for B. An example of a prediction is the statement that the maximum temperature in Brussels tomorrow is expected to fall within the error bands produced today by ECMWF's ensemble model prediction system. I will not consider C.

The Intergovernmental Panel on Climate Change (IPCC) also introduced the concept projection[3]. ***A projection is a conditional prediction***, conditional on assumptions concerning social economic developments and/or future emissions. Conceptually this is not very different from ensemble weather prediction where individual members of the ensemble are conditional to details in the initial state.

2.2 Climate change scenarios

According to IPCC (2001, 2007) a scenario is 'a plausible *and often simplified* description of how the future could develop'. In IPCC (2013) the *and often simplified* was dropped: 'A scenario is a plausible description of how the future may develop based on a coherent and internally consistent set of assumptions about key driving forces (e.g., rate of technological change, prices) and relationships.' However, in IPCC (2013) the word scenario is mainly used to denote the Radiative Concentration Pathways that have been defined as a basis for future climate change projections, rather than a complete description of a possible future state of the climate.

A set of scenarios provides a plausible description of how the future *may* develop. Although usually no explicit probabilities are assigned to the different scenarios, it is implicitly suggested that the future may be (approximately) in either state. **Therefore, with my definitions, *a scenario is a prediction***[4].

Hurk et al (2006, 2007, 2014) describe four climate change scenarios for the Netherlands as 'plausible and internally consistent pictures of how the climate may look like in 2050'. They are supposed to span the model

[3] In the blogosphere there exists a considerable confusion, see e.g., [accessed 27 February 2014] http://pielkeclimatesci.wordpress.com/2006/05/25/what-is-the-difference-between-a-multi-decadal-climate-projection-and-a-multi-decadal-climate-prediction/, http://scienceblogs.com/stoat/2007/08/23/projection-prediction/, http://klimazwiebel.blogspot.nl/2013/07/prediction-or-projection-nomenclature.html. See also Hammond (1996) ['*Scenarios are neither predictions nor forecasts of future conditions. Rather they describe alternative plausible futures . .*'] and Pielke sr (2001) ['*Thus, the IPCC model runs should only be interpreted as sensitivity experiments, not forecasts, projections, or even scenarios'*.]

[4] Here I deviate from IPCC (2013), which states 'Note that scenarios are neither predictions nor forecasts'.

uncertainty, and have been developed to help adaptation decisions. No statement has been made as to which scenario is more likely.

2.3 Models and observations

Predictions are made with the help of models. These may be simple conceptual models, statistical (downscaling) models or very complex computer models. Modern numerical climate models have been constructed by extending the general circulation models used for weather prediction. All models are wrong (Sterman, 2002), but one can quantify the model discrepancy by comparing historic simulations with observations. The exact numbers will, of course, depend on the available observations and the way in which the comparison is done. A special metric is the skill, measuring how accurate model predictions were in hindsight. Skill is always defined with respect to a particular forecast period, and skill scores obtained in the past are no guarantee for future skill.

There is wide intersubjective agreement about the general modelling approach, but mainstream experts differ in their treatment of details. A major research effort has gone into the study of these differences in Climate Modelling Intercomparison Projects, such as CMIP5 (Taylor et al. 2012). These intercomparisons are being used for making some of the uncertainties explicit. There are however different opinions as to the suitability of climate models for making predictions.

All models predict an increase in the average global temperature on a centennial timescale. However, there is considerable disagreement about the actual rate of warming. Hindcast studies show some agreement in the simulation of global mean temperature but large differences in the simulations of changes in precipitation on a local scale.

Confidence in climate model predictions is based on the model's ability to reproduce important aspects of observed climate. A lack of confidence is based on the failure of current climate models to reproduce (other) important aspects of climate observations.

2.4 Model testing and development

It is not at all obvious that the behaviour of a complex system, such as the climate system, can be understood in terms of the behaviour of its constituents (Anderson 1972). I favour a pragmatic approach in which one accepts that there are limits (practical or fundamental, I don't care) to the understandability and predictability (see e.g., Tennekes 1990, 1992 and 1996) of complex systems, but also acknowledges that one does not know where these limits are. It's my belief, based on my modelling experience, that it is useful to try and see how far you can get. This has sometimes been called pragmatic reductionism[5].

Applied to model development this means that one tries to reduce the model discrepancy by improving the models (see also Oreskes et al. 1994). Strategies for model improvement involve better use of old and new observations, error reduction in computer code, better numerics, higher resolution, increase in the number of dynamical variables, extension of the domain, better external forcing, and improved parameterisations of unresolved processes (using results from additional, dedicated observations). Essential are an open mind, transparency and eagerness to change one's opinion if new information (observations, arguments) becomes available. This then underpins an approach to theory and model improvement, which respects the basic laws of physics, observational evidence and sound reasoning. This approach has been extremely successful in numerical weather prediction. Of course, this does not guarantee that it will be successful in the case of climate prediction, but one can try.

[5] See e.g., Reductionism in Meteorology and Climatology. Lecture presented at the Dutch Philosophy of Geosciences Symposium, April 16, 2004, Utrecht, The Netherlands. Unpublished, but abstract available online.

Nevertheless, I conjecture that for complex systems like the climate system it will be impossible to prove, and meaningless to refute a model. Therefore we have to live with imperfect models, incomplete understanding and limited predictability. Also, we have to accept that there are important subjective aspects to confidence in model predictions because of subjective assumptions concerning e.g., the choice of metric to assess model discrepancy, and because past skill does not guarantee future skill.

3. Philosophical and probability theory perspective

Climate change scenarios are descriptions of possible future states of the climate system. David Hume (1748) pointed out that statements about the future are not deductive, that is, they do not follow in a logical way from observations. Sunrise tomorrow does not follow logically from the fact that sunrise has been observed many, many times. To logically conclude that the sun will rise tomorrow we must make an assumption, such as - suggested already by Hume - similarities between past and future, but then the problem shifts because there is no form of reasoning that can prove this assumption. So one may conclude that in these situation predictions cannot be proven. Popper (1985) pointed out that they can be refuted by observations, but this is not very helpful in the case of climate prediction.

Sometimes climate models are used to establish predictions. There are several different views about the kind of reasoning that should be involved in doing so and about the appropriate attitude to have towards the predictions. An interesting overview was given by Katzav (2014), who distinguished five different views: 1. standard; 2. conservative; 3. adequacy-for-purpose; 4. Bayesian; and 5. possibilist. Rather than attempting to summarize these different views I would like to note here that in all of these approaches assumptions have to be made. Different people may make different assumptions and this introduces a certain amount of subjectivity in statements about the future.

3.1 Subjectivity

There is a rather large body of literature on the concepts of 'subjectivity' and its counterpart 'objectivity' (see e.g. Porter 1992; Quine 1992; Megill 1994; Douglas 2004, 2009; Yamamoto 2012; Lloyd and Schweizer 2014) from which it is apparent that these concepts have been used in many different meanings. My definition was given in the introduction as 'depending on the qualified judgement of individuals or groups'. This raised a number of questions, e.g. which groups of individuals should be considered, and what makes a judgement qualified.

To discuss these questions I find it useful to refer to the work of Douglas (2004, 2009). Douglas sees objectivity as a basis for trust and distinguished three categories: objectivity1, referring to interactions with the world such as observing and measuring; objectivity2, dealing with psychological aspects such as detachment and value-neutrality; and objectivity3 which deals with social aspects. Objectivity is not something absolute, but it can be assessed and graded.

Traditionally, in the physical sciences, observations play an important role. For example, if one repeatedly measures the speed of light in vacuum one always finds the same result (i.e. within the error bands of the measurement). This gives the determination a high degree of objectivity1, and naturally leads to intersubjective agreement among experts and a high degree of objectivity3.

In climate modelling things are a little different. Measurements are also essential but their availability is limited for several reasons, as discussed above: the system is unique, complex and non-stationary, which makes it impossible to monitor it in all its details, and - importantly for the present discussion - predictions can only be checked in the future. Therefore, the degree of objectivity1 is necessarily smaller than in the previous example, which enhances the relevance of objectivity3.

Douglas makes a distinction between two types of intersubjective agreement: concordant objectivity3 and interactive objectivity3. A typical example of the first is the recent publication by Verheggen et al. (2014) in which the degree of consensus was measured with the help of a survey among 1868 scientists. An example of the second type is the Fifth Assessment report of IPCC (2014). In both cases there emerges a broad agreement among experts on certain statements (including uncertainty estimates).

How does this impact on the question raised in this paper? First of all, it provides some clarification of my definition of subjectivity ('depending on the qualified judgement of individuals or groups') as the counterpart of objectivity3. Further Impact derives from two remarks by Douglas (2004) concerning intersubjective agreement: 1. "There is always the chance of a group illusion"; and 2. "There is a second limitation: how one decides on the composition of the group." In practice these concerns may be closely related, which is best illustrated by returning to the quotes given in the introduction of this paper.

One might be tempted to argue that IPCC judgements are not just those of individual researchers but those of a community of experts, which make them more reasonable (less subjective) than those of individuals like Bengtsson, Gray, Lindzen and Pielke Sr. However, one could imagine that IPCC scientists collectively place too much confidence in models, which would render value to individual dissident expert judgements. This is exemplified by statements concerning attribution, where IPCC writes "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 Lennart Bengtsson writes "The reason to the slow warming is in fact more or less unknown".[6]

This example also underscores the problem of group selection. Verheggen et al. (2014) carefully specified the group of experts that they approached for their survey, but it is obvious that other choices could have been made, and similar studies with different groups have let to different outcomes (see the discussion in Verheggen, 2014). The selection of IPCC authors similarly follows a carefully defined process, but it is a fact that there are other processes which result in different group composition and conflicting predictions, as is apparent when one compares the following statements:

- Equilibrium climate sensitivity [the change in global mean surface temperature at equilibrium that is caused by a doubling of the atmospheric CO_2 concentration] is likely in the range 1.5°C to 4.5°C (high confidence), extremely unlikely less than 1°C (high confidence), and very unlikely greater than 6°C (medium confidence).[IPCC, 2013]
- Doubling the concentration of atmospheric CO_2 from its pre-industrial level, in the absence of other forcings and feedbacks, would likely cause a warming of -0.3 to 1.1°C . . . [NIPCC[7], 2013]

From an epistemic point of view it is important that judgements 'qualify', which I propose to interpret here as being made by experts. This then shifts the problem to determining which people can be considered relevant experts. To give an example compare Bengtsson and Dyson (quoted in the introduction). Bengtsson has a scientific record in atmospheric modelling, was director of the prestigious ECMWF and Max-Planck Institute for Meteorology, so it would be hard to argue that he is not qualified. Freeman Dyson is a theoretical physicist and mathematician, famous for his work in quantum electrodynamics, solid-state physics, astronomy and nuclear engineering. In his case one could argue that he has no direct experience with climate modelling. However, this could also be seen as an advantage, in particular, if one suspects group illusion (i.e. overconfidence in models). From this example I conclude that the selection of experts is non-trivial and an additional source of subjectivity.

[6] Statement on http://klimazwiebel.blogspot.nl/2013/07/heinz-wanner-die-globale-klimadebatte.html (accessed 30 August 2014).

[7] NIPCC stands for Nongovernmental International Panel on Climate Change, a group of sceptical scientists.

3.2 Probability

In some approaches it is possible to assign probabilities to predictions. A Bayesian framework (see e.g., E.T. Jaynes, 2003) provides a formal, logical way of assigning probabilities to hypotheses, starting from some prior hypothesis, which is updated when new information is available. Unfortunately, for complex problems it is quite difficult or impossible to formally describe all relevant assumptions. Therefore, strict applications of the Bayesian approach to the climate prediction problems are limited in scope, (see e.g., Murphy et al. 2004; Hegerl et al. 2006; Tebaldi et al. 2005; Rougier 2006; Stephenson 2012; Williamson and Goldstein 2012; Collins et al. 2012) and a discussion of this approach in Katzav et al. (2012).

In all of these studies subjective choices have to be made which will influence the probabilities one ends up with. In traditional physics this does not play a role, because the (prior) assumption that the basic laws of physics will also hold tomorrow has been very successful and is accepted by almost everyone. However, when studying unique, complex non-stationary systems the assumptions become important. As a result there are diverging views on the suitability of climate models as tools for quantitative predictions of the climate system (see quotations in the introduction above), and confidence in predictions will be influenced by assumptions. From a logical point of view, it is perfectly legitimate when different actors disagree about the probability assigned to a prediction (see also Fenetti 1937, and Morgan et al. 2009).

Cartoon by Frank Modell in The New Yorker,1983.

In a frequentist approach probabilities would not suffer from subjectivity in the Bayesian sense. However, the climate system is unique and non-stationary, which makes it difficult to apply frequentist methods. In practice, in this approach assumptions about uncertainty are often hidden (Zwiers and von Storch 2004). Of course, one can derive frequentist probabilities from an ensemble of models, but this approach has also a subjective element. To illustrate this, think of a situation in which you have 5 different models, 4 of which predict a particular change and one does not. In this case one group of experts could argue that there is an 80 % chance that this change will occur. However, another group of experts might reject one or more of the available models as insufficiently realistic, and they would come up with another probability.

There are more sophisticated ways of assigning probabilities to model forecasts. In these approaches one translates assumed probability information on parameters and/or the initial state, emission and forcing scenarios and/or policy action into probabilities of the model forecast. Even more sophisticated are the multi model ensembles. There is a huge body of literature on this subject (see e.g. Walley 1990; Rougier 2006, 2007; Bayarri 2007; Knutti 2008; Rougier and Crucifix 2012; Stephenson 2012; Williamson and Goldstein 2012; and Katz et al. 2013). In all cases the results are depended on the assumed input pdfs, other implied assumptions and used methods. Also, the results do not account for the possibility that all models miss something essential, something that is sometimes referred to as 'structural uncertainty' and has been illustrated in many different ways (e.g.: woodworm, Komen 1994; black swan, Taleb 2007; hawkmoth, Thompson 2013, Thompson and Smith 2014).

One might be able to develop an advanced statistical model that deliberately factors in the probability that 'all models are wrong' (Daniel Williamson, private communication; see also Luo and Caselton 1997; Craig et al. 1998, 2001; Chandler 2013). This probability could be assessed by interviewing expert panels about their beliefs. This would be useful and helpful to make assumptions more explicit. However, one would expect the final results to depend on the definition of expert, and on the particular choice of expert panel. Therefore, results would still not be robust when you change expert groups, so that the question remains as to why the probability estimate of one person or group would be better than the estimate of someone else.

My conclusion: Advanced probabilistic techniques can quantify (some of) the uncertainty due to underlying assumptions, but are misleading when they do not make these other assumptions explicit.

IPCC seems to be well aware of the role of 'expert judgement' i.e. the role of subjectivity in the formulation of probabilities. The Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties (Mastrandrea et al. 2010) is based on earlier work by Morgan and Hernion (1990)[8], Risbey and Kandlikar (2007), Morgan et al. (2009) and others. IPCC (2014, chapter 1) correctly notes that expert judgement is needed (and even asserts that experts tend to be overconfident), but this remains somewhat hidden when probabilistic statements of the type 'there is x % chance' are made, where it would have been more accurate to write 'we estimate an x % chance'.

4. Psychological and social perspective

Common experience suggests that psychological and social factors play a role in confidence development. It is a striking fact that different individuals develop different levels of confidence. One point was already noted in the previous section. Climate models have strong and weak points (which is a 'fact'), but different people tend to focus on different aspects. Some derive confidence in climate model predictions, because the models are able to reproduce important aspects of observed climate. Others develop a lack of confidence, from the same information, because of the failure in reproducing (other) important observations. I would speculate that individual experiences play a role here. Pilkey and Pilkey-Jarvis (2007) have collected a long list of failed environmental predictions. This could surely affect one's thinking and foster a pessimistic attitude. Meteorologists, who can pride themselves of the enormous progress in numerical weather prediction, may be more optimistic about the modelling *approach* (although, aware of the sheer complexity of the climate system, they may be less optimistic about the robustness of the model results).

I'm not an expert in the field of (social) psychology, but I would like to speculate that other relevant individual factors might be: personality, values and level of education. I further speculate that social factors such as communication, tradition, group conformity, and authority also play an important role (see e.g., Castellano et al. (2009), Feygina et al. (2010), Hamilton (2011) and Kahneman (2011) for some striking examples). Social epistemologists are trying to find the facts, see e.g. http://www.hps.cam.ac.uk/research/se.html (accessed 27 February 2014). In my opinion, the psychological and social perspectives deserve much more attention.

5. How to deal with the subjective nature of climate change scenarios?

There are different reactions to the recognition that specification and/or quantification of uncertainties in climate change scenarios is subjective, as it depends on the subjective judgement of individuals or groups. I will discuss two:

- Seek other strategies for policy development,
- Make underlying assumptions more explicit and better visible.

[8] See particularly Chapter 6, 'Human judgement about and with uncertainty'.

5.1 A bottom-up, resource-based vulnerability approach

It has been suggested (Dessai and Hulme 2003; Pielke and Wilby 2012) that one should simply refrain from using model generated scenarios for adaptation policy guidance. Dessai and Hulme write

> . . there is a growing literature that argues that scenarios of climate change, least of all probabilities of climate change, are not needed for climate adaptation policy. Instead, a strategy of resilience and adaptive environmental management that enhances coping capacity is preferred (Pielke Jr. 1998; Adger 1999; Handmer et al. 1999; Kelly and Adger 2000; Barnett 2001; Burton et al. 2002; Clark and Pulwarty 2003; Tompkins and Adger 2003). These authors argue that in the face of the considerable uncertainty over climate change projections and its impacts, one is better off adapting to the present day (or recent historic) climate variability as this is assumed a good proxy for near term climate change.

I believe that this is a viable approach, but it obviously hinges on the assumption that recent historic climate variability is a good proxy for near term climate change. In weather prediction this would be called persistence. From weather prediction we know that dynamic numerical models based on the laws of physics can do much better. Therefore, personally, I would rather rely on a diversity of different scenarios generated with the best available methods (i.e. observations *and* models), and spanning known uncertainty.

Pielke sr argued against the use of climate change scenarios by arguing 'If predictions (projections) on multi-decadal climate predictions are going to be given to the policymakers and impacts communities and claimed to be robust, they must show skill at predicting CHANGES on multi-decadal time scales in global and regional climate statistics in hindcast runs.' [statement on climatedialogue.org and Die Klimazwiebel]. This argument raises several questions. First of all skill (Pielke's definition seems to differ from mine) is a measure of model discrepancy, but it would have to be made more specific to be useful. Secondly, past skill does not guarantee future skill, so what underpins confidence in future skill?

5.2 Other approaches

Several (non-probabilistic) alternatives have been proposed for dealing with the assessment of climate change scenarios, see e.g. Lipton (2004), Kandlikar et al. (2005), Stainforth et al. (2007a,b) and Parker (2009, 2011). A useful overview can be found in Katzav et al. (2012). Additional approaches were discussed at the workshop 'The roles of climate models: epistemic, ethical and socio-political perspectives' in Eindhoven.

Here, I would like to draw attention to the knowledge quality assessment method developed by Jeroen van der Sluijs (1997, 2002, 2008) for exploring the quality of evidence for complex and contested policy decisions and others (van der Sluijs et al. 2008). A concise summary, discussing reasons for belief in models, was by Janssen and Petersen in 2011 in a lecture[9] at a workshop entitled 'All models are wrong, Model uncertainty & selection in complex models'.

Knowledge quality assessment is a specific methodology for the analysis of assumptions in assessments. Essential elements are the identification of explicit and implicit assumptions in the calculations, assessment of potential value-ladenness of key-assumptions, robustness of the underlying science, agreement among peers, identification of 'weak' links in the calculation chain and agreement among stakeholders. Details are given in Kloprogge et al. (2011) where the method was tested ex post on the indicators 'change in length of the growth season' and 'deaths and emergency hospital admittances due to exposure to ozone' in the fifth Dutch Environmental Outlook.

[9] Model structure uncertainty. A matter of (Bayesian) belief? http://www.math.rug.nl/stat/models/files/petersen.pdf. Accessed 26 February 2014.

Personally, I believe that there is an important role for knowledge quality assessment in statements about climate change attribution and prediction, and in particular the development of climate change scenarios. To the best of my knowledge this has not yet been done in a systematic way. Van der Sluijs (2008) recommended the setup of an Ignorance Platform, 'whose primary task is to explore the policy relevance of our scientific ignorance on climate change and to design strategies based on resilience to achieve climate proofing under the radical ignorance of future climates', but so far this has not seen any follow-up.

Knowledge quality assessment provides a useful protocol for making underlying assumptions more explicit (see e.g. the examples given by Kloprogge et al. 2011). This is useful to experts because it will help identify points that one may want to pick up in future research. It may also be useful in the more general debate, when it provides groups or individuals with diverging views on the usefulness of climate models with a joint (less subjective) basis for their discussions. Such a discussion might well lead to individual recalibration of confidence. Nevertheless, it seems unavoidable that different people will differ in their final judgement.

6. Conclusions

The title of this paper asked the question: Why have trust in climate change scenarios. The answer, somewhat surprising perhaps, appears to be that people (can) - collectively or individually - choose whether they have trust or not. This does not imply that one can believe whatever one likes regarding some phenomena. Empirical evidence, logic, and conformity with the basic laws of science should continue to be guiding. However, in practice, empirical evidence is incomplete (because it may be impossible or too expensive to collect the necessary observations, or one may have to wait for a century before one can test a prediction) and there may be intrinsic limits to the reducibility (as in the case of chaotic flow). These factors leave room for subjective judgement.

In this paper I attempted to demonstrate the additional value of a multidisciplinary (physical, philosophical, social science) perspective. Improved communication between these different disciplines would be useful, and will hopefully lead to further understanding of the meaning of climate change scenarios.

The paper began with an overview of current practice in climate modelling, in which I tried to clarify the meaning of the concepts prediction, forecast, projection and scenario. With the definitions given in this paper forecasts, projections and scenarios *are* predictions, but with different implied assumptions. However, I also noted that there is some confusion about this in the literature. Therefore, a first conclusion is that the various concepts would benefit from a better philosophical foundation, and a more consistent semantic usage.

The development of climate predictions involves a certain amount of inductive reasoning, and therefore they cannot be 'proven' in a logical way. Instead, philosophers have been discussing various other ways of justifying - not proving - climate change predictions. In some approaches it is possible to assign probabilities to scenarios, but, importantly, this always involves subjective choices that have an influence on the probabilities one obtains. Therefore, different people in climate science may end up with different probabilities. Also in non-probabilistic approaches assumptions have to be made, so that different people may make different assumptions and these affect which predictions they trust.

In section 4 I argued that the answer to the question 'What determines someone's trust in the usefulness of scenarios?' does not only lie in the physical or philosophical domain, but should also be considered from a (psycho)social perspective. It seems plausible that both personal (personality, values, experience, level of education) and social aspects (communication, tradition, group conformity, authority) could play a role. Research on these aspects should shed more light on the role of these aspects.

Accepting that judgements about the meaning of climate change scenarios have subjective elements, another question came up, namely: ‘How should one deal with this?’ I discussed three responses, the so-called bottom-up approach, attempts to quantify subjective aspects in a probabilistic approach, and several alternative non-probabilistic approaches. This led to a number of specific conclusions which can be summarized as follows:

- The bottom-up approach also involves subjective assumptions.
- Advanced probabilistic techniques can quantify (some of) the uncertainty due to underlying assumptions, but are misleading when they hide other assumptions.
- It would be useful to make a more systematic knowledge quality assessment of top-down (scenario) and bottom-up (vulnerability) approaches.

Knowledge quality assessment provides a useful tool for making underlying assumptions more explicit and may facilitate discussion between believers and non-believers, and between providers and users of climate change scenarios. It seems unavoidable that different groups and different people will differ in their judgement, and in the trust they have in specific predictions. This is not bad. It provides fuel for science, and allows policymakers to give proper weight to their political and personal values.

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