

On Climate Change Research, the Crisis of Science and Second-Order Science

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> Context • This conceptual paper tries to tackle the advantages and the limitations that might arise from including second-order science into global climate change sciences, a research area that traditionally focuses on first-order approaches and that is currently attracting a lot of media and public attention. **> Problem** • The high profile of climate change research seems to provoke a certain dilemma for scientists: despite the slowly increasing realization within the sciences that our knowledge is temporary, tentative, uncertain, and far from stable, the public expectations towards science and scientific knowledge are still the opposite: that scientific results should prove to be objective, reliable, and authoritative. As a way to handle the uncertainty, scientists tend to produce “varieties of scenarios” instead of clear statements, as well as reports that articulate different scientific opinions about the causes and dynamics of change (e.g., the IPCC). This might leave the impression of vague and indecisive results. As a result, esteem for the sciences seems to be decreasing within public perception. **> Method** • This paper applies second-order observation to climate change research in particular and the sciences in general. **> Results** • Within most sciences, it is still quite unusual to disclose and discuss the epistemological foundations of the respective research questions, methods and ways to interpret data, as research proceeds mainly from some version of realistic epistemological positions. A shift towards self-reflexive second-order science might offer possibilities for a return to a “less polarized” scientific and public debate on climate change because it points to knowledge that is in principle tentative, uncertain and fragmented as well as to the theory- and observation-dependence of scientific work. **> Implications** • The paper addresses the differences between first-order and second-order science as well as some challenges of science in general, which second-order science might address and disclose. **> Constructivist content** • Second-order science used as observation praxis (second-order observation) for this specific field of research. **> Key words** • Second-order science, climate change research, observation theory, theory-dependency, causality, production of knowledge.

Introduction: How the high profile of climate change research challenges the sciences

« 1 » From as early as the 1980s, science seems to have been in a crisis. This crisis started at that time within the natural sciences, whose research subjects have been more and more shaped by societal (political/economical) activities and challenged by socio-ecological questions (cf. Becker & Jahn 2006: 68f). As a consequence, science is no longer unanimously regarded as a guarantor of a better future (Becker & Wehling 1993: 11ff). The crisis of science comes in the form of a loss of public trust, confidence, legitimacy and, thus, power. And – at least

partly as a consequence of this loss – a crisis of self-concept (von Storch & Krauß 2013; Leuschner 2012; Ravetz 2004; Nowotny, Scott & Gibbons 2003; Gibbons et al. 1994). The loss of trust goes beyond science; it is also part of a growing awareness of an increasing destabilization of former apparently stable aspects of society: nation states, political institutions, our perception of and relation to nature and the physical environment, the economy, gender roles and gender relations, families, etc. This perception and acknowledgment of destabilization seems to be inherent to our epoch of the “second modernity,” in which contingencies increase (since almost every aspect is dependent on decisions, Luhmann 1992b) and in which all certainties become debatable as a kind of unwanted side-effect of the industrializa-

tion of almost all parts of our planet (Beck 2009; Beck & Bonß 2001; Beck, Giddens & Lash 1996). Whilst this understanding of increased and inherent contingencies is incrementally assumed within most scientific disciplines, and especially so in social sciences and humanities, the public expectation towards the explicitness and correctness of scientific results has predominantly stayed the same (cf. Leuschner 2012: 39ff; Neverla & Schäfer 2012). This tense situation between science and public expectations sets the frame for the highly polarized public debates on climate change, and due to its concurrent high profile, climate change research thus acts as a catalyser for the destabilization of science in general. This is further enhanced by an increasing awareness of the tentativeness of all knowledge,

seeping into natural sciences from humanities and social sciences.¹

« 2 » After a first acknowledgement in 1992, with the World Summit in Rio de Janeiro as the starting point for the debates on how to create sustainable societies, climate change research successfully reached the public agenda in 2007. This was due to an arranged series of actions (Egner 2007) beginning with the widely discussed Stern Review (“The Economics of Climate Change,” Stern 2006), followed by the globally launched documentary movie “An Inconvenient Truth,” produced by the well-known politician Al Gore and embraced by scientific confirmation in the form of successive presentations of the Fourth Assessment Report of the Intergovernmental Panel on Climate Change (IPCC AR4) in 2007 (Solomon et al.

1 | We owe thanks to one of our anonymous reviewers for pointing out that what we call a “crisis of science” seems to be a European topic. She/he argued that there was no crisis in (the philosophy of) science in the U.S. because everyone was a realist and, thus, there was no alternative perspective on truth, at least not among scientists, including social scientists. Regarding the public debate on the reasons for climate change, she/he stated that the highly polarized debate, which also existed in the U.S., was interpreted as pure self-interest. That is, scientists who had published saying that human-caused climate change was occurring had to maintain this position in order to continue receiving research funds. Opponents of the idea of human-caused climate change were said to be coal and oil companies and scientists receiving funds from them. The rancour of the debate caused people on both sides to become more committed to their positions. She/he concluded that although this was an issue in which “the observer” is a factor in the results, self-interest or mendacity was a more familiar or comfortable interpretation in the U.S. If this were the case, we, from our “European” perspective, would also call this a “crisis of science,” though a different one: what she/he described as the motivating force of scientists denies, ultimately, any difference between “science” and “economy,” which originally are clearly distinguishable social systems (cf. Luhmann 1992a). By adopting the “codes” of economy, science would disappear as a specific social system and would merge with economics. We do not know whether this is true for the sciences in the U.S., even if we notice some evidence for tendencies in this direction.

2007; Parry et al. 2007). Additionally, it has become apparent that the human dimension, and with it the social sciences and humanities, cannot be excluded from climate change research, adding to the already existing challenges of achieving reliable knowledge of complex and/or non-linear systems within the physical world (cf. Grundmann, Rhomberg & Stehr 2012). With its increased public profile and the need for interdisciplinarity, along with the highly polarized and antagonistic political debate on the causes and effects of climate change, climate change research, involuntarily, has thus made an important aspect of science and academic work publicly visible: that scientific knowledge and academic insights are in principle preliminary, uncertain and fragmented. It has also revealed to a broad public audience that we only have a very narrow understanding of the far-reaching consequences of our actions in complex and non-linear systems, on a global as well as on a regional scale.

« 3 » For many scientists, this insight is not at all surprising; they would agree on this as the basis of their scientific doing. Hence, differing opinions and, thus, hard debates on the plausibility and adequacy of research questions, methods and results are essential parts of scientific work (this is true in principle, of course there is ignorance and ostracizing within the scientific community as well, but this will not be addressed here). However, there are relatively few scientists in general who are willing and prepared to reveal and discuss their epistemological presuppositions. Climate scientists are no exception to this general rule and thus rather adopt a position on the basis of an “objective truth” (instead of insisting on theories, see below) (cf. Pielke 2007). By insisting on the undeniable truth of their findings, they take the stance of an authority, presenting themselves as “indisputable experts.”² Since most of the parties who are involved in the climate change debate, with their differing positions on the causes and effects in climate change, argue alike, the initial academic debate on our current knowledge about cli-

2 | “Brauchen wir ein neues umweltwissenschaftliches Ethos?” by Reiner Grundmann and Hans von Storch. Retrieved from <http://klimazwiebel.blogspot.co.at/2013/11/brauchen-wir-ein-neues.html> on 4 May 2014.

mate change turns into a “religious” debate on the “right kind of faith” when this topic is discussed in the “agora” (cf. Hoffman 2012, who calls it a “culture war”). Josef Mitterer (2011: 9) calls this an “impasse that can only be solved by use of force” (translation by the authors). Moreover, science lurches into a serious crisis when results are discussed as “mistakes” or “betrayal,” as happened in the debates on climate change. Thus, we agree with Jerome Ravetz when he comments on the “climategate” controversy in 2009 with the following remark: “Politics will doubtless survive, for it is not a fiduciary institution; but for science the dangers are real.”³

« 4 » Figure 1 points to a further aspect in those cases where academic debates reach into the public sphere: the question of how to communicate scientific findings with their corresponding assumptions, implications and uncertainties. This question becomes even more urgent when the scientists are convinced that societal (political, economic, etc.) actions or decisions are needed. As the two short texts in the figure indicate, within the climate change debate a productive exchange of ideas has obviously become impossible as both sides operate with double standards: whilst climate change researchers recommend addressing emotions in order to facilitate actions, the argumentation of climate sceptics is denounced as being emotionalizing.

« 5 » With this paper we try to tackle the question of whether second-order science, and especially its aspect of self-reflexion, could help to enable a “more scientific” or a “less polarized” scientific and public debate on climate change (i.e., coming back to theories instead of insisting on “faith” or “truth”). We believe (keeping in line with our previous religious metaphors) that it is of particularly high value for scientists as well as for the addressed public, media and politicians to disclose the conditions of scientific knowledge. Within academia, such a disclosure could be used to rework our approaches and our epistemologies as well as

3 | “Climategate: Plausibility and the blogosphere in the post-normal age,” by Jerome Ravetz. Retrieved from <http://wattsupwiththat.com/2010/02/09/climategate-plausibility-and-the-blogosphere-in-the-post-normal-age/> on 4 May 2014.

Veronika Wirth et al.

COMMUNICATING CLIMATE CHANGE

Adaptation communication formats are numerous, but are they effective? An analysis of 278 formats in ten OECD countries yields good practice examples and a list of success factors. One recommendation is that addressing emotions, norms and values helps in motivating for taking action.

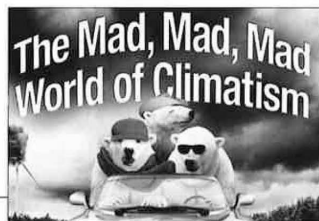


30–39

Jens Soentgen, Helena Bilandzic

WIE KLIMASKEPTIKER ARGUMENTIEREN

Klimaskeptische Argumente könnten mit steigenden individuellen Kosten der Energiewende salonfähig werden. Deshalb lohnt eine Auseinandersetzung mit der emotionalisierenden Argumentationsstrategie der Klimaskeptiker, die sich vor allem auf Verschwörungstheorien stützt.



40–47

Figure 1 • Screenshot of the table of contents of the scientific journal GAIA, March 2014, Topic: Climate Change. The figure illustrates the strict distinction between “us” and “them” in the climate debate: in the abstract of the article on “communicating climate change,” addressing emotions is presented as particularly suitable in promoting the idea of anthropogenic climate change, whereas the abstract of the article “the structure of climate sceptical arguments” (“Wie Klimaskeptiker argumentieren”) criticizes exactly this use of emotions by climate sceptics.

to rethink how we communicate our findings. Outside academia, it might be necessary to develop an “understanding” of the modalities and practices of scientific work in order to enable a critical public – from our viewpoint, a responsible and reasonable public counterpart would be of high value for scientific research. Thus, the current crisis of science (to which the highly polarized public and political debates on climate change contribute) could lead to a redefinition of our scientific practices as well as our self-concepts as scientists moving towards second-order science. In a first step, we differentiate first-order from second-order science. Subsequently, we focus on the connections of theories, the production of knowledge and society, which plays a crucial role in doing science. In our understanding, theories and their development are not conceivable without their specific societal context, they are not beyond or outside society, but rather they are an essential part thereof. Theories contain and represent the general ideas and assumptions regarding in what

way and how a specific society perceives and positions itself within and to the world (or nature, the cosmos, etc.). Thus, theories and society develop in a kind of co-evolution, which finds its expression in the type of “knowledge” that is produced. In that way, this paper is not at all a critique of climate change research or pointing to its contribution to the current crisis of the sciences. Instead, we use the example of climate change research to indicate important challenges in scientific work in general. We close with a discussion of the potential of second-order science as a new way of doing science.

First-order and second-order science

« 6 » Climate change research can be understood as an example of a traditional form of science (which is called first-order science in this context) that has been forced by its high public profile to reflect on how it is actually conducting science

(Weingart, Engels & Pansegrau 2008). It has yielded various considerations about how traditional science should change to be able to tackle the huge challenges of globally changing environments and societies. At the same time, the acknowledgement of the crisis of science has triggered considerations of alternative ways of doing science, and the so-called second-order science has emerged. It can be classified into four main strands:

« 7 » *Firstly*, “mode-2 research” (Nowotny, Scott & Gibbons 2001, 2003; Nowotny 1999; Gibbons et al. 1994), which is characterized by a strong orientation towards societal needs. It demands socially relevant research (socially distributed research, mostly application-oriented scientific practice, transdisciplinary methods, which include societal parties concerned in the research process, and the demand for research to be subject to multiple accountabilities and not only to scientific experts, cf. Becker & Jahn 2006: 319ff). In addition to that, mode-2 research demands that science has to contribute directly to the well-being of society (Nowotny, Scott & Gibbons 2003; Beck, Giddens & Lash 1997).

« 8 » *Secondly*, “post-normal science” (Funtowicz & Ravetz 1993), which argues less normatively than mode-2, but also suggests a research methodology that is appropriate for cases where “facts are uncertain, values in dispute, stakes high and decisions urgent” (ibid: 744). Post-normal science opts for a democratization of science by including an “extended peer community,” i.e., including societal stakeholders within a kind of transdisciplinary methodology as well as in the mode-2 approaches. To us, attached to this demand is a crucial and yet unacknowledged question: If knowledge is in principle preliminary, uncertain and fragmented (see below), who should decide on whether a specific research question, method or result is especially advantageous for society? However, while mode-2 research is only partly acknowledged within climate change research, post-normal science is vividly discussed (e.g., Krauß, Schäfer & von Storch 2012; van der Sluijs 2012; Ravetz 2011; Saloranta 2001; Bray & von Storch 1999). Both strands can be summarized by the term “transdisciplinarity” (cf. Table 1).

« 9 » Thirdly, self-reflexivity, which focuses on changes within science itself to increase the self-reflexion of science (cf. Umpleby 2010), including the observer, being aware of the dependencies of all research from the selection of theories and, thus, the basic assumptions for the observation, changing the attitude of science, etc. Hence, the difference between first-order science and the aspect of self-reflexion in second-order science refers to the question of how we know what we know. It includes the knowledge of knowledge and pays attention to how theories affect the phenomenon being studied or how the phenomenon shows up in a different light by applying different theories. This is an essential part of a system praxeology needed in the governance of complex issues (cf. Ison 2010). Being aware of the implied theories in each analysis of a phenomenon inevitably points to the observer who decided to apply this specific set of theories instead of an alternative perspective in the first place. Therefore, the aspect of self-reflexion in second-order science focuses on the processes of observation, including the observing person her/himself. Most people would probably agree that we know because we observe, and that it is the specific scientific observation methodology that leads to the distinction between laypersons and scientists, assuming that the latter provide privileged and reliable facts about the world. However, science in general, and climate change science in particular, is not only about “stating facts,” but is rather concerned with questions of future developments. With this shift from retrospective and current observation to prediction – especially in addition to the acknowledgment of the complexity of non-linear systems – there is a sensitizing to the uncertainty as well as the limits of scientific knowledge. Within the last three decades, the awareness has grown that future system states are not only unknown, but most probably even principally unknowable (Funtowicz & Ravetz 1990: 12) and so, our practices in science as well as our self-concepts as scientists might need reconsideration.

« 10 » Regarding climate change research, or even the greater picture of global environmental change research, we have to include a *fourth* strand of second-order science, i.e., that of complex, non-linear

	first-order science	second-order science
scientific practice	stating facts, referring to reality	including the observer (sr), theory-dependency (sr), fragmental section (sr, td, cs)
epistemology	objectivity, truth	self-reflexivity (sr, td), multiple realities (sr, td, cs)
causality	objective causes	contingent causes (sr, td, cs)
aims of science	predictability, prognosis, explaining functions	unknowable future (sr, cs), well-being of underprivileged in a society/social and political change (td), contingent scenarios (cs)
scientific attitude	certain (expert knowledge)	knowledge is uncertain, fragmental, tentative (sr, td, cs)
production of knowledge	increasing (linear) knowledge	circular knowledge (sr, cs)
communication modus	expert knowledge	participation, interaction (td)

Table 1 • Observable differences between first-order and second-order science. The letters in parentheses (sr, td or cs) refer to the three distinguishable foci (sr) self-reflexivity, (td) transdisciplinarity and (cs) complex, non-linear systems.

systems: Self-reflexivity will surely help, but as with transdisciplinarity, it will certainly not contribute to the analysis of our climate, which shows many examples of complex system behaviour (such as bifurcation points, threshold-abrupt change, bi-stability domains, cf. Lenton et al. 2008). All four postulations attribute to the concepts of second-order science in different ways. Table 1 tries to point to the differences between first-order and second-order science in quite a “black-and-white” manner.⁴ We are well aware of all the shades and nuances in between, which can also be detected in parts of the climate change research. In the following we will mainly focus on the strand of self-reflexivity and the theory-dependency of all knowledge and come back to all four strands at the end of this paper.

« 11 » In summary, the various suggestions for second-order science can thus be distinguished in the following three central postulations (cf. Table 1):

4 | Cf. “Science 2: Is a broader conception of science still science?” by Stuart Umpleby. Retrieved from http://www.gwu.edu/~umpleby/recent_papers/2010%20WMSCI%20science%20%20abstract.doc on 13 May 2014.

- self-reflexivity,
- transdisciplinarity (encompassing mode-2 research and post-normal science), and
- complexity and/or non-linearity.

The first approach differs from the others insofar that it proceeds from the assumption that there is neither a direct nor an indirect access to the truth; that there is an influence of the observer on the observed object (e.g., due to the contingency of the selection of theories, of the basic assumptions or of the epistemology); and that attributed causes to an observed effect may reveal a lot about the implicit assumptions and concepts of the observer and less about how nature works. However, to our knowledge, all approaches share the epistemological assumption of multiple realities, that is, the recognition that the future is highly contingent, which in complex systems could be due to bifurcations and multiple attractors. The knowledge that is produced against the background of such a scientific attitude is of a more circular type and, moreover, aware that knowledge is tentative, uncertain and fragmental.

« 12 » Due to predominantly (and moreover, to a large extent, implicitly) positivist epistemologies, it is quite unusual to disclose and discuss the epistemological foundations

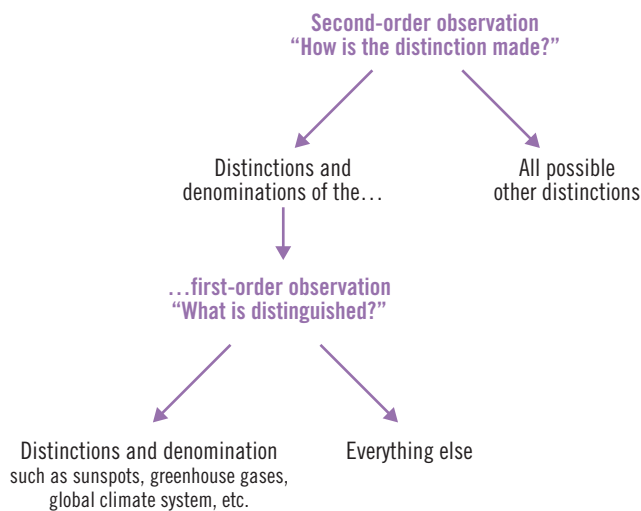


Figure 2 • First-order and second-order observation (Adapted from Egner 2010: 45).

of the research questions, methods and ways to interpret data within climate change research. Accordingly, it is not well received within climate change science to highlight the contingencies of presuppositions or the normative assumptions of the research and the inherent uncertainties of climate models (cf. for instance McGrail 2013). In general, it is quite unusual to reflect on (let alone publicly discuss) questions such as:

- How do I know that something is a “fact” (and not only an interpretation or a momentary observation to which I attribute a contingent cause)?
- In what way am I (personally) related to my field of research? Am I aware of all the various dimensions of this relationship?
- Why am I doing what I am doing?
- Do my approaches, my methodology or my personal motivation impact the results, and if so, in which way?
- How can I keep my mind open for results I do not like? How do I handle findings that do not fit my expectations?
- When and how do I know that I have reached a result and that I can conclude my research?

«13» One crucial distinction between first-order and self-reflexive second-order science can be found in the different understandings of “observation.” The aspect of

self-reflexion in second-order science adopts Heinz von Foerster’s (1984) theory of observation, which goes well beyond “seeing” and “distinguishing” something from everything else. Observation is defined as the twofold practice of distinction and simultaneous indication of one side of what we have distinguished before (cf. Luhmann 1998, 69 and Figure 2). As something is distinguished, everything else is left aside and is thus no longer observable. Observation therefore needs an action (Foerster 2006: 27): whatever we observe has to be actively distinguished from everything else. It has to be perceived as being distinct from the rest, e.g., a cumulonimbus as being distinct from cirrostratus in terms of the immediate weather prospects. Otherwise, both phenomena would simply be clouds. In climate research, to take another example, climate models act as the core experimental devices. The models can be understood as mathematical constructs that are designed to simulate the functioning of the full climate system by combining components that are assumed to describe the most significant physical processes. To construct these models it is crucial to distinguish and to define the main components and their processes, e.g., the atmosphere has to be distinguished from the hydrosphere, the hydrosphere from the cryosphere, etc. It is this act of distinction and indication that

renders climate modelling at all possible and that literally brings the “global climate system” (as model and as “system”) into being. The results of climate models depend significantly on how the distinction of the components and the mathematical description of their internal processes is carried out. For example, against the background of the current discussion about the “anthropocene,” it could be argued that societies and human activities can no longer be viewed as external to the climate system, but rather have to be included as one of the components of it – most likely, this would lead to very different results. This is why observation theory emphasizes the importance of including the observation of the observer in the scientific endeavour. Thereby, the observation of the world is shifted from first-order observation (question “what is being observed?”) to the level of second-order observation (with the question “how is something being observed?” Figure 2). The benefit and importance of second-order observation for science is that the respective blind spot of the observer comes into focus. The term “blind spot” is the epistemological equivalent to the physiological blind spot, as we are unaware of both when we observe. In observation theory, however, the term blind spot refers to the underlying distinction of an observation. While this distinction guides what is being observed and what is not being observed, the observer is unaware of it. Second-order observation (as an observation of the observation) can reveal the distinctions that have been applied in the first-order observation. In such a way, first-order observation brings forth an object (e.g., the climate models in our example above), while second-order observation brings forth the acting (which would include the reflection and disclosure of the presupposition, theoretical decision, limits, uncertainties, etc.; cf. Foerster 2006). When applied to climate research, the underlying distinction seems to be “natural” versus “anthropogenic,” which is why the human dimension as a driver of climate change is regarded as external within the modelling approaches. If this distinction were changed, a “new reality” would unfold (since the observation of “the world” has changed) and would bring new, previously unseen phenomena and relations into view.

« 14 » Second-order observation can be easily misunderstood if it is concluded that by its application we are able to find “the truth behind the things,” once and for all. Unfortunately this is not the case, which can be shown by applying second-order observation to itself. It appears that any second-order observation utilizes again two distinctions: it not only distinguishes which kind of distinction the observed observer is using, it also utilizes the distinction between the observer and her/his observed object (Fuchs 1992: 46f), which is – again – a first-order distinction. As a consequence, each observation results in a new blind spot. Thus, the “truth behind the things” is still hidden, even with elaborated practices of observation. This refers to the non-accessibility of the so called “Archimedean point”: an impartial point of view from which everything can be seen and known is simply impossible to achieve. Rather, the world only comes into being through observation (Spencer-Brown 1969). This leads us to the main difference between first-order science and self-reflexive second-order science, i.e., the possibility or impossibility of having a true theory or a true and objective result. From the perspective of self-reflexive second-order science, everything we know leads back to an observer and to the chosen theories.⁵

« 15 » In terms of practicing science, self-reflexive second-order science offers certain benefits that could be utilized and become especially useful when and if the tools of first-order science seem to block science instead of allowing new insights. It offers a specific viewpoint, an additional reflectiveness that facilitates the inclusion of the conditions and prerequisites of scientific endeavour. It will not *per se* result in a better science, and (quite obviously) results will not be more true than before. Still, it has the potential to facilitate scientific discussion as neither side can claim to own the truth; and it has prospects to return to what we (the

authors) perceive as best-practice science: scientific studies that disclose the respective assumptions, limitations and uncertainties as the “human” behind the researcher is allowed to enter the arena. In this, we fully agree with Silvio Funtowicz and Jerome Ravetz (1990: 14), who claim with reference to research on global environmental issues “that this work cannot be done in neglect of human values and interests.”

On theories, scientific knowledge and society

« 16 » We start our reflections on the interrelation of theories, scientific knowledge and society and their implications for first-order and second-order science with an observation of the development of climate change research. It is an attempt to observe scientific epistemologies, practices and connected styles of communicating scientific findings of science, in general and in climate change research in particular, in the context and against the background of essential assumptions of second-order science. By doing so, we are very well aware of our position and responsibilities as *observers* of climate change research instead of being actively part of it. With this, of course, we face certain limitations – we cannot see our own blind spots and, thus, our observation cannot be “true” in an objective way. Furthermore, it influences how we view the capabilities and feasibility of the development of different scientific practices, of changing the basic scientific attitude and of exploring epistemological questions. However, the position of an observer allows a different view of things, as mentioned in the section where we distinguished first-order from self-reflexive second-order science. We would like this paper to be understood as an act of second-order observation that might bring forth the *acting*, while climate change research brings forth the *object*. It might well provoke objections from within the group of climate change researchers as well as from their opponents. However, it is our starting point for carving out the challenges of doing science in general and for our considerations relating to the connection of theories, the production of knowledge and society.

Observing climate change research as starting point

« 17 » The theory of anthropogenic global warming, for example, is the outcome of research by climate scientists who observe the physical and chemical composition of the atmosphere. Since CO₂ has long been known to be a greenhouse gas, the observed rise in CO₂ concentrations within the atmosphere has suggested itself as the main cause of global climate change. Thereby, (and probably due to its political sensitivity), the observed correlation between CO₂ concentrations and global mean temperature was transformed into a causal relation. Whilst this causal relation seems very straightforward and thus is easy to explain to the public, scientists are aware that it is a simplification: if and when more energy from the sun is trapped within the atmosphere, this does not necessarily have to transform into thermal energy solely, but can also transform into kinetic, potential and other energy forms. However, via the concept of “detection” and “attribution” (cf. Stehr & von Storch 2009: 88ff.), the observed rise in temperature can reasonably be attributed to the rise in greenhouse gases, and thus the increase in global mean temperature most probably is due to industrialisation. When translated into public communication, however, this “political climate hypothesis”⁶ is often transferred into a certainty. An instructive example is the career of the so-called “hockey stick graph.” The graph was first published in 1998 and shows the mean temperature of the northern hemisphere over the past 1000 years. It is based on statistical climate reconstructions using climate proxy records, i.e., tree-ring data (Mann, Bradley & Hughes 1998). The form of the graph has the appearance of a hockey stick tilted sideways. The long “shaft” forms the (flat) mean temperature between the years 1000 to 1850 and is followed by a sharp and steady increase until the year 1998 forming the “blade” of the stick. The conducted climate reconstruction is riddled with many prerequisites, which were clearly pointed out by the authors when the graph was first published. However, these important remarks about the presuppositions and methodological ambiguities of the study were increas-

5| Even if second-order science, especially the science of complex non-linear systems, does not automatically lead to a constructivist stance, the strong demand for self-reflectivity that comes with second-order science will mostly lead to some constructivist ideas about “reality” (Hacking 1983: 21ff, 167ff).

6| We owe thanks to Egon Becker for this summarizing expression.

ingly disregarded during the popularization of the graph. Thus, the hockey stick was transformed into an iconic image purporting to be incontrovertible evidence for global warming. It made its way into important publications such as the “Summary for Policy Makers” of the Third IPCC Assessment Report⁷ (cf. von Storch & Krauß 2013: 81ff).

« 18 » Thus, by and by, the idea of anthropogenic climate change shifted from a scientific theory to an undoubtable description of the world “as it really is.” Whilst it is crucial to disclose this step of internalization, this disclosure is at the same time far from trivial. It means a step in the direction of second-order science, as it emphasizes the theory-dependency of all observations (and thus, as a matter of course, including this observation of the observation, too).

Challenges in doing science

« 19 » In the following, we will use the example of the developments in climate change research as outlined above to point to some significant challenges in doing science in general.

« 20 » All theorising and researching in science starts with the question of why things are as they are, or why a phenomenon occurs precisely in the way it does and not in some other way. It is thus the question of cause and effect that builds the starting point of all scientific research as well as the core of all theories, be it in science or everyday life. At the same time, the concept of causality has often been challenged and considered not only to be the basis, but also something like the core problem of science (e.g., Russell 1912). Additionally to our thinking that appears to be inevitably causal (and linear), we seem to be in dire need of certainty and truth, which is expressed in the concept of “knowledge,” above all in the ideas of “scientifically assured knowledge.” To date, the relationship between science and society is based on the “knowledge of experts” who are responsible for telling the public, the politicians, the media, or generally all interested persons “what is the case” (to borrow

7| “Climate Change 2001 Synthesis Report. Summary for Policy Makers.” Retrieved from <https://www.ipcc.ch/pdf/climate-changes-2001/synthesis-syr/english/summary-policy-makers.pdf> on 14 August 2014.

an expression Niklas Luhmann used in his farewell lecture at the University of Bielefeld in 1993). So far, the question “what is behind all this” (as the title of Luhmann’s lecture continues) mostly remains unaddressed. This would be one step towards self-reflexive second-order science, since it refers to the theories and presuppositions, which led to this specific piece of knowledge and includes considering contextual-bound, fragmental and uncertain findings, instead of using the “objective truth” as an argument.

« 21 » In climate change research, in contrast, it is often common practice to communicate with the aim of “transporting” expert knowledge to the political and public sphere (Pielke 2007). Currently, some climate change scientists expect the public (and above all politicians and economists) to ground their actions on the communicated scientific findings (cf. Bray & von Storch 2014: 61ff) and are bewildered at the large extent to which the various scientific perspectives result in a different public (political) understanding and interpretation of the findings (ibid: 93 ff, answers to the space for open expression in the survey, see especially comments #14, 17, 27, 46 and (with a self-critical touch to climate change research) #15, 18, 20, 68). Ultimately, from the point of view of climate change scientists, the current (political, economic, societal, etc.) decisions and actions are in discordance with what would be scientifically “correct” (for instance, the debate on the “two-degree target”).

« 22 » In this context, we detect at least three pitfalls or challenges for scientists in general, on which we will ponder briefly in the following:

- 1 | the tendency to take theories as “reality,”
- 2 | the belatedness of all knowledge and
- 3 | our seemingly inescapable need for causation that often (mis-)leads us to easily interpret correlations as causation.

We are convinced that all three aspects are essential for the currently deepening crisis of the sciences. We argue that the assumptions of self-reflexive second-order science might help to reshape scientific and science-to-public interactions.

« 23 » *Ad 1:* Within the process of knowledge-building, there seems to be a tendency to leave theoretical grounds as well as an impetus to shift to “reality,” where

“facts” can be stated and in which the world seems to be just “as it is.” This is not only a tendency within media, politics or economics, but this assumption of a direct access to reality through (accurate) observation obviously can also be found within the sciences. This can be exemplified by yet another controversial and publicly debated dispute, i.e., the dispute on evolution versus creationism. In this context, the biologist Josef H. Reichholf stated that “evolution as history of life is an objective fact, as is the history of the Earth and the cosmos; just natural history – and nothing else” (Reichholf 2009: 165, translation by the authors). In his answer to this statement, Wolfgang Sonne emphasized how important it is to differentiate between facts and theories. He pointed out that this is a crucial aspect of the epistemology of scientific work, as otherwise scientific debates would turn to religious wars that can only be lost: “Those who turn scientific theories to questions of faith, and this is done by declaring them as facts, have already lost the battle with religion” (Sonne 2009: 272, translated by the authors). Seemingly, the very same has happened within the scientific debates on climate change: in the first decade of this century, they turned into the “evangelical science’ of global warming” (see footnote 3). What we can thus observe is a charged atmosphere in which scientific discussions turn into religious wars (“battle with religion,” as Sonne stated) where protagonists no longer change their minds or come to different conclusions, but are “changing sides”⁸ as they are “reversing beliefs.”⁹ To us,

8| “Streit über Erderwärmung: Angesehener Meteorologe wechselt zu Klimaskeptikern” by Axel Bojanowski in Spiegel online 7 May 2014. Retrieved from <http://www.spiegel.de/wissenschaft/natur/klimawandel-meteorologe-lennart-bengtsson-wird-klimaskeptiker-a-967602.html> on 7 May 2014.

9| “Prominent scientist reverses belief: ‘One of the most eminent climate scientists, the Swede Lennart Bengtsson, has defected to the camp of climate skeptics.’ Blog by M. Morano at Climate Depot, 7 May 2014. Retrieved from <http://www.climatedepot.com/2014/05/07/prominent-scientist-reverses-belief-in-global-warming-one-of-the-most-eminent-climate-scientists-the-swede-lennart-bengtsson-has-defected-to-the-camp-of-climate-skeptics/> on 8 May 2014.

it is crucial to leave the insistence upon “being objective” and upon “the truth” behind and to return to theories and scientific approaches with all their arguable assumptions and presuppositions. A commitment to self-reflexive second-order science might help, as it demands the continuous questioning of one’s own findings and presuppositions.

« 24 » *Ad 2:* Knowledge of something requires the analysis and an understanding of phenomena that have been observed in the *past*. Thus, we do not “know” about the future. With regard to climate change and the urgent need for decisions on how to proceed with our global activities, Peter Sloterdijk stated in a speech at the United Nations Conference on Climate Change in Copenhagen in December 2009: “Intrinsically, when compared to reality, knowledge always lags behind – yes, one could say that it is principally delayed.”¹⁰ Furthermore, to wait for “scientifically assured knowledge” (which then, of course, is still contingent as we argue above) as the grounded basis for decisions might lead to a “point of no return”: changed actions would no longer be necessary since they would not affect the already ongoing processes in the global context. This is exactly the dilemma of the highly polarized debate on the causes and effects of climate change and it might be conducive to science if scientists insisted on the tentativeness of scientific knowledge instead of arguing with “true results.” Of course, this will not match the expectations of the public (nor politics, nor the media) who ask for certainty and clear results. To us, it is obvious that we all (scientists as well as all parts of society and each individual) urgently need proper training in withstanding the urge for “final truths” and in coping with uncertainties, instabilities, vagueness, impermanence – in short: “fluid conditions” of all kinds. If nothing else, “keeping the door open to doubt” (Rovelli 2012: 102) is one of the most prominent tasks of science. The concept of self-reflexive second-order science could very well contribute to that.

10| “Wie groß ist ‘groß?’” by Peter Sloterdijk. In: Die Welt, 17 December 2009. Retrieved from http://www.welt.de/welt_print/debatte/article5556427 on 8 May 2014. Translation by the authors.

« 25 » *Ad 3:* One of the core aims of the sciences is to find the causes of observed phenomena. Causal attribution is not entirely restricted to science, but rather it is an inevitable operation, in everyday life, too. We strive to know “why” (Rathmann 2008). Moreover, earth system scientists proceed from the basic assumption of the uniformity of nature in which causal attribution also allows us to conclude from past events to future development as well as to derive past processes from present instances: “the present is the key to the past” and “the past is the key to the future” (Archer & Rahmsdorf 2009: 105). However, as soon as causation is assessed empirically with regard to such assumptions, problems arise that are very difficult to address. The philosopher Nancy Cartwright (1999) states that the strict causality of physics, which states that a physical law has to be always true is rarely exactly the case except under laboratory conditions. Hence, this strict causality, which claims universality, should be replaced by a weaker causality, which refers to the capacities within the things, i.e., a law might be true, but not universally so. For example, a painkiller does not always kill the pain, but only has the capacity to do so (cf. Caldwell 2000). This notion of capacity takes into account the circumstances, or rather (extrinsic) influences that may even reverse physical laws, as these generally only hold *ceteris paribus* (Cartwright 1999: 50). For example, Coulomb’s law states a constant for the repulsion of two negatively charged particles. However, under non-laboratory circumstances, the measured values of the so-called constant differ, and the two negatively charged particles might even approach instead of repel (ibid: 59f). These problems of strict causation become amplified when complex systems are analysed, where it is nearly impossible to distinguish extrinsic and intrinsic forces (Coombes & Barber 2005: 305). Since we cannot be certain about the “facts” of scientific results (in the sense of trueness or objectivity), any deduction for a future applicability of today’s results only shows a probability that the processes might be the same in the future. Hence, a different understanding of causality is necessary, i.e., one that takes into account that correct predictions are a rarity and, most of all, that

there is not only one predictable outcome to a cause (cf. Cartwright 1999: 152ff). In the end, the problem of causality is a problem of induction, as Russell already stated in 1912:

“That is to say, when a law exhibiting, e.g., an acceleration as a function of the configuration has been found to hold throughout the observable past, it is expected that it will continue to hold in the future, or that, if it does not itself hold, there is some other law, agreeing with the supposed law as regards the past, which will hold for the future. The ground of this principle is simply the inductive ground that it has been found to be true in very many instances; hence the principle cannot be considered certain, but only probable to a degree which cannot be accurately estimated.”⁹ (Russell 1912: 15f)

« 26 » Due to the principally inductive character, any causal relationship can be falsified by one single different event. Furthermore, due to positive and negative feedback loops, thresholds, etc. in the climate system as well as in other complex systems, it is impossible to distinguish cause and effect: the system can indeed be successfully described by means of mathematical equations, but only because the framework conditions are set during calculation (cf. Russell 1912, and for an example on the difficulties of discerning cause and effect, Coombes & Barber 2005). This, precisely, seems to be one of the core difficulties faced by climate change researchers, who need to argue that the increase in CO₂ causes global warming and not, for instance, vice versa, i.e., that global warming causes an increase in CO₂. Within climate change research, this challenge is aggravated, as for most of what is currently known, climate scientists have to rely on correlations. The very specifics of how causation and correlation relate to each other – summarized under the key phrase that “correlations are not causations” – may play into the hands of climate sceptics, who in turn tend to forget that the same conditions apply to their results.

« 27 » Whilst heated debates about scientific findings and assumptions are common and even necessary for the scientific process, the controversy about causes and effects of climate change and the trueness of the findings in climate change research

exemplifies that this can lead to discussions that cannot be won and that are blocking rather than advancing our progress in scientific knowledge. Furthermore, if fought out publicly, they undermine the expert status of scientists, which however, is insisted upon from both sides. First-order science obviously does not hold any mechanisms or practices that offer a way out of these dead-end discussions. In contrast, with its focus and emphasis on theory-dependency, observer-dependency and general awareness of the tentativeness of knowledge, the different scientific attitude of self-reflexive second-order science might offer possibilities for a return to a mutually productive or at least less obsessive discussion.

Conclusion: Second-order science as a new way of doing science?

« 28 » As already mentioned above, the term second-order science combines such diverse aspects as

- 1 | self-reflexivity,
- 2 | transdisciplinarity and
- 3 | complex, non-linear systems.

These three aspects refer to very different objectives of science and thus lead to different conclusions and demands:

« 29 » *Ad 1:* Self-reflexivity together with a general awareness and acknowledgment of the theory- and observer-dependency of scientific doing refers to processes and requisitions *internal to science*. It is up to us scientists to be vigilant about our certainties and comforting knowledge. However, whilst thinking and discussing our epistemological research foundations will certainly slow down the research process, this slowing may not hold only disadvantages (as, for example, emphatically argued by the so-called slow-science movement, <http://slow-science.org/>). Self-reflexivity surely will lead to some loss of confidence in the results (or in the basic assumptions and, thus, the methods, etc.) and this could be understood as an inappropriate self-weakening of the expert status of scientists (which might also result in a further weakened position in public perception of science). Nevertheless, we are confident that self-reflexivity will contribute to more openly conducted

discussions and might enable us to return to theories instead of an (ultimate) truth (with a tendency to turn to “religious wars” when conflicts arise). Rethinking what we already know in a self-reflexive way might slow down the current scientific rush with its focus only on innovations and might question the perceived necessity to produce ever more knowledge (Fuller 1999) – to us, it seems wise to evaluate and re-examine already existing knowledge to get to know what we know. It might be the difference between “going forward” in our knowledge and “going into depth” – both are sorely needed, but currently there is a strong tendency to “going forward” (for a discussion of why “innovation is not the holy grail,” cf. Seelos & Mair 2012). It will surely take some time, but it might even result in a re-strengthening of the self-concept of researchers and, beyond that, might entail some sort of relief: the possibility (also publicly) to communicate that the presuppositions are contingent and that the results are uncertain. This transition would, of course, require a public and societal counterpart that is able and willing to understand science in this way.

« 30 » *Ad 2:* Transdisciplinarity, with its demands for the participation of stakeholders outside the scientific community and, thus, the socially distributed production of knowledge and the application of transdisciplinary methods, refers to *science policy*, which also includes well-operating science-to-public communication. One first convincing attempt to establish an adequate “boundary communication” between climate science and stakeholders and decision makers at different levels is the development of so called “climate services” (cf. Vaughan & Dessai 2014). It is a sophisticated conceptualization – particularly considering the presuppositions, limitations and uncertainties inherent to climate research – that has been developed from the perspective of post-normal science (von Storch et al. 2011). The idea of “climate services” could easily be linked to our considerations against the background of second-order science. However, an explicit suggestion on how to build up an adequate boundary communication between science and public in terms of second-order science would have gone far beyond the scope of this paper.

« 31 » *Ad 3:* The term “complex and/or non-linear systems” refers to a change within the theoretical scientific perspectives in *systems theory* and, thus, is also a debate *internal to science* in a specific theoretical field. The shift from a linear understanding of systems to a perspective of complexity and non-linearity is still in the making since there are still very many notions around what complexity is (cf. for instance Kwapien & Drozd 2012; Mainzer 2003; Reitsma 2003). Complexity in the wake of complexity theory does not mean “being more complicated,” for instance including even more parameters or indicators into an analysis of a phenomenon – so far still a common interpretation (cf. Keiler 2011). Applying complexity theory and analysing complex dynamics (such as the climate system or other social-ecological systems) rather requires the observation of the concurrence of hierarchic as well as of heterarchic structures in non-linear processes, with the consequence that the outcomes of events are quite hard (or even impossible) to predict. Chaos theory has already shown that tiny variations of initial conditions can have strong and surprising effects, and that even systems with simple rules can show unpredictable behaviour. Consequently, complexity theory focuses on non-linearity and erratic processes, evading simple mathematical descriptions. Hence, specific phenomena can show unexpected effects, often distant in time and space from where they occurred. The implication of complexity is not only to overcome the traditional, hierarchical and linear thinking about what constitutes socio-natural or socio-spatial order and how it is preserved, it rather indicates that systems do not necessarily move towards an equilibrium and that events are mostly unpredictable and could be irreversible in their effects.

« 32 » Thus, second-order science can be understood as a generally different scientific attitude, which leads to other scientific practices and – hopefully – to more successful science-to-public communication. A strengthening of the specifics of the sciences is urgently needed as well as an awareness of the sophistication of our research objects, which have become much more complex within the last thirty years or so. Science in the 21st century strives for an understand-



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ing of uncertainties, contingencies and non-linear dynamics. The expectation on scientists to tell politicians what and how they should decide – as is currently happening with climate change researchers – assumes that even in a complex world there are (technical, political, societal, etc.) solutions to a problem that do not themselves induce new “collateral damage” and unforeseen and unforeseeable side-effects. In other words, as Craig Dilworth (2009) puts it, we might simply be “too smart for our own good,” and

our search for ever more solutions might just lead us into a vicious cycle of developing ever new technology and thereby producing ever more problems for which new technology is needed. However, it is just this search for “solutions,” coupled with an indestructible linear thinking that has also led to the set of crises we currently face since the proposal of a technical solution then tends to be discussed as the only alternative. Whenever there is a (political) debate about the “lack of alternatives” in a decision processes, we

should take this as a serious warning – there is always an alternative. Global environmental change and the accompanying (global) social transformations can no longer be addressed with linear thinking, the search for easy applicable technical solutions and the promises of security and certainty based on assured scientific knowledge. It is high time for a change of our attitude.

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Open Peer Commentaries

on Philipp Aufenvenne et al.'s "On Climate Change Research, the Crisis of Science and Second-Order Science"

Doing Second-Order R&D

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> **Upshot** • Bringing second-order understandings to the doing of climate science is to be welcomed. In taking a second-order turn, it is imperative to reflect on reflection, or report authentically our doings and thus move beyond sterile debates about what ought to be or what second-order doings are or are not. The field of doing second-order R&D is not a terra nullius, so exploring the full range and domains of praxis is warranted.

« 1 » I am very sympathetic to the arguments being mounted in this target article as it is an area of praxis (theory-informed practical action) that is undeveloped yet very relevant to our times (Ison 2010). In my commentary I do not propose to dwell on the overall thesis but to embellish certain arguments and point to underdeveloped or absent considerations. I do so because I believe it important that readers understand that what is being proposed by the authors is not novel, or untested, but does remain subjugated. And collectively we need to devise strategies to break out of this subjugation. In making this claim, I think it is fair to concede that climate science has, to date, been little influenced by second-order understandings. In turn, this appreciation raises concerns about what is conveniently hidden behind the term "climate science."

« 2 » I have no doubt of the need for greater reflexivity (reflection on reflection, a second-order notion) on the part of those practitioners who claim to be scientists

(Ison et al. 2014; Ison 2008). Authenticity arises when there is reflection on experience, because the congruence between espoused theory and theory in use can then be experienced by an other (a reader, a colleague etc.). I was initially left with a sense that the authors had not yet tried to do second-order science – so for me there was an absence of authenticity, e.g., a tendency to speak of science as if such a state, with agency, existed rather than offering a reflection on their own practice as practitioners of science, as embodied doers of science (if indeed this is what they do). However, towards the end of the paper there is a nice list of questions concerned with the doing of science, which have the potential to trigger reflexive insights grounded in praxis. From my perspective, Humberto Maturana's aphorism "all knowing is doing" is apposite, but what science is in its doings is a contested space. A question that needs to be asked is: Do the authors really mean knowledge or do they mean processes of knowing? (see Cook & Wagenaar 2011).

« 3 » Science as a domain of practice can be understood as generating what are or are not accepted as scientific explanations. What is or is not accepted unfolds in social relations. This is very apparent in the field described as "climate science," as can be appreciated at this historical moment as peoples of the world struggle to come to terms with the explanations offered. Framing the doing of science with the term "climate" has also revealed and concealed in ways that may not be helpful. Climate, and its subsidiary, weather, are pervasive, i.e., their effects operate in every domain, from river catchment management to sewage engineering. It is thus problematic when a particular framing captures the discursive

space, as well as investment resources, and generates new institutions (e.g., the production of climate change adaptation reports by siloed or isolated departments in local government agencies) to the detriment of all the other domains where climate variations will operate. Practitioners of a second-order science need to be aware of the historicity of their understandings, and how this history informs framing choices, practices and the institutions (norms, rules of the game that humans invent) that surround them (Russell & Ison 1993, 2000; Ison, Blackmore & Iaquinto 2013).

« 4 » It would be a shame if readers of this paper were left with the impression that there was not a developed and evolving praxeology in second-order science. The authors refer to several important lineages but fail, in my view, to mention other important sources, for example the early work of Horst Rittel, who with Melvin Webber called for the development of second-generation systems approaches (Rittel 1972). Also not mentioned, except obtusely through reference to Stuart Umpleby's work, is the whole sub-field of second-order cybernetics and the work of such important scholars as Gregory Bateson, Humberto Maturana, Heinz von Foerster, Klaus Krippendorf, Gordon Pask, Ranulph Glanville, etc. (see the frequent references to these and contemporary authors in papers in this journal). Drawing on these traditions, there has been considerable attention paid to second-order praxis within the fields of agricultural and rural development research (Russell & Ison, 1993; Ison & Russell 2000, 2011), and now systemic innovation (Ison 2015). Other important fields with second-order influences include systemic design (Metcalf 2014), systems science (Ison 2010) and systemic fam-

ily therapy (Poerksen 2004). These lineages are all praxis lineages, as is science, so there is much to be drawn upon and institutionalised in ways that are socially valued and that attract investment

« 5 » Let me conclude with a plea. Let us not become stuck in a discursive trap about what second-order science is or is not. As Heinz von Foerster might say, how can we move forward in ways that maximise our choices? One way of doing this would be to address the question: What world(s) do we bring forth when we take responsibility for our observing? Or in Maturana's terms: What is it that we do when we do what we do when we claim to do second-order science/R&D (see Ison 2010)?

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On Detection and Attribution

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> **Upshot** • I discuss the concepts of detection and attribution as they are used in scientific discussions about the cause of global warming.

« 1 » In my commentary on Philipp Aufenvenne et al.'s target article I want to focus on §17, i.e., on "detection and attribution." I claim that their assertion "Since CO₂ has long been known to be a greenhouse gas, the observed rise in CO₂ concentra-

tions within the atmosphere has suggested itself as the main cause of global climate change" is inaccurate as it applies only to the climate change that began to emerge in the 20th century, when it became clear that human activities would significantly increase atmospheric CO₂ concentrations. For older geologic eras, one would see parallel developments of CO₂ concentrations and temperatures, as derived from proxy records. But the accuracy and temporal resolution of these records was hardly sufficient to decide whether one would lead the other. Indeed, since no external cause for elevated or reduced CO₂ concentrations could be given, it is plausible that it CO₂ follows temperature. In the popular literature, the correlation of the two was made into an argument for elevated CO₂ levels being the cause of the ongoing process of global warming, but not in scientific circles.

« 2 » To deal with the recent change of climate, the concept of "detection and attribution" of Klaus Hasselmann (1979) was invoked, as rightly described by the authors.

« 3 » "Detection" means to identify a change as beyond the range of natural internal variations within the climate system; the presence of variations "without causes" is difficult to understand for lay people, who often enough insist that "where there is smoke, there is fire." The climate system is full of non-linear processes, which as a sum appear as something that is well-described by the mathematical construct of randomness (red noise, pink noise) with significant long-term variations (Hasselmann 1976). "Detection" means, if a dead body is found, that when the death cannot be explained by natural causes, detectives are then asked to look for suspects and to determine who may have done it.

« 4 » The second step is called "attribution." While detection represents a stringent statistical hypothesis test (with the difficulty of determining the appropriate null-distribution), attribution is a plausibility argument, namely: Which of the suspects best fits the profile of the crime? Of course, it can be that the series of suspects that is examined does not contain the real murderer, so that a misattribution takes place. In the end, an assertion is made that "we can explain the ongoing change" best by attributing x% of the change to process X, and y% to Y, etc.

If done properly, a caveat "given our present understanding of the system and its sensitivity" is added.

« 5 » The expectations, or "signals" of how a certain possible "cause" may act on the climate system are derived from simulations with dynamical climate models that quantitatively describe these expectations (or "guess patterns"). The output of such models is also used to estimate the range of natural variations. Except for these two applications, the process of detection and attribution does not make use of climate models; instead it is an assessment of observed data.

« 6 » The detection and attribution efforts began to become successful in the mid-1990s (e.g., Hegerl et al. 1996), when analyzing global decadal trends in air temperature. In the meantime, other variables have also emerged as influenced by elevated atmospheric greenhouse gas presence (The International ad hoc Detection and Attribution Group, 2005). Approximately 1/2 or more of the centennial change is attributed to increased CO₂ concentrations and other greenhouse gases, while 1/2 or less may be due to changes in solar forcing, volcanism and aerosol forcing.

« 7 » In hindsight, in the 1980s we may have already detected a global change that needs explanation through external causes (Rybsky et al. 2006). Regionally and locally, the detection and attribution is more complicated (Barkhordarian, von Storch & Bhend 2013), as more "suspects" are present, such as massive changes in aerosol generation and land-use changes (urban developments).

« 8 » In summary, the issue of whether the recent climate change, in particular in terms of air temperature, is related to changes in the presence of greenhouse gases is not based on the co-variability of the presence of such gases and temperature, but on the detection of changes beyond the undisturbed regime, and the determination of the most plausible mix of causes. In terms of air temperature, the recent changes cannot be explained without making use of elevated greenhouse gas concentrations; this explanation is consistent with physical theory, but remains conditional upon the present body of scientific knowledge.

« 9 » In the public domain, this rather sophisticated assessment transforms to the

assertion that the cause of climate change, and increasingly violent weather extremes, is due to the ongoing human emission of greenhouse gases. Such a transformation of scientific assessments is not surprising when post-normal conditions prevail, as in the case of climate sciences and climate policies (von Storch 2009).

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First Aid for Climate Research with Second-Order Science

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> Upshot • On an epistemological level, Aufenvenne, Egner and von Elverfeldt argue convincingly for an increasing role for second-order science in climate research. However, the authors partially underestimate the already increasing role of reflexive critique in climate discourse, and they do not yet fully take into account the radical changes in our conception of climate change through the deployment of a second-order approach.

« 1 » The target article by Philipp Aufenvenne, Heike Egner and Kirsten von Elverfeldt article makes a highly welcome and necessary contribution to the debate of the current status of climate research in the climate debate. Climate science has had a spectacular career since human induced green house gas emissions were singled out through detection and attribution. After the 2007 Nobel Prize for Peace for the IPCC and Al Gore, a series of scandals and public debates haunted climate research. From then on, its public reputation suffered. Nonethe-

less, climate politics relied on science-based goals (such as the 2-degree target) and turned into an “anti-politics machine” (Ferguson 1994), while the political debate subsequently shifted into climate science.

« 2 » This is where the target article comes in. The authors argue that in the course of the climate debate, climate science has lost public trust. Knowledge about climate change is partially uncertain, tentative and temporary. According to the authors, this undermines public expectations towards science and scientific knowledge. They see this as part of a general feature of “second modernity” (Beck, Giddens & Lash 1996), which is characterized by an increasing destabilization of values and institutions in society (§1). While climate research might be an indicator for the ills of a “second modernity” or not, there is certainly more to the current crisis, as the authors also suggest in their article, even though tentatively and somewhat reluctantly.

« 3 » The authors frame the communication problem mostly in terms of epistemology. In order to improve public communication, they suggest supplementing or even replacing first-order climate science with second-order science. Consequently, they discuss and convincingly suggest applying mode-2 research, post-normal science, self-reflexivity and a change in theoretical scientific perspectives to “complex and/or non-linear systems” in order to overcome the current problems in public-science communication. This is well argued and serves as a necessary and provoking contribution to the debate about the role and status of climate research in climate politics and communication. The authors spend a great deal of time on explaining the difference between first-order and second-order science; in doing so, they sometimes reduce climate science to just another example of science in general. This reduction does not always do justice to the prominent and special role of climate science; they neglect the fact that climate science has a troubled history of its own. In my opinion, a more ethnographic approach in terms of science and technology studies could provide a more detailed insight into the workings of the current status and dynamics of climate research.

« 4 » In the following, I would like to extend further the argument that a second-

order science approach should also take into account the cultural and political history of climate research; a dimension that is only sporadically highlighted by the authors. Their focus on mainly epistemological and generalizing aspects tends to miss out some of the specific features that distinguish the climate debate from other debates and climate science from other sciences. Most of all, there is more to the debate than only smoothing out communication between science and the public; from a second-order science approach, the definition and understanding of the climate change problem itself possibly has to change. The understanding of anthropogenic climate change as catastrophic and carbon-based, as Jerry Ravetz¹ characterizes the dominant science-based climate discourse, reduces the climate problem to a governance problem using technological criteria such as mitigation, adaption and resilience. The social and political dimension of unequal access to and use of fossil fuels, of social inequality and environmental justice, for example, are excluded from this discourse.

« 5 » Thus, the article is somehow trapped in an unsolved tension between epistemology and politics. The authors tend to attribute the polarized nature of the climate debate to epistemological problems and to the unwillingness of scientists to disclose and discuss uncertainty. But how does this relate to their statement that from the beginning, climate change was a political hypothesis (§17)? If this is true – and I have no doubt it is – there is more to the crisis in climate research than only epistemological problems in communicating uncertainty. Of course, there is: the authors rightly mention the (in)famous hockey stick debate as an example of the crisis of climate research (§17). A well-chosen example, as it serves as an indicator for the increasing politicization and scandalization of climate research. But the authors tend to underplay the political and cultural context of these “religious wars” (§29) and how climate research turned into

1 | “Climategate: Plausibility and the blogosphere in the post-normal age,” by Jerry Ravetz. Retrieved from <http://wattsupwiththat.com/2010/02/09/climategate-plausibility-and-the-blogosphere-in-the-post-normal-age/> on 23 October 2014.

an “evangelical science” (see footnote 1). In fact, there is a cultural and political history of climate science that originates in the science wars of the eighties, in the cultural wars mainly in the US and with the rise of the Tea Party and finally in the symbolic equation made by Jerry Ravetz, who compared the “war on carbon” to the “war on terror” (in Krauß, Schäfer & von Storch 2012). There are deep implications in this drastic analogy: the war on carbon might be as wrong as the war on terror was, rendering climate politics fruitless and ineffective.

« 6 » After the failure of the COP15 climate summit in Copenhagen, “the dangerous relationship between climate research and politics” (von Storch & Krauß 2013) became an even more widely addressed topic in the social and political sciences. Second-order science perspectives at least occasionally entered climate discourse; there were reflexive, critical and constructivist analyses of the IPCC reports (Beck 2012), of its documented errors (van der Sluijs 2012) and of the hacked emails from climategate (Grundmann 2012; Ryghaug & Skjølsvold 2010). These contributions from science and technology studies and social sciences have already left a significant impact on the recently published new IPCC report.

« 7 » Bruno Latour (2004) also provides an interesting discussion of the prospects and consequences of analyzing the production of climate knowledge in terms of constructivism. Climate skeptics easily adopted the constructivist approach and the notion of uncertainty and turned it into an argument against climate politics. In his recent book, Latour (2013) reconsidered his position and stated that a statement such as “the sea-level is rising” is already political, as it changes the conditions for politics fundamentally. From this perspective, he adopted Carl Schmitt’s metaphor of war for himself and argued for “climate wars” against those who deny climate change or consider it as a minor problem. He did not abandon constructivism, but he added politics, with diplomacy as its practice and war as its ultimate rationale.

« 8 » Finally, Aufenvenne, Egner and von Elverfeldt suggest that climate services provide a good example of a new approach to improving science-public communication. Again, I suggest that there is a history to be considered. Climate services are forms

of governance that originate in top-down approaches and a linear understanding from science to politics; more often than not, they simply extend first-order knowledge into the practice of climate politics (Krauß & von Storch 2012). This often results in new forms of green colonialism, replacing indigenous knowledge systems with “Western” climate science and values (Mahony & Hulme 2012). As a result of this critique, the chapter on climate services in the new IPCC report includes a reflexive approach and argues for the dialogue between different kinds of knowledge, for knowledge exchange and the identification of pathways instead of top-down solutions.²

« 9 » Aufenvenne, Egner and von Elverfeldt link the problem of uncertainty in climate science with problems in science-public communication. In doing so, they convincingly suggest that second-order science is better suited to communicating climate research. Their approach is far from utopian, as current examples from science and technology studies as well as from social science and ethnographic studies demonstrate. They also tentatively make a first and important step towards laying bare the implicit authoritative approach and even evangelical mission inherent in the policies of climate research. The more radical implications of this approach are only randomly mentioned in the article; it would be great to see them fully developed in subsequent articles. Nonetheless, the article provides a useful epistemological first aid kit for climate research in crisis.

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2] “Climate change 2014: Impacts, adaptation, and vulnerability.” Retrieved from <http://www.ipcc.ch/report/ar5/wg2/> on 23 October 2014.

The Social and Political Context of Science

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> **Upshot** • Second-order science primarily focuses on perception and cognition. However, social contexts, including political interpretations of science, are also included because they are part of the interpretations of the observer. To understand a scientific theory, it is helpful to understand neurophysiology, the history of the individual and the social and political context in which the scientist was operating.

« 1 » In their target article, Philipp Aufenvenne et al. hope that second-order science will ameliorate the stridency of the climate change debate. In my opinion, second-order science, by pointing out the limitations and biases of an observer, can help people to be more tolerant of and more patient with the views of others. However, the importance of the debate and the strong interests of actors involved in the discussion will sustain debating methods that go beyond a normal scientific conversation.

The debate over climate change

« 2 » Just as conceptions of scientific conversations have been changing, conceptions of public conversations have also changed. Consider the fields of advertising, propaganda and political campaigns. Advertising resulted from the combination of a market economy and mass media – newspapers and magazines, radio, television and now the internet. In advertising, the criterion for success is not truth or objectivity but rather whether customers buy the product. It was not long before the methods of advertising were applied not just to selling soap or cars but also to electing political candidates (McGinniss 1969).

« 3 » World War II and the Cold War led to numerous innovations in propaganda. (Brown 1975) Propaganda during wartime is different from advertising in that the reputation of the source (e.g., a company advertising a product or a candidate running for office)

		Message		
		white	gray	black
Source	white	Truthful message	Mixed message	False message
		Source is given	Source is given	Source is given
	gray	Truthful message	Mixed message	False message
		Unclear source	Unclear source	Unclear source
	black	Truthful message	Mixed message	False message
		False source	False source	False source

Table 1 • Types of propaganda.

need not be protected. Complete hostility is already assumed. Methods of propaganda can be classified as white, gray or black on two dimensions – the message and the source (see Table 1). The message can be truthful (as perceived by the source), a mixture of truth and falsehood, or a deliberate lie. The source can be truthfully given, obscured, or attributed to an incorrect source. Just as advertising has made the transition from selling products and services to political campaigns, so too have the methods of propaganda sometimes entered the political arena when some stakeholders feel very strongly about the consequences of the debate.

« 4 » Restrictions on the burning of fossil fuels threaten the profits of large industries, and not offering flood insurance for homes and businesses in some areas threatens property values. Furthermore, relocating cities and farms, even if done over a period of decades, is psychologically disruptive and can lead to denial. Hence, human caused climate change is not a normal scientific debate and we should not be surprised if methods of discussion go beyond what is common in the scientific community. Table 1 shows that the possibilities for styles of communication are larger than just the top left square.

« 5 » The opinions of scientists have been politically important for hundreds of years. Galileo was placed under house arrest. Copernicus decided to publish his conclusions posthumously. Some geneticists in the Soviet Union who resisted the views of Stalin were murdered (Medvedev 1969). In the United States, scientists who disagree with the policies of the administration not infrequently become whistle-blowers. When scientists engage in policy relevant research,

they are not only thinking about theory, methods and data. They are also estimating the consequences for their careers of reaching “politically incorrect” conclusions. Hence, second-order science can encompass issues that go beyond scientific uncertainty and humility about what scientists can know. When vital interests are involved, the issue for stakeholders becomes how to shape the information environment so that political decisions will favor the interests of particular groups – for example, coal producers, or those who live near power plants, or the general public.

« 6 » These remarks were stimulated by the example of climate change. If we turn our attention to the possibility of a second-order science, we are dealing primarily with academic traditions, egos and preferences.

The discussion of second-order science

« 7 » Second-order science assumes that science is an instrument in the regulation of social systems and that human beings are purposeful systems. Claiming that science operates outside of social systems or that the purposes of scientific observers will not affect their observations is not believable in cases where strong commercial or political interests are concerned. Second-order science suggests that we should recognize the personal and social context of observations and research, acknowledge these and, when appropriate, discuss them in research reports. We already do this to some degree. Including brief biographies is a step in this direction. Also, a common practice is to acknowledge the funding source for a study. Usually this is done to give credit to the funding organization rather than to indicate the bias or per-

spective underlying the research. But the latter purpose is served as well. Such disclosures may become more common (Morgan 1983). In the case of climate change, or the earlier debate over the safety of cigarettes, knowing who funded a study can be a better indicator of the conclusions of a report than knowing the theory, method or data used.

« 8 » Advocates for a realist philosophy often argue that any move away from objective science opens the door not just to emotions but to political or self-serving arguments, apparent or disguised. However, if scientists do not acknowledge political interpretations and uses of science, these interpretations will be discussed in a forum separate from science, usually by journalists or “watchdogs” such as the Union of Concerned Scientists. Second-order science suggests that the social and political context is relevant to the science itself, that interpretations and their policy implications are not separate from science (see Figure 1).

« 9 » One hears two arguments against second-order science. One claim is that second-order science would weaken the objectivity and authority of science and hence undermine the effort to create reliable knowledge. A second claim is that by pointing out the perspectives, including interests, of observers and institutions, one legitimizes a no-holds-barred contest over scientific results, at least as perceived by the public. A reply is that the interests of stakeholders need to be known and discussed. Second-order science would acknowledge and comment on the psychological, social and political context of scientific research, when needed, in order to explain the implications of a report.

« 10 » There are several audiences for scientific research. First, the scientific community shares an understanding of research methods and understands the various sources of uncertainty in results, but scientists from one field may have little understanding of the methods in another field (Umpleby 1990). Second, decision-makers, who commonly have a background in law, may know little about a specific scientific issue. Usually they will listen to the judgment of scientists but will also be influenced by lobbyists and campaign supporters. Third, science journalists will listen closely to what scientists say and try to communicate results and un-

certainty to the general public. Fourth, the public will be influenced by many messages about a scientific issue from scientists, journalists, political leaders and affected interest groups.

« 11 » It is important to understand that for several of the actors in a social system, the criterion of success is not truth, certainty or reliable results but rather whether public decisions are consistent with one's interests. Any means of communication that furthers that goal will be favored and considered legitimate by some people. An expanded conception of science would give scientists a larger regulator (in terms of requisite variety) for contributing to the wise management of society.

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Figure 1 • A cartoonist's view of science in the social and political context. With kind permission of the Union of Concerned Scientists, <http://go.ucsusa.org>, and the artist, Kevin Cannon. Each year the Union of Concerned Scientists sponsors a contest for the best cartoons depicting the interaction between science and politics.

Authors' Response: Communicating Second- Order Science

Philipp Aufenvenne,
Heike Egner
& Kirsten von Elverfeldt

> **Upshot** • For communicating second-order science, von Foerster's ethical imperative provides a viable starting point. Proceeding from this, we plead in favour of emphasising the common grounds of diverging scientific opinions and of various approaches in second-order science instead of focussing on the differences. This will provide a basis for communication and stimulate scientific self-reflection.

« 1 » We owe thanks to our commentators as they illuminate aspects of our argumentation in a specific and especially insightful manner. Our response will be organized around the topics of:

- a | maximising choices (Ison §5),
- b | the transformation of scientific findings as soon as they enter the public agenda (von Storch §9), and
- c | the notions of "history matters" and "personal and social context matters" (paraphrasing Krauß §§3ff and Umpleby §7 and §9).

« 2 » We will start with a response to Ray Ison's legitimate plea to "not become stuck in a discursive trap about what second-order science is or is not" (Ison §5). This is why we focussed on a current scientific-public debate and left the history of second-order science and a tracing of the full range and domains of its praxis (Ison §4) to other au-

thors and other papers. We completely agree with his conclusion. To quote Heinz von Foerster's ethical imperative, "Act always so as to increase the number of choices" (Foerster 2003: 227), the question is indeed: "How can we move forward in ways that maximise our choices?" (Ison §5).

« 3 » One strong possibility for maximising our choices in scientific debate would be to overcome the tendency to mark differences and delve into dichotomies and dualities (as it is still common in scientific practice). Emphasising common ground instead of focussing only on the differences opens the doors to communication and fruitful debate. As soon as scientists of different disciplines and backgrounds find ways to exchange and discuss their assumptions and findings in a constructive manner (in the sense of being based on a shared ground), space for self-reflexivity unfolds. In

the context of climate change research and second-order science, a shared notion could be that of the impossibility of finding “true results.” At a first glance, this might create the impression of being (yet another) starting point for epistemological disputes between the representatives of “realistic” and “anti-realistic” approaches. However, it has to be seen against the background of the theoretical set of complex and non-linear systems. Even though the majority of climate change researchers does not share the constructivist epistemology¹ that prevails throughout most strands of second-order science, they strongly support the notion that direct cause and effect relations cannot be found, as a consequence of the principal non-linearity of the climate system. Whilst starting from completely different research designs, the findings of constructivists and climate scientists thus both culminate in the notion of precluding final truths.

« 4 » This understanding is also the basis of the concept of detection and attribution (as has been very clearly argued in the commentary by **Hans von Storch**). Within this concept, scientists attribute probabilities to detected changes that are “beyond the natural internal variations within the climate system” (**von Storch** §3). In a nutshell, “attribution is a plausibility argument” (**von Storch** §4). This requires negotiation processes within the scientific arena, and finally “what is or is not accepted unfolds in social relations” (**Ison** §3). Thus, despite widely different epistemologies, the respective scientific research processes lead to the same conclusion: we cannot make final statements about truth(s), only about probabilities and plausibilities. Differing epistemologies therefore do not always make a difference, and thus we absolutely agree with **Werner Krauß** (§5) that “there is more to the crisis in climate research than only epistemological problems in communicating uncertainty.”

« 5 » At the same time, however, climate change researchers are especially challenged

1 | At least that is what we assume, although climate skeptics seem to like the constructivist idea of the uncertainty of all knowledge (**Krauß** §7) and tend to apply that idea to unwelcome findings of climate researchers. At the same time, they do not necessarily accept this perspective as a basis of their own criticism as well.

to explain their findings to the interested public, as climate change is a prime issue on the public agenda. Thereby, climate change science and politics “have established a dangerous relationship, with science setting the political agenda [...], thus de-politicising the political agenda” (**Krauß** 2015: 60). As “the presence of variations ‘without causes’ is difficult to understand for lay people” (**von Storch** §3) this “dangerous relationship” demands a transformation of scientific unknowables to simple cause and effect relationships, which will then be easily understood as statements of truth. If indeed statements such as “the sea-level is rising” are seen as political statements (**Krauß** §7, citing Latour 2013), this urge might become more understandable. From that perspective, communicating truths is indeed a simple necessity in order to (i) be understood outside the scientific realm, and (ii) display expertise in order to be able to act in a politically correct manner “rather than admitting uncertainties and open questions” (**Krauß** 2015: 72). At the same time, it indicates why epistemology, nevertheless, might be helpful: we do not share Latour’s point of view that a statement is political just because it changes the conditions for politics. Instead, we see it as a prime example of the dangerous relationship between climate research and politics, which feeds on scientific findings that are translated into the language of “the wrong and right” of ethics and political decisions. Self-reflexivity due to an epistemology that questions truth might act as a means to defuse the booby traps of this dangerous relation (to stick to the metaphors of war, **Krauß** §5). Instead, it could lead to a less dangerous relation and contribute to a re-separation of scientific and political realms. Hence, whilst there is more to the crisis than epistemological problems, an epistemologically based “state of doubt” might help.

« 6 » **Krauß** repeatedly points to the “history” of climate change research and its related debates and aspects. We would even push his notion of “history matters” a bit further and argue that it could serve as yet another strand of common ground between radical constructivist notions of second-order science and the complexity scientists within climate change research. Whilst, yet again, coming from completely different sides, the insight is the same. For example,

observation – and especially second-order observation – is at the core of radical constructivist approaches. It employs the question “how is (something) being observed?” This includes at least some understanding of the history of how something has been and is being observed, and which other paths of observation could have been chosen but were not, thereby bringing “some things” into being but others not. Furthermore, second-order science acknowledges “the person” behind the scientist: it is always a specific “somebody,” who is doing her or his research in a very specific way. This way of doing science cannot be seen as being independent from the personal biography of that somebody – an aspect **Stuart Umpleby** (§7) points to. Thus, history matters, even in this indirect way. Studying complex and non-linear systems such as the climate system brings with it the same insight: history matters, because the probability of future system states depends on the initial conditions of the system and on the subsequent paths that have been “chosen” by the system.

« 7 » Besides the necessity to include history, **Umpleby** stresses the point that context also matters (§9). The commentaries to our article might serve to provide a simple example: the commentators come from rather distinct disciplines and, thus, “one’s interests” (**Umpleby** §7) are the context in which our article has been read. This context is based on the personal scientific context but is also, of course, influenced by the specific momentary personal context, including the current active knowledge, current personal research ideas, daily news coverage, reported events, etc. It is this context that influences what is selected in which way and which aspects are regarded as being important or not. The understanding that communicative success within society depends on whether the outcome is “consistent with one’s interests” (**Umpleby** §11) is certainly a lesson learned the hard way, especially by climate change scientists. So far, politicising climate change research seems to be the main reaction to this lesson. However, the insight that “political interpretations and uses of science” (**Umpleby** §8) are inevitable indeed should not be reduced to simply extending “first-order knowledge into the practice of climate politics” (**Krauß** §8). Instead, scientists have to understand that as

soon as they publish their results, their work enters the “agora.” There, the findings compete with a multitude of different kinds of knowledge (Krauß §8), and the political decision on which action to take is a democratic negotiation process, not a scientific one.

« 8 » On the basis of the commentaries as well as fruitful discussions with colleagues from different scientific paths, we conclude: the notion of second-order observation that

we have chosen as a starting point for our article as well as our specific contexts (scientific, social, and personal) allowed specific conclusions, while precluding others. By focussing on epistemology as a framework for the crisis of climate change research, we did not include and thereby probably underplayed “the political and cultural context” (Krauß §5) of the crisis. We agree that this has to be explored in further research.

We are very grateful for all four commentaries, since they add to our argumentation by deepening different crucial aspects from different perspectives. This we understand as an amplification of our statement that within science “it is high time for a change of our attitude” (our §32).

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Combined References

- Archer D. & Rahmsdorf S. (2009) The climate crisis. Cambridge University Press, Cambridge.
- Barkhordarian A., von Storch H. & Bhend J. (2013) The expectation of future precipitation change over the Mediterranean region is different from what we observe. *Climate Dynamics* 40: 225–244.
- Beck S. (2012) Between tribalism and trust: The IPCC under the “public microscope.” *Nature and Culture* 7(2): 151–173
- Beck U. (2009) *World at risk*. Polity Press, Cambridge UK.
- Beck U. & Bonß W. (2001) *Die Modernisierung der Moderne*. Suhrkamp, Frankfurt am Main.
- Beck U., Giddens A. & Lash S. (1996) *Reflexive Modernisierung*. Suhrkamp, Frankfurt am Main.
- Becker E. & Jahn T. (2006) *Soziale Ökologie*. Campus, Frankfurt am Main.
- Becker E. & Wehling P. (1993) *Risiko Wissenschaft*. Campus, Frankfurt am Main.
- Bray D. & von Storch H. (1999) Climate science: An empirical example of postnormal science. *Bulletin of the American Meteorological Society* 80(3): 439–455.
- Bray D. & von Storch H. (2014) A survey of the perceptions of climate scientists 2013. *Helmholtz-Zentrum Geesthacht, Geesthacht*.
- Brown A. C. (1975) *Bodyguard of lies*. Harper & Row, New York.
- Caldwell R. (2000) The dappled world. *Philosophy Now* 28: 42–43.
- Cartwright N. (1999) *The dappled world*. University Press, Cambridge.
- Cook S. D. N. & Wagensaar H. (2011) Navigating the eternally unfolding present: toward an epistemology of practice. *The American Review of Public Administration* 10(5): 1–36.
- Coombes P. & Barber K. (2005) Environmental determinism in Holocene research: Causality or coincidence? *Area* 37(3): 303–311.
- Dilworth C. (2009) *Too smart for our own good: The ecological predicament of humankind*. Cambridge University Press, Cambridge.
- Egner H. (2007) *Überraschender Zufall oder gelungene wissenschaftliche Kommunikation: Wie kam der Klimawandel in die aktuelle Debatte?* GAIA 16(4): 250–254.
- Egner H. (2010) *Theoretische Geographie*. WBG, Darmstadt.
- Ferguson J. (1994) *The anti-politics machine. “Development,” depoliticization, and bureaucratic power in Lesotho*. University of Minnesota Press, Minneapolis.
- Foerster H. von (1984) *Observing systems*. Intersystems Publications, Seaside CA.
- Foerster H. von (2003) On constructing a reality. In: Foerster H. von, *Understanding understanding: Essays on cybernetics and cognition*. Springer, New York: 211–227. Originally published in 1973.
- Foerster H. von (2006) *Sicht und Einsicht*. Carl-Auer, Heidelberg.
- Fuchs P. (1992) *Niklas Luhmann – beobachtet*. Westdeutscher Verlag, Opladen.
- Fuller S. (1999) *The governance of science*. Open University Press, Buckingham, Philadelphia.
- Funtowicz S. O. & Ravetz J. R. (1990) *Global environmental issues and the emergence of second-order science*. Report EUR 12803 EN. Commission of the European Communities, Luxembourg.
- Funtowicz S. O. & Ravetz J. R. (1993) *Science for the post-normal age*. *Futures* 25 (7): 739–755.
- Gibbons M., Limoges C., Nowotny H., Schwartzman S., Scott P. & Trow M. (1994) *The production of knowledge*. Sage, London.
- Grundmann R. (2012) The legacy of climategate: revitalizing or undermining climate science and policy? *Wiley Interdisciplinary Reviews: Climate Change* 3(3): 281–288.
- Grundmann R., Rhomberg M. & Stehr S. (2012) *Der Klimawandel und die Rolle der Sozialwissenschaften*. In: Egner H. & Schmid M. (eds.) *Jenseits traditioneller Wissenschaft*. Oekom, München: 95–111.
- Hacking I. (1983) *Representing and intervening*. Cambridge University Press, New York.
- Hasselmann K. (1976) Stochastic climate models. Part I: Theory. *Tellus* 28: 473–485.
- Hasselmann K. (1979) On the signal-to-noise problem in atmospheric response studies. In: Shaw B. D. (ed.) *Meteorology over the tropical oceans*. Royal Meteorological Society, Bracknell, Berkshire UK: 251–259.
- Hegerl G., von Storch H., Hasselmann K., Santer B. D., Cubasch U. & Jones P. D. (1996) Detecting anthropogenic climate change with an optimal fingerprint method. *Journal of Climate* 9: 2281–2306. Available at <http://journals.ametsoc.org/toc/clim/9/10>
- Hoffman A. J. (2012) Climate science as culture war. *Stanford Social Innovation Review*, Fall: 29–37. Available at http://www.ssireview.org/pdf/Fall_2012_Climate_Science_as_Culture_War.pdf
- Ison R. L. (2008) Methodological challenges of trans-disciplinary research: Some systemic reflections. *Natures Sciences Sociétés* 16: 241–251.
- Ison R. L. (2010) *Systems practice: How to act in a climate-change world*. Springer, London.
- Ison R. L. (2015) What is systemic about innovation systems? The implications for poli-

- cies, governance and institutionalisation. In: Francis J. & van Huis A. (eds.) *Innovation systems: Towards effective strategies in support of smallholder farmers*. CTA/WUR, Wageningen. In press.
- Ison R. L. & Russell D. B. (eds.) (2000) *Agricultural extension and rural development: breaking out of traditions*. Cambridge University Press, Cambridge UK.
- Ison R. L. & Russell D. B. (2011) *The worlds we create: designing learning systems for the underworld of extension practice*. In Jennings J., Packham R. P. & Woodside D. (eds.) *Shaping change: Natural resource management, agriculture and the role of extension*. Australasia-Pacific Extension Network (APEN), Australia: 66–76.
- Ison R. L., Blackmore C. P. & Iaquinto B. (2013) *Towards systemic and adaptive governance: Exploring the revealing and concealing aspects of contemporary social-learning metaphors*. *Ecological Economics* 87: 34–42.
- Ison R. L., Carberry P., Davies J., Hall A., McMillan L., Maru Y., Pengelly B., Reichelt N., Storzaker R., Wallis P., Watson I. & Webb S. (2014) *Programs, projects and learning inquiries: Institutional mediation of innovation in research for development*, *Outlook on Agriculture* 43(3): 165–172.
- Keiler M. (2011) *Geomorphology and complexity – inseparably connected? Zeitschrift für Geomorphologie, Supplement Band 55(3): 233–257*.
- Krauß W. (2015) *Anthropology in the anthropocene: Sustainable development, climate change and interdisciplinary research*. In: Greschke H. & Tischler J. (eds.) *Grounding global climate change. Contributions from the social and cultural sciences*. Springer, Berlin: 59–76.
- Krauß W., Schäfer M. S. & von Storch H. (2012) *Introduction: Post-normal climate science*. *Nature and Culture* 7(2): 121–132.
- Krauß W. & von Storch H. (2012) *Post-normal practices between regional climate services and local knowledge*. *Nature and Culture* 7: 213–230.
- Kwapien J. & Drozd S. (2012) *Physical approach to complex systems*. *Physics Reports-Review Section of Physics Letters* 515 (3–4): 115–226.
- Latour B. (2004) *Why has critique run out of steam? From matters of fact to matters of concern*. *Critical Inquiry* 30(2): 225–248.
- Latour B. (2013) *An inquiry into modes of existence. An anthropology of the moderns*. Harvard University Press, Cambridge MA.
- Lenton T. M., Held H., Kriegler E., Hall J. W., Lucht W., Rahmsdorf S. & Schellnhuber H. J. (2008) *Tipping elements in the Earth's climate system*. *Proceedings of the National Academy of Sciences* 105: 1786–1793.
- Leuschner A. (2012) *Die Glaubwürdigkeit der Wissenschaft*. Transcript, Bielefeld.
- Luhmann N. (1992a) *Die Wissenschaft der Gesellschaft*. Suhrkamp, Frankfurt am Main.
- Luhmann N. (1992b) *Beobachtungen der Moderne*. Westdeutscher Verlag, Opladen.
- Luhmann N. (1993) *“Was ist der Fall?” und “Was steckt dahinter?” Die zwei Soziologien und die Gesellschaftstheorie*. Bielefelder Universitätsgespräche und Vorträge 3. StadtBlatt, Bielefeld.
- Luhmann N. (1998) *Die Gesellschaft der Gesellschaft*. Suhrkamp, Frankfurt am Main.
- Mahony M. & Hulme M. (2012) *Model migrations: Mobility and boundary crossings in regional climate prediction*. *Transactions of the Institute of British Geographers* 37(2): 197–211.
- Mainzer K. (2003) *Thinking in complexity*. Springer, Berlin.
- Mann M., Bradley R. S. & Hughes M. K. (1998) *Global-scale Temperature patterns and climate forcing over the past six centuries*. *Nature* 292(6678): 779–787.
- McGinniss J. (1969) *The selling of the president*. Penguin, New York.
- McGrail S. (2013) *Climate change and futures epistemologies: Tensions, trends and possibilities in climate discourses epitomised by three prominent climate experts*. *Journal of Futures Studies* 17(3): 21–40.
- Medvedev Z. A. (1969) *The rise and fall of T. D. Lysenko*. Columbia University Press, New York.
- Metcalfe G. S. (ed.) (2014) *Social systems and design*. Springer, Tokyo.
- Mitterer J. (2011) *Das Jenseits der Philosophie*. Velbrück Wissenschaft, Weilerswist.
- Morgan G. (1983) *Beyond method: Strategies for social research*. Sage Publications, Beverly Hills CA.
- Neverla I. & Schäfer M. S. (ed.) (2012) *Das Medien-Klima*. VS Verlag für Sozialwissenschaften, Wiesbaden.
- Nowotny H. (1999) *The need for socially robust knowledge*. *TA-Datenbank-Nachrichten* 8(3/4): 12–16.
- Nowotny H., Scott P. & Gibbons M. (2001) *Re-thinking science: Knowledge and the public in an age of uncertainty*. Polity Press, Cambridge.
- Nowotny H., Scott P. & Gibbons M. (2003) *“Mode 2” revisited: The new production of knowledge*. *Minerva* 41: 179–194.
- Parry M. L., Canziani O. F., Palutikof J. P., van der Linden P. J. & Hanson C. E. (eds.) (2007) *Climate Change 2007. Impacts, Adaptation and Vulnerability*. Contribution of Working Group II to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge UK.
- Pielke R. A. (2007) *The honest broker: Making sense of science in policy and politics*. Cambridge University Press, Cambridge UK.
- Poerksen B. (2004) *The certainty of uncertainty. Dialogues introducing constructivism*. Imprint Academic, Exeter.
- Rathmann J. (2008) *Kausalität in der Systemtheorie: Ein Problemaufriss*. In: Egner H., Ratter B. M. W. & Dikau R. (eds.) *Umwelt als System – System als Umwelt? Oekom*, München: 55–71.
- Ravetz J. R. (2004) *The post-normal science of precaution*. *Futures* 36(3): 347–357.
- Ravetz J. R. (2011) *“Climategate” and the maturing of post-normal science*. *Futures* 43: 149–157.
- Reichholf J. H. (2009) *Wehret den Anfängen*. *Forschung & Lehre* 16(3): 165.
- Reitsma F. (2003) *A response to simplifying complexity*. *Geoforum* 34(1): 13–16.
- Rittel W. H. J. (1972) *On the planning crisis: Systems analysis of the first and second generations*. *Bedriftsokonomien* 8: 390–96.
- Rovelli C. (2012) *The uselessness of certainty*. In: Brockman J. (ed.) *This will make you smarter. New scientific concepts to improve your thinking*. Harper Perennial, New York: 102–104.
- Russell B. (1912) *On the notion of cause*. *Proceedings of the Aristotelian Society*. New Series 13: 1–26.
- Russell D. B. & Ison R. L. (1993) *The research-development relationship in rangelands: An opportunity for contextual science*. In: Gaston A., Kernick M. & Le Houerou H. (eds.) *Proceedings of the Fourth International Rangeland Congress, Montpellier, 1991*. Volume 3. Service Central d'Information Scientific et Technique, Montpellier: 1047–1054.

- Russell D. B. & Ison R. L. (2000) The research-development relationship in rural communities: An opportunity for contextual science. In: Ison R. L. & Russell D. B. (eds.) *Agricultural extension and rural development: Breaking out of traditions*. Cambridge University Press, Cambridge UK: 10–31.
- Rybski D., Bunde A., Havlin S. & von Storch H. (2006) Long-term persistence in climate and the detection problem. *Geophysical Research Letters* 33: L06718.
- Ryghaug M. & Skjølsvold T. M. (2010) The global warming of climate science: climate-evidence and the construction of scientific facts. *International Studies in the Philosophy of Science* 24(3): 287–307.
- Saloranta P. M. (2001) Post-normal science and the global climate change issue. *Climatic Change* 50(4): 395–404.
- Seelos C. & Mair J. (2012) Innovation is not the holy grail. *Stanford Social Innovation Review*, Fall 2012: 45–49.
- Solomon S., Qin D., Manning M., Chen Z., Marquis M., Averyt K. B., Tignor M. & Miller H. L. (eds.) (2007) *Climate Change 2007. The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge UK.
- Sonne W. (2009) Ein Lob der Theorie. *Forschung & Lehre* 16(4): 272–273.
- Spencer-Brown G. (1969) *Laws of form*. Allen & Unwin, London.
- Stehr N. & von Storch H. (2009) *Climate & Society*. World Scientific Publishing, London.
- Stern N. (2006) *Stern review on the economics of climate change*. Cambridge University Press, Cambridge UK.
- The International Ad Hoc Detection and Attribution Group (2005) Detecting and attributing external influences on the climate system. A review of recent advances. *Journal of Climate* 18: 1291–1314. Available at <http://journals.ametsoc.org/toc/clim/18/9>
- Umpleby S. A. (1990) The Scientific Revolution in Demography. *Population and Environment*, Spring 1990: 159–174.
- Umpleby S. A. (2010) From complexity to reflexivity: Underlying logics used in science. *Journal of the Washington Academy of Sciences* 1: 15–26.
- van der Sluijs J. P. (2012) Uncertainty and dissent in climate risk assessment: A post-normal perspective. *Nature and Culture* 7(2): 174–195.
- Vaughan C. & Dessai S. (2014) Climate services for society: origins institutional arrangements, and design elements for an evaluation framework. *WIREs Climate Change* 74 (11).
- von Storch H. (2009) Climate research and policy advice: Scientific and cultural constructions of knowledge. *Environmental Science & Policy* 12: 741–747.
- von Storch H. & Krauß W. (2013) *Die Klimafalle. Die gefährliche Nähe von Politik und Klimaforschung [The climate trap. The dangerous proximity of politics and climate research]*. Hanser Verlag: Munich.
- von Storch H., Meinke I., Stehr N., Ratter B. M. W., Krauß W., Pielke R. A., Grundmann R., Reckermann M. & Weisse R. (2011) Regional climate services illustrated from experiences from Northern Europe. *Zeitschrift für Umweltpolitik & Umweltrecht* 34(1): 1–15.
- Weingart P., Engels A. & Pansegrau P. (2008) *Von der Hypothese zur Katastrophe*. Verlag Barbara Budrich, Opladen.