2. Concluding remarks

In the last four chapters, I have argued that certain ideals about how science operates cannot be fulfilled by modern science. It has been shown, by example of climate science, that this becomes particularly apparent in the context of science that deals with very complex systems. These ideals are, nevertheless, very pervasive in public debates about the reliability of certain scientific research. This makes it easy for specific interest groups that would like to avoid regulations to take advantage of the widespread presence of these ideals to sow doubt about particular research results. So fostering these ideals in the public understanding of science is in the interest of these stakeholders, which is one reason why these ideals are so persistent in the public perception of (climate) science, despite the fact that science can rarely if ever live up to them.

While these ideals might take slightly different forms in different public debates about different fields of science, three popular ones, in the form in which they arise in the context of the public debates about climate research that were examined here, concern: the value-freeness of science, the relationship between theory and model, and observations and the ability of science to produce clear, binary predictions.

In the process of investigating why these ideals cannot be applied to climate science, many of the epistemological difficulties of climate science were discussed. However, it should be again emphasised: the notion that there are particular epistemic challenges does not mean that these are so overwhelming that they prevent climate scientists from making meaningful and useful statement about the climate in general or anthropogenic climate change more specifically. Instead, what has been shown in this book is that the process of evaluating and estimating climate-change hypotheses and making projections and predictions is not an impossible task, but it is more complicate than often portrayed. One example of such a hypothesis that was discussed here concerns the value of ECS. It was shown how – with careful reasoning and taking into account many different lines of evidence – scientists can come to epistemically well-justified conclusions despite all epistemic hurdles. In this context it was also pointed out that improvements in understanding the inner-workings of the models and target system is seen by climate scientists and philosophers as essential to increasing confidence in climate change related hypotheses (Bony et al., 2013; Knutti, 2018; Winsberg, 2018).

I have also argued that one aspect of scientific practice, in the context of scientists dealing with highly complex systems, is the increasing relevance but also visibility of specialist tacit knowledge. As Chapter 4 has shown, while climate scientists, on the one hand, have to deal with the epistemic opacity of the climate models and their relationship to the target system, the scientists can (at least to a certain degree) circumvent these obstacles through the "feeling" or "compass" that scientists acquire by working with their models (Lenhard, 2020; Soler, 2011). Scientists acquire this skill through their training, sustain it by practicing as scientists and expand it in conversation with and in company of other scientists. What the investigation of the role of tacit knowledge has shown is that the quality and reliability of scientific research results can only adequately be assessed by other scientists who work in the same or an adjacent field because only they are in command of the necessary specialist tacit knowledge.

However, I have also argued that recognising the role of tacit knowledge in science also gives us access to a practically useful criterion for when outsiders to a specific scientific community have good reason to be sceptical about a claim made by an apparent 'expert'. That is, when they make a claim that is contradictory to something the majority of other experts agree on and they further have no experience of practicing in the specific research field. In my opinion connecting expertise and experience in this way offers a helpful framework for how to think about expertise considering that the above described ideals cannot be consulted to determine whether or not to trust the scientists in question. Some practical implications of this will be discussed in the following.

5.1 Where to go from here?

By way of concluding, I will examine what follows from the failure of the three ideals about science and the increasing significance of tacit knowledge in more general terms. In order to do so, I will look at the situation from three different perspectives:

1. philosophy of science
2. science
3. public understanding of science

5.1.1 Philosophy of science

Firstly, considering the role of philosophy of science, I agree with Winsberg's assessment that philosophers will not "be able to tell a normatively grounded story that will secure the unassailable reliability of the results of climate modelling" (Winsberg, 2018, p. 161). There is no overall argument philosophers can make to the effect that climate models provide reliable and trustworthy results. Whether the models are reliable is a question wrongly put. Instead, the question needs to be whether the models can assess or contribute to assessing a specific hypothesis; that is, whether the models are adequate-for-purpose (Parker, 2009; Winsberg, 2018, p. 202). Thus, any concrete epistemological deliberation about the reliability of climate models is only possible on a distinctly local level and only with the caveat that such elements as expert knowledge and methodologically not fully constrained decision making are not fully accessible to the philosopher. Nevertheless, philosophers can contribute to getting a better understanding of epistemological difficulties in climate science and how they arise. However, climate scientists still remain "the best experts regarding what *should be believed*" (Winsberg, 2018, p. 163). That is, philosophers might reconstruct certain reasoning structures as we have seen in Winsberg's case study about RA and ECS. However, determining whether the necessary conditions for accepting a specific hypothesis is fulfilled is a task that can only be accomplished by scientists who have the relevant expertise.

For this reason Winsberg has advocated for philosophy of science instead to concentrate on the underlying social structures when trying to foster trust in climate science (2018, p. 161). I come to a similar conclusion, albeit from a slightly different angle. In Chapter 4 I have argued that tacit knowledge is not just at the root of all knowledge but that its relevance as well as its visibility increases the more complexity becomes an epistemic obstacle. Thus taking a closer look at the social structures and institutions, which facilitate the acquisition of this tacit knowledge, can be a helpful way to 'circumvent' the opacity the tacit-knowledge component of science leaves behind, as the social structures and institutions of science usually perform an important role in safeguarding against inadequate science. Better understanding how these structures and institution function can also foster trust in the scientific process, especially because, as I will argue below, they further understanding what specific features and characteristics can help identify experience in an expert.

Some specific aspects of the social structure of climate science have already been scrutinised by philosophers of science. One specific institution that has attracted quite a bit of attention from philosophers of science in this context is the IPCC. One reason for this is the unique position of the IPCC as a scientific institution, which provides policymakers with reports, summaries and reviews of the current state of research (the IPCC does not do its own research) but to some degree also integrates policymakers into the assessment process. This is quite unusual for science, which usually tries to draw quite a distinct 'demarcation line' between itself and politics, so as to not give the impression to be value-laden. Therefore, philosophers of science have shown specific interest in those aspects of the IPCC where the sphere of politics and of science intersect, such as the review process, where governments are invited to participate (Kosolosky, 2015) and the rules by which the authors are chosen (Leuschner, 2012b).

Some other aspects that are noteworthy in climate science from a social epistemology perspective that philosophers of science have turned their attention to concern, how to establish that there is actual consensus among experts (see Intemann, 2017) and what the epistemic consequences of distributed epistemic labour mean for attributing authorship when no single author can possibly be in possession of the whole range of necessary knowledge (Huebner et al., 2017). The question what influence public scrutiny has on how climate scientists do their work and communicate their knowledge to the public has also gained the attention of philosophers of science. In respect to the problem of artificially manufactured doubt in public climate-change debates (Oreskes and Conway, 2010), Biddle and Leuschner (2015), for instance, examine when dissent has an epistemic beneficial effect and when not.[1] As stated above, these kinds of research topics cannot just improve our understanding of the role of tacit knowledge but can contribute to learning how to attribute expertise to those who actually are experts.

1 See also Winsberg (2018, pp. 208–226) for a good first overview on different debates concerning climate science from the perspective of social epistemology.

5.1.2 Science

Secondly, it seems clear that science does not do itself any favours by upholding obsolete ideals instead of openly communicating how actual scientific research is done. In the short term it might seem convenient to resort to one or more of these ideal in order to strengthen one's argument. Insisting on the specific virtue of either the scientists or the scientific methods might be an easy way out of a particular debate. The ideals represented in Chapter 3 are attractive to science; they turn science into something that is 'above' other human undertaking, seemingly irrevocable and 'objective'. However, in the long run, these ideals can easily – as has already happened, not just in the case of climate science – be turned against science, by those wishing to undermine specific research because it has ethical, social or political implications that would be inconvenient to those agents.

More open communication from all branches of sciences about the actual process would make it harder to attack one specific field of research as an outlier. If science as a whole refrains from resorting to these ideals in science communication and instead choses to explain thought processes, background assumptions, methods, procedures and uncertainties more openly, it would make it harder to sell the 'failure' of some scientists to follow these ideals as a distinct misconduct of those scientists.

What makes this difficult is that these kind of ideals are also widespread among scientists. Specifically, the ideal of science being a value-free enterprise is a well-established assumption in science. Asserting the opposite, that is, that science cannot avoid value-decisions is often refuted vocally (see, e.g., Schmidt and Sherwood, 2015). Here research being *value-laden* is generally mixed up with the research being *biased* (Winsberg, 2018, pp. 150–151). Overcoming this wrong preconception is a difficult task as it is deeply rooted both in science and in the public understanding of science. On the other hand, as has been shown in Chapter 3.1, methodological not fully constrained decisions become more ubiquitous and tracing all decision-making processes, to lay open the reasoning process behind them, becomes impossible in practice with increasing complexity. Thus, it will become inevitable that science will be more vulnerable to attacks of apparent inappropriate value-ladenness, the more complicated the issues sciences tries to tackle become, specifically when the research has significant social relevance.

The other two ideals are less deeply ingrained in science. The notion that observations are underdetermined and that the relationship between model and observational data is complicated is not new to science, as has been shown in Chapter 3.2. On the other hand, the relationship between theory and observation is often oversimplified in situations where scientific research results are communicated to the general public. Here a concept of observations, akin to a "direct empiricists" (Lloyd, 2012) perspective on science, is often draw upon to emphasise the trustworthiness of a research result. Accompanying problems like underdetermination, theory-ladenness and measurement uncertainties are rarely talked about in this context. Here, again, oversimplifying the relationship between observation and theory risks making science vulnerable to attacks from outside forces.

As far as expressing of uncertainties goes, climate science has done a lot over the years to become more adept at dealing with and communicating uncertainties though there are still some difficulties. The calibrated language as stated in the IPCC's *Guidance Note for Lead Authors* is the best example for this. Finding a consistent but also easily understandable framework for how to communicate uncertainties has been a long and not yet finished project (see also Landström, 2017). As has been noted in the most recent IPCC report, while a clearly calibrated language is employed, the terminology adopted in the Guidance Note still leaves room for misinterpretations (Chen et al., 2021, pp. 168–171).

Another aspect concerning uncertainty, where some scientists as well as philosophers see room for improvement that also infringes on the topic of communicating uncertainties in the IPCC report, concerns *expert judgement*. The climate science community has widely acknowledged the relevance of expert elicitation in making uncertainty assessments. There are also many proposals how to make the process of expert elicitation more explicit. However as I have argued in Chapter 4 the question remains if particular schemes of structured expert elicitations would not also risk just shifting at least some background assumptions to another level and ignore the fundamental tacitness of these kinds of judgements.

It has been a well-documented tactic from climate change sceptics to call for 'better' science and emphasising uncertainties in order to argue that it is still to early for regulatory policy. This is in stark contrast to the well-established insight shared both by philosophers of science as well as scientists that science is always fallible and that in that respect scientific research results are always preliminary. Further, as Howe (2014) points out scientists giving in to these demands has proven counterproductive in the past, hindering progress on taking actions. This does not mean that climate science should not try to proceed to reduce uncertainties, rather the history of climate change policy has shown how risky giving in to these excessive and unsatisfiable public expectations can be.

5.1.3 Public

And thirdly, on the flipside of this coin, when it comes to the issue of public understanding of science, it has become clear that the lack of insight into actual scientific practice makes it easy for specific interest groups to sow doubt about scientific research results disadvantageous to them. One way of counterbalancing this is on the level of science education – to not just teach scientific knowledge but also knowledge about the methods, procedures and structures of science. Improving public understanding of these feature of science would further the trust in science. Here philosophy of science could also play an active role in facilitating the exchange between science and the public.

The second aspect regarding the general public's relation to science discussed in this book concerns expertise and how to recognise it. I have argued for a definition, introduced by Collins and Evans (2009), that defines expertise through experience in a specialist field. This definition at least gives those who are not members of a specific scientific community a criterion when to be sceptical about the claim of 'apparent' experts, specifically when their claim is contradictory to that of the majority within the scientific community. It offers a pragmatic solution to the gap left by the failure of the ideals discussed in Chapter 3. In Chapter 4 I have noted how in the context of climate science prominent climate sceptics who declare themselves 'experts' in the field of climate science actually have not practiced in the field of climate science. This is not a problem that is unique to public debates of climate research but also a feature of other public disputes about science.[2]

In Chapter 4 I have stated, following the argument by Collins and Evans (2009), that other conceptions of expertise like track records or reputation can

2 Oreskes and Conway's book "Merchant of Doubt" also discusses other such cases from the second half of the 20th century beyond the specific case of climate science. For a more recent example one might also look at the COVID-19 pandemic. During the pandemic, e.g., the German virologist Christian Drosten has publicly voiced concern about the way that (otherwise not named) scientist from other disciplines were passed off as experts in the media, despite them only having knowledge of the topic in question (that is, coronaviruses) "that does not go above superficial textbook knowledge" (Henning and Drosten, 2020).

potentially exclude certain kinds of people who actually have expert knowledge. Nevertheless, reputation or track records can be good indicators of experience. After all, as already discussed, the main way that this expertise in science is acquired is through studying the subject at university, by practicing in the field and being a member of the scientific community. This usually goes along with gaining a reputation, e.g., through specific career steps or through authoring publications. Publications can also be a way to gain some knowledge of the track records of scientists.[3] However, one has to keep in mind that reputation as well as track records on their own can be misleading as a criterion of when to consider someone an expert. For one, there are, as noted in Chapter 4, cases where people who have not gone through the traditional 'channels' of specialist expertise acquisition (e.g., getting an university degree) but, nevertheless, have assembled specialist expertise through other routes. For another, it might also not always be possible for non-experts to correctly evaluate the track record or reputation, because it requires, for example, knowledge which journals are taken seriously by the specific scientific community and which are considered fringe journals. Here an outsider to the scientific community might risk wrongfully assuming expertise.[4] That is, the criterion of experience is not a fail save principle by which a layperson can 'separate the wheat from the chaff', however, it can be a case-specific pragmatic solution to get some idea when to be sceptical about the claimed expertise of someone. Further, an additional assessment of the specific social structures of climate science can be helpful to better understanding how such characteristics as track records and reputation come about. In this sense an analysis of the social structures of climate science cannot just help to further understanding of the acquisition and relevance of tacit knowledge and experience in science but also play a role in strengthening trust in climate science.

3 It also has to be noted that there are also other features that are more external to the scientific process, which can be an indicator for inadequate research, such as when the research is financed by stakeholders who have an interest in a specific research outcome. However, as scientific research is increasingly financed by industry, how science is financed is also not a failsafe way to evaluate the reliability of the research and can, from an outsider's perspective, merely give an indication when there is disagreement amongst scientists (for examples of such cases of inadequate research financed by special interest groups in respect the climate science, see Oreskes and Conway (2010), for an example from medical studies, see Douglas (2000)).

4 For examples of such cases, see Collins (2014).