4. Tacit knowledge, skill and expertise

When in conversation with outsiders to the climate-science community and asked how they go about making this or that decision, for example, about determining the possible range of a specific parameter in the tuning process, it is not uncommon for the climate scientists to explain that they "have experience" with the model they work with. This sort of experience forms a vital part, not just of climate science but of any kind of scientific endeavour.

It is a well-established insight in philosophy of science and epistemology that not all knowledge can be made explicit either for practical reasons or in more general terms. This knowledge, typically either called *tacit knowledge*, *non-propositional knowledge* or *knowing-how*, is considered to be an essential part of knowledge acquisition overall. However, considering that it is usually assumed not to be just part of everyday life but also crucial to science, the discussions about this 'phenomenon' in philosophy of science are relatively scarce. One explanation for this is that, although tacit knowledge is considered an indispensable feature of science, it is also an element of science that is "difficult to investigate" (Collins, 1974, p. 182). After all, tacit knowledge is often described as being the kind of knowledge that eludes explication for the person who is in possession of it. For instance, most people would say they "know how" to ride a bike when they are able to ride a bike down the street even though they might not be able to actually explain the *exact* physical principles making it possible for them to balance on a bike. Further, knowledge of those principles will not help the bike rider to be successful at riding a bike. Considering one can be in possession of knowledge that one at the same time (either in principle or for practical reason) cannot explain to someone else, it does not seem surprising that it might be challenging to make out exactly what constitutes this type of knowledge.

A second reason is that it often seems to be, in a way, uncomfortable for scientists and philosophers of science alike to admit that our understanding of the world relies so significantly on a kind of knowledge that is hard to put into words. It goes against the self-perception of science, which is in many ways built on the notion that knowledge is independent from the specific scientists who acquires it. Conventionally, the replicability of scientific results (at least in theory) is considered to be essential to doing science (Fidler and Wilcox, 2021). However, case studies made by sociologist Harry Collins have shown it is not an uncommon occurrence in science that researchers can only recreate experiments successfully when they are directly demonstrated how to perform them. Thus, it might be more comfortable to focus on those aspects of knowledge that can be explicated, like it is done in scientific articles. Furthermore, for a philosophy of science that emphasises the context of justification, relying on knowledge that cannot be clarified or is at least difficult to clarify, is unsatisfying as it at most seems to be impenetrable for a full logical reconstruction of the argument.

Consequentially, a third reason is that the term *tacit knowledge* in itself is rather imprecise and not very well defined. The origin of the term is usually ascribed to Michael Polanyi ([1958] 1962, 1966a, 1966b). He sees tacit knowledge as an activity that is not just 'silent' but also one that 'cannot' be expressed. Since it was first developed, the term *tacit knowledge* has become common in other fields beyond philosophy, such as economics and management (Nonaka and Takeuchi, 1995). Besides *explicit* and *tacit knowledge* philosophers also often make use of the phrases *knowing that* and *knowing how* (Ryle, [1949] 1973). In a similar vein, the distinction of *propositional* and *non-propositional* knowledge is used.[1]

If one goes way back in the history of philosophy, some similarity can be found in the distinction between the concepts of *technê* and *epistêmê*. Fantl (2017) argues that at least the definition of the distinction found in Aristotle's *Nicomachean Ethics*, where *epistêmê* is usually translated as "scientific knowledge" and *technê* as "skill, art, or craft", can be seen as a predecessor to modern concepts of *knowledge how* and *knowledge that.*[2]

1 Other related concepts are the distinctions between *practical* and *theoretical* knowledge and *procedural* and *declarative* knowledge (for more information on the terminology, see Fantl, 2017).

2 Fantl (2017) especially sees parallels between Ryle's ([1949] 1973, p. 47) "knowing that" and "technê". He specifically refers to Aristotle seeing technê as "identical with the characteristic of producing under the guidance of true reason" (Nic. Eth. 1140a.10). Fantl concludes: "Such a conception of technê as skill guided by norms or rules anticipates

As far as modern philosophy is concerned, the concepts of *tacit knowledge* or *knowing how* are also often linked to Wittgenstein and Kuhn, who are considered to be "providing important insights into tacit knowledge and related epistemic issues" (Soler and Zwart, 2013, p. 7).[3]

After Polanyi and Ryle, the debate has (sporadically) been picked up by others in the fields of philosophy, history and social studies of science, most prominently by those coming from a background of the *new experimentalism* and the *practical turn*, whose representatives were most interested in questions of scientific practice. However, there have been few in recent years who turned specifically to the issue of tacit knowledge – one has to assume due to the problems already discussed above (see Soler, 2011).

Philosophy of climate science is here, with a few exemptions, no outlier. While, for instance, Winsberg (2018, p. 161) notes that there are some aspects of climate modelling that evade description, Lenhard (2020) mentions "the feeling" that climate scientists have for the models and Hillerbrand (2014, 2010) explicitly discusses non-propositional knowledge in climate-change uncertainty assessment. The significance of tacit knowledge in climate science has so far not been explored in more detail.

Climate scientists, on the other hand, point out on occasion aspects of their work that indicate an acknowledgement of these tacit components in the practice of climate science, even if they are not named so explicitly as will be discussed further below. This is unusual insofar as *tacit knowledge* has a bit of a bad reputation in science, at least as long as it comes to elements of justification procedures. The crux of the matter is that (at least in practice) tacit knowledge is usually difficult to make explicit and has a personal or subjective component; both features are conventionally not seen as signs of 'sound' science. Thus, even though tacit knowledge, as will be discussed below, is part and parcel of science, those aspects of science usually do not make it into scientific publications. However, in climate science the scientists themselves sometimes hint at those tacit features of their work. Therefore, one has to assume that the reliance

Ryle's identity of know-how with a disposition whose 'exercises are observances of rules or canons or the application of criteria' (Ryle, [1949] 1973, p. 47)" (2017).

3 Wittgenstein's (1953, 201) contribution is usually seen in his writings about rule following. Ludwig Fleck's ([1935] 1979) conception of "habits of thought" is also commonly seen as an early influence on the development of the idea of tacit knowledge.
Kuhn (1962, p. 44) himself refers to Polanyi in *The Structure of Scientific Revolution* arguing that the rules constituting a paradigm do not have to be made explicit in order for there to be a paradigm. See also Soler (2011, pp. 397–398).

on tacit knowledge is far more widespread. In fact, I will argue in the following that it is to be assumed that the necessity for tacit knowledge is significantly more prevalent in sciences that deal with additional epistemic challenges coming from highly complex systems. The claim I will make is that in those cases where the system explored and the instruments used are so complex that they are not fully transparent in all instances and all aspects to the scientists tacit knowledge gains an even more important role.

4.1 Tacit knowledge

In the following, I will briefly discuss the arguments made by Polanyi and Ryle as both are the most common reference points on the topic. Then I will also take a closer look at the in-detail analysis of tacit knowledge by sociologist of science Harry Collins, who explicitly discusses tacit knowledge in the context of modern science, before I will return to the topic of climate science and examine how specifically tacit knowledge applies there. Collins distinguishes three types of tacit knowledge, of which two, he argues, constitute tacit knowledge which could be made explicit at least in principle, but are not because either of the way society is structured or due to the limitations of our body. However, the goal is not to specifically explain in detail every single way that tacit knowledge is of significance in the context of climate science nor whether or not this tacit knowledge could, at least in principle, be made explicit. Rather it is to show how it permeates science at every step of the way and how the significance of this kind of knowledge increases under the framework of a science dealing with increasingly complex systems.

4.1.1 Michael Polanyi: tacit knowledge

Based on his personal experience as a chemist with a long and distinguished career, Michael Polanyi (1966a, p. 4) coined the term *tacit knowledge* to describe the circumstances that "*we can know more than we can tell*" (Polanyi, 1966a, p. 4). Polanyi's motivation is his opposition to an objectivist philosophy of science that sees science being a non-personal and non-subjective undertaking as a major characteristic of science (Polanyi, [1958] 1962, pp. 15–17). Contrary to the prevailing opinion of his time, Polanyi is convinced that knowledge in the end can only be understood as "personal knowledge" (Polanyi, [1958] 1962). For Polanyi, knowledge is personal insofar as it cannot be made fully explicit and is based on experience and skill acquired in practice. He claims that "all knowledge is *either tacit* or *rooted in tacit knowledge*" (Polanyi, 1966b, p. 7).

There are two, now famous, examples from Polanyi's writings illustrating how he sees tacit knowledge operating and permeating every day life. The first example concerns face recognition:

> We know a person's face, and can recognize it among a thousand, indeed among a million. Yet we usually cannot tell how we recognize a face we know. So most of this knowledge cannot be put into words. (Polanyi, 1966a, p. 4)

Although we do not have the words to express how we recognise them, we, nevertheless, certainly possess a kind knowledge what the faces of people we are acquainted with look like and we put that knowledge to good use in daily life.[4] And there are ways to relay this knowledge, even though we have difficulty putting into words what makes us recognise a face. Polanyi specifically highlights the, at that time new, identikits used by the police to create pictures of suspects where witnesses can select from different templates of specific facial characteristics without having to give detailed descriptions of the suspect's face to an artist. However, our knowledge about other people's faces is not dependent upon the invention of techniques like this. The difficulty here is communicating the knowledge, not accessing it. Tacit knowledge is a kind of knowledge that one can be in possession of regardless of whether one finds a way to circumvent the linguistic barriers.

The second example concerns learning how to ride a bicycle. As already observed above, it is a common experience that one sometimes can do things, such as riding a bike, without needing to understand or be aware of the underlying (physical) processes:

> If I know how to ride a bicycle [...], this does not mean that I can tell how I manage to keep my balance on a bicycle [...]. I may not have the slightest idea of how I do this, or even an entirely wrong or grossly imperfect idea of it, and yet go on cycling [...] merrily. (Polanyi, 1966b, p. 4)

What is more, riding a bike (for a human being) can only be learnt by practicing it. One cannot learn how to do so by reading about it in a book. I can spend a lot of time studying the underlying physical principles making it possible for a

4 To understand how much this knowledge simplifies everyday interactions one only has to take a look at some accounts of people who suffer from face-blindness.

human being to balance on a bicycle, however, this will not give me the skill of being able to ride a bike (Polanyi, 1966b, p. 7).[5]

Yet Polanyi sees tacit knowledge not just as part of daily life but also part and parcel of science. Scientists, argues Polanyi, rely on the specific skill they developed in their specialist field. The acquisition of skill is a necessary and time-consuming part of the training as a scientist. Polanyi notes that skill is something that can be *"achieved by the observance of a set of rules which are not known as such to the person following them"* ([1958] 1962, p. 49). Experience is at the heart of this. And in science, like in other occupations requiring connoisseurship, it can only be obtained through practice and in company of those who already have the ability:

> To become an expert wine-taster, to acquire a knowledge of innumerable different blends of tea or to be trained as a medical diagnostician, you must go through a long course of experience under guidance of a master. Unless a doctor can recognise certain symptoms, e.g. the accentuation of the second sound of the pulmonary artery, there is no use in his reading the description of syndromes of which this symptom forms part. He must personally know that symptom and he can learn this only by repeatedly being given cases for auscultation in which the symptom is authoritatively known to be present, side by side with other cases in which it is authoritatively known to be absent, until he has fully realized the difference between them and can demonstrate his knowledge practically to satisfaction of an expert. (Polanyi, [1958] 1962, pp. 54–55)

Further, Polanyi also notes, and what will be significant further down below, that this also has implications for how the training of future scientists is done:

> The large amount of time spent by students of chemistry, biology and medicine in their practical courses shows how greatly these sciences rely on the transmission of skill and connoisseurship from master to apprentice. It offers an impressive demonstration of the extent to which the art of

5 Inspired by *Gestalt psychology*, Polanyi ([1958] 1962, pp. 53–55) sees tacit knowledge rooted in the distinction of *subsidiary awareness* and *focal awareness*. In the same way that a pianist has to concentrate on the entire piece of music they are playing (*subsidiary awareness*) and not on the specific actions their hands are performing (*focal awareness*) in order to successfully play music, tacit knowledge requires this kind of shift in focus awareness from the distinct to the whole, where attention must be unspecific and invisible so not to fail, Polanyi argues.

> knowing has remained unspecifiable at the very heart of science. (Polanyi, [1958] 1962, p. 55)

Thus, for Polanyi, in many ways tacit knowledge is central to practicing science. It is not just the primary way for an apprentice to acquire the necessary knowledge and skill that makes a scientist a scientist but also facilitates new scientific insight.

4.1.2 Gilbert Ryle: knowing how and knowing that

For the sake of completeness, it is worthwhile mentioning that, around the same time as Polanyi developed the idea of *tacit knowledge*, Gilbert Ryle came up with the related concept of *knowing how* and *knowing that* ([1949] 1973). Whereas Polanyi came to the issue from a philosophy-of-science perspective, Ryle looked at it from the point of view of philosophy of mind. What unites Ryle and Polanyi is an opposition to what they consider to be the dominant perspectives at that time in their respective fields. While Polanyi is concerned about an objectivist perspective on science, Ryle voices worry about the "intellectualist legend", which proclaims that "the intellectual execution of an operation must embody two processes, one of doing and another of theorizing" ([1949] 1973, p. 32). He claims that the intellectualist legend would ultimately lead into a vicious regress:

> The crucial objection to the intellectualist legend is this. The consideration of propositions is itself an operation the execution of which can be more or less intelligent, less or more stupid. But if, for any operation to be intelligently executed, a prior theoretical operation had first to be performed and performed intelligently, it would be a logical impossibility for anyone ever to break into the circle. (Ryle, [1949] 1973, p. 31)

Thus, Ryle argues that knowing how cannot, by default, require conscious reasoning as that would mean one would end in a situation where it is not clear how the first initial step should be initiated.

Instead, he sets out to offer a "positive account of knowing how" ([1949] 1973, p. 40). For Ryle knowing how to do something constitutes a disposition to behave a certain way:

> Knowing *how*, then, is a disposition, but not a single-track disposition like a reflex or a habit. Its exercises are observances of rules or canons or the applications of criteria, but they are not tandem operations of theoretically avow-

> ing maxims and then putting them into practice. Further, its exercises can be overt or covert, deeds performed or deeds imagined, words spoken aloud or words heard in one's head, pictures painted on canvas or pictures in the mind's eye. Or they can be amalgamations of the two. (Ryle, [1949] 1973, p. 46)

For Ryle, thus, like Polanyi *knowing how* is something that requires training or more general a practical learning process. Ryle also sees knowing how as an intelligent activity that is more than mere habit, instead it displays a degree of flexibility and adaptability to changes of circumstances (one might think of, for example, the car driver who reacts spontaneously in a perilous situation). This knowledge might have been obtained by some direct verbal instructions, but Ryle ([1949] 1973, pp. 47–50) emphasises that this does not mean that we do consciously follow these rules in our mind.

The debate concerning *knowledge how* and *knowledge that* and whether one can be reduced to the other is ongoing as an argument of *intellectualism* versus *antiintellectualism* in philosophy of mind (for an overview, see Fantl, 2017). In the following I will, however, be using the term *tacit knowledge*. Not least because it is the one most commonly used, not just by philosophers of science but also in science itself (insofar as it is discussed at all), while the dualism *knowing how* and *knowing that* is historically closer associated with debates in philosophy of mind. The term *tacit knowledge*, however, also conveys, in its opposition to the *explicit* or *explicable*, that it is a kind of knowledge that is, for practical or more fundamental reasons, not put into words, which will become an important feature in the case of (climate-)science practice discussed below. To that end, a closer look at specific aspects of the role of tacit knowledge in modern science seems prudent.

4.1.3 Harry Collins: a taxonomy of tacit knowledge

One person who has extensively explored the unique role that tacit knowledge plays in actual scientific practice in the recent decade is sociologist[6] Harry

6 As a sociologist, Collins sets a different goal for his analysis of tacit knowledge than a philosopher might do. Collins describes his approach as being "just a plumber" (2013, p. 26) in creating a scheme to explore and structure tacit knowledge. Collins (2010, p. 146) also specifically criticises most philosophical approaches to tacit knowledge for having put the human body at the centre of any investigation of knowledge.

Collins. In multiple case studies and over several decades, Collins (2014, 2013, 2001, 1974; Collins and Evans, 2009), specifically in the field of gravitational-wave physics, has studied how physicists rely on tacit knowledge in their everyday work life. Collins has also written broadly about the concept of *expert* and *expertise*, a topic that, as already discussed and will be further explored in the following, is intricately connected to tacit knowledge. Exploring what constitutes *expertise* also has specific bearings in the context of public perception of climate science, where the expertise of the scientists has been questioned in the past by those who wanted to sow doubt about anthropogenic climate change. In this context Collins provides a helpful framework to look at the intricate connection between expertise, practice, experience and tacit knowledge in the context of increasing complexity in science.

In his book *Tacit and Explicit Knowledge* (2010) Collins introduces a taxonomy of tacit knowledge that is useful to get an understanding of the variety of functions and forms that tacit knowledge can take in science. Collins broadly defines three different types of tacit knowledge, each referring to different intensities of 'tacitness' and a way in which something cannot be made explicit:

1. Relational Tacit Knowledge (RTK)
2. Somatic Tacit Knowledge (STK)
3. Collective Tacit Knowledge (CTK)

Before taking a closer look at each of these types of tacit knowledge, a few words need to be said about Collins' definition of tacit knowledge to avoid misunderstandings later. While for Polanyi the opposite of tacit knowledge is knowledge that is *explicable*, Collins defines *explicit* knowledge as the opposite to tacit knowledge. For Polanyi, tacit knowledge is that kind of knowledge that cannot be made explicit. Collins, on the other hand, defines tacit und explicit knowledge in the way it is transmitted:

> The tacit is communicated by "hanging around" with such persons. In children and older students tacit knowledge is acquired by socialization among parents, teachers, and peers. In the workplace it is acquired by "sitting by Nellie" or more organized apprenticeship. In science it is acquired during research degrees, by talk at conferences, by laboratory visits, and in the coffee bar. (Collins, 2010, p. 87)

That is, for Collins tacit knowledge is defined by being acquired through close proximity to those who already are in possession of this knowledge, whereas explicit knowledge can be transmitted over longer distance.[7] However, this does not mean that explicit knowledge cannot also be transferred directly, in close contact, and that this might not enhance the learning process, for example, in a classroom situation, according to Collins (2010, p. 87). Further, he also recognises that some types of tacit knowledge could be transformed into explicit knowledge under the right conditions. As a matter of fact, from the three categories of tacit knowledge that Collins defines only the last one (*Collective Tacit Knowledge*) constitutes tacit knowledge that could not be turned into explicit knowledge, even in principle at some point in the future.[8]

4.1.3.1 Relational Tacit Knowledge

Relational Tacit Knowledge (RTK) is the weakest kind of tacit knowledge that Collins (2010, pp. 85–98) identifies. It refers to types of tacit knowledge that could theoretically be made explicit but is not done so in practice because of particular limitations of the structure of our society. It is knowledge that is

> experienced by humans as tacit knowledge and acquired as tacit knowledge, even though it is not the "ontology" of knowledge, nor even the structure of the human body and brain that have made them transferable in this way only. (Collins, 2010, p. 96)

7 Collins explains the transmission of explicit knowledge in terms of what he calls "strings". Strings are "bits of stuff inscribed with patterns: they might be bits of air with patterns of sound waves, or bits of paper with writing, or bits of the seashore with marks made by waves, or irregular clouds, or patterns of mould, or almost anything" (2010, p. 9). Though the strings themselves do not have meaning, they carry information that can be turned into meaning through interpretation of the strings. Collins argues that explicit knowledge is an economically "cheap" kind of knowledge because it can be "broadcasted" into the world at a considerable low cost (Collins, 2013, p. 27). However, Collins also point out that this does not mean that broadcasted explicit knowledge is automatically also understood. The "receivers of explicit knowledge have to be fluent in the language of the transmission medium and fluency in language is acquired as tacit knowledge" (Collins, 2013, p. 28). In this respect Collins agrees with Polanyi that all knowledge is tacit at its core.

8 Collins notes that in this context there are different meanings of the term *cannot*. He identifies eight different interpretations of "cannot" (2010, pp. 88–91). Some of these – that span from *logistic practice* and *technological impossibility* or *technical competence* to *somatic limit* and *contingency* – are of relevance in Collins' conceptions of tacit knowledge (see below).

This might happen for several reasons, argues Collins: sometimes knowledge is just kept concealed deliberately (*concealed knowledge*). For instance, it is not an uncommon occurrence that scientists from one lab try to conceal or at least not completely reveal their knowledge how to perform an experiment successfully. This knowledge could be put into words but is intentionally kept from others and, thus, could only be acquired by outsiders through "infiltrating the group" (Collins, 2010, p. 92). There is also that kind of RTK that is transmitted by directing the attention to a specific practice or object, for example, through touching or inspecting an object (*ostensive knowledge*). This knowledge could also be made explicit in theory but is too complex in practice. Further, there are situations where the logistics of the situations is so demanding that it is not feasible to turn it into explicit knowledge (*logistically demanding knowledge*). Such a situation might be the knowledge a worker in a big warehouse has who can locate every product in the warehouse in an instance by physically walking there, though they might have difficulty giving a description to someone else. Such a person could in principle be substituted by a computer system, but this might be considered to be too costly. In certain cases, knowledge is also kept tacit accidentally because there might be a misunderstanding concerning how much background knowledge the person who wants to acquire knowledge from another person has (*mismatched salience*). If person *A* tries to communicate *X* to person *B* and *A* assumes that *B* has some knowledge relating to *X* which *B* in fact does not have than *X* cannot be transmitted. Last but not least, Collins argues, there is that kind of RTK where a person *A* themselves is not certain how they actually perform a task insofar as they do not know what actions are actually important to succeed in carrying out the task, even though they are successful in doing it (*unrecognised knowledge*). Though the knowledge could in principle be made explicit, in this case, *A* is not able to do so because they are not aware of it. However, it is still possible that the relevant knowledge can be transferred through close proximity to *A* and even become explicit over time.

Even though RTK is neither in principle tacit nor will it in practice necessarily always remain so, Collins argues that one could still call it tacit as our experience of it is that it *is* tacit:

> In society as we know it there will always be secrets, mismatched saliences, and things that are unknown but may be about to become known. […] the fact is that whatever you do there will always be knowledge that is not made explicit for these contingent reasons and it, therefore, will be an ever-present

> feature of the domain of knowledge as it is encountered even though its content is continually changing. (Collins, 2010, p. 98)

Collins notes that, though not all RTK could be made explicit at the same time, there is nothing preventing any individual piece of RTK to be made explicit in general. Therefore, according to Collins, the "principles to do with the nature of knowledge are not at stake" (2010, p. 98).

4.1.3.2 Somatic Tacit Knowledge

Somatic Tacit Knowledge (STK) is a stronger form of tacit knowledge than RTK (Collins, 2010, pp. 99–117). It is tacit knowledge that cannot be made explicit due to the limitations of the human body.

The most well known example for STK, according to Collins, is Polanyi's famous example of bicycle riding (see Chapter 4.1.1). Riding a bike is learnt through practice and usually through proximity to people who already know how to do so. And while it is possible to read and learn about the relevant physical laws in a book, this does not contribute to acquiring this particular skill. However, as Collins stresses, it is not impossible to imagine circumstances under which reading or being told about the physical principles of balancing on a bike might actually make it possible to acquire the skill to ride a bike:

> if our brains and any other elements of our physiology involved in balancing on a bike worked a million or so times faster, or, what is the equivalent, if we rode our bike on the surface of a small asteroid with almost zero gravity so everything happened much slower, we ourselves could probably use [...] rules to balance. Under these circumstances, balancing on a bike would be like assembling flat-pack furniture: as we began to fall to the left or the right we would consult a booklet and slowly adjust the angle of steering according to the instructions for remaining upright. (Collins, 2010, p. 100)

Abilities that rely on STK are usually carried out unconsciously and are often done better unconsciously, notes Collins (2010, p. 104). This might give an "appearance of mystery" (Collins, 2010, p. 117). But Collins claims that such concerns are unfounded.

For one, tasks that humans perform by relying on STK could still be done by artificial intelligence. For another, Collins notes that there are always things that specific objects or animals (including humans) are better at doing because of the specific way they are built.[9] Thus for Collins humans rely on STK to perform certain complex tasks because of reasons that are inherent to *them as humans*, not the knowledge. He concludes that it would be a "mistake is to see all problems of human knowledge acquisition as problems of knowledge" (Collins, 2010, p. 105). STK just like RTK could, in principle, be made explicit, argues Collins, but is not done so due to the specific circumstances of being human (for example, having a limited brain capacity on this specific planet).

Another form of STK, Collin identifies, he demonstrates using the example of playing chess. While it is often claimed that computers can beat humans at chess, Collins argues that whether this is the case or not actually depends on how one defines *playing chess*, how one judges whether this task has actually been done by a computer. So far computers have only been able to beat humans at playing chess by a *brute force* approach. That means that the computer is able to calculate a few steep ahead of the humans through sheer computer power and some general heuristic, which is enough to win against the best human chess player. However, if one defines the ability of being good at playing chess not as "wining a game of chess" but as "playing the way humans play chess", then the answer to the question whether computers can beat humans at playing chess is a different one (Collins, 2010, pp. 106–113). Collins considers this to be the difference between what he calls "somatic-limit tacit knowledge" (winning a chess game) and "somatic-affordance tacit knowledge" (playing chess the human way). Humans, contrary to computers, rely on pattern recognition when playing chess. Until now computers have not been able to mimic this kind of pattern recognition, but, in theory, at least one could imagine a machine doing just that. What hinders us in creating such a machine at the moment is our inability to reproduce the functionality of a human mind or body.

Both kinds of STK can, thus, Collins stresses, at least in principle, be made explicit[10] but are not done for practical reasons. The reason that some researchers, nevertheless, consider this kind of tacit knowledge to be an ex-

9 As examples of this Collins notes that while humans are better at doing a lot of cognitive tasks such as calculating or copy-typing, than sieves, trees or dogs. Sieves commonly better sort stones and dogs are better at acting in reaction to smell than humans (2010, p. 105).

10 Collins defines *explicit* here as "expressed scientific understanding of causal sequences" (2010, p. 117).

ceptional kind lies for Collins in the importance we put on making things explicit:

> In sum, there is nothing philosophically profound about Somatic tacit knowledge, and its appearance of mystery is present only because of the tension of the tacit with the explicit: if we did not feel pulled towards trying to say what we do, and if we did not make the mistake of thinking this is central to the understanding of knowledge, we would find nothing strange about our brains' and bodies' abilities to do the things we call tacit. (Collins, 2010, p. 117)

Here, like in the case of RTK, the tacitness in STK is not insurmountable. However, the barrier to overcome might in practice be more challenging and it might not (yet) be possible.

4.1.3.3 Collective Tacit Knowledge

The third kind of tacit knowledge that Collins (2010, pp. 119–138) differentiates is *Collective Tacit Knowledge* (CTK). Contrary to RTK and STK, CTK is defined as a type of tacit knowledge that cannot be made explicit because it is solidly situated in the social sphere. It is the kind of tacit knowledge that is required not just to ride a bike but navigate it in traffic. Collins argues that it calls for a certain kind of knowledge to drive a car in traffic, where there are other drivers, that goes beyond knowing the traffic rules and knowing how to use a steering wheel or to change gears. Further, this kind of knowledge depends on where in the world one is. The experience of driving a car in China or Italy is quite different from driving in the UK and requires some "social judgment", notes Collins (2010, p. 122).

There is a certain "social sensitivity" and "degree of flexibility" (Collins, 2010, p. 123) needed for many things that we do on a daily basis. It is the thing we rely on when, for instance, we have to improvise. This type of knowledge is tacit in nature and, Collin argues, specific to humans insofar as we are able to interpret context-dependently:

> What is being argued is that humans differ from animals, trees, and sieves in having a unique capacity to absorb social rules from the surrounding society – rules that change from place to place, circumstance to circumstance, and time to time. (Collins, 2010, p. 124)

This knowledge is located in the realm of the collective social sphere, argues Collins. We all share in it, but we cannot possess it without being part of the collective.[11] It is, according to Collins, an "enduring mystery" (2010, p. 123) how we have access to it. But as he concludes, it is a necessity to be human to take part in it, yet it is not essential to have a (full and able) human body. A person with a missing limb can still "know what it is to possess the collective human body shape [...] through the medium of a language that has been part formed through the physical interactions with the world of all those other human bodies" (Collins, 2010, p. 136).

Thus, one can also obtain CTK without actually participating in a collective practice, according to Collins. He calls this interactional expertise (see Chapter 4.2.1). That is, a sociologist of science could acquire interactional expertise about a subject just by being around and talking to scientists about how to do research in that particular field, even though the sociologist does not participate in that research. This means, Collins argues, that one can, at least in principle and after spending a significantly long time within the specific scientific community, engage in conversations on a highly specialised level without actually being scientists in that field.[12] In a similar way, leaders of big research project can acquire knowledge about various aspects of the project in order to make decisions about the research project's future without actually contributing any research. Though, Collins notes, it might sometimes still be helpful to engage in practice to acquire CTK, because of how our bodies or societies are constructed, it is "a matter of the nature of humans not the nature of knowledge" (2010, p. 138). But importantly, one still has to be immersed in the particular community.

11 Collins explicates this by a modified version of John Searle's (1980) *Chinese Room* thought experiment. The question that Collins puts forward is if it were possible for the person in the room to continue to engage in the exchange of questions and answers over a long period of time. Collins denies this because language is not fixed but dynamic and changes after a relatively short amount of time. This would make it impossible to pass as a native speaker to the people outside the room after a certain time.

12 An example Collins gives of such a situation from his personal life concerns how he has acquired interactional expertise in the field of gravitational wave research, which he has shadowed and observed for several decades as a sociologists. He claims to have actually managed to pass a kind of 'Turing Test' where he and an actual scientist separately and anonymously answered a number of in-depth questions concerning the research. The answers were then given to other experts in the field of gravitational physics who were not able to tell conclusively whether the answers were given by the actual gravitational-wave physicist or by Collins (Collins and Evans, 2009, pp. 104–109).

Compared to RTK and STK, CTK cannot be made explicit, even in principle, and there are no machines (we can imagine) that can imitate it, argues Collins:

> As far as knowledge is concerned, the deep mystery remains how to make explicable the way that individuals acquire collective tacit knowledge. We can describe the circumstances under which it is acquired, but we cannot describe or explain the mechanism nor build machines that can mimic it. Nor can we foresee how to built such machines in the way we can foresee how we might build machines to mimic somatic tacit knowledge. In the second case we know what we would need to do to make them work, in the first case we will not know how to start until we have solved the socialization problem. (Collins, 2010, p. 138)

For Collins CTK is the "central domain of tacit knowledge" (2010, p. 153).

4.2 Tacit knowledge in climate science

The reason for examining Collins' categorisation of tacit knowledge in detail here is that it illustrates nicely the variety of roles tacit knowledge can take, not because I now plan to move on to analyse every instance of tacit knowledge that might be significant in the working life of a climate scientists. In fact, I think this would be rather tedious and somewhat missing the point, considering that tacit knowledge by its nature is simultaneously omnipresent and frequently hard to detect.[13] In general, however, I agree with Collins' assess-

13 However, if one wants to better understand how tacit knowledge permeates all areas of science, in general it is worthwhile to first take a quick look at one of many case studies Collins did to explore tacit knowledge in the context of actual scientific practice. In this case study Collins (1974) examines the struggle of several different groups of physicists trying to construct a "Transversely Excited Atmospheric Pressure CO2 laser" (TEA laser) in the early 1970s. Collins observes the difficulties of a group of scientists to replicate a TEA laser just from reading the articles published on this subject by another, already successful research group. Only once the former got into contact with that later, through laboratory visits and other communication, do they figure out how to build a functioning TEA laser. The reasons for this, according to Collins, are manifold. For one, the scientists who originally created the laser were not so keen to outright reveal their knowledge due to competitiveness. But, as Collins emphasises, it also turned out (in hindsight) that the scientists had knowledge that they were not aware of initially but which was necessary to build the laser, which they were only able to pass along through showing others. Studying the publications on this topic was not merely

ment that all types of tacit knowledge he identifies are integral to doing science (2010, p. 150). Instead, the rest of this chapter is dedicated to the following two questions:

1. why the dependency of science on tacit knowledge is more visible in climate science than other more traditional fields of science
2. how and to what extent the pervasive presence of tacit knowledge can give us a definition of expertise that can function as workaround for the failed ideals from Chapter 3, as I implied at the end of Chapter 3.4

One particular feature of tacit knowledge that Collins' analysis has shown is how a lot of the knowledge tacit to us, or we acquire as such, might not be inherently tacit.[14] It is tacit for us because of some more mundane reason such as particular social structures or because of the limits of the human body to deal with significant complexity in an explicit way. Particularly the latter explains why the reliance on tacit knowledge is especially visible in climate science. It seems reasonable to assume that, when dealing with a system as complex as the climate system and equally complex models, scientists rely even more on tacit knowledge. The experience that scientist have with the models they work with or the "feeling", as Lenhard describes it, fulfils an important role, without which developing ESM would not be possible in practice. In such cases where, for instance, specific parameters are otherwise not very well constricted, the high complexity of the model makes it impossible to test the full range of possible parameter values as this would be far too time consuming. In these cases

enough to successfully recreate the TEA laser. In a later publication, Collins states that, although he had not yet developed the above classification at the time of the aforementioned case study, "building a TEA laser is a matter of RTK + STK + CTK" (2010, p. 152). See also Collins (2001) for a similar case study on tacit knowledge of the measuring of the quality factor of sapphire for the use in gravitational-wave detection.

14 I would like to note that while Collins might be right to claim that much of the knowledge that we come across as tacit is not tacit in principle but due to the limits of the human body or because of the way that society is (currently) structured. This might be right in principle. However, the assertion that knowledge might have an explicit form under quite different premises, such as on a different planet where people have a different brain capacity or in a completely differently structured society, might be useful when the aim is to point out that there is nothing 'mysterious' about this kind of tacit knowledge, as Collins (2010, p. 117) does. It is less so when one is concerned with science as it is done in practice at this point in time and the epistemological problems scientists are confronted with right now.

the *experience* with the models can be a helpful 'tool' scientists can resort to. More generally speaking, Alexander and Easterbrook conclude that climate-modelling institutions retain "a deep but tacit knowledge base about their own models" (2015, p. 1222; see also Easterbrook and Johns, 2009).

One might, nevertheless, come to the conclusion that these instances of tacit knowledge are only a feature of the process of the construction of models or development of experiments and question whether the insight that tacit knowledge is part of the daily practice of science has any implications on the justification of scientific research results. However, as already noted at the end of Chapter 3, in the context of climate modelling the realm of discovery and justification can no longer be separated as easily as such a suggestion might imply. Further and more significantly, as Léna Soler (2011) argues, tacit knowledge in general plays a significant role in the context of justification of procedures and products of science.[15] Soler emphasises that scientists develop a kind of "scientific 'sense' or 'instinct'" (2011, p. 406) that they make use of when, for instance, scientist O is faced with the question why they consider two experiments done at different times as 'the same' or why they decide at some point in the experimentation process there to be 'enough' evidence requiring no further testing:

> Faced with such questions, O will again, at some point, encounter insurmountable limitations in his attempts to clarify his reasoning. He will come to see that he is not able to put forward crystal-clear reasons. At some point, he will rely upon a personal intuition, a scientific 'sense' or 'instinct' which cannot be further analyzed by linguistic means and which refers to him as a particular individual. [...]
> O's intuition or scientific sense that is involved here can be viewed as a personal compass. This compass is not transparent, even to O himself. It points in a certain direction but it is a black-box (or at least contains some residual black boxes). We commonly assume that O's compass has been calibrated through O's previous experience, and that it has increased in sensitivity in proportion to the duration of O's first-person involvement in similar kinds of scientific practice. Moreover, we commonly assume that O's individual,

15 Soler defines justification in this context in the following broad way: "'S provides justifications in favor of X' means: 'S gives his own motives to believe X or to perform X'" (2011, p. 407).

> specific talents might play a role. However, the process of regulating the compass remains largely opaque. (Soler, 2011, p. 406)

It is easy to see similarities between the "compass" described here that scientists draw on when assessing the merits of an experiment and the "feeling" that Lenhard describes climate researchers establish for the models they work with.

Soler argues that all of this leads to an "opacity of experimental practice" (2011, p. 403) that goes beyond an opacity in the realm of discovery and has to be seen as contrary to the widespread "rationalist ideal of completely self-transparent knowledge" demanding "a fully explainable justification of human knowledge, a justification in which no step would be left in the shadows, in which each link in the reasoning chain could be exhibited and scrutinized" (2011, pp. 406–407). This opacity is at least in actual scientific practice, if not more deeply, anchored at the core of science insofar as it concerns a kind of scientific 'intuition' – though a consequential part of experiment development and justification – that is rarely attempted to discuss or make more explicit (Soler, 2011, p. 413).

One specific place where this kind of experience plays a particularly visible role in the context of climate science is the reliance on expert judgement to assess different lines of evidence. That is, evaluating the strength and weaknesses of different data sets and types of data, different types of models and ensembles or methods (such as emergent constraints) as discussed in Chapter 3.3.3.4). This requires, as Zickfeld et al., note not just assessing the specific literature but also "knowledge that is not explicit in the formal literature" (2007, p. 237). Common expert judgement when evaluating MIPs, for example, concerns assumptions about the quality and independency of different models (Hillerbrand, 2010; Lee et al., 2021, p. 568).

It seems reasonable to assume that this is a kind of background information that is primarily acquired in practice, not just for pragmatic reasons, but also because it requires some knowledge that is at least difficult to make explicit as it is a very context specific synthesis of a wide variety of pieces of information.

Lam and Majszak come to a similar conclusion in an analysis of the role of expert elicitation in the identification of tipping points (critical thresholds, which once crossed result in considerable, oftentimes abrupt and irreversible change to the climate system) about the necessary expert judgements in this process:

> in many cases it seems related to the experts' own experience and interpretation of certain nonclear-cut, possibly ambiguous, situations. For instance, this knowledge may involve practical experience of model behavior, interpreting ambiguous data and the relative relevance of feedback processes, drawing connections and building links between disciplines, among other things. (Lam and Majszak, 2022, p. 8)

Climate-change assessment is more than a simple calculation. Instead the scientist's expertise developed over time through their experience of working with the models and creating data sets is a significant and non-neglectable aspect to evaluating models and observations as well as assessing climate-change hypotheses.

Because expert judgement is usually seen as something 'subjective', concerns have been raised in the respect to how the elicitation of expert judgements is handled and structured expert elicitation protocols have been proposed with the aim to avoid or mitigate this 'subjectivity' (Hanea et al., 2021; Oppenheimer et al., 2016; Thompson et al., 2016). While there are certainly advantages to such procedures as making the selection of experts more explicit and possibly reducing some specific biases[16], it seems questionable such a protocol could ever make expert judgements fully transparent, as these expert judgements in themselves are still fundamentally based on the tacit knowledge gained through the practical experience of the scientists.[17]

To be able to judge the adequacy of a scientific argument, more is needed than just reading journal articles. Having specific tacit knowledge is constitutive to being a scientist. However, this also gives us the option to draw a connection between tacit knowledge and a concept of *expertise*.

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16 Lam and Majszak (2022), however, note that, considering the variety of ways social values can play a role in the model building and evaluation processes, such structured expert elicitation would make it not a value-free process (see Chapter 3.1.3) and that there are also certain value-laden trade-offs to be made in the development of these protocols.

17 The argument I have made here for the most part concerns tacit knowledge in the process of climate-model development and evaluation. However, one must assume that tacit knowledge, experience and skill take a similarly prominent role in the gathering and evaluation of observational data in the same way that skill has been noted to be an important quality of a successful experimenter. Anecdotally, I can report that in a conversation with a young climate scientist talking about her work, when asked how she would go about filtering noise from the actual signal, she answered if she did not know, she would "ask older, more experienced" scientists at the institute.

1.2.1 Connection between tacit knowledge and expertise

Collins (2014), Collins and Evans (2009), and Collins, Evans and Weinel (2016) distinguish two kinds of expertise that characterise scientists (and other professionals and people that have acquired a distinct skill).[18] A closer look at these two types of expertise, *contributory expertise* and *interactional expertise* will be helpful to understand how expertise is intricately connected to tacit knowledge. It will also shed some light on the question we were left with at the end of chapter 3 of what actually constitutes an expert.

Both are forms of *expertise* which require *specialist tacit knowledge*[19] but show, according to Collins and his co-authors, differences in the way they can be accessed and utilised. Contributory expertise refers, as the name already says, to those who provide a piece of knowledge to an area of specialist expertise "and is, generally, what people think of when they hear the word 'expert'" (Collins, 2014, p. 64). Collins emphasises that this kind of specialist expertise requires practice. One becomes a contributory expert by becoming an apprentice and by practicing in the specific field of expertise, in the company of others who are already experts in this field and learning from their abilities. As Collins puts it: "one does not become 'a scientist' without practice, and a lot of practice" (2014, p. 58).

The immersion in the scientific community cannot be substituted by reading scientific books and journal articles. Although one might (theoretically) ac-

18 From this perspective, being a scientist requires no different type of expertise than that which, for instance, a doctor or an engineer has. But this kind of expertise can also be attributed, e.g., patients with rare chronic diseases who not uncommonly develop "knowledge about the treatment of those diseases that compares with or even exceeds that of their doctors" (Collins, 2014, p. 64).

19 The authors also acknowledge that there are other more ubiquitous kinds of expertise including "all the endless indescribable skills it takes to live in a human society" (Collins and Evans, 2009, p. 16), that is, skilful abilities we all have but which are often not considered to be noteworthy. Further, Collins and Evans argue there are also kinds of specialist expertise that are solely build on *ubiquitous tacit knowledge*, not *specialist tacit knowledge*, such as popular understanding of science or knowledge acquired by reading scientific papers without being a member of the scientific community. These kinds of expertise, however, have clear limits, as discussed in this chapter. Collins and Evans also point out a meta-expertise that enables discrimination between two or more experts (2009, pp. 18–23). The problems, particular for laypersons to recognise expertise are discussed further below.

cumulate substantial knowledge[20] this way, as Collins and Evans note, it also bears a significant risk of misjudging the material at hand:

> what is found in the literature, if read by someone with no contact with the core-groups of scientists who actually carry out the research in disputed areas, can give a false impression of the content of the science as well as the level of certainty. Many of the papers in the professional literature are never read, so if one wants to gain something even approximating to a rough version of agreed scientific knowledge from published sources one has first to know what to read and what not to read; this requires social contact with the expert community. Reading the professional literature is a long way from understanding a scientific dispute. (Collins and Evans, 2009, p. 22)

This can also cause problems for effective science communication, when some research is of particular public interest and people who have no specific training in the particular field of research but, nevertheless, consider themselves experts because they have read some papers and are convinced they can judge the adequacy of the reasoning process behind the argument without any training as a specialist or current inclusion in the specific scientific community. Some of the most prominent climate-science critics have been scientists who also claim to be experts in the connection between smoking and cancer, the origins of acid rain and the increase of the ozone hole (Oreskes and Conway, 2010). Considering what it takes to become a true expert in these times, it is doubtful that they have actually acquired specialist expertise based on specialist tacit knowledge in the way described here in all those quite different research topics.[21] Assuming Collins' and Evans' assessment of the connection between tacit knowledge and expertise is right, it seems prudent to assume that in these cases these climate science critics are, amongst other things[22], actually missing the required tacit knowledge vital to assessing reasoning processes in

20 Collins and Evans call this kind of knowledge *primary source knowledge* (2009, pp. 22–23).

21 Oreskes and Conway write about the scientists in question (most prominently Fred Singer and Fred Seitz), though once "prominent researchers" in their own rights, "had no particular expertise in environmental or health questions" and "did almost no original scientific research on any of the issues" they attacked (2010, p. 8).

22 Oreskes and Conway (2010) also uncover not just strong financial ties between this group of scientists and specific interest groups from the affected industries but also strategies to artificially amplifying their voices. They further ascribe the scientists a strong personally motivated rejection of any kind of governmental regulation.

science which can only be acquired by being immersed in a specific scientific community. In a similar vein, it is necessary to be part of the scientific community to know which people working in and around the field in question are considered to be serious experts and what reputation the specific journal a scientific paper is published in has. All of this is crucial knowledge to judge the adequacy of an argument that cannot be simply gained from reading papers.

Hence, just reading books and papers clearly does not make one a specialist. However, this might make one question whether it does not significantly limit the number of people who can judge the adequacy of scientific arguments. Here the second kind of specialist expertise which Collins and Evans define, *interactional expertise*, comes to into play. This term refers to "the expertise in the *language* of a specialism in the absence of expertise in its *practice*" (2009, p. 28). Absence of practice, however, does not mean that interactional expertise can be acquired in isolation. It still requires immersion into the specialist community to obtain the necessary tacit knowledge and is, thereby, far from being a quick and easy undertaking.

One instance from the history of climate science where not all people involved were in possession of the required interactional expertise, also highlighted by Collins (2014, pp. 80–91) was the *Climategate* 'scandal' in 2009 (see Chapter 1). Interactional expertise, argues Collins, was needed to know that the "trick" the scientist were talking about in the leaked emails was not an attempt to mislead the public about the severity of climate change through deliberately and illicitly manipulating data. According to Collins, one needs 'inside information' about the 'language' that climate scientists speak to know that *trick* had a different meaning than the common connotation of 'deceiving'. This is something that one can only learn when associating with the specific community of scientists, not from reading some journal articles.

While interactional expertise can be acquired all on its own without engaging in practice, like for instance a sociologist of science who spends years with scientists of a specialised field, this is rather the exception, Collins and Evans note (2009, pp. 104–109). The much more common way to acquire international expertise is through establishing contributory expertise, argues Collins. In science interactional and contributory expertise are usually obtained together as "learning to become a contributory expert in a narrow technical domain is mostly a matter of acquiring interactional expertise because it is through talk that one learns how to act in practical matters" (2014, p. 72). In fact, Collins argues that interactional expertise fulfils a highly important role in science and "is key to most of what happens in science" (Collins, 2014, p. 72). Interactional expertise is, for instance, what makes it possible for scientists to evaluate the arguments made by other scientists in peer-review processes, without having done exactly the same research (Collins, 2014, p. 72; Collins and Evans, 2009, p. 60), although the interactional expertise referred to here is established most likely in the process of acquiring contributory expertise.

Interactional expertise gains particular significance in times where the increasing complexity in research subjects and questions means a widespread distribution of the workload between different researchers and research groups. In modern scientific research projects, specifically those requiring a high number of scientists working on one and the same problem, scientists can never be contributory experts in every aspect but still have to be able to communicate with the other scientists in the project. Collins discusses this using the example of gravitational wave physics:

> There are around a thousand physicists working in the international, billion-dollar field of gravitational-wave detection. Each of them belongs to a sub-specialism within the area, [...]. In the main, no person from one subgroup could step in and do the work of a person from another subgroup – at least not within a long apprenticeship. If that were not so, they would not be specialists. And yet all these people have to coordinate their work. The way they coordinate their work is by sharing a common language which they learn when they attend the many international conferences that are part of their job and by visiting and spending time at each other's laboratories. What they are doing is acquiring interactional expertise in each other's specialities. (Collins, 2014, pp. 69–71)

It is easy to see how this relates to climate science. Climate simulations are commonly a product of many hundreds of scientists' contributions over more than one generation. Institutions that develop climate models are, usually subdivided into many different working groups, specialising in different aspects of modelling the atmosphere, ocean and land and so forth.

To coordinate this work, it requires regular meetings between the heads of different working groups. Especially considering the interdependency of different model components (see Chapter 2.1), so no research group for a particular model component can do their work in isolation from the other ones; coordination and organisation are key. Improvements and changes in the different model components done willy-nilly could set the whole model array. Interactional expertise provides scientists with a "common language" to negotiate theses issues. Similarly, interactional expertise also makes discussions and cooperation with scientists from adjacent areas of science possible.

Another element related to this kind of management and internal communication work in science is the expertise developed in other research projects or/and in other research field and then 'transferred' to the current conundrum, which Collins and Evans (2009, pp. 64–66) call *referred expertise*. This kind of meta-expertise enables the scientist, specifically those in leading positions, to judge how to proceed in a (large) research project:

> The experience in other fields is applied in a number of ways. For example, in other sciences they have worked in, they will have seen that what enthusiasts insist are incontrovertible techniques turn out to be controvertible; this means they know how much to discount technical arguments. […] They will have a sense of how long to allow an argument to go on and when to draw it to a close because nothing new will be learned by further delay. They will have a sense of when a technical decision is important and when it is not worth arguing about. They will have a sense of when a problem is merely a matter of better engineering and when it is fundamental. (Collins and Evans, 2009, p. 66)

Thereby, *referred expertise* is a kind of expertise that goes beyond but is also facilitated through *interactional expertise*; it is, however, different to *contributory expertise*, which is always distinctly local.[23]

Expertise, or at least the expertise we are interested in here, requires social engagement. This stands in contrast to the common ideal (or maybe more accurately the 'caricature') of the lonely, unsocial scientists working on his own in a lab. An ideal that is not very close to what is actually going on in science. Scientists work in community because the questions posed by modern science are just too complicated to be solved by just one person, but also because it is the place where expertise is gained, established and refined. "[W]hile something can be learned from instruction books and other kinds of literature, the heart of an expertise is acquired by picking up tacit knowledge", thus, by being in company of those who are already in possession of it (Collins, 2014, p. 60).

23 Collins and Evans note, that managing scientific research projects, of course, also requires all sorts of non-science specific expertise in terms of "financial management, human resources management, networking skills, political skills, and so forth; some of these will comprise the contributory expertise of management itself" (2009, p. 66).

Tacit knowledge has always been an essential part of science. This might not have been acknowledged as much in the past because the "rationalist ideal of completely self-transparent knowledge", as Soler (2011, p. 406) calls it, is strong in science and epistemology alike and the role of tacit knowledge was easier to overlook, specifically to people outside the scientific community. However, in the context of sciences dealing with more and more complex questions and systems and the resulting added epistemic difficulties, the significance of tacit knowledge also becomes more and more visible. In climate science this can be seen not just in the tacit knowledge needed in communicating and organising research in its widely dispersed state but also in the tacit knowledge coming into play when climate scientists exert expert judgement and in the "feeling" scientists develop for models. The complexity of the climate system and the models moves the dependency of science on tacit knowledge further into the 'visible spectrum'.

Thinking of expertise and tacit knowledge in this manner can be a helpful way out of the dilemma we were left with at the end of Chapter 3, where it became apparent that certain ideals about how science ought to operate that are usually appealed to as a guarantee for 'good science' fall short in the context of increased complexity in modern science. As tacit knowledge is at the centre of many of the methods and practices that are in contradiction to the aforementioned ideals, reconceptualising tacit knowledge not as something lacking a kind of transparency science requires but as fundamental to all knowledge and the basis of any kind of scientific expertise can instead ground these practices. I will come back to this in a bit, but first, I want to take a detour to look at one specific way the increasing relevance of tacit knowledge manifests and becomes visible in the case of climate science.

4.2.2 Climate modelling as engineering or craft

One way to examine the increasing specific relevance of tacit knowledge in climate science is to look at some of the descriptions climate scientists themselves use for their work. What becomes noticeable very quickly is that scientists (and philosophers of science for that matter) often revert to words that characterise climate modelling as something akin to an engineering task and/or requiring some kind of creativity. The most striking example of this is the tuning of models where the process has been repeatedly compared to a "craft" or an "art" (e.g., Mauritsen et al., 2012; Hourdin et al., 2017; see also

Edwards, 1999). In one of the most well-known papers about tuning Mauritsen et al. write:

> The model tuning process at our institute is artisanal in character, in that both the adjustment of parameters at each tuning iteration and the evaluation of the resulting candidate models are done by hand, as is done at most other modeling centers. (Mauritsen et al., 2012, p. 16)

Though the terminology is not uncontroversial. In an article called "The art and science of climate model tuning", Hourdin et al. (2017) state that despite the title that there is some ambivalence among the authors whether *art* is the appropriate term to describe the process of tuning:

> There was a debate among authors on the idea of using the word art in the title of the paper. Tuning is seen by some modelers more as a pure engineering calibration exercise, which consists of applying objective or automatic tools based on purely scientific considerations. Others see it as an experienced craftsmanship or as an art: "a skill that is attained by study, practice, or observation." As in art, there is also some diversity and subjectivity in the tuning process because of the complexity of the climate system and because of the choices made among the equally possible representations of the system. (Hourdin et al., 2017, p. 598)

Nevertheless, Hourdin et al. also link tuning to what is commonly considered a distinct artistic practice, i.e., being a conductor of an orchestra:

> Climate model tuning is a complex process that presents analogy with reaching harmony in music. Producing a good symphony or rock concert requires first a good composition and good musicians who work individually on their score. Then, when playing together, instruments must be tuned, which is a well-defined adjustment of wave frequencies that can be done with the help of electronic devices. But the orchestra harmony is reached also by adjusting to a common tempo as well as by subjective combinations of instruments, volume levels, or musicians' interpretations, which will depend on the intention of the conductor or musicians. (Hourdin et al., 2017, p. 590)

The comparison of climate-model tuning to reaching harmony is also interesting insofar as music is often considered to be an endeavour that requires some kind of tacit knowledge in the learning process (Polanyi, [1958] 1962, p. 56). For instance, it is hard to imagine how one should be able to learn how to play a trumpet without ever having hold a trumpet, just by reading or listening to instructions. Many of the characteristics of what is commonly attributed to a good musician, specifically one who makes music as part of a group, fall within the realm of what Collins calls CTK. It requires creativity, intuition and often the ability to improvise; all skills that are characterised by eluding explicability and also being fundamentally human in its nature.

As we have seen in Chapter 3.4.3 scientists concerned about tuning note that any kind of procedure that renders tuning in more automatic terms will not be able to fully rule out the subjective, artisanal aspect of tuning. It merely moves the subjective decision making to a different level. Scientists still have to make judgement calls concerning trade-offs (Mauritsen et al., 2012, p. 16). The subjective and personal expertise of the scientists thus is an unavoidable component of climate modelling.

But comparing techniques of model building to an art or a craft also draws attention to another aspect of climate modelling. A craft is something that has to be learnt through apprenticeship and requires training as well as experience. A successful craftsman is someone who has acquired expertise in a skill through exercising that skill. It emphasises that tuning complex computer simulations calls for being well acquainted with the model in question. Experience in working with the model is vital (Hourdin et al., 2017, p. 398).

This fits with a more general description of climate modelling overall "without any pejorative connotations intended whatsoever, as engineering, or even tinkering" (Held, 2005, p. 1611). This comparison seems valid not just in respect to tuning but more generally considering that many of the epistemic issues of climate modelling arise from features of software engineering such as modularity and kludging, as discussed in Chapter 2 and 3. Similarly the process of developing parametrisations can be described as "akin to an engineering problem" (Parker, 2018), in respect to the task of finding a way of adequately implementing a process that cannot be integrated into the model in a resolved way. As these models are not fully theoretical constructs, some aspects of climate modelling also have elements of a trial-and-error approach such as the iterative method of model development and evaluation. Particularly the latter has also been noted to have a creative element to it (Guillemot, 2010).

Struggling to fit computer simulation into traditional schemes of theory on the one side and experiments on the other side, William Goodwin points out that, while climate-science modelling does not adhere to these dualistic structure, climate science does resemble applied sciences and engineering:

> many of the same issues that arise in thinking about how it is possible to make reliable predictions about out future climate also arise when trying to understand how engineers are able to make reliable estimates of the flight characteristics of wings that no one has ever built, or to calculate the effects of turbulence in the pipes of a proposed chemical plant. (Goodwin, 2015, p. 346)

However, it should be noted that, contrary to what Goodwin implies, climate models are not just employed to assess anthropogenic climate change but are also used to explore much more fundamental question about the climate system in the same way that 'traditional' sciences do (Parker, 2018). There are also other differences to typical applied and engineering sciences, such as the degree to which both disciplines "apply techniques in ways that might turn out to be outside of the domain under which they have being directly tested" and the applicability as well as reliance on a *V&V* approach (Winsberg, 2018, p. 162, see Chapter 3.2.3.3). So the comparison to applied sciences has its limits and one should maybe resort to a more careful wording and say that climate-model developing involves some methods, techniques and epistemic obstacles resembling those known from applied sciences. However, what has been shown here is that the comparison of computer simulation development to engineering is especially used when describing that there is an element of 'trial and error', 'tinkering', 'skill', 'craft' or even outright 'tacit knowledge' as it is also commonly associated with questions of engineering or technology (see also Franssen et al., 2018).

4.3 Conclusion: expertise through experience

Philosophers of science interested in complex computer simulations have long noted the "epistemic opacity" and lack of "analytical understanding" that comes along with these kinds of simulations (Humphrey, 2004; Lenhard and Winsberg, 2010). These philosophers are mostly concerned with the prospect of acquiring understanding (or the lack thereof) in respect to the internal processes of the models and/or the relationship between model and target system. However, there is also a different kind of opacity that is not so new to science and does not just turn into an issue for science where science hits a "complexity barrier" (Lenhard, 2019) that can only be circumvented through computer modelling. Soler (2011) argues that there is an inherent opacity to any experimental practice in the sense that it is not possible for the experimenter to make the reasoning-process behind whether or not to accept an experiment as successful fully explicit (Soler, 2011, p. 404). Instead scientists develop through practicing their skill as a scientist what is described as a "compass" (Soler, 2011) or "feeling" (Lenhard, 2020), which functions as a substitute for explicable knowledge in these situations. This makes tacit knowledge an unavoidable and necessary feature of science. Though computer simulations are not comparable to traditional experiments in all respects (Winsberg, 2003), the opacity Soler alludes to here effects, as we have seen, the practice of climate modelling as well and is only intensified by the complexity of the models. Tacit knowledge has always been part and parcel of science – as all of human life taking place in community – but its presence becomes much more visible once a reduction in analytical understanding due to high complexity comes into the picture. The opacity of experimental or modelling practice, however, does not mean that this prevents scientists from assessing the work of their colleagues. Quite to the contrary, as Collins and Evans (2009) have argued, without expertise rooted in specialist tacit knowledge it is not even possible to evaluate the research of others, e.g., in peer-review processes. In these kinds of situations again the "feeling" (Lenhard, 2020) or "compass" (Soler, 2011) that scientists develop by participating and being part of the specific scientific community play a non-neglectable role.[24]

Despite the essential role that tacit knowledge takes in scientific practice, tacit knowledge is often an uncomfortable topic for scientists, specifically in those instances where science is under constant scrutiny from the public. The idea that every decision, every reasoning process can be made explicit, so it can be assessed by anybody, is deeply ingrained into how scientists see their work.

Nevertheless, as long as scientists are among themselves, the significance of tacit knowledge might not stand out very much. After all, all involved are in possession of the necessary tacit knowledge or, where it is missing, it can be acquired (by lab visits, for instance). However, once outsiders (or sometimes insiders) to the specific scientific community start voicing doubt, the impossibility to make everything that is going on in science explicit becomes apparent. The image of science as fully transparent and logically traceable to the last corner shows cracks.

24 This is not to say that often disputes among scientists can arise because of lack of some specific kind of tacit knowledge (Soler, 2011). However, these disputes are settled by (amongst other things) relying on tacit knowledge.

The point I would like to stress here is that the reliance on specialist tacit knowledge can be interpreted as a strength of science, much more than a weakness. It is, in the end, something that can be learnt. In that sense there is nothing 'mysterious' or 'esoteric' about it. It is not an ability that 'falls from the heaven' or that is only bestowed upon a few chosen ones. But to acquire the necessary skill to become an expert, one needs to acquire tacit knowledge which requires time and effort and being immersed in the specific specialist community, which (at the very least theoretically) anyone could access.

Tacit knowledge is part of everyday life. It is, to paraphrase Polanyi, at least at the root of all knowledge (Polanyi, 1966b). It is, for instance, what makes it possible for us to use language and take part in conversations. What sets the knowledge of experts, e.g., scientists apart from the tacit knowledge of daily life is that it requires *specialist* tacit knowledge (Collins and Evans, 2009, p. 14). One reason we rely so heavily on the expertise of others in all areas of life is that we cannot, for reasons of time constraints, obtain the necessary tacit knowledge in every instance.[25] What scientists, thereby, accumulate in their professional life is exactly that – together with more explicable knowledge – through training and immersion in the specific scientific community. This is the way that scientists commonly acquire expertise.

Before turning to the question what defining expertise in this way means for public controversies about science, there are two things with respect to the role of tacit knowledge in science that I would like to point out here.

First of all, recognising that tacit knowledge is fundamental to science is not to say that explicit or explicable knowledge does not also have a prominent and significant place in science. Getting some new piece of explicit knowledge is usually the ultimate aim of a research project. Particularly considering that, as has been noted in Chapter 3.2 and 3.3, many climate scientists conclude that improving explicit mechanistic understanding both of the climate system and the models is a way forward to further secure knowledge of future climate change. Still, while explicit knowledge is what science thrives towards, achieving it is in practice only possible through tacit knowledge.

Further, tacit knowledge, while being an indispensable feature of science is not what makes science *science*. What characterises science in general or more specific scientific disciplines are particular methodologies, rules, conventions,

25 In our daily life we, e.g., take advantage of the specialist tacit knowledge of others when we trust doctors to interpret ultrasound or X-ray scans.

etc.[26] Instead of a claim about the definition of science, the point I would like to make here is that tacit knowledge facilitates access to these methodologies, rules and conventions. That is, without the required specialist tacit knowledge one cannot acquire the necessary expertise to do science.

In some public debates in the last years, the use of the term *expert* has become almost derogative. The claim that the people are not in need of experts, that 'ordinary people' know better than experts – who do not seem to know anything anyway because they all seem to change their opinion all the time or because there seems to be no consensus even about critical, basic questions – or even worse that experts are all 'in cahoots' in order to suppress 'common folk' has been a reliable by-product of many public debates about scientific research. Particularly when these discussions are also connected to debates about policies which are perceived to be freedom-constricting and costly.

There are many reasons that such arguments enjoy a certain popularity in certain circles. One contributing factor, for sure, is the discrepancy between the representation of specific scientific debates in the media, where controversies and lack of consensus are artificially inflated (see Chapter 1).[27] A common problem that observers of the public climate-change debate have noted, for instance, is that for a long time the issue was often reported in the same way political arguments are conveyed: by purporting objectivity through reporting on both sides of an argument equally, negating that facts do not come with many sides (Oreskes and Conway, 2010, p. 7). Similarly, it is often more attractive for journalist to report on controversies than stable consensus.[28]

But another factor to consider contributing to the rejection of expertise, I would argue, is that *expert* is generally not a very well defined term. This makes

26 I will not define these rules and methodologies here any further, because, as Chapter 3 has shown, they show a certain adaptability and are always unique to a scientific community at a specific point in time as research objectives and questions often change over time. And, as we have seen in this chapter, whether or not these rules, methodologies and conventions are observed can (in the end) only be evaluated by members of said specific scientific community.

27 There are of course also particular psychological factor that make particular groups of people especially susceptible to reject experts, such as that accepting and following the expert advice would mean having to restrict one's personal life in a way that would be perceived as inconvenient and uncomfortable (see Chapter 1).

28 As has also been noted by journalists themselves see Rusbridger (2015).

determining whom to trust exactly as an expert more difficult for laypeople. It simultaneously leads to the problem of non-experts being in a position to claim the title, and at the same time 'ordinary people' not knowing which attributes to look out for to identify potential experts, particularly when it seems like there are many conflicting positions.

This leads us to the question: how does one then as a layperson recognise an expert? After all, the only fail-safe way to judge the expertise of others is by becoming an expert yourself. But are there ways from an external perspective to discriminate experts from non-experts? Having noted the connection between expertise and tacit knowledge, Collins and Evans (2009, p. 68) propose to see specialist expertise as directly connected to *experience*. Defining a specialist expert in this way, they argue, has the advantage, compared to other prominent criteria for judging expertise based on credentials[29] or track record (e.g., Goldman, 2001), that it does not exclude those instances where people acquire expertise without being formally trained, e.g., in the form of an university degree.[30]

However, I would argue, emphasising the experience as the distinctive feature of expertise has further benefits. This definition acknowledges that having acquired specialist tacit knowledge is the foundation of any (scientific) expertise. Putting experience and skill front and centre brings science practice and the institutions facilitating it into focus. It underlines the importance of being trained in something, being part of the scientific community and the social structures underneath all of this for becoming a scientific expert.

And most importantly it gives an (at least partial) answer to the question we were left with at the end of Chapter 3: how can a layperson discriminate between conflicting expert opinions. Chapter 3 has shown that neither specific methods nor virtues scientists bring to the job are an adequate way to determine what constitutes 'good' science. This chapter, on the other hand, has

29 Although it should be noted that experience and credentials, of course, often coincide, see also Chapter 5.

30 A prominent example of such a case is the expertise gained by activists during the AIDS-epidemic in the 1980s, Collins and Evans argue. These activists managed to acquire expertise that was on par with that of scientists working on potential treatments, in order to get into a position to advocate for quicker access to a possible effective drug. They were even able to contribute to the research, due to their unique knowledge about the habits of the patients (Collins and Evans, 2009, pp. 52–53; Epstein, 1995). For other examples see: Collins and Evans (2009) and Collins (2014).

highlighted the relevance of tacit knowledge to doing science. Resorting to *experience* as a criterion of expertise can be seen as the logical conclusion. When assessing whom to trust, determining expertise defined in this way is also significantly easier (though, of course, not infallible) for an outsider to the scientific community, while it is almost impossible for a layperson to assess if any internal standards or methods are adequately followed. The analysis of tacit knowledge in science done in this chapter has also shown that it cannot be the job of the public to pass judgement on the quality of the specific work done by scientists; the experience and knowledge to do so lie with the scientists themselves and it would be presumptuous to assume that a layperson could, on the spur of the moment, acquire the knowledge scientist need many years to amass in order to evaluate a scientific argument.

A potential counterargument against defining expertise in connection with experience, which I like to get out of the way here, is the claim that, as Kuhn (1962) has prominently argued in the history of science, scientific progress was often brought on by younger scientists. First, the young scientists might not have that much research practice, but they still have commonly gone through some sort of apprenticeship program. In science this usually means attending university, acquiring several degrees and by doing so becoming a member of and practicing in this community but there are also other ways. Secondly, the new perspective younger scientists bring to the debate is what makes them disagree with more established scientists. This perspective also constitutes a kind of experience. Thus, it is more a question of different forms of access to experience.

Nevertheless, expertise as experience, of course, is not a fail-safe way for laypersons to assess whom to trust; an expert or (more probable) a group of experts with a lot of experience can, of course, be wrong. In fact, this happens all of the time. After all, it is a hallmark of good science that it revisits knowledge in light of new evidence. But in the absence of any other criteria, it gives a good (first) indication. However, if one changes the question slightly and does not ask how to recognise an expert but how to recognise someone who claims to be an experts but actually is not, experience is a much more promising criterion. That is, it provides a good, practical strategy to 'sieve out' specific types of apparent experts that actually do not have any experience working in the field in question and/or are not immersed in the specific scientific community. As already discussed above, many prominent critics of climate science who were 'sold' to the public as experts by so inclined stakeholders are actually lacking this specific experience of working in climate science.

Expertise characterised in this manner, puts specialist tacit knowledge in the form of skill at the centre and makes it fundamental to doing science. This has advantages and disadvantages. The advantage is that it is a very inclusive definition, as it also includes those people who did gain expertise through unconventional channels. It further provides a good guideline when to be sceptical of claimed expertise. The disadvantage is that it does not provide a foolproof method for laypeople to identify experts. In my opinion it seems however highly questionable if it would ever be possible to establish a procedure or mechanism that would allow us to do so. As has been shown in this and the last chapter, the subjects and methods of science are just too complex to make this very likely. In the end, who is an expert and who is not can best be determined from within science. Only there the necessary tacit knowledge is given to make such judgements.