Networks of Data and Models — A Defence of Latour on Climate Science

HANNA TRÆLAND ROSTØL

§1. Introduction

HUMANITY IS CURRENTLY CONFRONTED with a massive ecological crisis. This has been said a million times in different variations1, and I believe the words are starting to sound painfully familiar to us all. We are seeing climatic changes in terms of rising temperatures, extreme weather, and changing weather patterns, and the changes inescapably seem attributable to our carbon emissions (Jones, Stott & Christidis, 2013). These changes are predicted to keep rising, although the magnitude will depend on our actions now (Lee et al., 2021). Furthermore, in addition to this so —called «climate crisis», we are also seeing a biodiversity crisis, losing millions of species of the world, in what some scientists are calling the sixth mass —distinction. Like the climatic changes, this biodiversity crisis seems attributable to further anthropogenic actions such as deforestation, habitat destruction and pollution.

Whether it be the changing climate, decreasing animal populations, or the many other ecological problems we are facing, these things have one thing in common: Humans. How did the situation get to this? Why have we been acting this way, that could ultimately lead to the destruction of life on this planet? The detrimental effects of human exploitation of nature and our carbon emissions are not exactly newfound knowledge. Already by the end of the 19th century, Svante Arrhenius documented the relation between carbon dioxide in the atmosphere and the temperature of the Earth (Arrhenius, 1896). So why have we not taken action? In Facing Gaia: Eight Lectures on the New Climatic Regime (henceforth: Facing Gaia) (2017), Latour argues that the answer to such questions lies in the character of our relationship to nature.

What is the defining feature of our relationship with nature? According to Latour, and many other philosophers and theorists (see for example Collingwood (1945), Williams (1980) and Merchant (1990)), it is the nature — culture divide, i.e., our tendency to draw a sharp distinction between «nature» on the one side, and human —made «culture» on the other. The separation between

H. Træland Rostøl (✉)
University of Bristol, Inglaterra
e—mail: traeland@ruc.dk

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the distinctly cultural or natural also gives rise to the association of certain characteristics within these domains. In modernity, humans have been seen as active agents with the capacity to will, desire, and act accordingly, whereas nature has served as the passive canvas against which our actions can take place (Latour, 2017, 1st lecture).

What Latour sets out to do in Facing Gaia then, is to redefine our conception of humanity and the «natural» world. He shows that to achieve his goal, the so—called «Gaia—hypothesis» can be used as inspiration to redistribute agency into a pluralistic conception of the Earth and every entity that belongs on it.

However, Latour's account does not simply provide us with a method for describing nature and culture. His project is more ambitious, and presents a complete, unified worldview containing this new conception of nature and culture. Such a thoroughgoing redefinition comes with various ontological, ethical and epistemological commitments. In particular, our relationship with the concept of «nature» is intimately linked to how we believe we can measure, describe and theorise about said nature, and will have inevitably have epistemological consequences applied to climate science. It is some of these consequences I will look at in this article.

Some aspects of Latour's theories are quite unconventional, and have been the subject of much debate. In this article, I will focus on criticisms raised by Philippe Stamenkovic (2020), which to some extent echo objections of Andreas Malm (2018). I shall focus on Stamenkovic's description of Latour's epistemology as invalid and unscientific, and argue that, on the contrary, Latour accurately describes how climate science is practiced today. Although he claims Latour is out of touch with reality, Stamenkovic himself fails to address the concrete realities of climate scientific practice. Instead, he bases his view on an outdated view of science reminiscent of what Latour (echoing Nagel (1986)) describes as a «view from nowhere». As I will show by situating the discussion in various examples from climate science, Latour does not make this mistake.

This article has two main parts. In the first part, I will briefly outline Latour's «Gaia theory», noting only what is necessary to facilitate the following discussion. I then quickly move on to the second part, in which I address Stamenkovic's criticisms. I read Stamenkovic as mounting three major lines of attack: The first is Stamenkovic's claim that Latour undermines the credibility of climate science and scientists, the second is that Latour makes it impossible to obtain any objectivity or universality, thereby making it impossible to establish climate scientific facts, and the third and last is based on Stamenkovic claim that science should be value—free. I will address all three objections in turn.

§2. Latour's Gaia Theory

Latour developed his account of the practice of science over many years and in many different pieces of work. Here, I will focus on his ideas presented in his book Facing Gaia (2017); this combines elements of his established work with a number of novel and unusual ideas. In particular, the focus of this article is on the first four chapter of the book. Even restricting myself to this, Latour's views are complex, and a thorough discussion would be too extensive for the current scope. Rather, I will first present three components I regard as central to Latour's theory, and then I will draw them together into a somewhat comprehensive account of the theory.

§2.1 The Bifurcation of Nature and Culture

The starting point of Latour's theory is his criticism of what is, according to him, an artificial separation between «nature» on one hand, and «culture» on the other. This separation is a central tenet of modernity and has been criticised by many philosophers and other theorists alike, as the so —called «bifurcation of nature and culture », a term inspired by Alfred North Whitehead's book *The Concept of Nature* (1920).

Latour embraces this idea of a divide between «nature» and «culture», and he spends the two first chapters of *Facing Gaia* showing how this separation is in fact unattainable. He stresses how the two domains of nature and culture cannot be separated: In order to define «nature», one must also define «culture», as the human is what escapes nature. Conversely, in order to define «culture», one must also define «nature», since the human is what cannot «totally escape» the constraints of nature (Latour, 2017, p. 15) Instead, Latour argues, these two domains are two halves of a pair making up one single concept. This can also explain why humans are unable act sufficiently against climate change: «it obliges us to experience the full force of the instability of this concept, when it is interpreted as the impossible opposition between two domains that are presumed actually to exist in the real world» (Latour, 2017, p. 19).

A by—product of the bifurcation of nature and culture is the «view from nowhere» that has been prevalent in science. In this bifurcation we have humans on one side, as active agents with the capacity to will, desire, and act — and perhaps somewhat «overanimated». On the other side, we have nature, which is inactive, passive and unable to respond on the other — hence «deanimated». In this conception, humans are therefore able to measure, observe and detect this nature from a detached space outside it, adopting a «view from nowhere». As

Latour writes, epistemologists dream about knowledge floating in a weightless space, but «once we restore the gravitational fields knowledge necessarily loses this mystical form» (Latour, 2017, p. 128). This means that science, by claiming objectivity and value—neutrality, takes on a «view from nowhere» by which it claims to be able to know everything from the outside.

However, if we do as Latour suggests and realise that the separation giving rise to this view is only a fabrication of our imagination to begin with, merely an unfortunate result of modernity, this view from nowhere is in fact impossible. Instead, we should look to the practice of science and see that the construction of facts takes place through complex relations between scientists, universities and academies, offices and laboratories, scientific instruments, and so on, all organised in so—called «networks» of actors, which are not necessarily «natural»(whatever «natural» means), but can also be social, institutional or political.

§2.2 The Problem of the Anthropocene

After outlining the characteristics of the bifurcation of nature, Latour goes on to explain exactly how unfeasible it is as a framework for the time we live in, that by many has been called the «Anthropocene», i.e. «the era of humans». Although not yet an officially established geological epoch, the fact that it is, as Latour documents, being discussed by geologists, shows just how deeply the domains of «nature» and «culture» have become intertwined as a result of human domination. Humans now have to realise that they are becoming likened to a geological force on par with volcanic eruptions and earthquakes (Latour, 2017, p. 114).

However, the Anthropocene forces us to realise not only that the agency of humans has become as powerful as a geological force, but also that the Earth has agency. As Michel Serres writes, in the Anthropocene, «the Earth is quaking new: not because it shifts and moves in its restless, wise orbit, not because it is changing, from its deep plates to its envelope of air, but because it is being transformed by our doing. Nature acted as a reference point for ancient law and for modern science because it had no subject. » However, now «[w]e are disturbing the Earth and making it quake! Now it *has a subject once again»* (cited by Latour (2017, p. 61).

Under these circumstances, it is evident that the Anthropocene requires us to think differently of the strict separation between «nature» and «culture». However, according to Latour, the categories of «nature» and «culture» are so powerfully ingrained in people that they simply interpret the Anthropocene as the simple superposition of «nature» and «humanity», each one taken as a whole. However, Latour's point is that we cannot simply draw a line between nature and humanity as we have done up until now, not now that we know that humanity itself is a geological force, and «nature» is so intimately tied to and influenced by human action. «We are gradually forced to redistribute entirely what had formerly been called natural and what had been called social or symbolic». [...] Neither nature nor society can enter intact into the Anthropocene, waiting to be peaceably «reconciled»., he writes (Latour, 2017, p. 120).

Instead, to cope with this realisation, Latour suggests that we have to completely abandon this bifurcation of nature, redefine this concept altogether, and to distribute agency elsewhere. To achieve this goal, Latour draws essential inspiration from the so—called «Gaia—hypothesis».

§2.2 The Gaia—Hypothesis

Latour spend the first two chapters of *Facing Gaia* discrediting the division between «nature» and «culture». The Gaia—hypothesis then, is presented by Latour as a possible alternative to this Nature/Culture divide, which is capable of solving the inconsistencies the bifurcated framework faces in the Anthropocene.

The Gaia—hypothesis is a hypothesis initially conceived by James Lovelock, and then developed in close collaboration with Lynn Margulis in the late 1960s and early 1970s (Boston, 2008). The central idea is that the Earth acts as a self—regulating mechanism as a result of the living organisms on it. This, perhaps abstract—sounding idea, can be illustrated by the simple, but remarkably effective simulation *Daisyworld* developed by Watson & Lovelock (1983). It is a bit lengthy but will provide a nicely intuitive idea of the hypothesis.

Daisyworld is a hypothetical planet with a very simple biosphere consisting only of two types of daisies: black daisies and white daisies, although they do not necessarily have to be perfectly black or white. A star provides light and thus heat to the planet, and the daisy growth rate is affected by the temperature on the planet. Whereas the black daisies absorb light, which leads to a heating of their environment, the white daisies reflect light, which leads to a cooling of their environment. No daisies can grow below 5 °C or above 40 °C. At the lower end of this temperature spectrum, the black daisies have an advantage because they grow more easily in these conditions. On the other hand, when the temperature is in the higher end of the spectrum, the white daises have an advantage. Consequently, at low temperatures, there will be more black daisies than white daisies. This effectively heats up the planet, because black daisies absorb light. When the temperature has risen to a certain value, the conditions are such that the white daisies have an advantage instead. Consequently, the white daisies will outgrow the black daisies. This cools down the planet again, since the white daisies reflect light, until the temperature falls enough for the black daisies to have an advantage again. This means that the temperature keeps on oscillating around a certain value, and that the daisies effectively act to stabilize the temperature. More details and mathematical descriptions can be added to this simulation (see (Watson & Lovelock, 1983) for more details), but the point remains the same: Any planetary system containing life necessarily creates a self—regulating system.

The essence of Lovelock's theory then, is that just like these daisies create a self—regulating system, so does any living organism. In this view, living organisms do not simply adapt to their environment, they constantly modify it, whilst they are at the same time being modified by it. These back—and—forth interactions mean that a system consisting of living organisms will be in disequilibrium.

In fact, this idea of disequilibrium was one of the starting points for Lovelock's theory, who realised that the chemical disequilibrium of the Earth's atmosphere must be because of the presence of life, and from this he concludes that the lack of disequilibrium in the atmosphere of Mars necessarily meant that there was no life on Mars, or other planets in the solar system. (Margulis & Lovelock, 1974). This was in a time where expensive missions to Mars were being planned in the search of life, whereas Lovelock and his supporters claimed the Gaia—hypothesis could successfully predict the answer to this without ever leaving Earth.

However, it is necessary to mention that the Gaia—hypothesis has also been a controversial scientific hypothesis, and is by no means universally accepted. It has been problematic partly because it is interpreted by many as being teleological and/or untestable (Boston, 2008) However, discussing the scientific consensus of the Gaia—hypothesis and its details are not important here, we simply need to acknowledge that Latour adopts this idea of self—regulation among actors that have previously been seen as passive to develop his theory.

§2.2 Combining the Elements — Latour's Gaia Theory[2]

Latour's mission is, as mentioned before, to develop a framework of viewing the world that does not entail a bifurcation of nature and the many consequences of this, that can accurately describe the merging of categories such as «nature», «humanity» and «culture» that we see so clearly in the Anthropocene. The Gaia—

hypothesis he argues, is a fruitful starting point for this (Latour, 2017, 3rd lecture).

According to the Gaia—hypothesis, organisms are no longer passive but actively and perpetually modify their environment in the process of self—regulation. This effectively leads to a fundamental redistribution of agency from previously only belonging to humans, to being a property of the many actors of the Earth. As Latour writes, «[i]f the compositions of the air we breathe depends on living beings, the atmosphere is no longer simply the environment in which living beings are located and in which they evolve; it is, in part, a result of their actions. In other words, there are not organisms on one side and an environment on the other, but a coproduction by both. Agencies are redistributed. » (Latour, 2018, p. 76). The Earth, which has previously been seen as a background against which human action can take place, is thus no longer unjustly reduced to this human stage, but is a dynamic collection of multiple entities that all respond to any alteration in it.

This is then what the concept of «Gaia» can represent, it is «the name proposed for all the intermingled and unpredictable consequences of the agents, each of which is pursuing its own interest by manipulating its own environment» (Latour, 2017, p. 142). It is, according to Latour, a particularly apt framework, as it neither deanimates nor overanimates humanity or the rest of the agents of the world. In it, no agents are deprived of their ability to act. Organisms are necessary for explaining the behaviour of the Earth, and they can no longer be reduced to the background of human action. Nor does it overanimate these agents, as there is no overall totality. This latter point is very important to Latour, and as he argues often a misunderstanding Lovelock's hypothesis faces, and it is therefore worth expanding upon.

Lovelock's theory is often described as taking the Earth as a single, living organism, and thus criticised for being teleological. This is perhaps not surprising, since Lovelock named his theory after an ancient Greek goddess and uses terms such as «superorganism» (Latour, 2017, p. 94). Latour highlights the criticism put forward by earth—system scientist Toby Tyrell for example, who accepts the theory of co—evolution but not the «existence of an all—powerful thermostat» (Latour, 2017, p. 132). However, as pointed out by Latour, this is simply not what Lovelock was saying. In fact, in response to such criticisms, Lovelock clearly states that «nowhere in our writings do we express the idea that planetary self—regulation is purposeful, or involves foresight or planning by the biota» (Lovelock, 1990, p. 1). By stressing this point Latour claims to retrieve a fully *secular* notion of Gaia, a concept that cannot be reduced to its individual parts — *nor* to the totality these.

There are still a myriad of interesting components and important points to make about Latour's Gaia theory. However, at the expense of these, I will simply move forward by asking: Where does this leave us? Latour started by showing the inadequacy and inconsistencies of the bifurcation of nature as a framework for living in the Anthropocene. He therefore wanted to abandon the consequences such a bifurcation entails, such as the divide between the concepts of «nature» and «culture», the overanimation of humanity and the deanimation of the rest of the world, and the view of nature as something passive that humanity at will and without consequences could alter, interfere, and extract resources and information from. This has some obvious consequences for the epistemology of science, which will be the topic of the next section.

§3 Latour's Account of Climate Science

Now that I have outlined Latour's view, one can also understand why his perhaps unconventional ideas have been the topic of much debate. Whether praised or critiqued, Latour's conception has received a considerable amount of attention by numerous contemporary philosophers and other theorists alike.[3] However, to my knowledge, not many of these recent analyses of Latour's framework have focused specifically on how his epistemological stances work with climate science. This is surprising, as it is the case despite the fact that in *Facing Gaia* Latour spends a considerable amount of time discussing both facts and methods from climate science. An exception to this is the paper which I argue against in this article argue, namely «The contradictions and dangers of Bruno Latour's conception of climate science» by philosopher and historian of science Philippe Stamenkovic (2020). He is echoing some of Andreas Malm's (2018), and some of these will also be reviewed. However, my objection to Stamenkovic's points of criticism will be the main subject of the rest of this article.

In his paper, Stamenkovic (2020) puts forward a particularly harsh criticism, in which he undermines most aspects of Latour's work. There are many different points being raised in this paper, and I will not address all of them in detail. Stamenkovic's paper consists of two main parts, namely the first one which addresses Latour's general philosophical conception, and the second part, in which Stamenkovic analyses the way he specifically deals with climate science. Although the first part introduces some questionable objections and seems to personally attack Latour, describing his writing style as «narcissistic» (p. 14), even questioning his «good intentions» (p. 13) and labelling his views as «pseudo—profound bullshit» (p. 10), this will not be the focus of my study. Instead, I will focus on the second part (pp. 14—30), in which he attempts to provide some concrete examples of Latour's views of climate science and claim to show these are invalid. However, I will argue that Stamenkovic's arguments are based on an outdated and traditionalist view of science and that his analysis fails to do exactly what he criticises Latour for doing: Taking the reality of the scientific practice and its methods into account.

I have identified three main criticisms against Latour's conception of science in Stamenkovic's paper, namely that 1) Latour's conception of science undermines the credibility of climate scientists and climate science 2) his framework makes any universality or objectivity impossible and consequently it is impossible to refer to any «facts», and that 3) subjective values do not, as Latour claims, belong in science. I will address each of these in turn now.

§3.1 Credibility of Climate Science and Scientists

One of Stamenkovic's main objections are towards Latour's appeal to the strength of the scientific network as giving scientific hypotheses their credibility. These networks are one consequence we see from Latour's shift from «the view from nowhere», and scientific knowledge is instead situated within networks that consist of both physical, institutional, and social components. Stamenkovic firmly argues against such a conception, an argument he develops mostly in section 3.2.1 (Stamenkovic, 2020, pp. 21—22). He writes that in Latour's conception «it is the power of the scientific *institution*, and the fact that scientists are assembled in a «political body», that founds scientific authority — and not the fact that scientists tell the truth about the world (p. 22). So to Stamenkovic, these networks are not the way climate scientists gain their credibility, which instead is through some separate kind of process, although he fails to describe exactly what this process should look like. However, although vague, it seems to be defined by some process that cannot be social in nature or rest on the networks in which the scientific hypotheses are entangled, i.e., what we may call a traditionalist conception of science. By examining a few examples from climate science, I will highlight how hypotheses about the climate really are being produced in the virtue of these networks, and cannot be reduced to this undefined, unsocial and «scientific» process Stamenkovic seems to be referring to.

The knowledge production in climate science relies heavily on a multitude of data and models, and the organisation of these in a what we can call a «network», social and institutional in structure. As Latour writes,

> «the scientific disciplines that have come together to develop these facts [about the climate] that have become so sturdy do not come from the prestigious sciences such as particle physics or mathematics; they come from a multitude of earth sciences whose certainties have been achieved not by some earth—shaking fool—proof demonstration but by the weaving together of thousands of tiny facts, reworked through modeling into a tissue of proofs that draw their robustness from the multiplicity of data, each piece of which remains obviously fragile. » (Latour, 2017, p. 31)

As I will show, here Latour effectively describes the essence of climate science; namely the fact that knowledge does not only come from a simple testing and rejection of hypotheses, but the «weaving» together of facts in an intricate web.

A very direct example of this is the how the process of data collection works and has worked historically in climate science. Let us look at the example of global surface temperature, a very important variable for detecting global warming, but also a problematic variable in many ways. We can intuitively understand why it is problematic if we pretend to be faced with the task of calculating the global average temperature. Many considerations arise from this. Exactly how many temperature measurements do we include to get a sufficient coverage? Where exactly do we measure it? The temperature is surely different at ground level compared to at higher altitudes. However, many countries consist of great mountain landscapes, whereas some countries are almost completely at sea level. Not to mention that more than around 70 percent of the surface of the Earth is covered with oceans, meaning that it is difficult to have properly established weather stations in these areas. Then — at what time of the day do we measure it? And lastly, exactly how? How do we make sure that the instruments used in different locations function the same way, that they are calibrated to agreed upon standards to give homogeneous outputs, and then that this data output is stored in a consistent way? And not to mention that if we want to keep track of the temperature over time, we need to make sure that all of these factors are consistent not just across different places in the world, but also kept consistent over the time period of interest.

By imagining this task, we can begin to realise that measuring the global temperature is not such a simple task. Historically, weather stations were in some areas extremely sparse or completely lacking. Before about 1940, the most common method for measuring sea surface temperature was to throw a bucket attached to a rope overboard from a ship, pull it back up, and read the water temperature. This was not a good method as the temperature of the water would change in the measuring process, as the temperature of the bucket and the surrounding air would alter the temperature of the water. This means that when scientists use these data today, the raw data has to be processed to make up for these uncertainties (Kennedy et. al, 2011). After the second world war, although the bucket method became less common, measurements of the sea surface temperature was perhaps even more problematic, as the changed infrastructure and shipping routes of the world lead to great discontinuities in the measurements (ibid). This shows that even with better measuring devices, collecting data is not so straightforward, and can be influenced by social and political factors.

Today we have abandoned many of these traditional ways of measuring the temperature, and there are now considerably more weather stations on land that all have agreed upon standards for carrying out measurements and storing data, and buoys have replaced the wooden buckets in measuring sea surface temperature. This data is constantly being compared against each other and continually calibrated to be consistent, as well as being supplemented by other types of measurements, such as satellite data. The uncertainties in the data are today therefore diminishingly small compared to the historical measurements (Hooker, Duveiller & Cescatti, 2018).

However, the credibility of these datasets is not simply a result of scientists that «tell the truth about the world» as Stamenkovic puts it. The credibility of the datasets is due to a massive, collective effort of scientists to make better measuring devices and collectively putting them to use, to settle on international standards for calibration and measurement, and combining these data in enormous datasets, freely accessible to anyone who wishes to use them. If this example shows us anything, it is that you can actually tell the truth about the world in many different ways; the ships with their wooden bucket did tell something true about the temperature, just like a remote weather station on Greenland also tells the truth about the immediate world around it. However, it is ultimately by the superposition of all these truths in a well—established network that the climate scientists can provide credibility to their hypotheses about the climate.

And with this realisation, we can also even more clearly see one of the consequences of knowledge being produced in these networks, namely that the knowledge can be constrained by certain political conditions. Although weather stations are a lot more frequent and widespread today, the fact is that there are still more of them in developed than in developing countries (Maina, 2018). This means that there is a lot more uncertainty about the data covering certain regions, meaning that political conditions are effectively putting a constraint on the extent and quality of the knowledge produced. Stamenkovic's related claim that science and politics necessarily are separate processes, a claim that will also be revisited later in section 3.3, is therefore simply untrue in this case.

Let us also consider the converse to feel the full force of this argument, i.e., let us consider what happens if someone outside this network makes a «true statement about nature». A climate denier can also say something true about the world if she is pointing out a particularly cold winter and using this fact as an argument against global warming. Maybe she has even recorded the past five winters, systematically writing down the temperature at specific times every day, and making plots showing the temperature trend, all quite «scientifically», at least in Stamenkovic's sense of scientific being likened to telling direct truths about the world. However, of course what is lacking is to put this fact about the world into a wider context, to place it inside a credible network of other facts and successfully relate it to these other facts. The knowledge needs to be «situated» as Latour aptly puts it. This example might be a bit forced, however the point is simply that a truth in isolation is not necessarily credible by default, but is credible only when it is compared, combined with and contested by other facts and pieces of evidence in a credible network. And in the network that the climate scientists belong to, it would be trivial to point out that a cold winter in a specific location is not enough to draw a conclusion from, either because internal variability can temporarily disguise the general trend, or simply because the global trend really says something else. When the climate denier's facts are situated like this, her asserted hypothesis about the world is no longer credible.

As I have illustrated with these examples, by claiming that scientists gain their credibility simply by telling truths about the world, Stamenkovic is ignoring how these facts needs to be tied together in order to produce meaningful hypotheses about the climate. The scientists do not simply tell the truth about the world by default, they do so *because* of their networks. When it comes to a system as complex as the climate system, there are many ways to tell the truth about it, but it is by the combination and systematisations of these truths in networks that the climate scientists establish the credibility of their hypotheses. As I have shown, these networks are highly visible and well—established if one looks to the practice of climate science, and an essential part of its knowledge production. Rather than discrediting established scientific facts, as Stamenkovic claims, Latour points out and identifies the structures that are involved in the production of such facts, and consequently establishes their credibility. Stamenkovic's accompanying argument that Latour's view hinders climate action by discrediting scientific knowledge (Stamenkovic, 2020, pp. 19—20) therefore also falls short.

However, it is worth to mention that I am not arguing that science necessarily always and exclusively gains its credibility through these networks. I am simply asserting that this is an essential part of how some knowledge in climate science is produced, and that critiquing this aspect of Latour's theory as a reason to reject it is simply failing to pay attention to the practice of climate science.

§3. 2 Universality and Objectivity in Climate Science

The next main epistemological argument Stamenkovic provides against Latour's view is that of the problem of objective truth, developed mainly in section 3.2.1 (Stamenkovic, 2020, pp. 23—24) Stamenkovic claims that «[o]bjectivity and truth themselves are conceived by Latour as pure social, institutional products, which have nothing to do with an external reality, independent from the social.» (p. 23) This is then used to argue that since there is no objective truth, any claim put forward by climate scientists would be just as good as that of climate deniers. This is an argument also put forward by Malm, who writes that «the denialist should forget about the question of who is objectively right and instead put his trust in the enormous institutional apparatus of science» (Malm, 2018, ch. 4, cited by Stamenkovic (2020, p. 23)). However, as I will show, Latour's view does not deny the establishment of facts, these facts are just established in virtue of the networks of the scientists.

I will use the very same excerpt Stamenkovic uses to prove Latour's rejection of any objective truth, to show that Latour in fact does not argue that objectivity is inherently a social property, only that it can be achieved through social institutions. This difference is crucial.

Latour writes that with the advent of climate science, we see «the first time they [the scientists] have to count on the institutions of science as their own way of attaining objective truth.» He continues by saying that «[i]nstead of alternating abruptly between an impossible universality and the narrow limits of their own «point of view,» it is because they extend their set of data from instrument to instrument, from pixel to pixel, from reference point to reference point, that they may have a chance to compose universality» (Latour, 2017, pp. 215—216), quoted in (Stamenkovic, 2020, p. 23) Stamenkovic seems to be confusing the method for achieving objectivity with objectivity itself. If we look closer, Latour is saying that the scientists use the institutions as «their own *way* of attaining *objective* truth» (emphasis added). Clearly, he is highlighting the institutions as a method of reaching truth, which means that objective truth is very much achievable. Latour is not anywhere in this excerpt saying anything about the property of being objective, or what this universality the scientists can compose is. He is saying that objective truth can be institutional in its *composition*, but not necessarily in its intrinsic *nature*.

And thus we can realise how this difference is so important: A huge part of Stamenkovic's arguments rests on the fact that Latour on one side is saying that any objective truth is impossible and that as a consequence of this there can be no established facts, and on the other hand referring to these facts (as a part of his devious plan of appearing politically correct, Stamenkovic (2020) claims (p. 11; p. 17). This is according to him an epistemological contradiction and therefore deprives Latour's framework of any chance of endurance. However, as illustrated above, Latour is not saying that there cannot be established facts, he is just saying that they can be established by social, institutional structures; his view is epistemological rather than ontological. And in fact, the view Latour is putting forward here once again closely resembles the practice of climate science.

In the last section we saw how climate scientists have highly structured networks for data collection. However, these networks extend much further, also in establishing hypotheses about the physical processes of the climate, nowhere made more apparent than by the use of multi—model ensembles and model comparisons, which are an integral part of the knowledge production of the Intergovernmental Panel on Climate Change (IPCC). The IPCC is the United Nations body for assessing science related to climate change[4], and is highly representative of the practice of contemporary climate science. According to the IPCC, the confidence of the hypotheses about the climate «depends on the type, amount, quality and consistency of evidence (e.g. mechanistic understanding, theory, data, models, expert judgment) and the degree of agreement» (Mastrandrea et al., 2011, p. 1). Here it would perhaps suffice to simply point to the political structure of the IPCC as well as the reference to «expert judgment» to illustrate the social and institutional character of the process of establishing facts. However, I want to show that the role of the networks runs even deeper than this, and instead focus on the «degree of agreement» between climate models.

Let us look specifically at climate models, and how different models are used in conjunction to support hypotheses about the climate. In general, there is more confidence in a result if more models agree on it. Pirtle, Meyer and Hamilton (2010) found 118 articles in climate science where the authors refer to agreement between a variety of climate models to encourage confidence in the results of the models. Additionally, most of the IPCC's analysis based on models is based on the so—called «Coupled—Model Intercomparison Projects» (CMIPs[5]). It is an established fact that the output of these model comparisons is more reliable than the outputs of individual models (Flato et al., 2013). The models that constitute the CMIPs are produced by different research groups that take different components of the climate system into account. The models share many similarities, such as basic assumptions about the physical and chemical processes, how to model these processes mathematically, and they even share pieces of code. However, there are also differences in assumptions and implementation[6], and consequently the individual models possess different types of biases, and the combination of the models are thought to balance these biases out, producing an overall more accurate result (ibid). Through the combination and comparison of these model outputs, the scientists gain more and more understanding of the processes of the climate system (Knutti, 2018).

The CMIPs then, provide us with a highly illustrative example of how the process of establishing facts is based on a highly social and political structure. Something that resembles what we can call «the objective truth» in this case is therefore not accessible to the individual scientist or even research group, but can only be achieved through the concentrated effort of these structures. The objective truth is not hanging out there ready for a keen scientist to establish as a fact as Stamenkovic seems to imply, but is only something the scientists get closer and closer to through the collaborative, social, and institutionalised effort that the CMIPs represent. In light of this we can make sense of what Latour is saying, namely that it is only because the scientists extend their view «from reference point to reference point, that they may have a chance to compose universality». And importantly, once again we see that by paying attention to the practice of climate science, Stamenkovic's argument fails.

In this section we have seen that objectivity and universality are in fact according to Latour achievable through social and institutional methods, which does not necessarily mean that they are social in nature. Contrary to what Stamenkovic argues, Latour is not opposing the existence of established facts, he is simply describing the process of their establishment, and he does so quite accurately if we look to examples in climate science. We can say that facts do not get established as facts because a scientist yells that it is true loud and often enough, as Stamenkovic's impression seems to be. Facts get established as facts because of the painstaking effort of scientists using the totality of their networks to test, contest, and develop it in any way possible.

§3. 3 Values in Climate Science

It might not be surprising that accompanying Stamenkovic’s traditionalist view of knowledge production in science, the related idea of science being value—free also emerges. In Stamenkovic’s conception, an objective truth can always be isolated from the structures that made it, and consequently also from the scientist that made it. Latour on the other side, by abandoning this «view from nowhere», he also abandons the idea of the cold—headed, always rational scientist that can practice science free from the influence of values. And exactly this is the topic of the last main objection against Latour I identified in Stamenkovic's paper.

Stamenkovic does not refer to «values» explicitly. However, I have chosen to organise various statements that express criticism against any type of non—objective criteria to guide science as his «opposition against values in science». This approach is chosen because as opposed to the two previous objections that were fairly concentrated in specific parts of his paper, his opposition towards values is a point revisited numerous places in the article, but is best responded to collectively.

For example, when describing why we must not abandon the criterion of objective truth, Stamenkovic writes that «the only criteria we are left with are *subjective* criteria, such as personal, ideological or emotional preferences» (p. 25). Here it is pretty straight—forward to understand that Stamenkovic believes these types of criteria, or «values» as I have chosen to call them, do not belong in the scientific practice. Stamenkovic implies this reason enough to see Latour's conception as unfeasible.

Furthermore, in addition to scientists having values, Latour argues that these values can carry on into the prescription of scientific statements (Latour, 2017, p. 56). The fact that scientific statements also can contain social values and emotions seems absurd to Stamenkovic, who without much reason points at this as a fundamental flaw of Latour's theory. He writes that «Latour considers that scientific statements made by climate scientists are charged with “emotions” » (p. 17) and implies that this consequently discredits his framework. Elsewhere, Stamenkovic writes that «[f]acts (e.g. about climate change), according to Latour, are always prescriptive (of a morality, a policy[)]» (p. 8), again implying that this value—laden view of science proposed by Latour is enough reason to reject his ideas.

However, although frowned upon by Stamenkovic, values are in fact an established part of the scientific practice in general today, and multiple philosophers of science are discussing their exact role and function. Perhaps I am slightly cheating by putting Stamenkovic's opinion into this value—framework as it is such an established debate. On the contrary, perhaps Stamenkovic is cheating by not doing so. Whatever we choose to call them, such values are a visible part of the climate scientific practice.

Values are an accepted part of the scientific practice of climate science, exemplified by its inclusion in the latest IPCC report (AR6), which as mentioned before, represents the state of the art of the climate scientific practice. In AR6, it is stated that science has its own values, including openness, objectivity and evidence—based thinking (Chen et al., 2021, p. 151). However, in addition to this the role of subjective and social values «stemming from ethnic or national identity, traditions, religion or lived relationships to land and sea» are also recognised, as is their role in guiding decisions in the «construction, assessment and communication of information (high confidence) » (ibid, p. 151). This view that subjective values can carry on into scientific statements is reflected in Latour's conception as seen above.

However, it is important to note that even though the role of values is recognised in the scientific practice, this does not mean that their role so far is well defined. In the state—of—the—art philosophy of climate science this is a hotly debated topic, and the exact roles of values for example when guiding choices in model—making and data—collection is being investigated, see for example (Bender et al., 2022) and (Winsberg, 2018). Nevertheless, once again it is evident that Stamenkovic's argument falls short in the face of examples from climate science.

§3. 4 Further criticisms of Latour

One final remark, I have only been defending certain aspects of Latour's epistemological views applied to climate science, and not his view as a whole, or not whether this type of epistemology is fruitful applied to all types of science. As Stamenkovic (2020) highlights, it can be argued that climate science is not exactly what we would call «normal science», but rather «post—normal science», in the sense that it is highly politicized. (p. 27) Whether climate science is a particular type of science playing by different rules is a question I have not tried to answer.

It is also important to note that I have only defended Latour's view against the claims that his epistemology of science is unscientific, and I have done this by looking at examples of climate science. I have not tried to assess the validity of the ontological and ethical commitments that his theory entails.

An important ontological commitment criticized by Malm (2018, ch. 3) and also picked up by Stamenkovic (2020, pp. 20—21) is Latour's complete merging between categories such as nature and culture, human and non—human. In particular, the uniform distribution of agency between human and non—human actors arising from this has been discussed. A consequence of this is that it seems difficult to distribute any particular responsibility to humanity in slowing down the climatic changes, which obviously can be seen as very problematic in a time which is already painfully defined by inaction and political unwillingness to make big changes. I see such criticisms of Latour's view as potentially carrying more weight (though I am not trying to say that I necessarily agree with them). Nevertheless, a discussion of these is outside the scope of this article.

§4 Conclusion

In this article I have briefly reviewed Latour's Gaia theory, explaining how it emerges from a criticism of the distinction between «nature» and «culture», and how Latour breaks down these categories and the properties associated with them. According to Latour, this provides us with a suitable framework for making sense of the Anthropocene.

I have focused on a set of criticisms of Latour's approach to climate science made by Philippe Stamenkovic. According to Stamenkovic, Latour undermines the credibility of climate science and scientists, makes it impossible to establish climate scientific facts, and blends personal and political values with a scientific practice that should be value—free. I show that Stamenkovic's arguments are consistently based on neglecting the concrete realities of climate scientific practice. Stamenkovic claims that Latour's «epistemological position seems to be based on his personal, ideological preferences, rather than on the facts» (p. 29); I think this is perhaps a more appropriate description of Stamenkovic's own position. It is simply impossible to reduce climate science to a science without institutionalised networks, without values, and with an objective truth intrinsically accessible to scientists without the help of these structures. This is what Stamenkovic tries to do, which is not a difficult task when one does not take scientific practice into account, instead only appealing to an ill—defined, traditionalist view of science.

As I have shown in this article, climate science is a scientific practice that rests well within the framework that Latour provides. I am not arguing that Latour's philosophical position as a whole, nor even his epistemological views on all aspects of scientific practice are correct. Nonetheless, the framework outlines in *Facing Gaia* does provide the flexibility and complexity needed to accurately describe the practice of climate science, something that affects our lives today, and will do so even more in the coming years.

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Networks of Data and Models— A Defence of Latour on Climate Science

In his seminal work Facing Gaia: Eight Lectures on the New Climatic Regime Bruno Latour develops an extensive framework for defining the relationship between humanity, nature, and whatever other entities belong on this Earth. Latour has long been a controversial figure and this recent book also contains unconventional components. In this article, I will defend Latour's account of the practice of climate science against a set of criticisms put forward by Philippe Stamenkovic. I shall show that climate scientists, in line with Latour's conception, both gain their credibility and establish facts in virtue of their highly social and institutional networks, in which their data and models are developed and distributed. Furthermore, I shall show that the separation of climate science from political and subjective values that Stamenkovic advocates is unachievable: Values are an ineliminable part of the climate scientific practice. To drive my argument, I shall examine specific examples from the climate scientific practice, something Stamenkovic neglects to do. As we will see, Stamenkovic's arguments fail to provide a compelling reason to reject Latour's account of climate science.

Keywords: Climate science, Bruno Latour, epistemology of science, objectivity, values in science

HANNA TRÆLAND ROSTØL holds a BSc in Physics and Philosophy from the University of Bristol, UK, and an MSc in Physics + Philosophy and Science Studies from Roskilde University, Denmark. Her research interests include epistemology and philosophy of science, with a particular interest in climate science and climate modelling.

INFORMACIÓN DE CONTACTO | CONTACT INFORMATION: University of Bristol, Beacon House, Queens R, Bristol, Inglaterra. e–mail (✉): traeland@ruc.dk· iD: http://orcid.org/0000—0000—0000—0000.

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