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Was Anthropogenic Climate Change Falsifiable in 1925? Reflection on the Scientific Attitude

Nicolas BOULEAU
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In 1925, a French astronomer J. Mascart achieved the gigantic work of studying more than three thousand publications in order to sum up the available knowledge about the climate variability. He published his analysis and conclusions in a book. We base on this work to tackle anew some epistemological problems. In particular we weigh up the relevance of the popperian doctrine in the case of climate change. As eventually seen, the strict framework of falsifiability does not practically operate and the most rational position seemed to stake on improving measurements and scientific networking. We qualify this position as “narrowly scientific”. The case of climate change is, in fact, representative of any question of knowledge whose assessments and controls are postponed to the future, which is the case of every theory attempting to say more than available measurements.

This leads us to reconsider the narrowly scientific position, a wait and see policy which does not take into account the interests involved in contradictory representations. We discuss a more engaged attitude for research centres that is better placed to anticipate possible dangerous uses.

Le changement climatique anthropique était-il réfutable en 1925 ? Réflexion sur l'attitude scientifique.

Nous étudions la pertinence de la doctrine popperienne dans le cas du changement climatique telle que cette connaissance était appréhendée en 1925 en nous fondant sur une analyse approfondie et quasi-exhaustive de J. Mascart des travaux et théories disponibles à cette époque. On s'aperçoit que le cadre strict de la réfutabilité n'est pas opérant et qu'à cette époque la position qui se voulait la plus rationnelle, apparentée à des points de vue qui seront, après guerre, ceux de Lakatos, Quine et Feyerabend, est finalement attentiste. Nous la qualifions de scientifique au sens étroit. Le cas du changement climatique est, en fait, générique de toute question de connaissance dont les sanctions et contrôles sont repoussés à plus tard, ce qui est le cas de toute théorie qui prétend dire plus que les mesures disponibles.

Ceci nous conduit à reconsidérer la position scientifique étroite qui ne tient pas compte des intérêts en jeu dans les représentations contradictoires. Nous discutons de la part des laboratoires une attitude plus engagée et plus anticipative d'usages possibles éventuellement dangereux.

I. Introduction. The Popperian doctrine and its critics.

The philosophy of Karl Popper is famous principally for the idea that certain intellectual constructions are easier than true science because they avoid verification by experiment. Among them are theories which present themselves as scientific, such as the dialectical materialism of Marx and Lenin, or which claim scientific status in a special sense like the sociological method explained by Max Weber; and collections of interpreted observations such as psychoanalysis, which are scientific according to some (Freud), or structurable knowledge according to others (Lacan).

In support of the theses of John Stuart Mill on the value of the open criticism of ideas, Karl Popper developed “a corpus of doctrine” able to define what is scientific and what is not, as well as the direction a more rigorous science should take: the real content of a theory is the sum of the statements which refute it, its potential *falsifiers*. A theory becomes more vulnerable as its content increases. When a theory is rejected by experiment, a new theory has to be found which fits in with what remains valid in the former while proposing an alternative for where the difficulty occurred. It is not sure

that such a better theory can be found each time a problem occurs: this depends on the inventiveness of the researchers.

Since what scientists are going to discover is, by nature, unpredictable, the future of society is open and theories which claim to grasp the direction of History are non scientific. Popper's criticism of historicism should be considered as a polemical extension of his doctrine, with its sights firmly set on Marxism-Leninism, but it cannot easily be included in his epistemological thesis proper, since Popper does not deny the predictive value of scientific theories: on the contrary, Galle's discovery of Neptune using the calculations of Le Verrier is exemplary of the scientific approach. To avoid the contradiction, Popper is obliged to introduce his three-world theory, which is not easily acceptable, especially in the field of biology.

The failings of the Popperian doctrine have been set out by many authors, but they remain less well-known to the general public. School manuals only teach the idea of "falsifiability". The two most damaging critiques are those of Adolf Grünbaum, who pretends to believe in the doctrine in order to analyze the properties of the rationalism to which it leads¹, and of Paul Feyerabend, who does not believe in the doctrine at all, and whose main point is that things did not happen that way historically, not even in the case of Newtonian mechanics and relativity. To each of the questions: 1. Can we wish to live according to the rules of critical rationalism [the name given to the Popperian doctrine]? 2. Is it possible to keep both Science as we know it and these rules? Feyerabend answers no, basing his argument, and thus giving weight to it, on an analysis of real situations and the real human behaviour of scientists², an analysis which continues through his whole work. Grünbaum and Feyerabend examine in detail the question of the replacement of one theory by another, and show that the matter is far from being as simple as Popper suggests.

The transition from Newtonian mechanics to relativist mechanics is a difficult case to hold up as the archetype of the truly scientific. The costume is too tight. One of the assets of Feyerabend's argument that "everything goes" is that it can receive in the field of knowledge all the issues in which we are involved today, and not only those of physics. The very notion of theory, with its logical and mathematical framework, such as the epistemologists used it during the three first quarters of the 20th century, is too restrictive. For instance, what is called chaos theory today is not a theory in this sense, but a phenomenological collection of properties of certain systems.

Two circumstances lead us to take a certain distance from Popper and from his better known critics alike. One, very sudden, was the development of modelling that accompanied the computer science boom during the last third of the 20th century, a new form of knowledge whose characteristics have to be studied. The other, more gradual, is the growing presence of environmental issues (waste, biodiversity, climate) and ethical issues (cloning, perinatal innovation, human genetics). Do Popper, Lakatos, and Feyerabend set themselves the right problem? When Feyerabend writes that

¹ A. Grünbaum, "Is the method of bold conjectures and attempted refutations justifiably the method of science?" *Brit. Jour. Philosophical Sci.* 27 (1976) 105-326.

² P. Feyerabend, *Against Method*, New Left Books, 1975, Chapter 15 in particular.

theoretical anarchy is more apt to develop progress than doctrines based on law and order or again that the only principle which does not restrict progress is “everything goes”, we have a right to wonder whether these positions are not showing their age, and whether the real problem today is not rather to know if technical innovation can be fostered with more respect for the values of civilization, in its most common sense as the opposite of barbarism, and with greater lucidity about the risks being generated³.

In the following pages of this article, we carry through this reflection, basing ourselves on an analysis of the case of climate variability and its causes at the beginning of the 20th century. At that time, a synthesis of available scientific works was still possible, so that what was already a very complex issue has lessons for us when transposed to today.

II. How was the science of climate change to be taken forward in 1925?

The issue of climate change is peculiarly appropriate for putting Popper’s ideas to the test. Central among contemporary debates in which science is expected to be an arbitrator, climate change is the province neither of metaphysics nor of psychological or social interpretation, but of the natural sciences, whose outlines it is bound to question. It is located in the very place where the purification of facts requires the conditions of observation to be made specific to the most anecdotal of detail and frustrates true understanding.

In this perspective, there are two principal advantages to basing our study on the work of Jean Mascart entitled *Notes sur la variabilité des climats*, 383p, Lyon, M. Audin 1925. Firstly, the author makes no attempt to defend one particular explanatory theory but rather strives with true scientific rigour to evaluate the various chains of argument as well as the quality of the measures. Secondly, his study presents a synthesis of the works available at that time that if not exhaustive is nonetheless outstandingly complete: he analyses almost 3100 bibliographical references involving more than 1000 authors.

The landscape, as we are going to see, is complex. We meet the most varied points of view, more or less well supported, which often defy comparison due to the multiplicity of explanatory fields they appeal to, and the specificity of the data they interpret. This great mass of documents and opinion creates a situation close to that we confront on most subjects today on a permanent basis with the Internet: information overload.

Jean Mascart shows a great concern for exactness. He was an astronomer, son of the physicist Eleuthère Mascart whose experiments prior to Michelson’s against the existence of ether made him a pioneer in relativity theory⁴. But Jean Mascart is far from being interested only in astronomical factors. On the contrary, he attaches great

³ Actually, Feyerabend approaches the legitimacy of non-dominant knowledge and the democratization of its production in other works, cf. *Farewell to Reason* Verso 1987, *Wissenschaft als Kunst* Suhrkamp 1984.

⁴ Cf. M.-A. Tonnelat *Histoire du principe de relativité*, Flammarion 1971 and R. Mossery *Léon Brillouin*, Belin 1999, Pages 11-30.

value to all accounts and historical and economic testimonies, and in his conclusion, which betrays a certain epistemological unease, he stresses the importance of archival research and of the systematic collection of reliable data.

To situate his work, he first sets himself apart from the great narrative frescos such as that of Bertrand on the *Révolutions du globe* in which hypotheses on the formation of the earth's crust jostle together with Buffon's theory, Fourier's theory of heat, and Lyell's theory on the mass of waters, and so on. He criticizes the art of evocation without serious foundation, and he himself determines to draw on the experimental sciences: astronomy, geology, the physics of the globe, geography, oceanography, and meteorology. "It is necessary to retain in these matters", he writes, "a degree of scepticism, since it has been impossible until today to give an accurate definition of the very word *climate* that we use." For sure, the term climate sums up the invariability of the weather in a region once the daily and yearly variations have been abstracted, but the notion is vast in view of the physical parameters involved, and yet sins by omission in having no precise and clear marker as far as the long term is concerned. In other words, the signal is unknown as far as low frequencies are concerned. Mascart gradually adopts a position that we could qualify as positivist on this point, considering that it is rather useless to keep on trying to define this primitive word.

As recommended by Claude Bernard, Mascart looks for the *efficient cause* of the phenomena of glaciation by sifting the evidence of the plant fossil record. Ever the good astronomer, he finds it in the eccentricity of the earth's orbit, the precession of the equinoxes (variation of the earth's axis), and the variations of the activity of the sun. These causes being in any case indisputable and calculable, he deems it evident that the earth is not in a steady and constant state as far as its astronomical evolution is concerned, and that consequently *climatic variations are currently taking place*. His aim thus defines itself as "to search for the symptoms of climate change in the most recent *human period*". Naturally, he knows that "economic and political phenomena may intervene" - unbalanced irrigation, deforestation, and so on - and he cites Bourquin on that point: "the sunshine and desolation of lands where once bloomed great empires such as Tello, Babylon, Nineveh, Persepolis and Thebes were due, above all, to the lack of foresight or mistakes of men, and to political and economic events, rather than to natural phenomena."

After the astronomical causes, he approaches the physical causes and in particular the greenhouse effect. He credits the Belgian scholar W. Spring⁵ with being the first to insist on the role played by carbon dioxide in the thermal balance of the atmosphere, ahead of Arrhenius (1895). Mascart expresses many reservations about Arrhenius' theory, which suggests that the carbon dioxide content variation hypothesis may explain variations at the geological scale, without astronomical cycles. He observes that if this were the case, the causes of the variations would remain obscure, and disputes the role of volcanoes and solubility in the ocean. He also disputes the

⁵ W. Spring "Recherches sur les proportions d'acide carbonique contenues dans l'air" *Mém. de l'Acad. de Belg.* 1885.

orders of magnitude of the temperature changes connected with increases or decreases in carbon gas calculated by Arrhenius, which he finds excessive in both directions. But his principal disagreement is with Arrhenius' view that the recent and future increase of carbon dioxide content will be *beneficial*, by making winters milder and summers cooler, thanks to rain.

Mascart thinks that the proportion of carbon dioxide should be considered to be stable during historical time, *with the exception of* the recent period during which "the artificial combustion of coal has destroyed the balance, all the more as coal consumption has tremendously increased." Arrhenius' idea that the inescapable cooling of the Earth due to physical and astronomical causes is fortunately balanced by an increase in carbon dioxide of human origin (an increase which started in the 19th century and which makes winter softer, summer more rainy, and plants grow better) is, to Mascart, a mistake, since the two phenomena are from different time scales. "This cooling [of our globe] is so very slow that for it to be demonstrated, very precise observations would have to be made, and continued over at least several centuries – perhaps even ten or twenty centuries." (p.113)

"Geographical causes" are to be understood as the analysis of air stream influence (influences on water and temperature transport, influences upon precipitation, erosion and modification of relief), the analysis of ocean currents (influences such as that of the Gulf Stream, or the melting of polar ice providing cold fresh water), and the influence of local circumstances (lakes softening the climate, volcanoes). It is clearly apparent in the works of certain authors studied that the causes do not take the logical form of a tree diagram, but that some are linked by feedback. For instance, the position of the Earth's axis has an obvious influence on the variation of the main air streams, which in turn may influence the axis of rotation by an inertia effect. We begin to see that the complexity of the phenomena makes it impossible to organise them hierarchically as the different terms of a convergent series. Confronted by the profusion of diverse theories, Mascart continues to defend the plurality of ideas. "The most important question is: what is a hypothesis?" he writes, and adds "A hypothesis is nothing but a momentary construction: it has no absolute or final value [...] A hypothesis is good if it is fertile, if it stirs up research and criticism." Mascart would thus be closer to Lakatos' ideas than to Popper's. To him, the test of a hypothesis is not refutation by experiment. His position is more subtle: he has confidence in opinion that will prevail with time, and gives bonus points to what is likely to encourage future research. So, on a controversy which impassioned people at the time, he writes, "the very proof that this [the continental drift hypothesis] is right is the harshness of the criticism that it generates. And then, what is against it? Absolute facts, fundamental impossibilities? No: opposing it is only that its does not fit *some other hypotheses*, accepted today it is true, but what will be their own value tomorrow? [...] for if the hypothesis is weak, time will carry it gently away ..."

Mascart comments that the authors hold to explanatory reasons that are over-particular, and concludes the chapters on the geographical causes with the observation that "All the successive theories are insufficient."

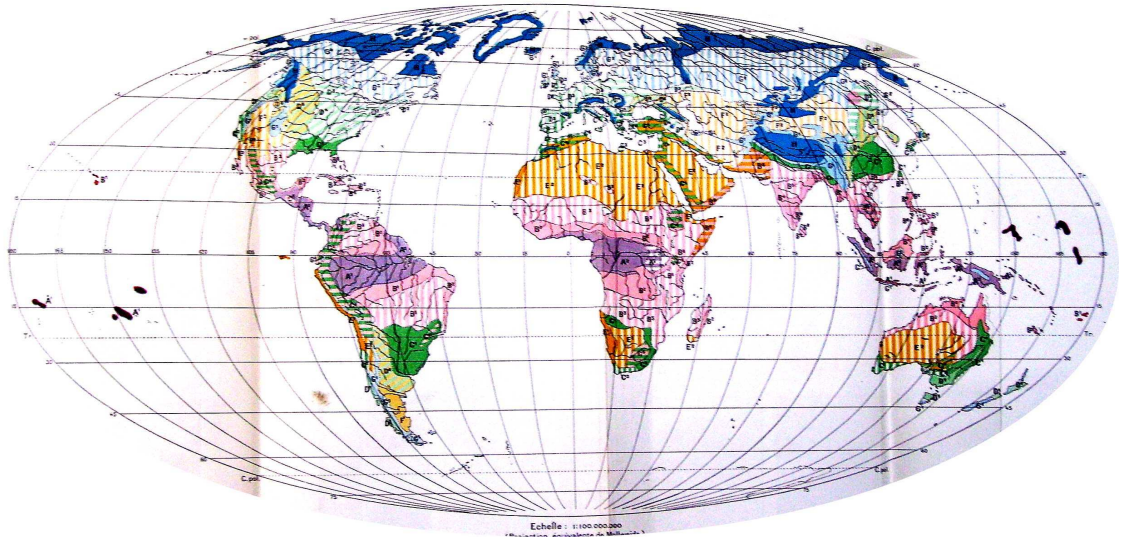
His approach to human causes, today called the anthropogenic effect, is a plea for a better management of old documents and archives. He explains how much the urban phenomenon changes the characteristics of the weather (temperature, rainfall) and that it has to be taken into account to maintain the comparability of measurements. He points out that it is the anomalies, the unusual phenomena of the climate, that should be put in correspondence with the economic facts, and that in this respect the averaging out of data often deletes the most interesting things. The poor quality of the measurements in 1925 had him write, "In these circumstances, given the brief duration of the few reliable observations, it seems a complete illusion to seek arguments for or against the hypothesis of climate variability among meteorological observations properly so called".

After several chapters devoted to detailing the astronomical causes that are his speciality, Mascart devotes a few pages to a discussion of the works relating sunspots to the price of wheat or other economic indicators (Herschel 1801, Arago 1825, Barral 1866, Chambers 1863, Hunter 1876, etc.). Whereas today we are concerned with the part played by the economy on the climate, Mascart considers it interesting that some causes of climate variation may shed light on economic science, which fits in with his permanent concern for scientific fertility.

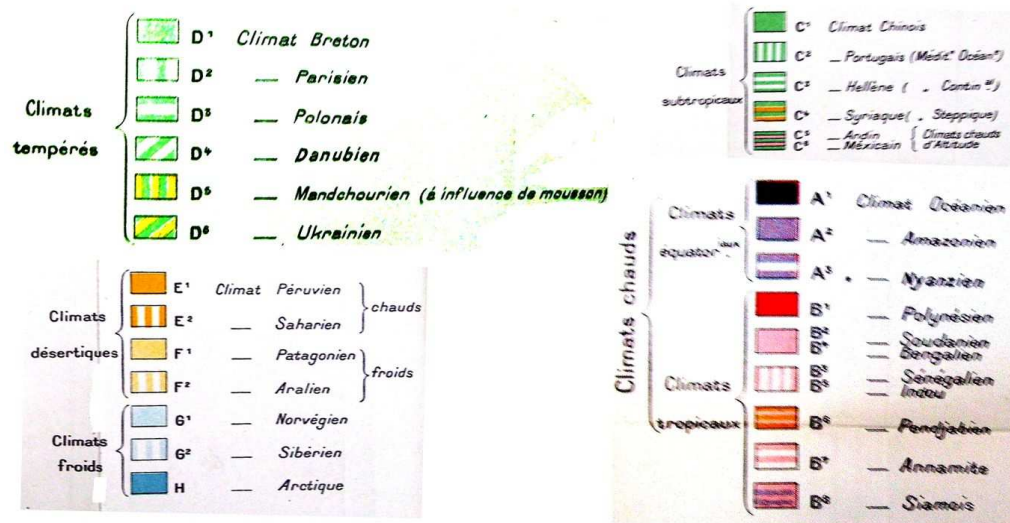
The general conclusion of his book deals with the necessity of improving observations, the care to be given to measurements and their comparability, conservation, and indexation. The net result of his huge work is scarcely inspiring, and Mascart knows it. Conscious that his work could be considered confused, he recognizes that he hasn't drawn "the general lines at a stroke, broadly brushing in the steps", and that he still lacks a general point of view. He esteems that faced with "successive contradictory works" and "opposed hypotheses", the "spirit of association and collaboration" should be developed further.

What is outstanding in this book is the constant modesty of the author before the complexity of the subject. This attitude leads him to consider as an unyielding reality the fact it is impossible to choose among different theories, and that this situation is not bad in itself. "[These testimonies] are sometimes [contradictory - and this is evidence of good faith on the part of the authors]", he writes. (p.33) Mascart seems to take it for granted that scientists are disinterested. Yet knowledge in climate matters may be involved in active money-making schemes: this was the case with the plan to immerse the Sahara (at that time part of the French empire) by diverting 172 billion of cubic meters of water from the Mediterranean with a daily evaporation of 28 million cubic meters. Mascart presents the case and discusses both its feasibility and the stakes involved.

As far as climate variability and the question of human influence are concerned, the multiplicity of effects and causes is impressive, and the mechanisms which could explain them still more so, so that many authors make the mistake of proposing theories. "All the authors seem to become intoxicated with the hope of finding simple and single origins [of climate variations]" but "each time a cause is considered, it at once appears insufficient and is completed by mere hypotheses".



1925 Climate map (after E. de Martonne), A. Chevalier and L. Cuenot, *Traité de géographie physique*, Paris, Armand Colin, 1925

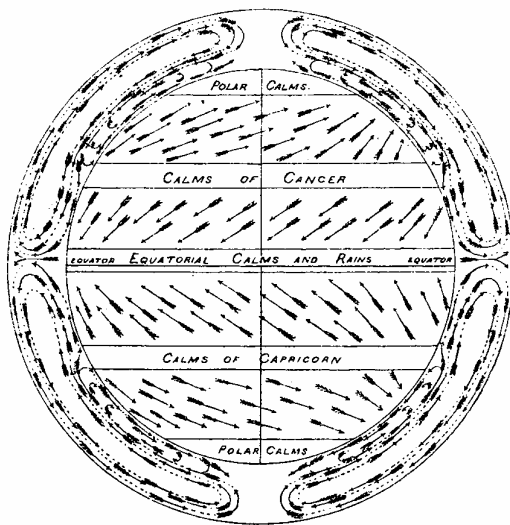


The scientists seem to be at a loss faced with this complexity, as if they do not want simply to recognise it, or do not know how to examine it as it is, and can do nothing other than propose explanatory theories. This is the most common form of discourse in scientific publications. The theories are fairly rapidly contradicted by certain examples. But Mascart clearly shows that these refutations *invalidate the numbers and the order of magnitude of the conclusions but not the theories themselves*, which retain a part of possible truth. This is thus effectively not a situation where the Popperian approach is practicable.

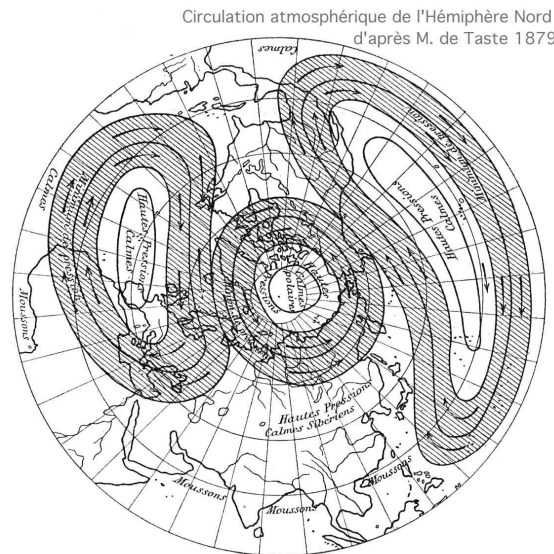
Furthermore, on the specific problem of the anthropogenic effect, we do not know what would have happened if human beings had not existed. This remark is significant. There is no reason why the path we have not followed should be thought of

in a more determinist manner than the path we are following. It could certainly have been warmer than it is at present: we can imagine, for instance that the oilfields not worked by human beings would have spread, naturally or after earthquakes, over the surface of the oceans, forming a film which would have obviously changed the evaporation and created consequences that are hard to assess ⁶.

Reading Mascart’s work, we realize that the epistemologists Popper, Kuhn, and Lakatos were mainly thinking of the case of physics, which they raised, consciously or not, to the rank of generic situation for scientific knowledge. For in the situation described to us, no direction clearly stands out as the rational path. The line adopted by our author is a sort of “wait and see”: he places his trust in the scientific community as an institution that will abandon weak hypotheses over time and organize itself to make use of accessible data⁷. This attitude has two failings, however: first its passivity, which is not the best way of tackling risks, and then the somewhat idyllic image of scientific society it relies upon. We shall return to that point later.



La circulation de l'atmosphère d'après J. Thomson.



⁶ In this respect, the imagination of climatologists is also quite productive “what will happen if the slow growth of the Bahama strait corals ends up by closing the passage of the Gulf Stream? [...] the European and American civilization will be transformed, rich cities will turn into wastes, deserts will become populated, and so many events will be the consequence of a mere polypary.” (Thoulet J. *Ann. de Géol.* t IV, p269, 1894-1895).

⁷ We should note that such a reference to the scientific community was also made by the contemporary pragmatist philosopher Richard Rorty as a model for a social organization (R. Rorty "Science as solidarity" in *The Rhetoric of the human Science*, J. S. Nelson, A. Megill and D. N. Mc Closkey eds, Univ. of Wisconsin Press 1987).

The epistemological landscape is here composed of many theories which sometimes oppose and sometimes complement one another. Mathematical formalisms are present through the astronomical and fluid thermodynamics calculations, and co-exist alongside geographical and economic explanations. The result is a vast entanglement of causal dynamics. It is noticeable that the first rational steps consisted *in simplifying so as better to understand*. For instance, in the 19th century atmospheric circulations were calculated on an ideal sphere without continents or oceans where only physical laws applied. But since such models gave a poor explanation of the observations, towards the end of the century (Maurice de Tastes 1874), an approach more respectful of the observed particularities was tried, and followed from one complexity to another. The situation as Mascart felt it can be described epistemologically as a miscellany of the points of view of Lakatos, Quine and Feyerabend.

It is indeed above all necessary to promote conditions for more cooperative research programmes that have a higher concern for accuracy and comparability of data (Lakatos). It should also be recognised that chronological series of numbers are always finite, and are likely to receive several interpretations, each of which can be improved so as to adapt to new data (Quine's underdetermination). Finally, it is unwise to reject anything definitively on methodological grounds *a priori*: it is better to give all ideas a chance (Feyerabend).

III. A Generic Situation

The chronological dimension is the principal axis of the problem of climate change, and one might imagine that this is why the Popperian doctrine fails to apply. Indeed, how can we know today if a theory is falsifiable if its consequences appear only in a century or two? Yet we cannot call all predictive theories unscientific, since prediction is of the essence of science itself, nor can we accept all theories if they contradict one another.

The chronological dimension pushes the sanctions or possible adjustments into the future, but the situation is the same in any field for theories that correctly satisfy today's checks. The deductions furnished by these theories are in advance of investigations requiring higher performance equipment and can only be evaluated after a better understanding of the field concerned and of its relationship with other domains of knowledge. This is evidently not only a question of precision. The refinement of instruments goes hand in hand with that of theories, and as mathematics demonstrates very well, it is ideas that are the factors of advance in the reading of complexity⁸. Alexandre Koyré has shown that at the time of Tycho Brahe the Copernican system was less precise than the Ptolemaic. As for what makes ideas take hold - what gives them this enlightening power - it has something to do with simplification but takes the most varied forms, as Feyerabend has analysed. To go beyond the available data by means of an interpretation which provides more than is strictly to be gleaned by

⁸ See N. Bouleau *Philosophies des mathématiques et de la modélisation*, L'Harmattan 1999, p.241 *et seq.*

observation: this is what is expected not only of theories in the classical sense, but of all models.

In effect, the epistemological question, “what is true knowledge?”, or what makes one set of representations better than another, should be posed today at the level of models. The notion of theory that was utilised so extensively by the philosophers of the 19th and 20th centuries is now somewhat dated and very academic in hue. Now that simple algorithms can engender configurations of astounding variety and unexpectedness that defy all theoretical *a priori* descriptions, we have too many artificial examples stemming from no theory⁹ not to think that natural things are also capable of astonishing, specific combinations and dynamics that the search for theoretical laws alone cannot reach.

Let us attempt to analyse the contemporary situation starting from the problem of climate change, similar to that analysed by Mascart, but thinking in the context of modelling.

First, it is useful to distinguish between the use of models and the modelling (or model creation) that we are considering here. Models can be used in classical scientific activity: the satellites of Jupiter discovered by Galileo were a model of the Copernican solar system; the Ehrenfest model helps us understand that the reversibility of the laws of mechanics is compatible with the statistical laws of thermodynamics. Modelling is a larger and more common activity, consisting in representations, usually but not always aided by computer, which are generally intended not so much to offer greater objectivity and universality as to help decision and action.

Modelling presents features which distinguish it from other forms of knowledge or representation.

First of all, it takes place in a precise social setting. It is done by someone (a designer, a research laboratory, a design consultancy) for someone, the client (private company or public organisation), in an economic and geographical space, at a certain date. This obviously does not mean that modelling cannot use tried and tested knowledge that is considered as universal and objective, since such knowledge is available to all. Rather, it can in addition draw heavily upon data known only to certain actors, and its conclusions can make sense only for the action of certain economic agents or of certain institutions.

Next, modelling is a hybrid language, comprising natural language, the language of the sciences, and often, the language of the engineering sciences whose several special terms fashioned by usage do not achieve perfect rigour (decennial high water level, for example). This hybrid language provides semantic input for mathematical and computer formalisms and connects them to the signifieds shared by the actors concerned.

Finally, modelling is usually under-determined relative to what it is trying to represent. The point made by W. V. Quine that theories can be understood in several incompatible ways is much more apparent and general in modelling. This is equally

⁹ By theory we mean deductively structured concepts linked to the real by correspondence rules or paradigmatic analogies.

evident in cases where only a finite quantity of observed data is used, as where a finite number of points can give rise either to a polynomial or to a linear combination of sine and cosine, or where other models are compatible with the data, and different families of models are indefinitely perfectible as the number of observations increases. The phenomenon is general: an act of modelling can be validated as much as one would like, but it remains one interpretation among others.

The result of these defining characteristics is that the quality of modelling work cannot be evaluated without taking into account the person or organisation the work was done for, the actor who will make use of it. Let us return to the problem of Jean Mascart, who wanted to move things forward, but who was confronted by complex phenomena (retroactive effects and symbolic representations) explained by theories (today acts of modelling) that were sometimes contradictory, and often imprecise and hypothetical.

IV. Partisan representations are neither unworthy nor dishonest.

Mascart thinks it appropriate to wait. Wait for better observations, and also for the scientific community to work in a more cooperative manner by exchanging information and criticism so that *ad hoc* hypotheses are ultimately abandoned.

Today it seems that a growing number of academics recognise that the Popperian norm is relevant only in highly idealised cases, and that in practice one is before a mixture bringing together among others the views of Lakatos, Quine, and Feyerabend, where the direction of the rationality is hard to follow. The most scientific attitude would thus be to wait before pronouncing upon this or that truth or such and such a consequence, while trying to contribute to the research in progress. It must be recognised that this “confined” research – “recherche *confinée*” as Michel Callon¹⁰ calls it - far from the stakes, interests and conflicts of actors, is highly agreeable: ideas are favourably received and disciplinary recognition is gratifying.

This attitude may appear wise, but it leads to the permanent victory of *faits accomplis*. As Ulrich Beck pointed out in the 80s, technological innovation emerges essentially through *faits accomplis*, on the one hand via the market release of products prepared in secret, and on the other via research laboratories which invent new possibilities without controlling who is going to use them¹¹. An epistemology which is based on collective knowledge tested within the framework of the world scientific community and which leads scientists to wait for progress before pronouncing on possible consequences, maintains a passive conformity which protects two pillars of contemporary positivism: freedom of initiative for market release of products, and freedom for the researcher to work on whatever he sees fit. It is perfectly

¹⁰ Cf. M. Callon, P. Iacoumes, Y. Barthes, *Agir dans un monde incertain, essai sur la démocratie technique*, Seuil 2001.

¹¹ U. Beck, *Risk Society, toward a new modernity* (1986), Sage 1992.

understandable how *faits accomplis* such as the spread of GMOs or the delinquent feeding of cattle leading to BSE are unstoppable in such a system. This has led to reactions such as the precaution principle, wholly understandably in the context.

But we can approach things differently. The narrowly scientific attitude, when the wait is over and science has progressed, leads to a situation that is just as confused as before. Simply, during this time, a certain number of *faits accomplis* have emerged, accomplished not by angels but by men of every stripe, including drug traffickers, unscrupulous egoists, terrorists, and the agencies of totalitarian regimes. And not less important are those accomplished anonymously, by everyone and no-one, due to the underlying evolution of mores just as the sociologists of modernity have described¹².

The narrowly scientific attitude, which relies upon scientific progress to eliminate the least relevant representations, appears to be indissolubly connected to the belief that men are good and that groups, nations, organisations, companies, and networks are harmless.

More precisely, this attitude implicitly admits two theses. Firstly, that knowledge creation will escape logical contradictions if we work more in a “spirit of cooperation”, with greater synergy between the modelling teams. In other words, the complexity encountered will dissipate if *conflicts of interpretation* are gradually reduced, for example by coming to agreements on experimental protocols which become incontestable standards; by eliminating all abstract or vague terms from competing theories so that only the strictly descriptive and factual skeleton is retained following the ideas of the neo-positivists of the Vienna circle; or by harmonising points of view by regular colloquia producing consensual syntheses. This irenism, which constantly risks becoming argument by authority, ignores the fact that differing interpretations are the principal means at our disposal for advancing in complexity. They are our guiding lights in the forest. Their mutual consistency is not a necessary condition for comprehension, which can happily accommodate “local maps”, as René Thom puts it. In the context of discovery, scientific irenism is an illusion which can scarcely win the approval of researchers.

The second implicit thesis is that the purification necessary for scientific detachment is a process that can unfold over time, reality remaining *unchanged*. This was in fact the case from antiquity until, say, the 18th Century: then the influence of science on the world through the intermediary of technology was relatively slow, this slowness in turn most probably favouring in us the emergence and the durability of philosophical categories of division (between the mind and the body, between facts and values, between knowledge and interest). But the appropriation of material power or the economic advantage that an innovation is likely to give is today extremely rapid. This surprises even the research milieu, where knowledge circulates at the rhythm of congresses and the reading of publications. The new link with possible social uses has

¹² For instance, A. Giddens, U. Beck, S. Lash, *Reflexive Modernization, Politics, Tradition, and Aesthetics in the modern social order*, Polity Press 1994.

modified the historic significance of laboratory work upstream of innovation¹³. In addition, the *intentions* of actors (other scientists, developers, etc.), have today become a permanent preoccupation, and a possible risk.

In front of the dangers currently run by our planet and its human and non human inhabitants, and projects to manipulate human nature itself¹⁴, a more active attitude on the part of scientists is indispensable. If he holds to the quest for objectivity, in which society tends to maintain him, the scientist will inevitably revert to the narrowly scientific attitude. But the moment the lobbies act to promote their strategies, academics who thus remain confined in their research (pure or applied) are transformed into the mere hired help of a logic of development that is insatiable, blind, and without fear.

We must therefore look to break out of this cycle of pure/applied research – development – expansion, which is the credo defended best by economic agents on the one hand and by the media on the other. Scientists have certainly a role to play in this change, for they bear the vague responsibility for all the absurdities that this motor can lead to. They have however only limited means at their disposal: they are ordinary citizens, with no financial or institutional power whatsoever. Their only advantage is that they know knowledge production well, and one of their most efficient instruments is their possession of a language able to deepen both preciseness and interpretations: modelling.

The way that suggests itself consists not only in criticising the results or methods of colleagues in order to improve them, but also in criticising the experiments and modelling, taking into consideration the interests these favour, and *shouldering the imaginative task of reviewing their possible consequences*.

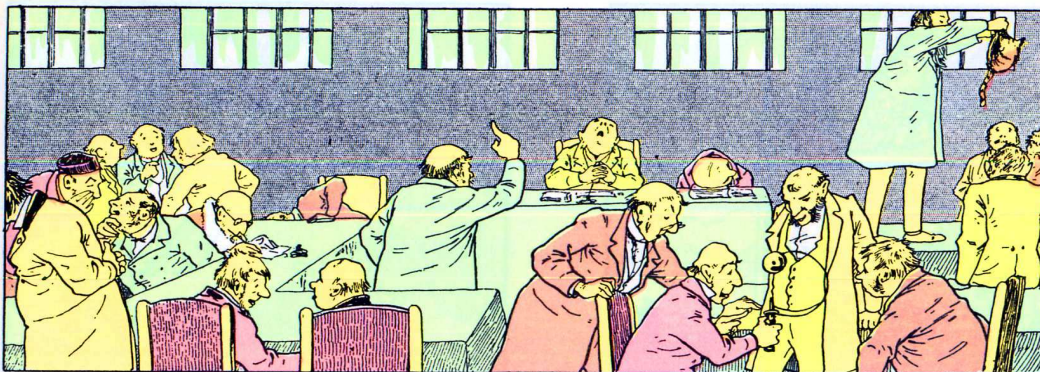
Those who engage in producing *counter-models*, in partisan cautions in the good sense of the term, who contribute to the biodiversity of ideas and representations by accepting their own cultural roots, are the true defenders of the collective good as this is properly understood. I consider that the most appropriate context for the production of these critical models is likely that of the research centre, and that it would be normal in this respect for the cultural values of the teams to be present and made explicit, the base from which the work of interpretation and anticipation can be conducted. It is in “grey literature” that this self-questioning and commitment take place, for the major reviews will probably remain politically correct for a considerable time to come.

¹³ Examples are numerous concerning patents and the community of biologists: cf. notably D. Pestre *Science, argent et politique*, INRA ed. 2003. But the purest science is not free of such surprises: when the International Mathematical Union presented the Gauss prize in September 2006 to Kyosi Ito, 92, the best known mathematician in the banking world today, he confessed his astonishment when the importance of the Ito calculus in trading rooms was explained to him in the 1970s. The work he began after the war was at the heart of a large research movement, stochastic calculus, in which the Soviet and the French schools were particularly distinguished (the members of the latter being relatively left wing or extremely left wing politically). Since 1973 stochastic calculus has become one of the principal capitalist tools on organised markets.

¹⁴ See for example the trans-humanist movement, which brings together brilliant academics in the USA with the declared aim of genetically improving man, cf. J. Brockman *The New Humanists* Barnes & Noble 2003.

This is the best real work for mankind: expose as much as can be exposed the risks that the technological adventure poses for it¹⁵. There is a loss to be accepted here: the era of knowledge gathering is over.

With regard to the greenhouse effect, as in other domains, there is no shortage of Mr. Homais¹⁶ to be found, the sort of person who has had some higher education and who will intone to anyone who will listen that “temperature is such a stochastic fractal that one can say everything and anything on the subject”. Attempting to make such interlocutors understand that the anthropogenic effect has been proved cannot be the only aim of scientists. The problem is how to uncover the interests behind the actions and the words that hide them, in the very place where the processes occur. Concretely, this poses a large number of questions, most evidently that of the right of collective scientific authorities to interfere in public and private research. But there is also the question of research management, which to me is philosophically fundamental: in France we speak a great deal of making universities financially autonomous, while implicitly maintaining everywhere the universality of the same positivist classical science. Is it not the precise opposite that should be done, namely make the universities autonomous in the values they defend, while maintaining public credits for critical rationalism as such ?



Aussi, le lundi suivant, l'astronome Scarmat (Jean), parrain du gigasélénanthropocynoïde, s'empressa-t-il de faire devant l'Académie attentive une communication sensationnelle commençant par ces mots : « Permettez à un modeste savant... » et se terminant par : « c'est, j'ose le dire, la plus belle découverte des temps passés et présents! » Après quoi les académiciens n'ayant plus rien à se dire, s'érigèrent en comité secret, ce qui leur permit d'expulser le public et de s'éclipser ensuite eux-mêmes à l'anglaise.

The cartoonist Christophe evokes the astronomer Jean Mascart under the pseudonym Jean Scarmat, and credits him with special rhetorical powers, *L'idée fixe du savant Cosinus*, librairie Armand Colin 1899.

¹⁵ Cf. A. Lebeau, *L'engrenage de la technique*, Gallimard 2005.

¹⁶ Cf. J.-Ch. Hourcade, V. Journé, "Monsieur Homais, les guides de montagne et le maître nageur, variations sur la négation des risques climatiques", *Critique internationale*, n°18, (2003) 65-79.