A Case of Irrationality?*

Anouk Barberousse
Institut d'Histoire et de Philosophie des Sciences et des Techniques
(CNRS – Univerisité Paris 1 – Ecole nomale supérieure), Paris

DRAFT. PLEASE DO NOT QUOTE WITHOUT PERMISSION OF THE AUTHOR

1. Introduction: An Episode in the History of Science

As soon as Maxwell published his first paper on the kinetic theory of gases in 1860, he noted that a major consequence of its principles, namely the equipartition theorem, conflicted with known empirical data about the specific heats of poly-atomic gases. According to the equipartition theorem, the total kinetic energy of an isolated system of particles at equilibrium is equally distributed among the particles' degrees of freedom. The equipartition theorem implies that the calculated values of γ, the ratio between the specific heat of a substance at constant pressure and its specific heat at constant volume, disagree with the measured ones, except for monoatomic gases. The calculated values of γ are too small compared to the measured ones (see table 1). This is called the "specific heats problem". It originates in the

* Many thanks to Denis Bonnay and Carlo Proietti, both at IHPST, Paris, for comments and suggestions about an earlier version of this paper, and to Pascal Engel who was at the origin of this reseach.

difficulty to guess, at the end of 19th century, how many degrees of freedom a polyatomic molecule possesses, and has only been solved with quantum mechanics. We now have a very different conception of molecules – a quantum conception, providing us with an explanation of the failure of classical statistical mechanics – but at the time, the structure of molecules was completely opaque.

In spite of the theory being empirically refuted, Maxwell and Boltzmann went one developing it. True, Maxwell has pessimistic sentences at the end of his 1860 paper:

> "This result [the equipartition theorem for a system of polyatomic molecules] […] seems decisive against the unqualified acceptation of the hypothesis that gases are such systems of elastic bodies."

And here is the last sentence of the final summary concluding the paper:

> "Finally, by establishing a necessary relation between the motions of translation and rotation of all particles not spherical, we proved that a system of such particles could not possibly satisfy the known relation between the two specific heats of all gases."

Maxwell nevertheless:

- published his results,
- and went further in the exploration of his theory.

Moreover, Boltzmann did the same.

According to the main current logical-philosophical accounts of belief change, they should have given up their theoretical enterprise and change their beliefs about the promise of the kinetic theory of gases. However, for all we know, they did not change their beliefs and placed more and more confidence in the theory. Prima facie, Maxwell and Boltzmann have thus been guilty of a rather severe kind of irrationality, or incoherence.

How are we to described and analyze this case? We cannot just tell the story of Maxwell and Boltzmann inventing statistical mechanics and explaining more and more phenomena or the story of statistical mechanics being progressively developed and refined as if there was no epistemological anomaly. In order to account for the beginnings of statistical mechanics, our story has to be based on an underlying view of how scientists reason and conceive of the relationships between the hypotheses they consider and the available data. Moreover, this underlying view cannot be the usual one coming either from methodology or from the epistemology of belief change.

My aim in this paper is to contribute to the working-out of such an underlying view. Given the available theoretical proposals in epistemology and in the formal study of belief change, which are much too abstract to be directly applicable to such a case, my enterprise is as much one in the methodology of history of science as one in epistemology and philosophy of science. In section 2, I present two methodological options as regards Maxwell's and Boltzmann's rationality and argue in favor of the one leaving open the possibility that they have not been guilty of irrationality. In section 3, I briefly present some theoretical approaches to belief change in order to indicate which ones could provide the best account of the historical case if only they were slightly adapted. In sections 4-6, I come back to the description of the case in order to refine it and point to the aspects of it that according to me are in need of formalization or theorization. In section 7, I propose some clues to such a formalization.

2. Methodological setting

Let us first of all try and locate this inquiry within the different available domains. Two main domains appear as possible *loci* for such an inquiry: methodology and epistemology. I could either focus on the internal development of statistical mechanics and explore how it has been partly confirmed and partly disconfirmed, what have been the consequences of a contradiction with empirical data, etc., or on Maxwell's and Boltzmann's epistemic attitudes. For sure, methodology and epistemology are close to one another: as V. Hendricks emphasizes, “methodological recommendations for ‘rational’ scientific inquiry, truth-conduciveness, reliability, convergence, strategies for winning games, changing beliefs economically and reliably, and the like, are at the very *core* of many formal epistemological approaches” (2006, 24). Rationality is precisely at the meeting point between methodology and epistemology. As one puzzle in the historical episode I focus on is precisely about rationality, I claim this episode is best analyzed by taking the actors’ beliefs and other epistemic attitudes into account, and not just the internal development of the theory. The specific heats problem is less an abstract problem for the kinetic theory than a problem about the analysis of Maxwell’s and Boltzmann’s views on the dynamics of scientific theorizing.

The next methodological point is to set up a strategy for handling the rationality puzzle. Two options are available: (*i*) assume that Maxwell and Boltzmann have been irrational; (*ii*) suspend our judgment and try to build up a sufficiently flexible theoretical framework allowing us not to decide beforehand that they were irrational. In other words, such a framework would leave open the possibility that Maxwell and Boltzmann were not irrational to develop statistical mechanics further in spite of its contradiction with available data. Option (*i*) would amount to a paradox: the development of statistical mechanics would be seen as rational (because of its impressive empirical success) whereas the individual scientists developing it would be seen as irrational. Such an option would thus create a new problem instead of clarigying one.

Accordingly, I think the second option is worth trying out, at least in this case which seems to be one whose only components are the physicists, the theory and the data. No extra-scientific factors, like a anti-scientific *Zeitgeist*, are involved that could result in irrational decisions. In other cases, as the one analyzed in Cushing (1994), historical contingency seems to have exerted an important influence on scientists facing a difficult choice between two theoretical approaches, namely the Bohrian interpretation of quantum mechanics on one hand and its deterministic counterpart on the other. (Cushing would not admit that his analysis implies that scientists are irrational in using non evidential criteria for theory choice; however, following Saunders (2005), I think that Cushing's book results in claiming that the physicists who accepted the Bohrian interpretation of quantum mechanics *were* irrational not to compare it seriously with its deterministic counterpart).

As far as the specific heats problem in concerned, Maxwell's and Boltzmann's can be idealized as pure scientists, insensitive to extra-scientific influences, and their epistemic attitudes about statistical mechanics can be idealized as being isolated from other factors. This seems to be a pure case in epistemology, against which to test various theories.

3. Approaches to belief change

The mother of all theories of belief change, so to speak, is the AGM theory of belief revision (Alchourrón et al. 1985). Its starting point is the modeling of a belief-state as a set of propositions (or sentences) closed under logical entailment. This is of course an idealization. According to Gärdenfors (1988), this idealization "is judged in relation to the rationality criteria for the epistemological theory" (9). This implies that for a rational agent, it is required, within the limits of her cognitive abilities, to believe the sentences implied by any sentence she believes. Of course the precise characterization of human cognitive abilities are crucial here; another useful complement would be also to specify whether other epistemic attitudes than belief are concerned by this idealization.

Within this model of belief-state, belief revision is said to be the change in the belief-state occurring in response to epistemic inputs, namely the encounter of the agent with new information, be it by experience or by testimony. The AGM theory sets out principles for belief revision. Let *K* be a belief set and *P* a proposition corresponding to an epistemic input. $K{*}P$ is the result of revising *K* by adding *P* and accordingly (that is, minimally) changing *K*.

Two cases have to be distinguished, according to whether *P* is logically consistent with *K* or not. When *P* is logically consistent with *K*, *P* is simply added to *K* as well as the propositions it logically entails (in symbols: $K+P = \{Q \mid K \cup \{P\} \rightarrow Q\}$, where "+" is the expansion operator and "+" denotes logical entailment.)

P can also be withdrawn from *K* (the result is $K - P$ where "–" is the contraction operator). However, in general, if *P* is withdrawn from *K*, other beliefs also are. AGM theory includes a principle of informational economy governing which beliefs have to be removed together with *P*, according to which the set of removed beliefs must be minimal. However, this principle is not enough to choose, among all beliefs which are "tied" to *P*, the ones which will be removed. Purely logical considerations are insufficient for that matter. Other principles are required. Gärdenfors has proposed an ordering of the conserved beliefs in terms of a relation of *epistemic entrenchment*; whereas Spohn (1987) argues in favor of a relation of *epistemic plausibility*. These relations can reflect personal preferences.

The main claim in AGM theory is that expansion and contraction are interdefinable within the Harper and Levi identities:

Harper identity: $K - P = (K * \neg P) \cap K$

Levi identity: $K * P = (K - \neg P) + P$

Belief revision is always, according to AGM theory, the composition of a contraction and an expansion.

The AGM theory has been refined and revised several times. In most of its successors, however, the principle according to which when *P* is logically inconsistent with *K*, several beliefs in *K* have to be removed before including *P* remains a basic condition of rationality. In order to fulfill our task, namely in order to formalize Maxwell's and Boltzmann's epistemic attitudes without *a priori* suspecting them of irrationality, the AGM theory thus has to be amended. I will examine some of the most recent proposals in section 5, after having worked out a more precise description of Maxwell's and Boltzmann's epistemic attitudes.

Belief change can also be modeled within a Bayesian framework. To put it briefly, the principles of Bayesianism imply a redistribution of the agent's degrees of belief when she accepts a new information which is at odds with her prior beliefs. However, neither Maxwell or Boltzmann seem to have diminished their confidence in the validity of statistical mechanics, that is, attributed lesser degrees to their belief that it is a satisfactory theory. Consequently, the Bayesian framework has from the same drawback, relative to our historical case, as AGM theory: it is difficult within it to account for Maxwell and Boltzmann both accepting the specific heats data and working out statistical mechanics.

4. A web of beliefs, acceptances and doubts

In this section, I try to give the richest description I can of Maxwell's and Boltzmann's epistemic attitudes about statistical mechanics and the specific heats problem (given that our only access to these attitudes is through their papers). Elements of different nature are parts of this description: data, mathematical results, hypotheses of various generality, models, fundamental theories (e.g. Newtonian mechanics), speculations. The relationships between these elements and Maxwell's and Boltzmann's minds may be equally diverse.

In section 4.1, I present Maxwell's own reflexive views in 1872, at a time where statistical mechanics has already taken off. In section 4.2, I propose a tentative classification of Maxwell's attitudes vis a vis statistical mechanics in 1860. This is a first step toward the development of an informal equivalent of Gärfenfors' relation of epistemic entrenchment or of Spohn's relation of epistemic plausibility. In section 4.3, however, I show that no simple, unified picture of Maxwell's epistemic state can be given, since his beliefs make up disconnected sets and are cannot be classified according to a single axis of entrenchment or plausibility. I insist that my aim in this section is to present the relevant elements in an *informal* way, as a preparation to the more formal proposals of section 5.

4.1 Maxwell about molecules in 1872

A specific features of the historical case I focus on is that its main actors are all but naïve about their own epistemic states and the hypotheses they are entertaining. Here is a striking example of Maxwell's reflexive stance about molecules, whose existence was still uncertain at the time. In a lecture he gave in 1872 at the British Association for the Advancement of Science, and published in *Nature* the year after, Maxwell examines the following questions: What is known about molecules? What is hypothesized, or speculated? He proposes the following ranking of the available results concerning molecules:

> "We may divide the ultimate results [of molecular science] into three ranks, according to the completeness of our knowledge of them.
>
> To the first rank belong the relative masses of the molecules of different gases, and their velocities in meters per second. These data are obtained from experiments on the pressure and density of gases, and are known to a high degree of precision.
>
> In the second rank we must place the relative size of the molecules of different gases, the length of their mean paths, and the number of collisions in a second. These quantities are deduced from experiments on the three kinds of diffusion. Their received values must be regarded as rough approximations till the methods of experimenting are greatly improved.
>
> There is another set of quantities which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate,

> as in the second, but is only as yet of the nature of a probable conjecture. These are: The absolute mass of a molecule, its absolute diameter, and the number of molecules in a cubic centimeter."

In this lecture, Maxwell thus carries out a careful and self-conscious evaluation of the various available results about molecules. However, this evaluation occurs quite late after the beginning of the whole enterprise of statistical mechanics. What would be mostly relevant is a similar evaluation of Maxwell's own hypotheses in 1859-1860, when he was working at his first paper on the kinetic theory of gases.

4.2 Maxwell about the possibility of statistical mechanics in 1860

The only resource we have about Maxwell's views in 1860 is indirect: it consists in what we can infer from his paper. Here is a tentative reconstruction:

(1) *About the applicability of the principles of mechanics to microscopic objects*: Maxwell evaluates it as highly plausible, even if he regards the analogy from the visible to the invisible as problematic.

(2) *About the atomic hypothesis*: The validity of the atomic hypothesis is indirectly at stake in the paper. Undoubtedly, Maxwell was profoundly convinced of its truth, and this conviction was the mainspring of his investigations in statistical mechanics. Since only indirect and far away consequences of it are affected by the specific heats problem, it is not seriously questioned.

(3) *About the "mechanical analogy"*, namely the hypothesis that the gaseous molecules are reasonably similar to their surrogates in the model, *i.e.*, billiard balls. Assessing the plausibility of the "mechanical analogy" is one of the aims of Maxwell's paper. His conclusion is cautious. For the sake of analysis, the mechanical analogy should be decomposed into three elements: the ideal gas idealizations, the probabilistic hypotheses

underlying the statistical computation, and the available empirical data. Let us examine Maxwell's attitude about these three elements in turn.

(*a*) Ideal gas idealizations (according to which the volume of the molecules is negligible compared to the whole volume of the gas; the collision time is negligible; the intermolecular forces are negligible). Clausius considered that these idealizations were safe, in the sense that for him, they did not introduce any distortion in the representation of gases. As for Maxwell, he gave them up in 1866-1867. Here are the reasons why he changed his mind about these idealizations. Whereas he thought in 1860 that the model based on these idealization is equivalent to a model where molecules are represented by centers of forces, namely material points interacting with each other according to repulsive forces at short distances, and attractive and quickly decreasing forces at larger distances, he demonstrated in 1867 that the ideal gas model and the centers of forces model are not equivalent. We can thus infer from the 1867 paper, that is, from Maxwell's change of mind, that in 1860, the validity of these idealizations was uncertain but slightly plausible.

(*b*) Probabilistic hypotheses used in the statistical computation, namely equiprobability of the directions of bounce after a collision, and independence of the three directions of a molecule's velocity. These hypotheses are held as entirely safe by Maxwell; he even gives arguments to justify them.

(*c*) Available empirical data: Some were almost fully accepted by Maxwell, as by every physicist at the time, like the thermodynamic relations and the measures of specific heats; some were very doubtful, like the dependence of the viscosity coefficient on density.

The main conclusion we can draw from this description is that the meaning of the expression "acceptance of a hypothesis" is highly ambiguous. Moreover, it crucially depends on the type of hypothesis considered. For instance, Maxwell only accepts in 1860 the hypothesis according to which the ideal gas idealizations are safe because he makes a mistake about the kind of model they allow for and believes this model to be equivalent to a centers of forces model. This is therefore a *default* acceptance, which can be attributed to the fact that he was not logically omniscient: he could not grasp, in 1860, the set of all the logical implications of the ideal gas idealizations. This default acceptance is different from the positive, well thought-out acceptance of the probabilistic hypotheses Maxwell uses in his statistical computation. Consequently, any formal analysis of Maxwell's, and more generally of scientists' epistemic attitudes has to carefully differentiate between either types of acceptances or degrees of acceptance.

4.3 Disjoined hierarchies of beliefs

In the last section, I have shown that there are several ways in which an item of science (hypothesis, model, set of data) can be said to be accepted by a scientist. Acceptance in this context is not a univocal notion. The picture of Maxwell's epistemic state has still to be complicated further, since the various items in section 4.2 cannot be ordered along a single axis of acceptance, or represented by a relation of epistemic entrenchment possessing the property of being a total pre-order . These items form disjoined groups the relationships among which are complex. For instance, the evaluation of the "mechanical analogy" depends on the validity of the atomic hypothesis *and* on the validity of the hypothesis of the applicability of Newtonian mechanics to molecules. Whereas the validity of the latter depends on the validity of the former, the former might be true but the latter false.

A complex picture of chains of dependence relations may be drawn, according to the following elements:

(*i*) The three fundamental hypotheses underlying statistical mechanics, namely: applicability of Newtonian mechanics to the description of molecules, atomism, and applicability of probabilistic tools, are partly independent. Consequently, they generate three chains of dependence relations.

(*ii*) Different *types* of dependence relations come into play, according to their starting point: fundamental hypotheses or idealizations involved in models' tractability.

Point (*ii*) can be illustrated with Maxwell's 1867 paper, in which he investigates a model in which molecules are centers of forces. In order to get results from this model and compare them to empirical data, it is necessary, at some point, to give a specific form to the intermolecular force law. Absolutely no clues to such a mathematical function were available at the time, since molecules were highly hypothetical, even speculative entities. In this state of total uncertainty, any mathematical function for the force law would do (within the domain of the usual force laws in mechanics). Now, only one function makes the computation possible: the equations are not solvable unless the intermolecular force is proportional to $1/r^5$. The justification for the adoption of the hypothesis that the intermolecular force is proportional to $1/^{r5}$ is thus strictly model-dependant.

These examples indicate that a faithful formalization of Maxwell's epistemic state in 1860 should be represented by distinct hierarchies of belief and acceptance sets. This epistemic structure is probably not exceptional among scientists.

5. The conditional nature of the elements of the Maxwell's and Boltzmann's epistemic states

The next step in my analysis of Maxwell's and Boltzmann's epistemic states is to take into account an important aspect of most propositions which are accepted of fully believed by scientists, namely their *conditional* form, for instance: "If such theory or hypothesis or such empirical data is correct, then …". The unconnected hierarchies that the propositions accepted by a scientist build up derive from this conditional nature, as well as from the various degrees of acceptance which are assigned to the antecedents.

It might seem that in the historical case I focus on some beliefs or accepted propositions are not conditional, for instance Newton laws. Well, it could be that for Maxwell and Boltzmann, Newton laws are true without any doubt *at the macroscopic level* - but this is not relevant for the specific heats problem. The validity of Newton's laws at the *microscopic* level is highly dependent on other hypotheses concerning the behavior of molecules. Maxwell and Boltzmann are conscious that an analogical transfer from the macroscopic level to the microscopic one is a risky process in a state of total uncertainty or darkness about the existence and nature of molecules. However, they also know that applying such an analogy is a very good way to overcome this “profound darkness” (cf. Boltzmann's quote below).

Maxwell and Boltzmann are also conscious that within these conditionals, the dependence of the consequent on the antecedent comes with degrees. Boltzmann, in a 1895 paper, discusses the various modes of this conditional dependence, underlining the fact that even when a proposition is true *if* another one is, in a case where one does not know whether the antecedent is true, one should not retain from *attempting* to develop the consequences of the consequent. Here are some quotes illustrating Boltzmann’s methodological advice.

Boltzmann, in his paper “On certain questions of the theory of gases”, first quotes Lord Salisbury claiming that nature is a “mystery”:

> “What the atom of each element is, whether it is a movement or a thing or a vortex, or a point having inertia, all these questions are surrounded by profound darkness. I dare not use any less pedantic word than entity to designate the ether, for it would be a great exaggeration of our knowledge if I were to speak of it as a body, or even as a substance”.

Boltzmann then comments on Lord Salisbury’s quote:

> “It this is so - and hardly any physicist will contradict this - then neither the Theory of Gases nor any other physical theory can be quite a congruent account of facts …”

> “Every hypothesis must derive indubitable results from mechanically well-defined assumptions by mathematically correct methods. If the results agree with a large series of facts, we must be content, even if the true nature of facts is not revealed in every respect. No one hypothesis has hitherto attained the last end, the Theory of Gases not excepted. But this theory agrees in so many respects with the facts, that we can hardly doubt that in gases certain entities, the number and size of which can be roughly determined, fly about pell-mell. Can it be seriously expected that they will behave exactly as aggregates of Newtonian centers of forces, or as the rigid bodies of our mechanics? And how awkward is the human mind in diving the nature of things, when forsaken by the analogy of what we can see and touch?”

Boltzmann’s comment shows that he carefully evaluates the reliability of the various conditionals involved in statistical mechanics, and does so even though the epistemic status of one of the most important elements of statistical mechanics, namely, the atomic hypothesis, is a “mystery”. In the same way as a model of a scientist's epistemic state should account for the degrees of conditional dependence attributed to hypotheses, it should account for the scientist’s reflexive evaluation of the hypotheses the consequences of which she explores.

Maxwell, in his review of *A treatise on the kinetic theory of gases* by Watson (the book summarizes and synthesizes Maxwell's work), expresses the same kind of self-consciousness that constitutes an important aspect of a scientist's epistemic attitude toward a theory whose foundations are uncertain:

> “The clear way in which Mr. Watson has demonstrated these propositions leaves us no escape from the terrible generality of his results. Some of these, no doubt, are very satisfactory to us in our present state of opinion about the constitution of bodies, but there are others which are likely to startle us out of our complacency, and perhaps ultimately to drive us out of all the hypotheses in which we have hitherto found refuge

into the state of thoroughly conscious ignorance which is the prelude to every real advance in knowledge."

This quote should engage us in including a special treatment for the category of beliefs bearing on mathematical methods and conclusions in our representation of epistemic states and belief change.

6. Belief change and theory change

Given the dynamic nature of our inner life, the study of its static structures can only be a first stage. In this section, I focus on belief *change*. Maxwell's case is again rich enough to bring out some characteristic features of belief change in a scientific context.

Between 1860 and 1867, Maxwell changed his mind about three topics:

(*i*) the scientific value of the kinetic theory of gases,

(*ii*) the comparison of the billiard balls model and the centers of forces model,

(*iii*) and the independence of viscosity coefficient on the density of the gas.

A comparison between these three cases will show that a rich description of Maxwell's epistemic state is necessary to account for belief change in his case – a case which is likely to possess sufficiently generic features.

(*i*) At the end of the 1860 paper, Maxwell is, to say the least, puzzled about the fate of statistical mechanics - more precisely, he expresses a balanced perplexity, considering the genuine successes of his enterprise (e.g., the microscopic explanation of the ideal gas laws, or the development of a unified analysis of transport phenomena) as well as its failure to account for the measured values of specific heats. He nevertheless overcomes the epistemic discomfort caused by the empirical contradiction encountered by his first attempt at giving a coherent picture of the dynamics of gases, and explores a new "realization" (that is, a new model) of the kinetic theory of gases.

Let us analyze Maxwell's decision further. The empirical contradiction does not lead him to give up his theoretical endeavor, his *theory* stricto sensu, but rather to try and build up another model mediating between theoretical hypotheses and empirical data. This implies that a crude formalization of Maxwell's epistemic state in which every proposition concerning the "dynamical theory of gases" would be put on the same level would be utterly unfaithful. It is at least necessary to distinguish between propositions relative to the general theoretical framework he is investigating and propositions relative to the various realizations of this framework in models, which contain other, sometimes more doubtful hypotheses.

(*ii*) Although Maxwell begins his 1860 paper by asserting that representing molecules as billiard balls and as centers of forces is equivalent in terms of the results computed from the two models, he realizes between 1860 and 1867 that it is not the case, and that the centers of forces model is more satisfactory - except for specific heats, and disregarding the fact that a central hypothesis remains unjustified in this model (namely, the hypothesis about the precise form of the intermolecular force law).

Maxwell's belief change is not the result of "observation", unless the concept of observation is so stretched as to include the results of mathematical computations – which would be paradoxical. This indicates that distinct sources of belief change may be involved scientific change, namely: empirical data, testimony, the possibility and results of calculation. Moreover, these distinct sources of belief change may have different effects according to the element on which they act. For instance, in our case, empirical data contradict the consequences of a physical model, but not the theoretical principles the model realizes. In that case, the acceptance of the theoretical principles is not at stake, but rather the acceptance of the validity of the model.

(*iii*) In 1860, Maxwell was agnostic about the dependence of the viscosity coefficient of a gas upon its density. The few available data at the time seemed to point to a dependence, but they were too inaccurate to be fully relied on. Maxwell's billiard balls model implied that the viscosity was independent of the density, but many features of this model seemed doubtful, to him in the first place. In 1866, Maxwell had acquired a full belief about this point: he was entirely convinced that the viscosity coefficient of a gas does *not* depend on its density. In this case, the cause of his belief change was "observation", or better data acquisition by careful and repeated experiments, together with model exploration.

From points (*i*)-(*iii*), we can conclude that observation, as of course an important cause of belief change – and consequently of theory change – cannot be considered, nevertheless, as the *sole* cause of belief change. Moreover, the effects of the various causes of belief change vary according to which subset of beliefs is concerned. In Maxwell's case, a distinction among beliefs concerning the theoretical framework of the kinetic theory of gases and beliefs concerning the various models which realize it seems necessary - but this distinction is not clear cut. In order to tell whether a given hypothesis belongs to the theoretical framework or to a model, the best method is sometimes to investigate how it is affected by a cause of belief change!

7. Some proposals

The (simplified) case-study presented in sections 4-6 suggests the following proposals, which aim at giving general answers to two questions:

- How should a scientist's epistemic state be represented in a formal theory of belief change?
- What are the possible sources of belief change?

7.1 Representation of a scientist's epistemic state

The case-study reinforces Friedman and Halpern's claim that "it is important to describe the underlying ontology or scenario for the belief change process", and to ask "what it means for something to be believed by an agent" (1997) (the two questions above are inspired by this paper). In order to represent belief change indeed, the first task is to describe what is affected by change. Now most available proposals, perhaps excepting Bochman's (1999), seem to be too idealized to account for the various aspects described in sections 4-6.

Notice first that in my historical case, the object we wish to represent is not, Maxwell's epistemic state proper, but only the part of it concerning statistical mechanics. This part may be conceived of as equivalent to a theory which is not yet finished, or a theory in progress, so to speak. The representation of such a theory in progress should take into account the unconnected character of the various sets of propositions composing it, since it would be simplistic to suppose that these propositions are organized by a unique ordering representing their relative strength or the relative confidence the agent has in their truth. One should leave open the way for including several orderings in the representation, as in (Lindström and Rabinowicz 1991). Lindström and Rabinowicz suggest to replace the usual functional notion of belief revision (introduced by AGM) by a relational notion, which results from a weakening of the original notion of epistemic entrenchment by not assuming it to be connected. Their motivation is that they want to allow that some propositions may be incomparable with respect to epistemic entrenchment. As a result, the family of "fallbacks" (representing the possible products of a revision) around a given theory needs not be nested.

The notion of a family of unnested fallback theories may help represent the variety of epistemic attitudes the agent entertains vis à vis the different propositions within its epistemic state (or the theory she is trying to develop). Intuitively, several uncertainty axes are needed to represent these attitudes, since the plausibility of the relevant propositions are evaluated according to various scales: for instance, the hypothesis that atoms are rigid bodies seems plausible when it leads to successful models of statistical mechanics, but not so plausible when confronted to the spectroscopic data, which suggest a large number of degrees of freedom. The family of unnested fallbacks can thus represent the dependence of plausibility assignments on the context and aim of the evaluation.

Here are some other examples of this point. As we have seen, some parts of the epistemic state (or of the theory in progress) can be described non defeasible, *e.g.*, Newtonian mechanics or the atomic hypothesis. However, the notion of non defeasibility is context relative: in other research domains than statistical mechanics or the kinetic theory of gases, the atomic hypothesis, for example, may be doubtful. It should be emphasized that the relevant context here is not even statistical mechanics in general, but statistical mechanics as it is developed when one deliberately neglects everything in it that would call the atomic hypothesis into question. This narrow context is a post-1860 context, corresponding to a selection of Maxwell's whole epistemic states in which he has gone beyond his doubts about the capacity of statistical mechanics to account for macroscopic matter.

One may object to the "several axes of epistemic entrenchment proposal" that it relies on a confusion between two distinct project, namely (*i*) measuring the epistemic preferences of agent and (*ii*) investigating into the justification of beliefs or hypotheses. The relation of epistemic entrenchment is the main tool of (*i*), which is a formal project. (*ii*), by contrast, is more a project in epistemology and it is not clear whether it can be formalized. This objection implies that whereas (*i*) is relevant to belief change, (*ii*) is not. My response to this objection is that the description I gave in sections 4-6 shows that (*ii*) is definitely relevant to belief change and that (*i*) does not suffice to study it. This is the reason why I believe that the picture of the epistemic state should be enriched. In the end on this section, I make two proposals toward such an enrichment.

Apart from the notion of non defeasible proposition, the notion of working hypothesis can also be used in the description of a scientist's epistemic state. It is context dependent as well. Some examples of working hypotheses are models, beliefs about mathematical tools, and beliefs about mathematical conclusions. The second category of "beliefs about mathematical tools and conclusions" is important in every case where theoretical physicists are dealing with not sophisticated enough mathematical tools and have to improve them. Usually, they are uncertain about the validity of their inventions.

A last feature of a scientist's epistemic state (or theory in progress) is that it is striving toward a fully coherent stage. This *dynamic component* is what differentiates it from the epistemic state, or a theory, of someone who is not interested in scientific progress. It should also be included in the representation of a scientist' epistemic state, perhaps in the guise of dynamic principles of exploration of the consequences of the non defeasible propositions (other examples of such principles would be analogies).

7.2 Sources of belief and theory change

As emphasized above, new empirical data (namely, controlled results of observation and experimentation) are obviously good candidates as sources of belief or theory change. The notion of empirical data is much more constrained than the notion of "observation" referred to in the current theories of belief revision. For instance, an isolated measurement result would count as a reliable motor of belief change only in very special cases. One should restrain from considering that new reliable, empirical data automatically force belief revision whenever they contradict what is previously known, believed or worked on as hypotheses. The above presented case shows that one should leave open the possibility that empirical data are included in the epistemic state even if they contradict other elements in it, with no charge of irrationality. We shall see how this can be (tentatively) done in the next section.

One should also take seriously the fact that the agents under consideration, even if they are scientists, are *not* logically omniscient. Logical operations on the elements of the epistemic state itself (deductions) should be allowed as providers of occasions of belief change. As Harman (2004) claims, theoretical reasoning is actually a way of changing one's epistemic state.

7.3. About Maxwell's and Boltzmann's rationality

According to AGM postulates, Maxwell and Boltzmann *are* irrational, because they do not give up statistical mechanics even though it is refuted by empirical data. Worse, they go on elaborating it. As AGM postulates are taken to be "general rationality postulates", Maxwell's and Boltzmann's persistency suggests that *other* rationality postulates should be considered instead.

I suggest that what I called the "dynamic component" of the epistemic state (or theory in progress) is responsible for Maxwell's and Boltzmann's rationality in spite of all appearances. This dynamic component is indeed governed by a rationality principle which is generally neglected, namely the principle of *positive* coherence (cf. Pollock 1979 and Harman 2004), according to which you not only have to avoid inconsistencies (this is the principle of negative coherence), but also to "find explanations of things in which you are interested". Maxwell's and Boltzmann's theoretical efforts are best viewed as guided by this principle, since their global scientific view of matter is more coherent (in the sense of "explanatory") when they put statistical mechanics into it than otherwise. The principle of positive coherence is thus stronger, in the case of scientific investigation, than the principle of negative coherence, which says that one should avoid inconsistencies. This is at least a tentative explanation of why Maxwell and Boltzmann were not irrational in developing an empirically refuted theory.

8. Conclusion

My starting point in this paper has been the apparent paradox of two physicists, Maxwell and Boltzmann, further investigating an already refuted theory, namely statistical mechanics. In order to leave open the possibility that they are not guilty of irrationality, in spite of the current theories in formal epistemology, I first described Maxwell's and Boltzmann's epistemic attitudes toward statistical mechanics at the time they both were working it out. In section 7, I presented some proposals, inspired by recent papers about belief change, accounting for the richness and complexity of scientists' epistemic states.

References

C. E. ALCHOURRÓN, P. GÄRDENFORS, AND D. MAKINSON, 1985, On the logic of theory change: Partial meet functions for contraction and revision, *Journal of Symbolic Logic* 50, 510-530

A. BOCHMAN, 1999, A Foundational Theory of Belief and Belief Change, *Artificial Intelligence*, 108, 309-352

L. BOLTZMANN, 1895, On certain questions of the theory of gases, *Nature*, 51, 413-415

J. CUSHING, 2004, *Quantum Mechanic. Historical contingency and the Copenhagen Hegemony*, University of Chicago Press

N. FRIEDMAN AND J. HALPERN, 1997, Belief Revision: A Critique

N. FRIEDMAN AND J. Y. HALPERN, 1999, Modeling belief in dynamic systems.Part II: Revision and Update, *Journal of Artificial Intelligence Research* 10, 117-167

P. GÄRDENFORS,1988, *Knowledge in Flux*, MIT Press, Cambridge, MA

G. HARMAN, 2004, Practical Aspects of Theoretical Reasoning, in *The Oxford Handbook of Rationality*, Oxford University Press

S. LINDSTÖM AND W. RABINOWICZ, 1991, Epistemic entrenchment with incomparabilities and relational belief revision', in A. Fuhrman and M. Morreau eds., *The Logic of Theory Change*, Springer

J. C. MAXWELL, 1860, Illustrations of the Dynamical Theory of Gases, *Philosophical Magazine*, **19** : 19-32, and **20** : 21-37, repr. in W.D. Niven (ed.) *The Scientific Papers of James Clerk Maxwell* (1890 /1961), vol. I, 377-409 , New York, Dover

J. C. MAXWELL, 1867, On the Dynamical Theory of Gases, *Philosophical Transactions of the Royal Society of London*, **157** : 49-88, rep. in W.D. Niven (ed.) *The Scientific Papers of James Clerk Maxwell* (1890 /1961), vol. II, 26-78 , New York, Dover

J. C. MAXWELL, 1873, Molecules, *Nature*, 8, 437-441

J. C. MAXWELL, 1877, Review of Watson, *A treatise on the kinetic theory of gases*, *Nature*, 20, 242-246.

J. POLLOCK, 1979, A plethora of epistemological theories, in G. Pappas ed. *Justification and Knowledge*, Reidel, 93-114

S. SAUNDERS, 2005, Complementarity and scientific rationality, *Foundations of Physics*, 35, 347-72

W. SPOHN, 1987, Ordinal conditional functions: A dynamic theory of epistemic states, in Harper and Skyrms (eds.), *Causation in Decision, Belief Change and Statistics*, Volume 2, Dorecht, Reidel, 105-134