Can science advance effectively through philosophical criticism and reflection?

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ABSTRACT

Prompted by Hasok Chang's conception of the history and philosophy of science (HPS) as the continuation of science by other means, I examine the possibility of obtaining scientific knowledge through philosophical criticism and reflection, in the light of four historical cases, concerning (i) the role of absolute space in Newtonian dynamics, (ii) the purported contraction of rods and retardation of clocks in Special Relativity, (iii) the reality of the electromagnetic ether, and (iv) the so-called problem of time's arrow. In all four cases it is clear that a better understanding of such matters can be achieved —and has been achieved— through conceptual analysis. On the other hand, however, it would seem that this kind of advance has more to do with philosophical questions in science than with narrowly scientific questions. Hence, if HPS in effect continues the work of science *by other means*, it could well be doing it *for other ends* than those that working scientists ordinarily have in mind.

Keywords: Absolute space – Inertial frames – Length contraction – Clock retardation – Special relativity – Ether – Time – Time directedness – Entropy – Boltzmann entropy

> The mind will not readily give up the attempt to apprehend the exact formal character of the latent connexions between different physical agencies: and the history of discovery may be held perhaps to supply the strongest reason for estimating effort towards clearness of thought as of not less importance in its own sphere than exploration of phenomena.
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> Joseph Larmor (1900), p. ix.

Hasok Chang's book *Inventing Temperature* (2004) offers in its last chapter a view of the history and philosophy of science (HPS) as "the continuation of science by other means". The need for supplementing normal scientific research in this fashion was impressed on Chang by his personal experience as a philosophical historian of science, which he describes as "a curious combination of delight and frustration, of enthusiasm and skepticism, about science". His delight in the beauty of conceptual systems and the masterfulness of experimental setups is mixed with "frustration and anger at the neglect and suppression of alternative conceptual schemes, at the interminable calculations in which the meanings of basic terms are never made clear, and at the necessity of accepting and trusting laboratory instruments whose mechanisms" he does not understand (p. 236). Chang claims that HPS can actually *generate* scientific knowledge in at least two ways. On the one hand, through the recovery of forgotten scientific knowledge, HPS can reopen neglected paths of inquiry. On the other hand, by applying the philosopher's scalpel to the thick and often opaque tissue of scientific discourse, HPS can positively contribute to clarifying or eliminating the confused, ambiguous or downright inept notions that bedevil innovative scientific thinking and appear to blunt its cutting edge.

Encouraged by Chang's exposition, I propose to examine here the possibility of obtaining scientific knowledge through philosophical criticism and reflection, in the light of four historical cases. The examples I have chosen concern

(i) the role of absolute space in Newtonian dynamics;
(ii) the purported contraction of rods and retardation of clocks in Special Relativity (SR);
(iii) the reality of the electromagnetic ether, and

(iv) the problem (or problems) that A.S. Eddington labeled with the catch phrase “the arrow of time”.

The first and the third question are closed. Question (i) is clearly a matter of conceptual criticism and I take it up chiefly to show how this worked in a case about which there is now general agreement. The advent of Relativity a century ago settled question (iii) not through conceptual criticism but rather by Einstein’s *fiat*. However, by showing that the assumption of an ether was not necessary for solving the scientific problems that were deemed to require it, Einstein signaled the presence of a conceptual confusion or mistake at the root of that assumption. Could such a misconception have been overcome earlier by professional philosophers? My answer will be: Yes in principle, but hardly so in practice, for the ether hypothesis was favored by powerful cultural forces.

When I began writing this paper I believed question (ii) was also closed, and I chose to deal with it as a positive illustration of the utility of HPS. When I first saw Harvey Brown’s new book (2005) I got the impression that he professed to have a different view of this question. The current significance of the issue itself was pleasantly enhanced in my eyes by this apparent discrepancy. However, after reading Brown’s book I made no changes in § 2 because, although I agree with almost all the many points he makes in it, I could not see that talk of ‘length contraction’ and ‘time dilation’ makes them more perspicuous.[1]

Question (iv) is still the subject of impassioned arguments and few would pronounce it closed. However, in the last few years, judicious HPS work by Sklar, Uffink, Earman and others has greatly clarified its terms and exhibited the true reach of the concepts and propositions at play. This allows one to hope that the conundrum will, at long last, be solved, or dissolved. It is not easy to clear a field charged with so many emotions and *weltanschauliche* commitments, but a few

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[1] Let me give just one example. Brown (2005), p. 25, says that, rather than conceiving relativistic spacetime geometry as a description of “the shape” of some thing-in-itself, “it is simply more natural and economical —better philosophy, in short— to consider absolute space-time structure as a codification of certain key aspects of the behaviour of particles (and/or fields).” I fully agree. However, once this codification is adopted, it is not longer fitting to say, e.g., that the negative result of Michelson and Morley’s experiment is due to the length contraction suffered by one of the arms of their interferometer. The status of this idiom in the special theory of relativity is similar to that of ‘sunrise’ and ‘sunset’ in Newtonian celestial mechanics.

philosophical considerations might perhaps help to disentangle one or two threads of the knot.

1. Absolute space in Newtonian dynamics

In the Scholium to the Definitions placed at the head of his *Principia*, Newton explains:

> Absolute space, of its own nature without reference to anything external, always remains homogeneous and immovable. Relative space is any movable measure or dimension of this absolute space; such a measure or dimension is determined by our senses from the situation of the space with respect to bodies and is popularly used for immovable space, as in the case of space under the earth or in the air or in the heavens, where the dimension is determined from the situation of the space with respect to the earth. Absolute and relative space are the same in species and in magnitude, but they do not always remain the same numerically. For example, if the earth moves, the space of our air, which in a relative sense and with respect to the earth always remains the same, will now be one part of the absolute space into which the air passes, now another part of it, and thus will be changing continually in an absolute sense.
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> (Newton 1687, p. 5; 1999, pp. 408s.)

Further on, he adds: “Absolute motion is the change of position of a body from one absolute place to another; relative motion is change of position from one relative place to another” (Newton 1687, p. 6; 1999, pp. 409). However, since such absolute places cannot be perceived and distinguished by our senses, “we define all places on the basis of the positions and distances of things from some body that we regard as immovable, and then we reckon all motion with respect to these places, insofar as we conceive of bodies as being changed in position with respect to them” (Newton 1687, p. 7; 1999, pp. 410s.). According to Newton, to use relative places and motions instead of absolute ones is all right when conducting ordinary human business (*in rebus humanis*), but would be inappropriate in science (*in philosophicis*). However, absolute and relative motions can be recognized by their respective causes and effects.

> The causes that distinguish true motions from relative motions are the forces impressed upon bodies to generate motion. True motion is neither generated nor changed except by forces impressed upon the moving body itself, but relative motion can be generated and changed without the impression of forces upon this body. [...] The effects distinguishing absolute motion from relative motion are the forces of receding from the axis of circular motion. For in purely relative circular motion these forces are null, while in true and absolute circular motion they are larger or smaller in proportion to the quantity of motion.
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> (Newton 1687, p. 9; 1999, p. 412)

This passage is followed by the description of the famous bucket experiment, which I presume is known to the reader.

Despite Newton's somewhat peremptory assertions, the causes and effects he mentions and the ingenious experiment he designed to exhibit them cannot distinguish true motion, as defined by him, from motion relative to a rigid body that travels with unchanging speed and direction, without rotation, in absolute space. This follows at once from Corollary 5 to Newton's Laws of Motion:

> *When bodies are enclosed in a given space, their motions in relation to one another are the same whether the space is at rest or whether it is moving uniformly straight forward without circular motion.*
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> (Newton 1687, p. 9; 1999, p. 423)

In particular, Newton's bucket will behave in exactly the same way whether its axis of rotation is at rest in absolute space or in a laboratory that moves "uniformly straight forward without circular motion". The upshot of this is that the notions of absolute space and absolute motion play absolutely no role in Newtonian dynamics. Therefore, the philosophical discussions provoked by the presence of these notions in the *Principia* —which began already in Newton's lifetime in the celebrated correspondence between Leibniz and Clarke— were wide off the mark and quite unnecessary. Newtonian dynamics makes an essential distinction between bodies acted upon by a force and bodies on which no net external force is acting. However, the latter class of bodies —and the reference frames based on them (the so-called *inertial frames*)— may move past each other with constant relative velocities of every conceivable magnitude and direction. Newton's theory makes no allowance for any *empirical or rational* way of telling which of them is truly at rest.

Therefore, for these bodies —and frames— the distinction between true and absolute rest and motion is vacuous.

As I have shown, a careful reading of the first few pages of Newton's book should have been sufficient to see this. It is merely a matter of understanding the concepts at play and the logical relations among them. Nevertheless, it does not appear that any physicists or philosophers became aware of the perfect idleness of Newton's absolute space before the advent and dissemination of Einstein's theory of relativity in the 20th century. Mach and other 19th century critics of Newton argued that physics can do without postulating an absolute space, but it does not seem to me that they ever dared to suggest that this postulate does not perform any job *in Newton's theory*. Such a lasting state of intellectual blindness was probably due above all to the resilience of the aboriginal —one is inclined to say instinctive— human ideas of motion and rest, which long stood in the way of the acceptance of Copernicanism and which Newton, with his talk about true motions, somehow sought to restore in a Copernican setting. It was —and still is— hard for humans to accept that the ground under their feet is in motion, unless they are accorded an ultimate standard of rest to which such motion can be referred. I suspect, however, that an explicit dismissal of absolute space by 18th and 19th century Newtonians was also prevented by their lack of an alternative mathematical structure to put in its place.

Let me try to explain what I mean. Newton said that space was neither a substance nor an attribute of substances, but had its own peculiar manner of being.[2] By virtue of it, "the parts of space are individuated by their positions, so that if any two could exchange their positions, they would also exchange their identities, and would be converted into each other qua individuals". Hence, "it is only through their reciprocal order and positions [*propter solum ordinem et positiones inter se*] that the parts of […] space are understood to be the very ones that they truly are; and they do not have any other principle of individuation besides this order and position" (Hall and Hall 1962, p. 103). In these words, Newton characterized not only the peculiar mode of being of space, but also an unprecedented ontological category, that takes pride of place in modern theoretical physics: the self-subsisting

2 Newton, *De gravitatione*, in Hall and Hall (1962), p. 99.

relational system, commonly known as a *mathematical structure*.[3] The structure that Newton identifies here with physical space is of course the so-called Euclidean space,[4] homeomorphic with $\mathbb{R}^3$. Now, each relative space determined by a rigid body (as described in my quotation from Newton 1687, p. 5) is an instance of this structure, but Newton would not countenance the real existence of more than one. However, as I have noted, he had no theoretical or experimental means of pinpointing the one absolute space among —or besides— the multiplicity of relative ones. But now, in the wake of Minkowski and Cartan, we can seize the whole thicket of relative spaces without sacrificing our yearning for oneness. More precisely, we can retain the whole uncountable set of Euclidean spaces determined by the inertial frames, conceive a single mathematical structure embracing them all, and identify this one structure with the physical habitat of bodies.

There are several ways of doing this, but for illustration one will be enough.[5] Let $\mathcal{W}$ be a parallelizable differentiable manifold, homeomorphic with $\mathbb{R}^4$, and endowed with a flat torsionless linear connection ∇. Define a foliation f indexed by Newtonian absolute time t, which partitions $\mathcal{W}$ into flat hypersurfaces homeomorphic with $\mathbb{R}^3$. Define a Euclidean metric μ on hypersurface $f(0)$. Call a curve in $\mathcal{W}$ *timelike* if it meets each leaf of the foliation once and once only. Consider the set $\mathcal{G}$ of all the timelike geodesics of $\mathcal{W}$, parametrized by Newtonian time t. Let γ and η belong to $\mathcal{G}$. If γ and η are parallel, we write $\gamma \parallel \eta$. Clearly, the

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3 The Bourbaki group defined 'species of structure' precisely, in set-theoretical terms. I would propose that philosophers use the term more loosely, to make allowance for progress in mathematics, such as is being made by category theorists. Still I see nothing wrong in using Bourbaki's definition as a guide to understanding, provided that one bears in mind —and discounts— the stiltedness of the set-theoretic approach.

4 It would be more proper to say *Cartesian space*, rather than *Euclidean space*. Why? Because Euclid's space only includes points that can be constructed with ruler and compass, and therefore lacks the continuity properties required for analysis.

5 The mathematical terminology used in this paragraph is standard and is explained in many textbooks of differential geometry or general relativity; also, cursorily, in the Mathematical Appendix to Torretti (1983). However, for the philosophical purposes of this paper the readers need not understand it. It is sufficient that they accept on faith my claim that a sophisticated mathematical conception can do the job for which Newton's absolute space was designed but turned out to be inappropriate.

relation $||$ is an equivalence (i.e. a symmetric, reflexive and transitive binary relation) on $\mathcal{G}$. Each element F of the quotient set $\mathcal{G}/||$ is a congruence of timelike geodesics (i.e. a set of timelike geodesics in $\mathcal{W}$ such that every point of $\mathcal{W}$ lies on one and only one geodesic of the set). The metric μ on $f(0)$ can be readily induced on each $\mathsf{F} \in \mathcal{G}/||$ by postulating that the distance between any two geodesics γ and η of F equals the μ-distance between the two points at which γ and η meet $f(0)$. Thus enriched, each $\mathsf{F} \in \mathcal{G}/||$ can stand for the relative space associated with an inertial frame in Newton's world. The structure $\langle\mathcal{W},\nabla,f,\mu,\mathcal{G}/||\rangle$ can then take in Newton's theory the foundational role that Newton assigned to absolute space, but which, by dint of his Laws of Motion, the latter could not play.

Two questions come to one's mind in connection with Chang's program:

(1) Is it at all likely that, before the development of these mathematical concepts in the 20th century, a solution of this sort could have been found in an HPS department, if such departments had been already founded —and funded— before 1900? I have a hunch that it is not, and that it was due to the lack of mathematical resources that a man like Ludwig Lange, who thoroughly understood the relativity of motion and the inanity of Newton's absolute space, did not seek an alternative geometrical structure to stand in its stead, but proposed his pseudo-operationalistic construction of a prototype inertial frame.[6]

(2) Did the delay in substituting a more suitable structure for Newton's absolute space hinder the progress of classical physics in any way? Again, I am tempted to say no. Philosophers and philosophically minded physicists repeatedly referred to absolute space as a bugaboo —"ein Unding", said Kant— and probably regarded it as a scientific scandal; but I do not see that any astronomical discovery or any application of mechanics to engineering was postponed because physicists paid lip service to Newton's phantasy. Perhaps the labors lost in figuring out the physics of ether could have been spared if the relativism inherent in Newtonian dynamics had sooner become explicit. (However, the demand for a material carrier of light waves was surely rooted on a cruder and sturdier metaphysical craving than Newton's promotion of a single pure relational system to be the playground of nature.) One may also suggest that conceptual criticism of absolute time (Einstein 1905, §1) would have been prompted at an earlier date if the useless idea of absolute space

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6 See Ludwig Lange (1885), James Thomson (1884). I explained this matter in Torretti (1983), pp. 17ff. See also DiSalle (1988).

had been dropped sooner. In Newtonian physics absolute time is certainly not an idle notion and local clocks can be regulated by it (e.g. due to the conservation of angular momentum, a rotating rigid top, isolated from external influences, will accurately keep such time). The difficulty lies in the global propagation of local time (the effective definition of the foliation f in the mathematical construction sketched above). According to Newton, "we understand any moment of duration to be diffused throughout all spaces, after its own way [*unumquodque durationis momentum…per universa spatia, suo more…diffundi intelligimus*]" (Hall and Hall 1962, p. 104). However, if the reference to *universa spatia* turns out to be equivocal, for there is more than just one universal space to which they may belong, then Newton's claim is not merely unwarranted —as James Thomson (1884, p. 569) perceived— but it is actually vacuous, and one may well expect that a suitable propagation of local time throughout space will yield a different global time depending on the inertial frame to which the space is anchored.

2. Rod contraction and clock retardation in Special Relativity.

In Newtonian mechanics, global time coordinate functions must agree up to a translation (choice of the zero of time) and a rescaling (choice of the time unit). As a result of this, the world can be partitioned into equivalence classes of simultaneous events in one way only. This property is represented by the uniqueness of the foliation $f\colon \mathbb{R} \to \mathcal{W}$ in the mathematical construction of §1. Einstein assumed that such uniqueness need not hold, so that the same events can be differently distributed among simultaneity classes, depending on the inertial frame with which the relevant time coordinate function is associated. By this bold and decisive move he was able to embrace together two physical principles that were incompatible under the Newtonian dispensation, viz., the Principle of Relativity, by which no physical experiment can tell apart a particular inertial frame as a true standard of rest, and the Principle of the Constancy of the Velocity of Light, by which light propagates in vacuum with the same constant speed relative to all inertial frames, regardless of the velocity of its source. The joint assertion of these two principles entails a drastic change in the basic rules for describing physical phenomena. For simplicity's sake I assume that durations are measured in seconds (s) —as usual— and lengths in light-seconds (Ls), where 1 Ls is the distance traveled in 1 s by a light signal *in vacuo* (in these units, the speed of light $c = 1$ Ls/s).[7] Let F and F′ be two inertial frames moving past each other with speed v Ls/s. Let x, y, z and x', y', z' be Cartesian coordinates defined on the spaces associated with F and F′, respectively, and let time coordinates t and t' be defined, respectively, in the primed and in the unprimed frame by the method explained by Einstein (1905, §1). Without loss of generality we can assume that (i) both the primed and the unprimed systems assign coordinates (0,0,0,0) to the same event; (ii) at time $t = t' = 0$, the homonymous axes of both systems are aligned with each other and coordinate values increase along them in the same sense; and (iii) the constant velocity of frame F′ relative to F has components (v,0,0). Einstein showed that, under these assumptions, his two principles imply that the coordinate systems (x,y,z,t) and (x',y',z',t') are linked by the following equations:[8]

$$\begin{aligned} x' &= \frac{x - vt}{\sqrt{1 - v^2}} \\ y' &= y \\ z' &= z \\ t' &= \frac{t - vx}{\sqrt{1 - v^2}} \end{aligned} \tag{1}$$

Suppose that the two endpoints of a measuring rod ρ′ at rest in F′ have spatial coordinates (x_1', y_1', z_1') and (x_2', y_1', z_1'). The spatial coordinates of ρ′ in F at time $t = t' = 0$ are then:

$$\begin{aligned} x_1 &= x_1'\sqrt{1 - v^2} & \qquad x_2 &= x_2'\sqrt{1 - v^2} \\ y_1 &= y_1' & y_2 &= y_2' \\ z_1 &= z_1' & z_2 &= z_2' \end{aligned} \tag{2}$$

Suppose that ρ′ is a standard meter rod. Then, obviously, the distance $|x_1' - x_2'|$ between its endpoints equals 1 m = 299,792,458 Ls in the frame F′ on which it

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7 By an international agreement in force since 1983, 1 meter equals 299,792,458 light-seconds, exactly.

8 This is the simplest case of a *Lorentz transformation* between coordinate systems adapted to inertial frames that move relatively to each another. They are thus called because H.A. Lorentz (1899, 1904) introduced this coordinate transformation in a context akin to Einstein’s (though with a conceptually altogether different outlook).

rests. Hence, by the transformation equations (1), the distance between the endpoints of ρ′ in the frame F relative to which it is moving with longitudinal speed v is given by

$$|x_1 - x_2| = |x_1' - x_2'|\sqrt{1 - v^2} \qquad (3)$$

and is therefore shorter than one meter by a factor of $\sqrt{1 - v^2}$.

This phenomenon was described as relativistic *rod contraction* in the scientific literature of the 1900's because it was readily confused with the physical contraction of rigid rods by *exactly* the same factor postulated (independently) about 1890 by G.F. FitzGerald and H.A. Lorentz to explain the negative result of Michelson and Morley's attempt to measure the velocity of the Earth in the ether. Lorentz and FitzGerald regarded the contraction named after them as a real change in the rod. They believed that the forces that keep the smallest parts of solid bodies in place are electromagnetic in nature, and expected those forces to be altered by a body's motion through the electromagnetic ether, then supposed to be the seat of the electric and the magnetic fields (see §3). The Lorentz-FitzGerald contraction was designed to ensure that one of the two perpendicular steel bars along which light travels in Michelson's interferometer is shortened by the precise amount required to conceal the motion of the Earth in the ether.[9] However, under Einstein's two principles, light travels on any inertial frame with the same speed in every direction, and Michelson and Morley's optical experiment —though accurate up to terms of the second order in *v*/*c*— cannot disclose the (almost) inertial motion of the Earth parallel to one of the interferometer's arms.

Relative motion will not make a solid body shorter in the way that, say, heat makes it larger. Indeed, if the motion of inertial frame F′ relative to inertial frame F effectively shortens the standard meter rod ρ′ that rests in F′ relatively to a meter rod ρ at rest in F, then we must also conclude that the motion of F relatively to F′ also shortens ρ relatively to ρ′ in exactly the same proportion. Some people relish such brainteasing descriptions, but others, more sensibly, perceive them as a misuse of language. Anyhow, a short reflection on the meaning of the terms that occur in

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[9] Michelson (1881), Michelson and Morley (1887). The experiment is described in many places, usually from a relativistic standpoint. To see what Michelson was actually up to I recommend reading his paper of 1881 (reproduced in Kilmister 1970, pp. 91-104).

eqn. (3) shows that in the present case there is no need for them. $|x'_1 - x'_2|$ denotes the distance between the endpoints of the meter rod ρ′ at rest in F′. $|x_1 - x_2|$ denotes the distance between two points of F each one of which coincides with an endpoint of ρ′. Now, as ρ′ moves along the *x*-axis of F, its endpoints coincide successively with indenumerably many different points of F, so, to know what we are speaking about we must impose one further condition on the two points in question. It goes without saying that the two points of F whose distance is denoted by $|x_1 - x_2|$ must coincide *simultaneously* with the two endpoints of ρ′. Choose any two points of F that meet this condition. I designate by E_1 and E_2 the two events at which the chosen points coincide with the respective endpoint of ρ′. Simultaneity in F agrees with the time coordinate *t* defined on F by Einstein's method. Under the conditions prescribed for our example, $t(E_1) = t(E_2)$ implies that $t'(E_1) \neq t'(E_2)$, so that E_1 and E_2 are not simultaneous on F′. Therefore, it is no wonder that

$$|x'_1 - x'_2| \neq |x_1 - x_2| \tag{4}$$

for —contrary to what one still reads in semipopular literature— each side of this inequality refers to a different physical quantity.[10] While the l.h.s. denotes the length of rod ρ′, that is, the distance between the two points on its rest frame F′ with which the endpoints of ρ′ invariably coincide, the r.h.s. denotes the distance between two points on the frame F which happen to coincide, respectively, with the two endpoints of ρ′, at some instant of time *t* (defined by Einstein's method on

[10] Already in his first paper on relativity, Einstein explained this with unsurpassable clarity. When one asks about the length of a moving rod one can think it as being ascertained by one or the other of the two following operations: "(*a*) The observer moves together with the given measuring-rod and the rod to be measured, and measures the length of the rod directly by superposing the measuring-rod, in just the same way as if all three were at rest. (*b*) By means of clocks at rest set up in the stationary system and synchronized in accordance with §1 [i.e. by Einstein's method of synchronizing clocks with light-signals], the observer ascertains at what points of the stationary system the two ends of the rod to be measured are located at a definite time. The distance between these two points, measured by the measuring-rod already employed, which in this case is at rest, is also a length which may be named as 'the length of the rod'." (1905, §2, pp. 895-896; Kilmister's translation, slightly retouched). One surmises that the writers who created the confusion on this subject never read thus far into Einstein's paper.

F), as ρ' moves along with constant velocity components (v,0,0). Since we assumed that ρ' is a meter rod, $|x'_1 - x'_2|$ equals the distance a light signal travels on F′ in 299,792,458 seconds. Therefore, by eqn. (3), the distance $|x_1 - x_2|$ between two simultaneous positions of the endpoints of ρ' on F is shorter by a factor of $\sqrt{1-v^2}$ than the distance a light signal travels in 299,792,458 seconds on F (which, by Einstein's two principles, also equals one meter). When the meaning of eqn. (3) is elucidated in this way, it appears like a rather unsurprising consequence of the relativity of simultaneity, which is neither paradoxical, nor incomprehensible, nor the effect of a physical force. The equation does not describe the contraction of a rod, but rather conveys the proportionality between (*a*) the distance that separates two points located in the space of a particular inertial frame, and (*b*) the distance that separates *another* two points located in the space of a different frame.

The Lorentz transformation eqns. (1) also entail the phenomenon known as special relativistic *clock retardation*. Again, this term has caused much puzzling and should be regarded as a misnomer; though it is a rather natural one, for how else would an air traffic controller, say, be supposed to describe the state of the atomic clocks that Hafele and Keating (1972) carried around the world on jetliners, when she compared them with the similar clocks that staid back at the airport?[11] The airport clocks were good atomic clocks, synchronized with the best in the world; the flying clocks had been carefully synchronized with them on departure, but were no longer synchronized with them on arrival: the arriving clocks showed a —very slightly— earlier time; in other words, they ran slower. Is not this precisely what we call *clock retardation* in English? Readers of this journal will at once remark that this ordinary manner of speaking presupposes a unique standard of universal time-reckoning, and that SR did away with that (or rather, showed that it had never

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11 Hafele and Keating's experiment was meant to test a prediction of General Relativity (GR), by taking atomic clocks to an altitude where the gravitational potential is significantly different from that on the surface of the Earth. However, in their calculations they also had to make allowance for the speed of the jet plane on which the clocks traveled. To separate the phenomenon predicted by SR from the gravitational GR effect, some of the clocks were flown around the world from West to East, the others from East to West. Upon their arrival, comparison of the two sets of traveling clocks among themselves and with those that remained on Earth nicely confirmed the combined predictions of both theories.

been in place). Still, to keep balance with the preceding discussion of rod contraction, I shall once more go through the derivation and elucidation of the relativistic phenomenon.[12]

Consider a standard clock κ'_0 affixed to point (0,0,0) of our frame F', which shows time $t' = 0$ as it passes by point (0,0,0) of F. By our earlier assumptions, the clock κ_0 affixed to this point of F should, at that instant, show time $t = 0$, and thus be synchronized with the clock κ'_0. After u seconds, κ'_0 will show time $t' = u$ and will be passing by the point $(vt,0,0)$ of F. Denote this event by E. Eqns. (1) enable us to calculate the time t(E), shown by the clock κ_{vt} affixed to this point, as κ'_0 passes by it. We have that

$$u = t'(\mathrm{E}) = \frac{t(\mathrm{E}) - vx(\mathrm{E})}{\sqrt{1-v^2}} = \frac{t(\mathrm{E}) - v^2 t(\mathrm{E})}{\sqrt{1-v^2}} = \frac{t(\mathrm{E})(1-v^2)}{\sqrt{1-v^2}} \tag{5}$$

Hence,

$$t(\mathrm{E}) = \frac{u}{\sqrt{1-v^2}} > u \tag{6}$$

Thus, the clock κ_{vt} shows a later time than κ'_0 and, if judged by it, κ'_0 must be said to be running slow. However, the observers at rest on F', who take κ'_0 as their standard of time, cannot infer from eqn. (5) that κ_{vt} is running fast, for κ'_0 has not been synchronized with κ_{vt}, but only with the clock κ_0 affixed to point (0,0,0) of F. At time $t' = u$, κ_0 travels with velocity $(-v,0,0)$ past the point $(-vu,0,0)$ of F'. Denote this event by E* and let κ'_{-vu} be a clock affixed to this point and synchronized by Einstein's method with κ'_0. As E* occurs, κ'_{-vu} displays time $t'(\mathrm{E}^*) = u$. The time $t(\mathrm{E}^*)$ displayed by κ_0 can be readily figured out from eqns. (1) after exchanging primed and unprimed coordinates and substituting $-v$ for v. We have then that $x(\mathrm{E}^*) = (x'(\mathrm{E}^*) + vu)\left|(1-v^2)^{-1/2}\right| = 0$ (as expected, for it was assumed that E* takes place at point $(-vu,0,0)$ of F'), and that

$$t(\mathrm{E}^*) = \frac{t'(\mathrm{E}^*) + vx'(\mathrm{E}^*)}{\sqrt{1-v^2}} = \frac{u - v^2 u}{\sqrt{1-v^2}} = \frac{u(1-v^2)}{\sqrt{1-v^2}} = u\sqrt{1-v^2} < u \tag{7}$$

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12 I have already done so in Torretti 1983, pp. 68-69, and again more succinctly in Torretti 1999*b*, pp. 73-75.

Therefore, judged by κ'_{-vu}—and by κ'_0, which runs in unison with it on F′—, it is κ_0 that runs slow. The same evidently holds for all clocks at rest on F and synchronized with κ_0 by Einstein's method, including of course κ_{vt}. Talk of "clock retardation" makes this sound paradoxical, for indeed, how can the same two clocks, viz., κ'_0 and κ_{vt},, at the same time run slower and faster than each other? There is no doubt that they cannot, but there is no question here of this happening *at the same time*: κ'_0 runs slower that the clocks on frame F when judged according to time t defined on F by Einstein's method, and κ_0 runs slower than the clocks on frame F′ when judged according to time t' defined by Einstein's method on F′. Note, in particular, that the event E*, which is, by hypothesis, simultaneous with E on F′, cannot be simultaneous with E on F. Therefore, the following inequalities, jointly implied by (6) and (7), should not come as a surprise:

$$t(\mathrm{E}^*) < t'(\mathrm{E}^*) = u = t'(\mathrm{E}) < t(\mathrm{E}) \tag{8}$$

The debate set off by (8) takes a picturesque turn if we consider the case of two twins, one of which rests on inertial frame F while the other one travels at a very high speed v, to and from a nearby star. If the latter twin travels practically all that time at constant speed $v = 0.9$ Ls/s while the former goes through 80 birthdays, then the traveling twin will enjoy less than 35 birthdays during the trip. If, as we may assume for simplicity's sake, they both remain in good health, their biological clocks will have been running approximately like atomic clocks and the difference in age will be manifest. Several confused and confusing arguments were prompted by this example. Some pointed out that, due to the relativity of motion, the twins were exchangeable, so that the latter aged to 80 while the former became 35. For them, this example furnished the ultimate *reductio ad absurdum* of Relativity.[13] Clearly, they disregarded the obvious asymmetry of the situation, in which one twin rests permanently on a single inertial frame, while the other must rest on two (at least), one in the outward voyage, another one while returning. Others claimed that, because changing frames requires accelerated motion, the example fell outside the scope of SR. This is, of course, quite false, for accelerated motion is difficult to handle but by no means intractable in this theory. Indeed, one ought to wonder

[13] The British scientist Sir Herbert Dingle was probably the most tenacious upholder of this standpoint; the story of his long fight for it is told with considerable sympathy by Hasok Chang (1993).

what could be the use of a physical theory in which there is no room for accelerated motion! Anyway, after Hafele and Keating successfully carried out a realistic version of the twin experiment (see footnote 11), one can in good conscience deal with it ideally and assume that the traveling twin, upon reaching the nearby star, jumps instantaneously from a frame that moves with velocity components (0.9,0,0) relative to a suitable Cartesian coordinate system adapted to F, to another frame that moves with velocity components (–0.9,0,0) relative to the same system.[14] In this case, the traveling twin's speed is equal to 0.9 *all of the time*.

Summing up: Clarity of mind suffices to rid oneself of the spooks of rod contraction and clock retardation. However, clarity of mind often is in short supply among educated people. In the case in point it was achieved only through long debates and intense reflection. Undoubtedly a physicist can apply SR in his work and even design experiments for testing it while he remains attached to the wrongheaded reading of equations (3) and (5) that I have discussed above. Still, I see a significant difference between such a physicist and the Newtonians who paid obeisance to absolute space. Whereas the latter inserted this idle notion in their scientific discourse as a purely verbal ornament, which could neither add nor subtract anything from the way they actually dealt with physical problems, the former adopts and disseminates a misconception of real physical phenomena.[15] Although such misconceptions need not stop progress within the bounds of a particular research, overcoming them surely contributes to the overall advancement of science. Thus, the examples of conceptual criticism presented in this section, though modest, corroborate the utility of Chang's program.

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[14] For a calculation based on this assumption and using the foregoing data, see Torretti 1999*a*, pp. 277-280.

[15] Prof. Miguel Espinoza (private communication) has prompted me to explain what I mean by a 'real physical phenomenon'. Rather than try my own hand at it, I seize on this opportunity to quote C. S. Peirce: "When an experimentalist speaks of a *phenomenon* […] he does not mean any particular event that did happen to somebody in the dead past, but what *surely will* happen to everybody in the living future who shall fulfill certain conditions. The phenomenon consists in the fact that when an experimentalist shall come to *act* according to a certain scheme that he has in mind, then will something else happen, and shatter the doubts of sceptics, like the celestial fire upon the altar of Elijah" (1931-1958, vol. 5, §425).

3. The ether hypothesis in pre-relativistic electrodynamics.[16]

> Die Einführung des Namens 'Äther' in die elektrischen Theorien hat zur Vorstellung eines Mediums geführt, von dessen Bewegung man sprechen könne, ohne daß man wie ich glaube, mit dieser Aussage einen physikalischen Sinn verbinden kann.
>
> Einstein to Marić, August 1899.[17]

In the introduction to the famous paper, "Zur Elektrodynamik bewegter Körper", in which Einstein laid down the foundations of SR, he wrote: "The introduction of a 'light-ether' will prove to be superfluous".[18] This statement reminds me of the little child's cry "But he's got nothing on!" in Andersen's tale about the emperor new clothes. Just as the false tailors disappear from the tale as soon as the child has spoken, so ether, which for centuries had filled the universe (in the dreams of Reason), vanished like an exorcised ghost after that short remark by the junior patent official in Bern. Well, I admit there is some exaggeration in this statement of mine, for Relativity was stiffly resisted at first by numerous scientists and philosophers. Still, it does not appear that they thought Relativity was false *because* it denied the ether.[19] By contrast, *ca.* 1905 a new physical theory that denied

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16 Nersessian (1984) is a book-length study of this subject, conducted from the philosophically significant standpoint of concept formation.

17 "The introduction of the noun 'ether' in the theories of electricity has led to the idea of a medium, of whose motion one could speak, without, I believe, being able to attach a physical meaning to one's utterance" (Einstein 1987- , 1: 226). I owe this striking quotation to John Stachel (2002), p. 171, whose English translation I reproduce with minor changes.

18 "Die Einführung eines 'Lichtäthers' wird sich insofern als überflüssig erweisen, als nach der zu entwickelnden Auffassung weder ein mit besonderen Eigenschaften ausgestatteter 'absolut ruhender Raum' eingeführt, noch einem Punkte des leeren Raums, in welchem elektromagnetische Prozesse stattfinden, ein Geschwindigkeitsvektor zugeordnet wird" (Einstein 1905, p. 277). Kilmister translates 'Lichtäther' as 'luminiferous ether'. I have preferred the more literal translation of the same expression by D.E. Jones in his authorized translation of Hertz (1893), p. 24.

19 At least I am not aware of anyone having raised this objection. H.A. Lorentz insisted to the end of his life that his own ether-based electrodynamics of moving bodies was,

radioactivity would have been laughed out of court. Surely, then, the existence of the light-ether was not supported by any hard facts. Indeed, it would seem that there was not much to be said for it, and that, therefore, a team of philosophers of science could have promptly got rid of it through conceptual criticism. This would have spared 19th-century science many fruitless efforts.

However, even a superficial glance at the history of ideas makes it doubtful that mere philosophers could have succeeded in suppressing the ether. For, as G.N. Cantor (1981) showed, the existence of one or more ethers was regarded by many scientists as a material aid to their Christian or spiritualistic beliefs, and we know all-too-well, after 350 years of Enlightenment, that ideas benefiting from this connection are dreadfully resilient and impervious to criticism. The religious associations of ether —or *æther*— are implicit in its name, a transcription of the Greek word αἴθηρ, which to Homer meant 'heaven' (*Iliad*, 16.365), and which Aristotle adopted as the name of the fifth element, the changeless stuff that the heavens are made of.

After Tycho, tracking the comet of 1572, showed that the planets are not affixed to impenetrable spheres, the word 'ether' became redundant and was ready to acquire a new meaning. The conception of ether as a fluid, transparent, extremely subtle form of matter that thoroughly fills the vast interstellar and interplanetary spaces is usually attributed to Descartes.[20] In the *Principles of Philosophy*, Descartes consistently refers to this form of matter as 'the second

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in its mature version of 1904, just as good as Einstein's, with which it is empirically equivalent (both yield *exactly* the same predictions); but he never claimed that the latter was disqualified by its denial of ether. On the other hand, I learnt recently that J.J. Thomson said as late as 1909 that ether is as essential to us as the air we breathe, and that the study of this substance is the most important task of physics! (quoted in French translation by Samueli and Boudenot 2005, p. 107.)

20 For instance, by Whittaker 1951/53, 1: 5: "Space is thus, in Descartes view, a *plenum*, being occupied by a medium which, though imperceptible to the senses, is capable of transmitting force, and exerting effects on material bodies immersed in it—the *aether*, as it is called". A careful reader will notice that Whittaker does not say that Descartes himself used this word to call it.

element' (1644, III, §§ 52, 70, 82, 123; 1996, 8: 105, 121, 137, 172).[21] However, in the same year in which this book was published, Sir Kenelm Digby used 'aether' in English to designate the transparent interstellar matter (*Oxford English Dictionary*, s.v. **ether**, 5.a), and the term was later employed in this sense, as a matter of course, both in England and in the Continent.[22] According to Descartes, this form of matter is immensely more abundant than the two other kinds acknowledged by him: the opaque matter of the Earth and the other planets, and the radiant matter of the Sun and the stars.

The Cartesian ether, which lacked even the faintest trace of empirical evidence, was postulated on purely a priori grounds, namely, that the existence of an empty space is a contradiction in terms.[23] It was held to be the vehicle of light, either as a rigid solid that transmits it instantly (according to Descartes 1637, pp. 3-6; 1996, 6: 83-86), or as an elastic fluid through which light propagates with finite speed in the guise of longitudinal waves (according to Huygens, 1690). But the ether also played a central role in the explanations of planetary motion and free fall proposed by Descartes himself and other Cartesian physicists. These explanations rest on the assumption that any portion of ether in motion will act by impulse on other portions of it and on other kinds of matter. So the planets are carried around the Sun by ether whirlwinds, like logs driven by a river. On the other hand, terrestrial bodies are made heavy by the relative lightness of celestial matter, which, however, is not an irreducible quality, but a consequence of the greater speed with which this subtle kind of matter moves upward, through the pores of gross terrestrial bodies, leaving them behind: "This celestial matter has more force to move away from the centre

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21 In the early, posthumously published book *Le monde*, he called it 'air', but not without warning the reader that it must not be confused with "this gross air we breathe" (Descartes 1996, 10: 28).

22 See Newton's posthumous papers in Hall & Hall (1962), pp. 94, 112, 220. Also Huygens (1888-1950), 19: 472, 473, 573; 21: 454; cf. 19: 463; Leibniz (1965), 4: 469.

23 "If you wish to conceive that God takes away all the air in a room, without replacing it with another body, then by the same token you must conceive that the walls of this room come together, or else there will be a contradiction in your thought"—Descartes to Mersenne, 9 January 1639 (in Descartes 1996, 2: 482).

around which it turns than any of the parts of the Earth, which makes that it is light relatively to them" (Descartes 1644, IV, §22; 1996, 9-2: 211).[24]

In Newton's *Principia*, much of Book II was designed to prove that the phenomena of the Solar System cannot be accounted for by ether vortices (not, at any rate, under Newton's Laws of Motion). And Newton's well-known declaration that he does not feign hypotheses, coming as it does right after his admission that he has been unable to assign a cause to gravity, can surely be read as a gibe at the ether-based Cartesian theories. In Part III, §44, of his *Principia*, Descartes declared that he did not claim to have found the "genuine truth" concerning the physical questions he dealt with, and that what he would henceforth write about them should be understood "as an hypothesis" (the French translation adds: "that is perhaps very far from the truth"; as I pointed out in footnote 24, this is probably from Descartes own hand). Nevertheless, if every consequence inferred from such an hypothesis fully agrees with experience, "we shall gather from it no less utility for life than from the knowledge of truth itself" (1644, III, §44; 1996, vol. 8, p. 99).[25] In unwitting (or was it deliberate?) opposition to Descartes' words, Newton's First Rule of Philosophy prescribes that no causes of natural things should be admitted *unless they are true* (Newton 1726, p. 387; 1999, p. 794). For the purposes of human science, it is enough that a natural thing like gravity "really exists and acts according to the laws than [one has] set forth and is sufficient to explain all the motions" that one links to it.[26] "To tell us that every Species of Things is endow'd

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24 I quote from Picot's French translation, which is more explicit than the Latin. Many of the additions in Part III, §§ 41ff. and in Part IV, were probably written by Descartes himself (see Adam's *Avertissement* in Descartes 1996, vol. 9-2, pp. ix-xix). For a sympathetic exposition of Cartesian theories of gravity, see Aiton 1972, or the shorter presentation in Aiton 1989.

25 Again, the French translation explains: "…for one will be able to employ it in the same way to dispose natural causes to produce the effects one will desire"; see Descartes 1996, 9: 123

26 In the century following Newton, the finest physicists, such as d'Alembert or Fourier, insisted that they did not seek for causes but for laws. But the run-of-the-mill scientific practitioners obscurely thought of gravity as a force of attraction exercised by, say, the Sun as it were through a bodiless arm that stretches all the way from it to the Earth to grab it and pull it, while at the same time prompting the arm of the

with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phænomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd" (Newton 1721, p. 377).

Newton's methodological attitude did not favor the ether, which, being intangible and invisible, can only exist hypothetically. During the 18th century, as Newton's influence grew, the ether became increasingly discredited. By 1771 the enlightened founders of *Encyclopedia Britannica* thought it appropriate to explain 'ether' as "the name of an imaginary fluid, supposed by several authors [...] to be the cause [...] of every phenomenon in nature".[27] Not without irony, Joseph Priestley extolled the "fine scene" that ether afforded "for ingenious speculation":

> Here the imagination may have full play, in conceiving of the manner in which an invisible agent produces an almost infinite variety of visible effects. As the agent is invisible, every philosopher is at liberty to make it whatever he pleases, and ascribe to it such properties and powers as are most convenient for his purpose.
>
> (Priestley 1775, vol. 2, p. 16; quoted by Laudan 1981, p. 159)

In 1784, LeSage, author of the last great ether theory of gravity (which many decades later James Clerk Maxwell judged favorably), declared he would stop publishing about it: "Since your physicists are so prejudiced against the possibility of solidly establishing the existence of my imperceptible agents, which nevertheless are most suitable (*très-propres*) for making intelligible the attractions, affinities and extensibilities (*expansibilités*) which currently constitute all of physics, I shall

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Earth to stretch as well and to grab and pull the Sun in turn. I have met engineers who appear to think so to this day.

27 First edition (1771), 1: 31, *s.v.* **ether**; quoted by Laudan (1981), p. 170. The article **éther** in Diderot's *Encyclopédie* is only mildly less sarcastic: "L'*éther* ne tombant pas sous les sens & étant employé uniquement ou en faveur d'une hypothèse, ou pour expliquer quelques phénomenes reels ou imaginaires, les Physiciens se donnent la liberté de l'imaginer à leur fantaisie". (This article is signed by the British authors Harris and Chambers.)

suspend still for some time the publication of the works I prepared about these agents."[28]

Newton's undisputed authority presided also over Ampère's construction of electrodynamics in terms of attractive and repulsive forces acting instantaneously at a distance between centers of force (which, however, to save the phenomena, Ampère had to conceive, paradoxically, as oriented line elements—not points!). From then on, until the experimental discovery of electromagnetic waves by Hertz late in the 19th century, French and German electrodynamics remained wedded to action at a distance —instantaneous or deferred—, while "la physique anglaise"[29] persistently took the contrary view, namely, that electric and magnetic action properly has its seat, not in the ordinary massive bodies that display it to our eyes, but in the intangible, pervasive ether in which these bodies are embedded. The "English" (in fact, mainly Scottish and Irish) approach was inspired by the wish to produce a physico-mathematical theory that would embody the ideas of Michael Faraday, whose mind-boggling discoveries had revolutionized the science of electricity, but who, for lack of a formal education, had been unable to convey his views in precise mathematical form. Yet Faraday's views —as far as we, assisted by our hindsight, can judge them now— were much closer to Einstein's than the ether-based theories of Maxwell and his followers. At any rate, Faraday did not hide his dislike for the ether hypothesis and openly favored the attribution of physical existence to curves in space that he termed electric and magnetic *lines of force*, but which, in today's mathematical jargon, we would describe as the integral curves of the electric and the magnetic vector fields.[30]

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[28] Quoted in French by Laudan (1981, p. 166), from Prevost 1805, p. 242. My translation.

[29] Duhem (1914), 1[e] partie, Ch. IV, §§IV-IX. For Duhem, "English physics", as practiced by the Victorian electrodynamicians from Maxwell to Larmor, is a paradigm of broadmindedness (*amplitude d'esprit*), which is not always a virtue; according to Duhem, a man in whom this form of intelligence, "which Pascal calls broadness and weakness of mind, was developed to an almost monstrous degree" was the un-French emperor of the French, Napoleon I (1914, p. 81).

[30] Given a vector field V on a smooth manifold M, every smooth curve in M whose tangent vector at each point of its range is identical with the value of V at that point is an integral curve of V. Cf. Kobayashi and Nomizu (1963), 1: 12; Choquet-Bruhat *et al.* 1977, p. 141.

Faced with this curious combination of facts, a thoughtful student of history cannot help wondering what could motivate the persistent attachment of Victorian physicists to the electromagnetic ether. To me at least, now that a full century has elapsed since Einstein declared it "superfluous", the 19th-century revival of this pre-Newtonian fantasy appears as a step back, unworthy of such refined mathematicians as William Thomson (Lord Kelvin), James Clerk Maxwell, George Francis FitzGerald and Joseph Larmor. Why did they so unhesitatingly take it for granted that, if electric and magnetic action happens chiefly in the space between the observable bodies, then that space must be filled by a material substance of a special kind, in which all other substances are soaked but which remains impervious to chemical analysis? More on line with our present inquiry, we may well ask the following question: Could an energetic, efficient HPS department, working in Cambridge, England, *ca.* 1850, have prevented this seemingly wrongheaded move? Questions of counterfactual history should no doubt be addressed only in a sportive mood. Still, by toying with the purely speculative answers that such questions allow, it is sometimes possible to improve our grasp of the concepts we use.

To begin with, let us recall that the light-ether, which Einstein found superfluous, had been revived, together with the wave theory of light, by Thomas Young, some fifty years before Maxwell's earliest publications on electromagnetism.[31] Young's proposal was strongly resisted, especially by the French scientific establishment, in the name of the corpuscular conception of light, which was thought to be backed by the authority of Newton. However, as far as I know, nobody doubted that *if* light is transmitted by waves, *then* there must exist a vibrating body that fills interstellar space and also permeates air, water, glass, and generally all transparent bodies. It should be noted that the same French scientists that opposed the wave theory of light and, with it, the light-ether, had no reservations against other imponderable fluids, capable of permeating ordinary bodies, notably caloric, which they held responsible for heat, in opposition to the kinetic conception defended about the same time by Rumford (1798). They all embraced the ether hypothesis together with the wave theory when Poisson, wishing to embarrass Fresnel, inferred from the latter's wave optics that a bright spot would be seen at the center of the shadow of a small circular screen and this seemingly implausible prediction was fulfilled (Whittaker 1951/53, vol. 1, p. 108).

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[31] Young 1800, 1802a, 1802b, 1804. I owe these references to Buchwald 1981, p. 235.

Indeed, no less a mathematician than Cauchy took it upon himself to dream up the molecular structure of ether in various ways.[32] His constructions allowed him to infer the optical phenomena of dispersion and double refraction, but little else. In a spirit very close to that of our present inquiry, Jed Buchwald asks why it was "necessary for Cauchy to embrace the hypothesis of a molecular ether if (as was almost certainly the case) he was primarily interested in discovering and solving differential equations for light?" Buchwald's reply deserves attention:

> Without the hypothesis of a molecular ether there would, at the time, simply have been no route at all to the mathematics. For, although the ultimate aim for Cauchy was always a mathematical proposition from which calculable phenomena could be deduced, this aim could only be achieved throughout most of the 1830s by deductions founded on a molecular ether. Moreover, there was then little to object to, because the hypothesis fitted so well into contemporary physical ideas; that is, it utilised the widely accepted concepts of material points and central forces.
>
> (Buchwald 1981, pp. 223-24)

Similar considerations can probably account also for other contemporary work on ether dynamics, notably by James MacCullagh (1839).[33]

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32 "Within the space of ten years the great French mathematician produced two distinct theories of crystal-optics and three distinct theories of reflection, almost all yielding correct or nearly correct final formulae, and yet mostly irreconcilable with each other and involving incorrect boundary conditions and improbable relations between elastic constants" (Whittaker 1951/53, 1: 137). As Poincaré (1901*b*) remarked in connection with a later ether theory: "Les hypothèses, c'est le fonds qui manque le moins" ("There is no dearth of hypotheses"; quoted from Poincaré 1968, p. 182).

33 For further references, see Whittaker (1951/53), 1: 137n. MacCullagh's paper of 1839 is discussed in Darrigol (2005), pp. 237-239 (followed by an extract in French translation, pp. 241-248). See also Stein (1981), pp. 310-315. Darrigol (2000), pp. 190f, 334f., explains the importance of MacCullagh's ether for FitzGerald and Larmor. Although it differed drastically from any material ever considered in the received physics of elastic bodies, MacCullagh's medium agrees with the boundary conditions required by Fresnel's laws of reflection and refraction; its equations of motion follow from Hamilton's principle and, suitably interpreted, they yield the Maxwell equations (in the absence of sources).

About the same time, the great Cambridge philosopher and historian of science William Whewell moved away from Newton's Rules of Philosophy and expressed his willingness to countenance hypotheses in the "inductive sciences", provided that they meet certain conditions. To be acceptable, an hypothesis must not only (*a*) be "consistent with *all* the observed facts"; but (*b*) it ought to correctly foretell "phenomena which have not yet been observed" (Whewell 1847, 2: 62). "Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded" (ibid., vol. 2, p. 64). The corroboration of wave optics by Poisson's bright spot at the center of the circular screen's shadow admirably illustrates condition (*b*). Larry Laudan (1981, pp. 176ff.) claims therefore that Whewell's restoration of the hypothetico-deductive method was inspired in particular by the success of wave optics and the ether hypothesis supposedly inherent to it. If true, this claim entails a negative answer to my question about the possible role of a counterfactual Cambridge HPS department in exorcising the ether; for Whewell, Master of Trinity since 1841, surely would have exercised an enormous influence on the composition and orientation of such a department. Indeed he probably played the opposite role in real life, by instilling in Maxwell — who was a student and later a fellow at Trinity in the 1850s— an enduring loyalty to ether, which seems unnecessary to us, given the mathematical resources at Maxwell's command. However, I am not sure that Laudan's claim is warranted, for even if Whewell's attitude to hypotheses had something to do with the successful prediction of unsuspected phenomena by Fresnel's wave optics,[34] that would not yet say anything about the assumption of an ether, which certainly did not occur as a premise in the deduction of those phenomena, but served rather as a means of reconciling the mathematical theory with the ordinary metaphysical prejudices of the time.[35]

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[34] They certainly inspired the similar attitude of John Herschel (1830, cf. pp. 32-33, 196-197, 207, 261-262).

[35] Laudan (1981) does not produce any textual evidence for the connection between Whewell's attitude toward hypotheses in general and the ether hypothesis in particular and I have not been able to find any in Whewell's *Philosophy of the Inductive Sciences*, where, by the way, he lists Cauchy's ether among several "precarious hypotheses" (1847, 1: 491).

Be that as it may, Faraday, who remained free from scholastic prejudices thanks to the same contingencies that prevented him from receiving a good mathematical education, had little sympathy for the light-ether and did not conceal his inclination to do without it when the Faraday effect[36] he discovered in 1845 confirmed his suspicion that light might be intimately related to electromagnetism. In a letter he wrote to Richard Phillips on 15 April 1846 and which was published as "Thoughts on Ray-Vibrations" (Faraday 1846), he explains that by regarding the "ultimate atoms" of matter "as centres of force, and not as so many little bodies surrounded by forces […] and capable of existing without them", he was gradually led to look at the lines of force, which issue from every atom on this view, "as being perhaps the seat of vibrations of radiant phenomena". This notion "will dispense with the aether, which in another view, is supposed to be the medium in which these vibrations take place." After briefly discussing the "lines of gravitating force" between ponderable particles, Faraday proceeds:

> The lines of electric and magnetic action are by many considered as exerted through space like the lines of gravitating force. For my own part, I incline to believe that when there are intervening particles of matter (being themselves only centres of force), they take part in carrying on the force through the line, but that when there are none, the line proceeds through space. Whatever the view adopted respecting them may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration.
> ……………………………………………………………………………………
> It may be asked, what lines of force are there in nature which are fitted to convey such an action and supply for the vibrating theory the place of the aether? I do not pretend to answer this question with any confidence; all I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. The lines of weight or gravitating force are, certainly, extensive enough to answer in this respect any demand made upon them by radiant phaenomena; and so, probably, are the lines of magnetic force.

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36 The Faraday effect is the rotation of the plane of polarization experienced by a beam of plane polarized light when it passes in the direction of the magnetic lines of force through certain materials —e.g. water, heavy flint glass, quartz— exposed to a strong magnetic field. See Faraday (1855), pp. 1-11.

> ....
> The view which I am so bold to put forth considers, therefore, radiation as a kind of species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavors to dismiss the aether, but not the vibration. The kind of vibration which, I believe, can alone account for the wonderful, varied, and beautiful phaenomena of polarization, is not the same as that which occurs on the surface of disturbed water, or the waves of sound in gases or liquids, for the vibrations in these cases are direct, or to and from the centre of action, whereas the former are lateral. It seems to me, that the resultant of two or more lines of force is in an apt condition for that action which may be considered as equivalent to a lateral vibration; whereas a uniform medium, like the aether, does not appear apt, or more apt than air or water.
>
> (Faraday 1855, pp. 450-451)

Lines of force made their first appearance within the field-theoretic approach to gravity and electrostatics developed earlier by some Continental mathematicians,[37] and Faraday is plainly alluding to them. However, such lines and their tangent vectors were apparently regarded by everyone except Faraday as powerful aids for calculating the actual and virtual effects of forces acting at a distance, and not as fitting representations of physical realities. Only the mathematically untutored English genius endorsed the now common view that physical realities may and indeed ought to be conceived as models (in the model-theoretic sense, i.e. realizations) of mathematical structures. By contrast, the most mathematical among

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[37] Duhem (1914, p. 99) observes that, in dealing with electrostatics, "a French or German physicist, such as Poisson or Gauss, mentally places in space [...] this abstraction called a material point, accompanied by that other abstraction called an electric charge, and thereupon seeks to calculate a third abstraction, viz., the force to which the material point is subjected; he gives formulas that allow one to determine the magnitude and the direction of that force, for every possible position of this material point, and infers a series of consequences from these formulas. He shows, in particular, that at each point of space, the force is directed following the tangent of a certain line, the *line of force*; that all the lines of force meet at right angles certain surfaces, whose equation he gives, the *equipotential surfaces*." A significant counterexample to Duhem's ethnography is the English mathematician George Green (1828), who generalized and extended Poisson's investigations concerning electricity and magnetism and even anticipated Gauss's use of the term 'potential' for the scalar field whose gradient is the field of vectors tangent to the lines of force.

the British physicists paid obeisance —or was it only lip service?— to Kelvin's criterion of intelligibility: "It seems to me that the test of 'Do we or not understand a particular subject in physics?' is 'Can we make a mechanical model of it?'" (Kargon and Achinstein 1987, p. 111).[38] Coming from someone who about the same time had declared that "we have no right to assume that there may not be something else that our philosophy does not dream of" (ibid., p. 41), this was indeed an amazingly restrictive demand; but it held Maxwell and the Maxwellians in awe.[39]

Mechanical modeling, as understood by Kelvin, involved looking for "the explanation of all phenomena of electro-magnetic attraction or repulsion, and of electro-magnetic induction [...] simply in the inertia and pressure" of matter (Thomson 1857, p. 200; quoted by Harman 1998, pp. 99-100). In the same Baltimore lectures of 1884 in which he stated the said criterion, Kelvin told his audience they "must not listen to any suggestion that we must look upon the luminiferous ether as an ideal way of putting the thing. A real matter between us

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38 The two occurrences of 'model' in the above sentences illustrate two current meanings of the word that should not be confused, for they are not merely different but point, if I may say so, in opposite directions. In the branch of logic known as model theory, a *model* of a structure of a given species is any set endowed with structural features that satisfy the requirements of that species. In the more ordinary acceptance in which the word is used in the Kelvin quotation, a *model* is a representation of an individual or generic object by a real or ideal object of a different sort, such as a cardboard model of a Greek temple, or the familiar model of a pendulum, consisting of a weightless inextensible string with a massive dimensionless particle at one end, affixed by the other end to a frictionless nail. The common inclination to confuse both meanings may be due to the fact that today theoretical physics generally represents physical processes and situations by models in the second sense which are models in the first sense of specific mathematical structures (generally not the streamlined ones that are studied by the several branches of pure mathematics, but ad hoc combinations of them).

39 The index to Hunt (1991), *s.v.* 'ether models', mentions Oliver Lodge's cogwheel ether, FitzGerald's paddlewheel ether, Poynting's turbine-spring ether, Maxwell's vortex and idle-wheel ether, and FitzGerald's wheel and band ether; pp. 96-104 of the same book describe FitzGerald's vortex sponge ether of 1885. Chapter IX of Whittaker (1951/53), vol. 1, is devoted to "Models of the ether". See also in Buchwald (1985), pp. 146-150, the discussion of "ether rupture" and Larmor's difficulty in dealing with it.

and the remotest stars I believe there is, and that light consists of real motions of that matter" (Kargon and Achinstein 1987, p. 12).

In his three major papers on electromagnetism, Maxwell (1856, 1861/62, 1864) carried to different lengths the enterprise of modeling, screwing "the focussing glass of theory" —as he said— "sometimes to one pitch of definition, and sometimes to another, so as to see down into different depths" (Maxwell, 1990-2002, 1: 377; quoted by Harman 1998, p. 82). But he never flinched in his adhesion to the ether hypothesis. The paper "On Faraday's Lines of Force" (1855/56) seeks only "to shew how, by a strict application of the ideas and methods of Faraday, the connexion of the very different orders of phenomena which he has discovered may be clearly placed before de mathematical mind" (Maxwell 1890, 1: 157-158). Although Faraday's style of thought was "very generally supposed to be of an indefinite and unmathematical character" (p. 157), Maxwell regarded Faraday's lines of force as evidence that he was "in reality a mathematician of a very high order" (Maxwell 1890, 2: 360). In Maxwell's formulation the electric (or, respectively, magnetic) lines of force form a congruence of curves in space, i.e. a set of curves such that one and only one of them passes through each point of space, and represents the direction of the force that would act on a positively electrified particle (or, respectively, an elementary north pole) placed at that point (Maxwell 1890, 1: 158). According to Maxwell, we "thus obtain a geometrical model of the physical phenomena, which would tell us the *direction* of the force, but we should still require some method of indicating the *intensity* of the force at any point" (ibid.). [40] However, "if we consider these curves not as mere lines, but as fine tubes of variable section carrying an incompressible fluid, then, since the velocity of the fluid is inversely as the section of the tube, we may make the velocity vary according to any given law, by regulating the section of the tube, and in this way we might represent the intensity of the force as well as its direction by the motion of the fluid in these tubes" (pp. 158-159). Despite any appearances to the contrary, this is not intended to be a mechanical model of the electrical (or magnetic) field. On this point, Maxwell is emphatic:

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[40] By a suitable parametrization of the lines of force one could ensure that the vector tangent to each such curve at each point reflects not only the *direction* of the force at this point but also its *intensity*. But in 1856 any parameter other than arc length may have seemed ungeometrical to Maxwell's readers.

> The substance here treated of must not be assumed to possess any of the properties of ordinary fluids except those of freedom of motion and resistance to compression. It is not even a hypothetical fluid which is introduced to explain actual phenomena. It is merely *a collection of imaginary properties* which may be employed for establishing certain theorems in pure mathematics in a way more intelligible to many minds and more applicable to physical problems than that in which algebraic symbols alone are used.
>
> (Maxwell 1856, in Maxwell 1990, 1: 160; my italics)

Thus, the "substance" in question is neither more nor less than a bundle of precisely defined properties (and relations), that is, the bare embodiment of a mathematical structure; and the familiar word 'fluid' is here stripped of "every meaning except that which is warranted by the phenomena themselves" (Maxwell 1890, 2: 359).

In his second paper, "On Physical Lines of Force" (1861/62), Maxwell valiantly proposes a mechanical model of the ether. Electricity and magnetism depend on the presence of molecular vortices in it.[41] The model allowed the existence of transverse waves propagating in the ether with a speed equal to the ratio between the electrostatic and the electromagnetic unit. Back to town from the country house where he worked out this result, Maxwell verified that this value, as established by Weber and Kohlrausch (1857) differed by less than 1.5% from the speed of light, as it was then known. Maxwell concluded:

> The velocity of transverse undulations in our hypothetical medium [...] agrees so exactly with the velocity of light [...] that we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium with is the cause of electric and magnetic phenomena.*
>
> (Maxwell 1890, 1: 500)

At the time Maxwell did not draw the further conclusion that light is an electromagnetic phenomenon. He conceived optical and electromagnetic processes in mechanical terms, the relation between them being "expressed by the molecular connection between ether and matter, in terms of different motions in the ether" (Harman 1998, p. 109). Thus, the generally acknowledged reality of the optical

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[41] We need not go here into details. The model has been repeatedly explained in the literature. In particular, the quaint illustration printed facing Maxwell (1890), 1: 488 (Plate VIII, fig. 2), can also be found in Tricker (1966), p. 118, Siegel (1991), p. 41, Harman (1998), p. 104, and Darrigol (2000), p. 150.

ether could be regarded as supporting the ether hypothesis in the theory of electromagnetism.[42]

Maxwell's mechanical model was clever but coarse. On 23 December 1867, he wrote to Tait that "the nature of this mechanism is to the true mechanism what an orrery[43] is to the solar system" (Maxwell, 1990-2002, 2: 337; quoted by Darrigol 2000, p. 154). In "A Dynamical Theory of the Electromagnetic Field" (1864) he made no attempt at creating a more accurate model. Instead he based the theory on Hamilton's principle.[44] By showing that his field equations comply with the requirements of this principle Maxwell proves that the electromagnetic phenomena described or predicted by the equations *can be* explained by the mechanical behavior of a peculiar substance that permeates all ordinary bodies and completely fills the space between them.[45] Never mind that he does not give us an inkling of

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42 In the paper of 1864, Maxwell proceeded "to investigate whether [the] properties of that which constitutes the electromagnetic field, deduced from electromagnetic phenomena alone, are sufficient to explain the propagation of light through the same substance" (1890, 1: 577), and he tentatively concluded that yes, "light and magnetism are affections of the same substance, and that light is an electromagnetic disturbance propagate through the field according to electromagnetic laws" (1: 580).

43 "**Orrery** [Named after Chas. Boyle, Earl of Orrery, for whom a copy of the machine invented by George Graham c 1700 was made by J. Rowley, an instrument-maker.] A piece of mechanism devised to represent the motions of the planets about the sun by means of clockwork" (*Oxford English Dictionary*).

44 Following this method, one constructs a Lagrangian function L, whose value at each instant represents the difference between the kinetic energy T and the potential energy U stored at that instant in the physical system under consideration, and one assumes Hamilton's principle, according to which the temporal evolution of the system is always such that the integral $\int L dt$ takes an extremal value, i.e. such that $\delta\int L dt = 0$. Since the value of this integral is conventionally known as 'action', Hamilton's principle is also referred to, with mild impropriety, as *the Principle of Least Action*.

45 Or, in Poincaré's lucid words: "*Maxwell ne donne pas une explication mécanique de l'électricité et du magnétisme; il se borne a démontrer que cette explication est possible*" (1901*a*, p. iv). Lorentz's words point in the same direction: "Maxwell fait voir comment les principes de la mécanique peuvent servir à élucider les questions d'électrodynamique et la théorie des courants induits, *sans qu'il soit nécessaire de*

how exactly such a mechanical explanation could be carried out. "To demonstrate the possibility of a mechanical explanation of electricity, we do not have to worry to find this explanation itself; it is enough for us to know the expression of the two functions T and U that are the two parts of the energy, to form the Lagrange equations with these two functions and to compare these equations with the experimental laws. [...] As soon as the functions $U(q_k)$ and $T(q'_k, q_k)$ exist, one can find an infinity of mechanical explanations of the phenomenon" (Poincaré 1901*a*, p. viii).[46]

Thus it is not wholly inaccurate to say that the ether in 1864, though still quite new to electrodynamics, was already being cast by Maxwell for the Cheshire cat part it later played so skillfully under Lorentz.[47] There was, however, one significant philosophical reason for Maxwell's retention of the ether hypothesis. He expected to account for electric currents and electrostatic charge distributions as epiphenomena of ether dynamics. This would spare physicists the need to postulate one or two special electric fluids (as they did in the 18th century) or to acknowledge electric charge as a primitive property of matter (as they have been doing since the 1890s). Maxwell's ether eludes our senses and was endowed by him and other researchers with either a far-fetched or an altogether unperspicuous mechanical structure, but it was only assigned properties or relations that could be conceived in classical mechanical terms. However, in the last quarter of the 19th century, the discovery of new effects (Kerr 1877, Hall 1879) and the progress of experimental research on

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pénétrer le secret du mécanisme qui produit les phénomènes" (1892*a*, in Lorentz 1935-39, 1: 164; my italics).

46 Maxwell was well aware of this. He writes near the end of his *Treatise* (1891, 2: 470): "The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions. Of these, some may be more clumsy or some more complex than others, but all must satisfy the conditions of mechanism in general", that is , the conditions entailed by Hamilton's principle, or by the Euler-Lagrange differential equations mentioned by Poincaré, which are necessary and sufficient for $\delta\int L dt$ to be zero. Cf. also Maxwell's classical description of a belfry as a mechanical system (1890, 2: 783-784; quoted *in extenso* in Buchwald 1985, p. 21).

47 I owe the comparison between Maxwell's ether and Carroll's cat to Tricker (1966, p. 109); but it may well have been intended by Carroll himself (just as his Humpty Dumpty mimics the speech of mathematicians).

the conduction of electricity in electrolytes and gases (see Darrigol 2000, ch. 7) led Lorentz (1892*a*) and Larmor (1894, 1895) to assume the existence of electrically charged particles of ordinary matter, which were the sources of the force fields located in the ether and also the objects of their accelerative action.[48] This important step received decisive and apparently irreversible experimental support when J.J. Thomson "discovered the electron" (1897),[49] i.e. when he successfully identified cathode rays with a spurt or stream of negatively charged elementary particles. After that, one could no longer expect to understand electric charges and currents as mechanical effects in the ether.

In the highly acclaimed electrodynamic theory —or ought we to say *theories*?— of Lorentz, the ether is completely motionless and its mechanical structure and behavior —if it has any— is of no concern at all. As Whittaker lucidly wrote: "Such an aether is simply space endowed with certain dynamical properties" (1951/53, 1:393). It is therefore no wonder that, despite Lorentz explicit warning to the contrary,[50] the general public, including most philosophers and even some physicists, identified his ether with Newton's absolute space, or at any rate assumed that it was at rest in it.[51] Still, the idea that this elusive form of matter was part of the

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48 This transition "from Maxwell to microphysics" was persuasively described by Buchwald (1985).

49 The word 'electron' was introduced by Stoney (1881) as a term of art for the quantity of electricity, "the same in all cases", which traverses an electrolyte "for each chemical bond which is ruptured within" it, in other words, the electrolytic quantum. Larmor (1894) designated with it his new elementary charged particles (§§114-125: "Introduction of Free Electrons"). Lorentz began using the word five years later (1899, p. 507; French translation in Lorentz 1935-39, 5: 139); in his German book of 1895 he spoke of "Ionen".

50 "When I say for brevity's sake that the ether is at rest, I mean only that no part of this medium is displaced with respect to its other parts and that all perceptible motions of the heavenly bodies are motion relative to the ether" (Lorentz 1895, §1, in Lorentz 1935-1939, 5:4). I gather from this that one should regard Lorentz's ether as a body at rest in the so-called firmament, i.e., the inertial frame of the fixed stars constructed by astronomers.

51 In the light of §1, the ironic implications of this are clear. If Lorentz's ether is absolutely at rest, then Newton's absolute space is no longer idle, but plays an essential role, not in mechanics though, but in electrodynamics (and optics). This conferred a non-relativistic character to Newton's physics and caused SR to appear

furniture of the universe had become deeply entrenched during the 19th century, and nobody seemed willing to dismiss it. Surely it is not easy for a highly respected profession to admit that one of its time-honored terms of art is a noun without a referent.[52] Even Poincaré, who in 1889 had predicted that "the day will doubtless come when the ether will be rejected as useless",[53] at the Paris Congress of Physics of 1901 argued thus for believing in it:

> We know where our belief in ether comes from. If we receive light from a distant star, for several years that light is no longer at the star and is not yet on the Earth. It must therefore be somewhere, sustained, so to speak, by some material support. The same idea can be expressed in a more mathematical and more abstract way. What we record are the changes suffered by material molecules; we see, for example, that our photographic film displays the consequences of phenomena staged many years earlier in the incandescent mass of the star. Now, in ordinary mechanics, the state of the system under study depends only on its state in an immediately preceding state; the system therefore satisfies differential equations. But if we did not believe in the ether, the state of the material universe would depend not only on the

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much more un-Newtonian than it is. If the spacetime structure underlying Newtonian mechanics had been duly acknowledged before 1900, the continuity with SR would have been obvious. In either system any congruence of parallel timelike geodesics determined a privileged frame of reference, equivalent to every other such frame; though, of course, the respective spacetime geometries have different symmetry groups, namely. the Poincaré-Lorentz group in SR, and the so-called Galilei group in Newtonian mechanics. But again this points to a continuity, inasmuch as the latter may be regarded as a degenerate limiting case of the former.

52 We see even today that some scientists and philosophers who share the common prejudices concerning the epistemic status of physics will not easily acknowledge that the ether, whose dynamics and molecular structure absorbed the attention of so many great minds, simply does not exist. Maybe they feel this is like saying that Cuvier or Darwin spent years studying the physiology and histology of the unicorn.

53 "Peu nous importe que l'éther existe réellement, c'est l'affaire des métaphysiciens; l'essentiel pour nous c'est que tout se passe comme s'il existait et que cette hypothèse est commode pour l'explication des phénomènes. Après tout, avons-nous d'autre raison de croire à l'existence des objets matériels? Ce n'est là aussi qu'une hypothèse commode ; seulement elle ne cessera jamais de l'être, tandis qu'un jour viendra sans doute où l'éther sera rejeté comme inutile" (Poincaré 1889. préface; quoted from Poincaré 1968, p. 215).

> immediately preceding state (*l'état immédiatement antérieur*), but on much older states; the system would satisfy finite difference equations. To avoid this derogation of the general laws of mechanics we have invented the ether.[54]

Poincaré went on to say that Fizeau's experiment makes you feel you are touching the ether with your finger ("on croit toucher l'éther du doigt"—Poincaré 1968, p. 181). I need not describe this experiment here.[55] It was designed to test Fresnel's ether drag formula. According to Fresnel, the ether is partially dragged by the bodies it permeates. Any body in motion carries with it precisely the amount of ether it contains in excess of what would be found inside the same volume of otherwise empty space. If n is the refractive index of a transparent material m, then, according to Fresnel (and Young), $n^2 = \rho_m/\rho$, where ρ_m is the density of ether inside a body made of m and ρ is the density of ether outside all bodies. The density of the ether dragged by this body is $(\rho_m - \rho) = (n^2 - 1)\rho$, while a quantity of ether of density ρ remains at rest. Let c be the speed of light in interstellar space. The speed of light inside the said body is then cn^{-1}. Let the body travel in interstellar space with constant speed v in the same direction as light is sent through the body. Then, the center of gravity of the ether contained in the body moves in this direction with constant speed equal to $(n^2 - 1)n^{-2}v$. Let c_v denote the speed —relative to the interstellar sea of ether— of light propagating inside a transparent body of refractive index n in the direction in which the body travels with speed v. Then, under Fresnel's assumption and the classical rule for the composition of velocities,

$$c_v = \frac{c}{n} + \frac{n^2 - 1}{n^2} v = cn^{-1} + \left(1 - \frac{1}{n^2}\right) v \tag{9}$$

54 Poincaré (1901*b*); translated by me from Poincaré (1968), pp. 180-181. I cannot repress the feeling that Poincaré the mathematician must have known (i) that if a given physical state depends on another in accordance with a system of differential equations, none of the two states can *immediately* precede (or follow) the other one, and (ii) that time dependent vector fields can be defined on space without assuming a material support for them to sit on. I suppose (ii) is the reason why he talks of *inventing* the ether at the end of this tirade about *believing* in it.

55 Fizeau (1851). The experiment is described in Darrigol (2000), pp. 315-316; also in Janssen and Stachel (in press), p. 13. Both this paper and Stachel (2005) contain important remarks about the significance of Fizeau's experiment.

Fizeau (1851) verified formula (9) by letting light travel along two parallel tubes in which water flowed in opposite directions. Fizeau's experiment, which was repeated with greater accuracy and the same positive result by Michelson and Morley (1886), is a true treasure-trove for philosophers, for it can be read in at least three very different ways, as confirming either (i) Fresnel's ether drag hypothesis, as explained above; or (ii) Lorentz's theory of the motionless ether, which also entails the so-called *drag factor* $\left(1-\frac{1}{n^2}\right)$, but does not attribute it to a partial ether drag; or (iii) Special Relativity. Indeed, under the SR rule for the composition of velocities, Fresnel's purported ether drag turns out to be a purely kinematic effect, a consequence of substituting the moving body's reference frame for that of the fixed stars, when both frames are related by a Lorentz transformation. (To be precise, the relativistic formula differs from Fresnel's by a term of the second order in v/c.) It seems to me, however, that someone less adventurous than Einstein would not have dared to substitute reading (iii) for reading (ii); certainly not a mere HPS research worker.[56] Indeed, there are good reasons to think that the scientific establishment would have continued to believe that Fizeau's experiment allowed one to touch the Lorentz ether with one's fingers, if Michelson and Morley's failed attempt to ascertain the motion of the Earth in the ether had not belied, already in 1887, the success of their improved Fizeau experiment of 1886.[57] Lorentz (1892*b*) sought to

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56 Not even Poincaré dared to do so (at least not openly), although in June 1905 he was in possession of all the mathematical and physical ingredients of SR. Giannetto (1998) provides sufficient evidence of this (though not enough, in my view, to establish his claim that "Poincaré must be considered the actual creator of special relativity").

57 Michelson and Morley's experiment with the Michelson interferometer is described in practically every textbook on SR, usually from a relativistic standpoint. To get the perspective from which the experiment was designed, I recommend reading Michelson (1881). From the negative result of his experiment, Michelson drew *neither* (i) the relativistic conclusion customarily associated with it to day, *nor* (ii) the geostatic conclusion that Tycho Brahe or Roberto Bellarmino could have based on it. Instead, Michelson concluded that Gabriel Stokes (1845, 1846*a*, 1846*b*) was right in assuming that the local ether is dragged completely by the solid Earth, and in decreasing proportions by the atmosphere that surrounds it. Michelson therefore eventually had his experiment repeated on a mountain top. Had he lived much longer, he might have persuaded NASA to put an interferometer aboard a space shuttle. However, Lorentz (1886) had proved that Stokes conception of the total ether drag is untenable: the

reconcile these experimental results with the contraction hypothesis — also proposed independently by FitzGerald (1889)— which I mentioned in §2: because every macroscopic solid body is held together by presumably electric intermolecular forces, a metal rod traveling across the electromagnetic ether with constant speed v will contract in the direction of motion by the exact amount necessary to make its motion impossible to detect through observations accurate to the second order in v/c. This hypothesis is not so implausible as some philosophers have later said.[58] However, as Lorentz (1895, 1899, 1904) gradually realized, to properly do its job the contraction hypothesis had to be supplemented with the introduction of "local time", which Lorentz conceived as a mere mathematical aid to calculation, but which will make very little sense unless real clocks in moving labs actually agree with it.[59] As is well known, Lorentz's mature theory of 1904, complete with rod contraction and clock retardation, is Lorentz-invariant[60] and, despite profound

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flow of an incompressible fluid around a moving solid sphere cannot be irrotational and yet adhere to the sphere (Darrigol 2000, p. 317; Lorentz 1892*c* demonstrated a lemma he had used in his proof, viz., that Stokes theory must assume a velocity potential).

58 They objected that the Lorentz-FitzGerald contraction hypothesis is "ad hoc", as if one could ever design a good scientific hypothesis without having specifically in mind the phenomena it is meant to explain and the problems it is intended to solve. As Janssen (1995, pp. 160, 183) aptly notes, this unfair and misguided criticism of Lorentz and Fitzgerald has precious little to do with Einstein's complaint (1907, pp. 412-413) that the contraction hypothesis was ad hoc in the sense that it was expressly devised to save the superfluous ether hypothesis. For a spirited and skillful defense of the Lorentz-FitzGerald approach see Brown (2005), ch. 4.

59 Lorentz (1916, p. 321, n. 72*) acknowledged this and gave Einstein credit for it. However, he could have learned it before 1904 from Poincaré. On this issue see the illuminating and well documented exposition by Michel Janssen (1995), §3.5.4, pp. 244-248.

60 Eqns. (4) and (5) in Lorentz 1904, §4 (1935-39, 5: 175), include as a factor a function of velocity $l(v)$, which is equal to 1 for $v = 0$ and "for small values of v, differs from unity no more than by a quantity of the second order". This entails that the theory is only approximately Lorentz-invariant, and that an experiment with a higher order of accuracy could one day disclose the motion of the Earth in the ether. However, a few pages later Lorentz shows, by a tortuous physical argument, that $l(v)$ must be constant and therefore equal to 1 (1935-39, 5: 187-188). On this condition,

conceptual differences, agrees exactly in every prediction with SR. This means that, although it postulates an ether, it does not afford any conceivable way of experimentally detecting its presence.[61] At this point, the time was ripe for someone to call the bluff, as in Andersen's tale.[62]

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the transformation proposed by Lorentz becomes what we now call a Lorentz transformation and the *exact* Lorentz-invariance of the theory is secured. However, Lorentz's argument for $l(v)$ = const. failed to convince Poincaré, who proved instead that the set of all transformations that satisfy Lorentz's eqns. (4) and (5) *form a group* if and only if $l(v) = 1$ (Poincaré 1906, pp. 144-146). This in turn implies that the inverse of a given transformation is on a par with it, so that no privilege can be claimed for one of the coordinate systems mutually related by the transformation. Interestingly, Lorentz (1914), commenting on Poincaré (1906), asserted that his theory of 1904 was not exactly Lorentz-invariant. "My formulas —he says— remain loaded with certain terms that ought to have disappeared. These terms were too small to exercise a perceptible influence on the phenomena and I could thus explain the independence from the motion of the Earth that observations had disclosed; but I have not established the principle of relativity as rigorously and universally true." (Lorentz 1935/39, 7: 263). In a lecture delivered in Leiden, Paul Ehrenfest expressed a different view: "The etherless theory of Einstein demands exactly the same as the ether theory of Lorentz. [...] As a matter of principle (*ganz principiell*), there is no experimentum crucis between these two theories" (1913, pp. 17-18). Lorentz attended this lecture and voiced no objections. See also Ehrenfest (1912), a discussion note concerning Einstein (1909), in which Ehrenfest, who apparently sympathized at that time with the etherless emission theory of light propounded by Ritz (1908), stresses that "the partisans of *Einstein's theory of relativity* must wish that [in a practically viable experimentum crucis between the theories of Ritz and Lorentz] the partisans of the *ether hypothesis* should prevail over the partisans of the proper emission hypothesis" (p. 319).

61 In his lectures concerning "Old and new problems of physics" (1910), Lorentz himself said, after describing the situation in electrodynamics that immediately preceded Einstein's SR: "Hat es dann überhaupt einen Sinn vom Äther zu reden? Schliesslich ist ihm nur noch soviel Substantialität geblieben, dass man durch ihn ein Koordinatensystem festliegen kann" (Lorentz 1935-1939, 7: 210). And yet even this alleged possibility is unclear, given that there is no way of telling relative to which coordinate system the ether is at rest.

62 Shortly before Einstein (1905), Alfred Bucherer (1903, 1904) and Emil Cohn (1904) put forward etherless theories of electromagnetic phenomena. However, they were not

Few will deny that, if the ether does not exist, the dismissal of the ether hypothesis was a major improvement of physics. It seems to me that, in principle, a critical thinker imbued in Newton's methods (or Fourier's!) and acquainted with the discoveries and ideas of Faraday could have perceived already *ca.* 1855 that the hypothesis was both groundless and superfluous. However, William Thomson, Lord Kelvin, who certainly was such a thinker, remained during the next half-century ether's most adamant advocate. Considering this and other aspects of the story summarized above, as well as the deference that is usually —and sensibly— shown by philosophers and historians of science towards scientific authorities of Kelvin's stature, I do not believe that a greater presence of HPS in 19th century academic life could have significantly speeded the abolition of ether. On the other hand, most historians agree that the ether hypothesis exercised a fruitful influence on research. From this point of view, it was perhaps just right that it survived as long as it did.

4. "Time's arrow".

The three foregoing sections refer to problems that I believe are no longer pending and in whose solution HPS had or could have had a say. That absolute space plays no role in Newtonian mechanics (§1) could be proved by merely pursuing the implications of the theory. That 'rod contraction' is a misnomer in SR (§2) can be readily understood if one bothers to ask what Einstein meant by 'the length of a moving rod' and reads his published answer to this question (see footnote 10). That the theory of the electromagnetic field does not have to postulate an ether (§3) was harder to see for 19th-century philosophers steeped in the *mentalité chosiste* of our flint carving forefathers. Although an HPS worker could, in principle, have reasonably abolished ether in the 1850s, it is unlikely that this move would have found acceptance in the philosophical or the scientific community.

In this final section I shall deal with questions of time, clustering around the so-called problem of time's arrow. The phrase "time's arrow" was coined by Eddington (1929, p. 69) presumably on the analogy of the arrows placed on street corners to

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predictively equivalent to Lorentz's mature theory and were therefore experimentally unsuccessful. See Darrigol (2000), pp. 366-372.

indicate the direction of traffic. The idea that time flows (in another time?) like traffic along a street is of course repugnant to a philosophically trained person, but it is old and persistently popular. Indeed, in an otherwise beautiful line, Vergil wrote that "time flies away",[63] without bothering to specify the medium in which this feat is performed. In the abundant professional literature about time's arrow, this expression does not usually refer to an attribute *of* time, but rather to the patterns of succession of natural events *in* time.[64] The strange impression we get from watching a videotape —e.g. of a football game— while it is being rewound would indicate that common physical processes follow patterns of occurrence that normally cannot be reversed. From this standpoint, Eddington's phrase might suggest that time, which "takes survey of all the world", sets a direction or order to events. However, this is not what the philosophers and scientists who write on time's arrow have in mind. In fact, some of them seem to relish the thought that time itself is unreal (Barbour 1999). The literature concerning time's arrow, besides gathering and describing patterns of succession that appear to be irreversible, generally seeks to explain their irreversibility as a consequence of the universal laws of nature. Such attempts must overcome one major difficulty. We normally assume that the fundamental equations of classical, relativistic and quantum mechanics and electrodynamics express the universal laws of nature to an approximation that is sufficiently good in their respective fields of application. Those equations are invariant under the *time reversal* transformation $t \mapsto -t$, which multiplies every value

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[63] "Sed fugit interea, fugit inreparabile tempus" — Vergil, *Georgica*, 3.284.

[64] Thus, Gell-Mann and Hartle (1994), p. 311, mention: "• The thermodynamic arrow of time – the fact that approximately isolated systems are now almost all evolving towards equilibrium in the same direction of time. • The psychological arrow of time – we remember the past, we predict the future. • The arrow of time of retarded electromagnetic radiation. • The arrow of time supplied by the *CP* non-invariance of the weak interactions and the *CPT* invariance of field theory. • The arrow of time of the approximately uniform expansion of the universe. • The arrow of time supplied by the growth of inhomogeneity in the expanding universe." Remarkably, they fail to mention the pattern of succession that most closely concerns us (and which may well be mainly responsible for our infallible sense of temporal orientation): we start living at birth and thereupon grow bigger and older until we finally die; not a single case is known of a person who rose from the grave and thereupon grew younger and ended by climbing up into his or her mother's womb.

of the time variable by –1. This invariance implies that for every temporal series of phenomena represented by a solution of the equations there is a matching series represented by another equally true solution, in which the corresponding phenomena succeed one another in reverse order. In many cases, however, only one of each such pair of solutions is exemplified in the natural world. To explain this selectivity of nature by deriving it from time reversal invariant laws is an ambitious undertaking which, to say the least, is not very likely to succeed.

Before looking more closely into this matter it will be convenient to make a few remarks about the meanings and uses of the word 'time'. Since 'time' is a noun it is plausible to ask for its referent. Reading some philosophers like Kant one even gets the impression that in its primary use the word designates a unique entity and therefore ought to be regarded as a proper name (although in English we seldom capitalize it). Its purported *denotatum* turns out to be even more difficult to pin-point than the referents of 'Homer' or 'Moses'. This, in turn, lends color to the verdict of unreality. Of course, in everyday conversation, 'time' is most frequently employed as a common noun, to denote particular instants ("At what time shall we meet?") or particular durations ("Due to the heavy traffic, it took me twice the usual time to get back home"), that very few people would dare to call unreal. Kant held that, in stark contrast with ordinary common nouns, the relation between the several objects called 'times', in the plural, and that which we call 'time', in the singular, is not that of individual *instances* to the *class* to which they belong, but rather that of *parts* to a *whole*. Yet, if the whole of Time remains elusive, the most one can grant to Kant is that, among those multiple items to which the common noun 'time' refers, some are related to each other as parts to wholes, while others — viz., the instants— are related to the former as their boundaries.[65] Something like this is probably what most of us would come up with when prompted to elucidate 'time'. However, I sense that any explication of 'time' that brings all its uses under one or two heads is surely narrow-minded. Indeed, I dare say that it is wrongheaded to think that 'time' must denote an individual object or a class of such objects merely because it performs like a noun. After all, only Platonists require such nouns as 'beauty' or 'justice' to sport substantive *denotata* (and not even Platonists believe

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65 Anyway, *the time* at which two people can sensibly agree to meet is never an *instant*, but at best a fairly short time *interval*, and therefore my first example may also be regarded as referring to a duration.

that the noun 'inanity' does it). Some continental philosophers employ the terms *Zeitlichkeit* or *temporalité* to elude the suggestions of definite reference implicit in the more familiar nouns *die Zeit* and *le temps*. Good taste bars the introduction of such ugly neologisms in English. On the other hand, I see no difficulty in taking 'time' as a fairly abstract portmanteau term that covers (connotes) a broad variety of aspects of our life in the world, but does not denote anything in particular. If such is the case, those who, like Augustine, ask "What is time?" cannot expect a simple, non-contextualized, reply any more than if they asked, say, "What is excellence?" or "What is validity?" For our present purposes, it will be sufficient to consider four pervasive features of our human experience that give good reason for describing it as experience *of* time and *in* time. I shall summarily identify them as *waiting times*, *time order*, *time points* (or *instants*), and the threefold display of *past, current and future times* at each actual instant. I have deliberately included the word 'time' in all four labels to underscore its polysemy. After the first item and before the other three I briefly touch upon *the whole of time*, which is not an element or an aspect of our experience but has come to be permanently associated with it.

(i) *Waiting times*: If you insist in spoiling your *espresso* by drinking it with sugar, then, as Henri Bergson shrewdly noted, you must wait for the sugar to dissolve in the beverage. In fact, much of our lives consist in waiting for one or the other thing to happen and, ultimately, of course, we always are —with mixed feelings—waiting for death. We spontaneously quantify waiting times, but our estimates are rough and highly dependent on context. However, our ancestors discovered that many readily typified natural processes have equal or proportional waiting times and began using them to measure time lengths intercontextually (or "objectively", as prepostmodern philosophers are fond of saying). Thus, one may presume that cavemen soon realized that they had to wait the same time for two equally sized pots of water to boil, after placing them over like fires. And the people of diverse civilizations, from Stohenge to Tiwanaku, found out that between any two consecutive summer solstices they had to wait for 365 noons. With the invention and improvement of clocks, the measurement of waiting times became ever handier and eventually took pride of place in our system of life.[66]

(ii) *The whole of time*: Waiting times can be divided into smaller parts and combined into larger wholes. At sunrise I wake up waiting for the next sunset but also for the next noon. The selfish son of a rich man continually waits — like everybody else— for his own death, but also, more impatiently, for that of his father, which he hopes will come first. Through an effortless idealization, we combine all waiting times into a single whole, "till Kingdom come". We view finished times as getting somehow packed into Life's attic. With a little imagination and a lot of abstract construction, we come to regard this storage place as reaching back to our birth, to the beginning of human history and prehistory, and even to "the creation of the world" (Friedmann 1922, p. 384).

(iii) *Points in time*: The parts of time are marked and bounded by *events*. In real life even the slightest event —e.g. the quick utterance of a monosyllabic word— lasts for a while and thus fills a *part* of time. However, the notion of a *point in time*, which takes no time but stands between two consecutive parts of time, played a role in the arguments of Zeno of Elea and was carefully articulated by Aristotle. With the generalized use of clocks and watches this notion became an important ingredient of ordinary common sense. Indeed, to reach it one needs very little mathematical sophistication. The shadow of a vertical stick shrinks continually as the Sun climbs, attains its minimum length at noon and thereupon slowly grows again. Given these circumstances, it seems reasonable to conceive noon as a point in time, the durationless instant at which the shadow stops shrinking and begins to grow.

(iv) *Time order*: Every *waiting* time begins, goes on and usually finishes (not infrequently with a disappointment). Thus, there is an inbuilt order of succession among its parts. This is readily extended to the whole of time, for whose beginning and end most of us therefore naturally —even if

66 We now know that time measurement by clocks also depends on context, insofar as their accuracy is controlled by atomic clocks, which measure actual waiting times *along their respective worldlines*. Thus, if Jack remains seated on an inertially moving spaceship while Jill takes a roundtrip from it to α-Centauri, Jack will wait longer than Jill for their reunion, *according to their respective standard clocks*.

impertinently— feel inclined to ask. Let us designate parts of time by lower case italics, *a*, *b*, *c*,…, and points in time by upper case italics *A*, *B*, C,… Then, for any two parts *a* and *b*, either (1) *a* is a part of *b* ($a \subset b$), or (2) *b* is a part of *a* ($b \subset a$), or (3) *a* and *b* share a part of time *c* ($a \cap b = c \neq \varnothing$), or (4) *a* and *b* do not have any part in common ($a \cap b \neq \varnothing$). In case (4), either *a* has already ended when *b* begins, in which case we say that *a* precedes b, or *a* begins after *b* has ended, in which case we say that *a* follows *b*. If *a* precedes *b* and *b* precedes *c*, then *a* precedes *c*. All this, I dare say, is fairly obvious. We have thus a linear order among non-overlapping parts of time. Points in time readily inherit this order if we make the following common though far from obvious assumption: If *A* and *B* are any two distinct points in time, there are always two non-overlapping parts of time *a* and *b*, such that *A* belongs to *a* and *b* belongs to *B* ($A \in a \wedge B \in b$). Under this assumption, a linear order is established among time points if we stipulate that, for any two such points *A* and *B*, *A* precedes *B* if and only if there are two non-overlapping parts of time *a* and *b* such that $A \in a$ and $B \in b$ and *a* precedes *b* (in the sense defined above). As far as I can tell, in every case in which I distinguish two given points in time *A* and *B*, a third point C can be discerned, such that either *A* precedes C and C precedes *B* or *B* precedes C and C precedes *A*. This familiar experience encourages one to conceive the linear order of points in time as *dense in itself*. Modern mathematical physics goes a large step further and regards it as continuous, and indeed as a linear order on a differentiable manifold (more on this below). Only on this assumption can the laws of physics be expressed as differential equations involving the time derivatives of physical quantities.

(v) *Past, future and current time*: Perhaps the most salient feature of our life in time is the partition of events and their times of occurrence into past, present and future. As far as I can judge, every normal four-year old child understands this partition and regularly applies it to matters of interest to him or her. My judgment may be biased by the fact that all the four-year olds I have talked to spoke either Spanish or English and had already mastered the use of tenses. Kant, who also spoke an Indo-European language, once noted:

> "All predicates have as copula: *is*, *was*, *will be*".[67] The partition is central to our consciousness and our behavior and most of our decisions would hardly make any sense without it. Nevertheless, it has been declared illusory by respectable thinkers (see footnote 71).

The partition of times noted under (v) is closely linked to the four acceptances of 'time' I commented under (i)-(iv). Thus, (i) one currently waits only for future events; to wait for the past to happen, though perhaps feasible for someone who adopts "the point of view from nowhen" (Price 1996), sounds crazy and even ungrammatical in ordinary English. Indeed our most primitive idea of a duration or length of time is how much we must wait now until an expected future event —e.g. the departure of a plane we have already boarded— becomes past. The partition naturally extends (ii) to points in time, and is linked (iii) to their time order, so that every past instant precedes all future instants. The partition provides the basic empirical criterion for establishing a time order among events: event A precedes event B if A is present or past when B is future, or A is past when B is present or future. Applied to (ii) the whole of time, the partition leads to the abstract conception of time already found in Aristotle. Using the clear and precise language of modern mathematics, we can say that, according to this conception, the whole of time is a linear continuum in which the present instant, *now* (τὸ νῦν), effects a Dedekind cut. There is an obvious difficulty —a contradiction, perhaps?— in any statement that uses the word 'now' to refer to the present point in time, inasmuch as the word will no longer denote its original referent when the statement is finally completed. Some philosophers believe that this difficulty can be evaded by avoiding all mention of *now* and using instead the so-called *tenseless present*. A coordinate system is defined on the whole of time, which assigns a unique numerical label to each instant. The point in time when a particular event *E* occurs can then be denoted by its label: 'E occurs at time *t*', say. This approach surely has fostered the opinion that all times are homogeneous and that their partition into past, present and future is illusory. However, the tenseless present remains a mere figment of the intellect, devoid of reference, unless the *t*-labels are anchored to the time of our life, which, as we know too well, is structured around that partition. The

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[67] "Alle praedicate haben zur copula: *est, fuit, erit"* (Kant, R. 4518; 1902– , 17:579). Cf. R. 4517: "Wir können das Wort *est* nicht anders als ein Zeitwort brauchen" (Kant, 1902– , 17:579).

zero of time of the Christian or "common" era must be fixed at so many years, days and hours before *now*, lest it should float timelessly in nowhen.[68]

Physics has created a mathematical structure that I shall denote by $\mathbb{T}$, to which it resorts for describing the evolution of phenomena and stating the laws that govern it.[69] $\mathbb{T}$ is a one-dimensional topological space homeomorphic with $\mathbb{R}$. Any bijective homeomorphism $t\colon \mathbb{T} \to \mathbb{R}$ defines an unique maximal atlas $\mathscr{A}_{\mathbb{T}}$ on $\mathbb{T}$, such that $\langle \mathbb{T}, \mathscr{A}_{\mathbb{T}} \rangle$ is a one-dimensional differentiable manifold. The charts in $\mathscr{A}_{\mathbb{T}}$ are known as *time coordinate functions*; "global time coordinates" if defined on all of $\mathbb{T}$. Every global time coordinate t induces on $\mathbb{T}$ a linear order $<_t$ such that, for any three real numbers a, b and c which satisfy the inequality $a < b < c$, either $t^{-1}(a) <_t t^{-1}(b) <_t t^{-1}(c)$ or $t^{-1}(c) <_t t^{-1}(b) <_t t^{-1}(a)$. Obviously, if t and t' are two global time coordinates, the orderings induced by them on $\mathbb{T}$ either agree or are the exact reverse of each other. The latter occurs, for instance, if $t'(x) = -t(x)$ for every $x \in \mathbb{T}$. In this case, we may denote the mapping t' by $-t$ or, for greater clarity, by $(-t)$. The coordinate transformation $(-t) \circ t^{-1}$ is usually called *time reversal*, although this name would perhaps suit better the matching point transformation $(-t)^{-1} \circ t$. It is worth noting that this point transformation is not only a automorphism of $\mathbb{T}$ (regarded both as a differentiable manifold and as a metric space), but also an isomorphism of orders between $\langle \mathbb{T}, <_t \rangle$ and $\langle \mathbb{T}, <_{-t} \rangle$: for any $x, y \in \mathbb{T}$ such that $x <_t y$, we have that $(-t)^{-1}t(x) <_{-t} (-t)^{-1}t(y)$. By contrast, the coordinate transformation $(-t) \circ t^{-1}$ is certainly not an automorphism of the complete Archimedean ordered field $\mathbb{R}$,

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68 Sunny Auyang (1998, p. 226) trenchantly makes this same point as follows: "Without a proper anchoring the *now*, a dating system is like a calendar in science fiction. 'Starday 4354.27' sounds scientific, but it floats in fantasy because it has no point of contact with reality. The utility of dating systems is based on their ability to synchronize the experiences and actions of individuals, so that each can say, 'Today is March 1, 1995,' or 'Now it is 4 o'clock'." I accidentally found this passage after I had written the text above, and I was pleasantly struck by the coincident wording.

69 The structure $\mathbb{T}$, as described in the main text, is specially tailored to fit Newton's "absolute, true and mathematical time" (1687, p. 5); but it also suits the universal time relative to an inertial frame defined by Einstein (1905, §1), as well as the mildly unprincipled domain of definition of the time coordinate that occurs in Schrödinger's equation. However, additional qualifications and caveats may be needed to speak sensibly about time in the context of General Relativity, as Gordon Belot (2006) has aptly noted.

inasmuch as it maps the neutral element 1 of the multiplicative group $\mathbb{R}\backslash\{0\}$ to the number -1, which does not enjoy any distinguished status in the structure of $\mathbb{R}$. This difference justifies the uncommon and seemingly artificial distinction I make between $\mathbb{R}$ and $\mathbb{T}$.[70] The standard definitions of a metric and a measure on $\mathbb{R}$ are readily transferred (in more than one way) to $\mathbb{T}$.

The invention of $\mathbb{T}$ can be traced back to the founder of Greek mathematical astronomy, Eudoxus, and perhaps even further, to Zeno of Elea (as reported by Aristotle), but obviously the characterization of $\mathbb{T}$ in the above terms would be anachronistic for any epoch of science before the second half of the 19th century. Still, the formulation of the laws of physics as differential equations involving smooth functions of time, initiated by Galileo and perfected by Newton and his contemporaries and successors, can only make sense to us if what is meant by "time" —i.e. the range of the independent variable of such functions— is none other than $\mathbb{T}$ understood in these terms. In particular, such formulation is out of question unless "time" is a differentiable manifold.

$\mathbb{T}$ affords a coherent representation of four of the five "time" features of human experience I listed above. By collecting them into a single structure, $\mathbb{T}$ justifies the use of a noun 'time', whose denotatum is any (or every) realization of $\mathbb{T}$, be it real or imaginary. *Time points* or *instants* (iv) are naturally identified with the points of the topological space $\mathbb{T}$, and *waiting times* (i) or durations with its open sets (or rather with $\mathbb{T}$'s *connected* open sets, which constitute a basis of its topology). On this understanding, *time order* (iii) necessarily agrees with one or the other of the two linear orders admitted by $\mathbb{T}$. Thus, there is apparently no problem in equating $\mathbb{T}$ or rather its intended realization with *the whole of time* (ii). On the other hand, there is nothing whatsoever in the structure of $\mathbb{T}$ that even hints at a distinction between one particular instant and the others. Moreover, the structure of $\mathbb{T}$ comprises nothing that, given the conventional choice of a particular instant, would mark an important difference between the instants that precede it and those that follow it. Indeed —as I emphasized above when I contrasted the linear order of $\mathbb{T}$ with that of the complete Archimedean ordered field $\mathbb{R}$—, there are no grounds in $\mathbb{T}$ for distinguishing one of its two admissible orderings from the other. Therefore, the fifth

[70] Since physicists and philosophers do not normally bother to distinguish between $\mathbb{T}$ and $\mathbb{R}$, the difference between a *point* transformation $\mathbb{T}\rightarrow\mathbb{T}$ and the corresponding *coordinate* transformation $\mathbb{R}\rightarrow\mathbb{R}$ generally eludes them.

item in my list, the trichotomy of times into past, present and future, though central to our conscious lives and crucial to our decisions, is not reflected in any way in the physico-mathematical representation of time. There are good reasons for this —on which I do not have to dwell here— and wakeful minds will not be led astray by it. All the same, this omission makes it a bit less surprising that men of normal or even superior intelligence who strongly believed in the epistemic powers of theoretical physics should have regarded the divide between the present, the future and the past as a stubborn illusion.[71]

Although some writers on time's arrow tend to identify it with the said trichotomy,[72] these are two very different (if related) matters. 'Time's arrow' or 'the direction of time' refers to the existence of a unique, or preferred, or intrinsically distinct time order. Obviously, anyone who, from his or her present vantage point, senses the blatant difference between past and future, recollection and expectation, times gone and times pending, can use this difference for distinguishing between the two possible linear orderings of the time continuum. On the other hand, if there is an intrinsic difference between these orderings, then the arbitrary choice of an instant as *the zero of time* determines a partition of all others into the two intrinsically distinct classes of *instants before zero* and *instants after zero*. This yields the partition of instants into past, present and future if the chosen zero is now. But the existence of an intrinsically distinct time order does not require or even imply the peculiar properties of this partition. Suppose you pick out a point p on a linearly ordered continuum. To establish an intrinsic difference between the set of points that precede and the set of points that follow p it is sufficient to find an intrinsically distinguished point q that belongs to one of those sets; q will automatically communicate its distinction to the set to which it belongs. This is what

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71 The most noteworthy among them was Albert Einstein, who wrote on 21.05.1955 to Michele Besso's widow: "Für uns gläubige Physiker, hat die Scheidung zwischen Vergangenheit, Gegenwart und Zukunft nur die Bedeutung einer wenn auch hartnäckigen Illusion" (quoted by Dorato 1995, p. 13).

72 For example, Albert (2000), p. ix, describes his own "relatively straightforward rehearsal of what is perennially referred to in the physical literature as 'the problem of the direction of time'" as a "careful discussion of what it means for a set of dynamical laws to distinguish, or to *fail* to distinguish, between the past and the future". I beg to note that the very notions of past and future only make sense with regard to a present which no "set of dynamical laws" can mention.

happens with the ordered field of real numbers $\mathbb{R}$: the neutral element for addition (0) stands between two classes of real numbers; the neutral element for multiplication (1) confers its intrinsic distinction on the class to which it belongs (the positive real numbers).

For all practical purposes, physicists are content to use the impoverished structure $\mathbb{T}$ to set up and solve their physico-mathematical problems. When the time comes to apply and to test their solutions, they put in "by hand" the link to the present and the attendant preferred time order. Indeed, physicists do this spontaneously and infallibly. If, as Einstein believed, they are yielding to a delusion (see footnote 71), it is a pretty stable and law-abiding one. I have never heard of a physicist who took what is currently going on in the lab for what went on yesterday or who failed to distinguish the outcome of an experiment from its preparation. Most working physicists accept that this is how things are and leave it at that; and sensible philosophers should presumably do the same. However, if you happen to be a physicalist driven by a metaphysical itch, you will naturally expect physics to account for every major facet of your experience, and a feature so pervasive as the trichotomy of times (at each instant) —even if it is a mere illusion— cannot be an exception. You are then bound to find a physical ground, if not for the fleeting singularity of the present, then at least for the steadfast and unmistakable difference between the direction from past to future and that from future to past. This can be provided if you secure a physical foundation for distinguishing between the time ordering in which, at any given instant, all past times precede all future times, which —for ease of reference— I propose to call the *forward* time order, and the opposite time ordering in which, at any instant, all future times precede all past times, which I shall call the *backward* time order.[73] There is nothing in $\mathbb{T}$ that can

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[73] Needless to say, the words 'forward' and 'backward' function here as metaphors which I do not particularly relish. But they provide concise expressions that will be readily understood by most people today. On the other hand, an ancient Greek would have misunderstood them utterly, for, according to the metaphor used by Homer, at all times we have our eyeless backs turned toward the future, and hence we are unable to see it (*Iliad*, 3.411, 4.37, 6.352, 6.450, 15.497; *Odyssey*, 1.222, 2.179, 6.273, 11.433, 14.137, 18.132, etc.; cf. Empedocles, DK 21.B.9). The standard practice of assigning a smaller real valued coordinate to event P and a larger one to event Q whenever P precedes Q in the forward time order is of course purely conventional. Therefore, as Price (2002, p. 88, n. 3) aptly notes, from merely examining two

represent such a foundation, but one may expect to find a suitable stand-in for it among the real-valued functions on $\mathbb{T}$ and other notional enrichments of the original structure, through which mathematical physics conceives the evolution of phenomena.

In fact, the world we experience teems with readily discernible processes that display time-asymmetric patterns of succession (cf. the list in footnote 64). However, the craving for unity that was still so very much alive in the 19th century did not favor the dispersion of explanatory grounds over a dappled collection of sources, but would rather focus on a single unidirectional universal law of becoming, from which one would then hope to derive the entire array of temporally oriented patterns. Since the 1860's, almost every philosopher-scientist who has pursued this question has put his or her stakes on the Second Law of Thermodynamics. As popularly understood, the Second Law says —or implies— that there is a physical property of the universe that Clausius (1865) called *entropy*, which takes a real value at each instant and increases monotonically with time.[74] This, if true, is

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solutions of the Friedmann equations, one with a singularity at the infimum of the time coordinate range and another with one at its supremum, it is impossible to tell which one we live in.

74 "One can express the fundamental laws of the Universe that correspond to the two main laws of thermodynamics in the following simple form: 1. The energy of the Universe is constant. 2. The entropy of the Universe tends to a maximum." (Clausius 1867, p. 44, as quoted in English by Uffink 2003, p. 129). Uffink notes that in his textbook of 1876 Clausius did not include this sweeping formulation of the Second Law, for which he obviously did not have a shred of evidence. Nevertheless it is untiringly repeated, often with great fanfare, in the philosophical literature, e.g. by Albert (2000, p. 32): "The third and final and most powerful and most illuminating of the formulations of the second law of thermodynamics [...] is that 'the total entropy of the world (or of any isolated subsystem of the world), in the course of any possible transformation, either keeps the same value or goes up'." Indeed Brush (1976, p. 579), who says that the statement about cosmic entropy was eliminated in the *third* edition of Clausius's treatise (1887), mentions this fact with a tinge of regret. More recently, Price (2002, p. 88-89) has suggested that "we could do without the notion of entropy altogether" and "hence by-pass a century of discussions about how it should be defined", or perhaps use the term 'entropy' only as a portmanteau word for "a long list of the actual kinds of physical phenomena which exhibit a temporal preference, which occur in nature with one temporal orientation but not the other".

sufficient physical ground for distinguishing permanently and globally a definite direction on $\mathbb{T}$. However, in the 1850's the conception of heat as a kind of motion[75] had finally prevailed over the notion that heat is a peculiar substance. By accepting that conception, physicists placed themselves under an obligation to provide a mechanical explanation of thermal phenomena, and in particular to derive the time-asymmetric Second Law from the time-reversal invariant laws of mechanics. This, in a nutshell, is the problem of time's arrow. A child or an Andean peasant who understood its terms would promptly conclude that it is insoluble.[76] But European adults are a stubborn breed, and some of them, from Ludwig Boltzmann on, have spent untold hours trying to figure out a solution.

Criticism of Boltzmann was promptly voiced by Loschmidt (1876), clarified by Burbury (1894) and backed —with a different argument— by Zermelo (1896*a*, 1896*b*). Their mathematical strictures eventually compelled Boltzmann to assign a regional scope (restricted *both* in space and time!) to the direction of time resulting from the evolution of entropy. Philosophers have been surprisingly complacent about this curious view, which they have sought to bolster with schemes of their own making.[77] On the other hand, it is only very recently that HFS research, mainly

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75 This phrase "the kind of motion we call heat" was introduced by Clausius in the title of one of his great papers on the subject (1857). In our days, Stephen G. Brush used it in the title of his monumental history of the kinetic theory of heat (1976).

76 Cf. Henri Poincaré (1893), p. 537: "Il n'est pas besoin d'un long examen pour se défier d'un raisonnement où […] l'on trouve en effet la réversibilité dans les prémisses et l'irréversibilité dans la conclusion."

77 Here is a small sample of texts from Reichenbach (1956, pp. 127-128, my italics): "The total entropy of the world in its present state is not too high: the universe has large reserves in ordered states, so to speak, which it spends in the creation of branch systems and thus *applies to provide us with a direction of time*. […] It follows that we cannot speak of a direction for time as a whole; *only certain sections of time have directions, and these directions are not the same*. […] Boltzmann has made it very clear that the alternation of time directions represents no absurdity. He refers our time direction to that section of the entropy curve on which we are living. If it should happen that 'later' the universe, after reaching a high-entropy state and staying in it for a long time, enters into a long downgrade of the entropy curve, then, for this section, time would have the opposite direction: human beings that might live during this section would regard as positive time the transition to higher entropy, and thus their time would flow in a direction opposite to ours. […] Life is restricted to the

by Uffink (2001, 2003; see also Brown and Uffink 2001; Callender 2001) has made it clear to philosophers that the thermodynamic concept of entropy can only be defined for particular physical systems under special conditions. This is sufficient to dismiss the popular understanding of the Second Law of Thermodynamics as a law of cosmic evolution, to disqualify thermodynamic entropy as the physical source of universal time order, and to remove the need for deriving Time's Arrow —*per impossibile*— from the mechanical or statistico-mechanical principles of thermal physics. I cannot give here a detailed and accurate picture of this complex affair, but the following sketch should be sufficient for my present purpose (and will, I hope, provoke a desire to read more about it in the references I give).

The Second Law of Thermodynamics can be traced back to Sadi Carnot's groundbreaking thoughts about heat engines (1824). A heat engine is a device by which heat is transferred from a hot reservoir —the furnace (*foyer*)— to a cooler one —the refrigerator (*réfrigerant*)— and which through this process yields mechanical work. According to the caloric theory of heat, which Carnot took for granted, heat is an indestructible substance, so that, if the process is carried out adiabatically, that is, in thermal isolation from the rest of the world, the amount of heat drawn from the furnace must be equal to the amount surrendered to the refrigerator. But Carnot's reasoning does not depend on this,[78] but on the assumption that the endless production of mechanical work (*création indéfinie de puissance motrice*), without consuming heat or any other agent whatsoever, is impossible (Carnot 1824, p. 21). From this assumption, he proved that a periodically operating heat engine which in each full cycle $\mathscr{C}$ performs an amount of work $W(\mathscr{C})$ by transferring heat $Q(\mathscr{C})$ from

temperate zones of transition in the entropy curve. Thus an alternation of time directions would involve no contradiction to experiences accessible to us. *Perhaps we are, indeed, inhabitants of a second section, in which the entropy 'really' goes down, without our knowing it*."

78 Carnot must have had grave doubts about the caloric theory, for his book contains the following rhetorical question: "Can one conceive the phenomena of heat and electricity as due to anything else than the motions of bodies? As such, must they not be subject to the general laws of mechanics?" (Carnot 1824, p. 21n; cf. p. 37n). In fact, the conservation of heat is incompatible with Carnot's proof, as Joule showed to Kelvin (in a letter of 8 October 1848, quoted by Crosbie Smith 1998, p. 83; the argument is also given in Maxwell 1883, pp. 146f. and reproduced in Torretti 1999*a*, p. 187 n. 56).

a furnace at temperature θ^+ to a refrigerator at temperature θ^- has an efficiency $W(\mathscr{C})/Q(\mathscr{C})$ equal to or less than a maximum $C(\theta^+,\theta^-)$, and that the value of $C(\theta^+,\theta^-)$ does not depend on the nature of the means employed but "is fixed solely by the temperatures of the bodies between which the transfer of heat ultimately occurs" (p. 38). Moreover, the maximum efficiency $C(\theta^+,\theta^-)$ can only be attained if the bodies involved in the process of producing work by heat transfer do not undergo "any change of temperature which is not due to a change in volume" (p. 23). Such changes can only be effected by outside intervention on an adiabatically closed system (e.g. by moving a piston very slowly). Carnot's results were applied with great success in the design of steam engines.

When the caloric theory was finally given up around 1850, the amount of work W was equated with the difference $Q^+(\mathscr{C}) - Q^-(\mathscr{C})$ between the heat extracted from the furnace and the heat surrendered to the refrigerator. In the new context the efficiency is defined as $W(\mathscr{C})/Q^+(\mathscr{C})$. Rudolf Clausius and William Thomson (later Lord Kelvin) derived Carnot's theorem (thus understood) from two differently stated "axioms", which I quote in Thomson's wording (1851; in 1882, pp. 179, 181):

> THOMSON: *It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.*
>
> CLAUSIUS: *It is impossible for a self-acting machine, unaided by any external agency to, to convey heat from one body to another at a higher temperature.*

Thomson notes that, although these axioms "are different in form, either is a consequence of the other". They became known as the Second Law —or Principle— of Thermodynamics (energy conservation being the First).[79] Their empirical warrant is the thermal phenomena that corroborate Carnot's theorem.

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[79] According to Thomson (1851) "the whole theory of the motive power of heat is founded on the two following propositions", viz. , "PROP. I. (Joule)", which amounts to energy conservation, and "PROP. II. (Carnot and Clausius).—If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed, it produces as much mechanical effect as can be produced by any thermodynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat" (1882, p. 178). Prop. II is then derived from the Thomson axiom quoted above, for which he argues thus: "If this axiom be denied for

In a series of papers, Clausius and Kelvin extended and reformulated the result. In 1854 Kelvin showed that the absolute temperature scale $T(\theta)$ can be chosen such that $C(T^+,T^-) = J(1 - T^+/T^-)$ [where J is Joule's constant], or equivalently

$$\frac{Q^+(\mathscr{C})}{T^+} = \frac{Q^-(\mathscr{C})}{T^-}$$

Generalizing the approach to cycles involving an arbitrary number of heat reservoirs, they obtained the formulation:

$$\oint_{\mathscr{C}} \frac{đQ}{T} = 0 \;\text{ if } \mathscr{C} \text{ is reversible}^{80} \tag{10}$$

and

$$\oint_{\mathscr{C}} \frac{đQ}{T} \leq 0 \;\text{ if } \mathscr{C} \text{ is not reversible} \tag{11}$$

Note that here T stands for the absolute temperature of the heat reservoirs; it is only in the case of (10) that T can be equated with the temperature of the system.

(Uffink 2003, pp. 126-127)

Since the integral $\oint_{\mathscr{C}} \frac{đQ}{T} = 0$ whenever the body, evolving from an initial state A_0 through any series of other states, returns to A_0, the integrand "must be the total differential of a quantity that depends solely on the present state of the body and not on the way by which it has reached that state" (Clausius 1865, §14). Clausius designates this quantity by S and calls it 'entropy' (*Entropie*, "from the Greek word

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all temperatures, it would have to be admitted that a self-acting machine might be set to work and produce mechanical effect by cooling the sea or earth, with no limit but the total loss of heat from the earth and sea, or, in reality, from the whole material world" (1882, p. 179n.)

80 I have renumbered the equations in Uffink's text. The symbol đ indicates that đQ might not be an exact differential. 'Reversible' here translates 'umkehrbar', as defined by Clausius (1864, p. 251): a process is reversible if it proceeds so slowly that the system always remain close to equilibrium; see Uffink 2001, p. 384. (Clausius' text is given by Uffink on p. 335.)

τροπή, transformation"—ibid.). Therefore, we have that

$$dS = \frac{dQ}{T} \tag{12}$$

> or, if we suppose that this equation is integrated for a series of reversible transformations, through which the body passes from the initial state to its present state, and if we denote by S_0 the value of S for the present state, then
>
> $$S = S_0 + \int \frac{dQ}{T} \tag{13}$$
>
> (Clausius 1865, §14)

The Second Law can then be restated as saying (i) that the entropy of a heat engine operating under conditions of maximal efficiency remains constant in each cycle and (ii) that if the engine works under any other conditions its entropy necessarily increases.[81] However, to speak of "the entropy of the universe" as Clausius went on to do right away (see footnote 74) is only an exercise in fanciful *Naturphilosophie*. Not only did Clausius lack any empirical warrant for his cosmic version of the law. The very science of thermodynamics was wholly focused on small thermally isolated bodies whose volume and shape can be altered adiabatically by outside intervention, and the concept of temperature and the related concept of entropy were defined only for systems in a state of equilibrium which cannot be seriously ascribed to the universe as we know it. The rigorous formulation of thermodynamics, pioneered by Gibbs (1876), carried out by Carathéodory (1909) and recently perfected by Lieb and Yngvason (1999) has made the Second Law "independent of models [...], Carnot cycles, ideal gases and other assumptions about such things as heat, temperature, reversible processes, etc." (Lieb and Yngvason 2003, p. 147), but still defines "the additive and extensive entropy function *S*" only for equilibrium states.[82]

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81 Uffink (2003, p. 127) recalls, however, that Kelvin never mentions the inequality (11) from which (ii) follows, and indeed calls eqn. (10) "the full expression of the second thermodynamic law".

82 More significantly, perhaps, for the present discussion: the rigorous treatment of thermodynamics *excludes* very small and very large material systems from its scope. "Physically speaking a thermodynamic system consists of certain specified amounts of

Although Clausius and Kelvin embraced the conception of heat as a kind of motion, initially they did not agree on what kind of motion it was. While Kelvin was inclined throughout his life to view matter as being ultimately continuous,[83] Clausius (1857) sought to derive the thermal behavior of gases from the hypothesis that a gas consists of “molecules”, conceived as very small, perfectly elastic spheres that move freely, without interacting among themselves.[84] Clausius (1858) rectified the latter highly unrealistic assumption by making allowance for intermolecular collisions, and calculated the mean free path of each molecule. The molecular-kinetic theory of heat took a big stride forward in a paper read in September 1859 to the British Association by 28-year old James Clerk Maxwell (1860). By boldly resorting to considerations of probability (which Clausius had timidly broached) in the discussion of velocity changes in molecular collisions, Maxwell derived “the Final Distribution of Velocity among the Molecules of Two Systems acting on one another by any Law of Force” (Maxwell 1866; in Maxwell 1890, 2: 43). This was subsequently modified by Ludwig Boltzmann (1868) and is therefore known as the Maxwell-Boltzmann distribution law. Maxwell and Boltzmann became thus the founding fathers of classical statistical mechanics.[85]

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different kinds of matter; it might be divisible into parts that can interact with each other in a specified way. […]. Our systems must be macroscopic, i.e, not too small. Tiny systems (atoms, molecules, DNA) exist, to be sure, but we cannot describe their equilibria thermodynamically […]. On the other hand, systems that are too large are also ruled out *because gravitational forces become important*. […] The conventional notions of ‘extensivity’ and ‘intensivity’ fail for cosmic bodies.” (Lieb and Yngvason 1999, p. 13; my italics).

83 In the Baltimore lectures of 1884, while presenting a model of an elastic solid built from bell cranks and springs, Kelvin asserts emphatically: “The molecular constitution of solids supposed in these remarks and mechanically illustrated in our model is not to be accepted as true in nature” (Kargon and Achinstein 1987, p. 110).

84 Clausius says that he was inspired by Krönig (1856), whose molecular-kinetic explanation of thermal behavior assumed however that each molecule in his model moved in a direction perpendicular to one of the walls of a cubic container. The molecular theory of gases can be traced back to Daniel Bernoulli (1738, §10). Versions of it were put forward by Herapath (1821) and Waterston (1846), but met a generally cold reception. See Brush (1976).

85 Boltzmann’s contribution is eloquently described by Uffink (2006, §4), where one will also find abundant references for further study. See also Uffink 2004

In the next few decades, Boltzmann vigorously pursued the "reduction" of thermal physics to classical mechanics.[86] As a part of this program, he introduced a generalized concept of entropy, which is also applicable outside states of equilibrium, and which allegedly supplied a statistico-mechanical foundation for time's arrow. The new concept turned up in connection with Boltzmann's proof that any gas, "whatever may be the initial distribution of kinetic energy" among its molecules, must in the long run approach the Maxwell-Boltzmann distribution and, once it is reached, keep it for ever. Despite the ambitious generality of the phrase I have quoted from Boltzmann (1872, in Brush 2003, p. 291),[87] his argument actually depends on several restrictive assumptions. Some of these are eventually relaxed or are at least declared relaxable, but others remain in place and determine the scope both of the said proof and of the ensuing demonstration that the (generalized) entropy of the gas continually increases until the gas acquires the Maxwell-Boltzmann distribution, and is constant thereafter. The following inescapable conditions are explicitly mentioned by Boltzmann:

(i) The gas consists of a large but finite number of molecules insulated and confined by rigid walls in a large but finite space *R*.

(ii) The molecules interact according to an unspecified law of force, which is however the same for all, it being assumed that "that the force between two material points is a function of their distance, which acts in the direction of their line of centers, and that action and reaction are equal" (1872, in Brush 2003, p. 279).

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86 I surround 'reduction' with shudder quotes because, contrary to many philosophers of my generation, I feel no sympathy for the idea of deriving the fullness of experience from dreams of reason. The obstacles met (and not overcome) by Boltzmann and his successors in their attempted reduction of thermodynamics to statistical mechanics are discussed with great acuity by Sklar (1993), Ch. 9, especially pp. 345-373. Sklar concludes equanimously (yet, I suppose, not without irony): "If we wish to claim that thermodynamics is reducible to statistical mechanics, we must have a subtly contrived model of reduction in mind."

87 As translated by Brush. Boltzmann's conclusion reads thus in German: "Es ist somit strenge bewiesen, daß, *wie immer die Verteilung der lebendigen Kraft zu Anfang der Zeit gewesen sein mag*, sie sich nach Verlauf einer sehr langen Zeit immer notwendig der von Maxwell gefundenen nähern muß" (Boltzmann 1909, 1:345; I italicize the phrase in question).

(iii) Interaction occurs only when the interacting molecules are very close and is therefore called "collision" by Boltzmann. Most of the time, however, the molecules move freely, i.e. with constant velocities along straight lines.

(iv) The probability that any particular molecule initially moves in a particular direction is the same as the probability that it moves in any other direction. (This can be formulated more precisely thus: let $\mathbf{x}$ denote the initial position of an arbitrary molecule; then, the probability that the unit vector $\dot{\mathbf{x}}/|\mathbf{x}|$ lies inside a particular solid angle α with its vertex at $\mathbf{x}$ is proportional to the size of α).

(v) The initial distribution of kinetic energies among the molecules is uniform on R. The exact meaning of this condition is explained by Boltzmann as follows: pick a connected space $r \subset R$ of any shape and unit volume; let $f(x,t)$ denote the number of molecules in r whose kinetic energy at time t is any real number in the interval $(x,x+dx)$; then, the distribution f is said to be uniform on R at time t if, for every real number x, the number $f(x,t)$ does not depend on the shape or the location of the unit volume space $r \subset R$.[88]

Uffink (2006, p. 45) mentions two additional assumptions that Boltzmann does not state but which he uses in his proof:

(vi) The distribution f is represented by a differentiable function, which Boltzmann also designates by f (without further ado); the number of molecules in R must therefore be large enough for this approximation to be viable.

(vii) f is allowed to vary only as a result of binary interactions; therefore the density of the gas must be low enough for n-particle collisions ($n > 2$) to be extremely rare. On the other hand, it cannot be so low that even 2-particle collisions are too infrequent for f to change.

According to Boltzmann "it is clear" that conditions (iv) and (v) will continue to hold forever, if they hold initially. I confess that I do not find this self-evident. I therefore tend to agree with Uffink when he lists the persistence in time of conditions (iv)-(vii) as a third unstated assumption (2006, p. 45). Boltzmann

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88 Condition (v) is tantamount to what Boltzmann (1964), pp. 40-41, describes as a state of *molecular disorder*.

argues that if conditions (iv) and (v) are not met initially, they will be satisfied "after a very long time", for then "each direction for the velocity of a molecule is equally probable" (1872, in Brush 2003, p. 267) and "each position in the gas is equivalent" (ibid., p. 268). Apparently, he thinks that one may in every case regard such "very long time" as already elapsed before whatever instant is the initial one in that case.

From these essential assumptions, plus a few other inessential ones,[89] Boltzmann is able to derive a differential equation for the distribution function $f(x,t)$, on whose left-hand side stands the partial derivative $\frac{\partial f(x,t)}{\partial t}$ and whose right-hand side sports a double integral. This differential equation is known as the *Boltzmann transport equation*. For brevity's sake, I shall denote $f(x,t)$ by $f_{MB}(x,t)$ if $f(x,t)$ corresponds to the Maxwell-Boltzmann distribution. It can be easily shown that $\frac{\partial f_{MB}(x,t)}{\partial t} = 0$. Thus, after the distribution f_{MB} is reached, it will never change.

By deft manipulation of his transport equation, Boltzmann (1872) proved that the quantity

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89 The provisional assumptions that Boltzmann invokes in his detailed proof (1872, § I), but which he later removes or pronounces removable, include the following:

(viii) All molecules in R are monoatomic and equal to one another. In § IV, Boltzmann extends his results to a gas consisting of polyatomic molecules of the same kind, "i.e. they all consist of the same number of mass-points, and the forces acting between them are identical functions of their relative positions" (1872, in Brush 2003, p. 318), the mass-points or atoms being held together by a force that depends only on their mutual distance and acts along the line that joins them. Then, towards the end of § IV, he observes that his calculation of entropy for such a polyatomic gas "can be carried out in the same way if several kinds of molecules are present in the same container" (1872, in Brush 2003, p. 334); in this case, the total entropy of the system is equal to the sum of the entropies computed for each subsystem formed by molecules of the same kind.

(ix) The wall of the container that encloses the gas reflects the molecules like elastic spheres. Boltzmann adds: "Any arbitrary force law would lead to the same formulae. However, it simplifies the matter if we make this special assumption about the container" (1872, in Brush 2003, p. 267).

$$H = \int_0^\infty f(x,t)\left\{\log\left[\frac{f(x,t)}{\sqrt{x}}\right]-1\right\}dx \tag{14}$$ [90]

"can never increase, when the function $f(x,t)$ that occurs in the definite integral satisfies" the Boltzmann equation (1872, in Brush 2003, p. 281). This is Boltzmann's famous (some might say "notorious") *H*-theorem. By virtue of it, the function *H* defined as in eqn. (14) in terms of any solution f of the Boltzmann equation satisfies the inequality

$$\frac{dH}{dt} \leq 0 \tag{15}$$

with equality holding if and only if $f = f_{MB}$. The latter is, of course, the equilibrium case, for which alone thermodynamic entropy is defined. Boltzmann noted that precisely in this case, *H* is proportional to the entropy. He therefore introduced a generalized concept of entropy, which is related with *H* by the same proportionality factor in the non-equilibrium cases, where thermodynamic entropy is not defined. Eqn. (15) entails then that the (generalized) entropy of any system to which it is applicable will increase while the distribution f differs from f_{MB} and therefore tends to become equal to f_{MB}, and that it will reach a maximum and henceforth remain unchanged as soon as $f = f_{MB}$. (Note that the factor of proportionality between *H* and the generalized entropy is such that the latter reaches its maximum when *H* attains its minimum).

A glance at conditions (i)-(vii) is sufficient to persuade one that a proof based on them cannot lead to conclusions about the universe. Indeed, condition (i) alone should dispel any such illusion. But even if we manage to forget it —as so many writers on time's arrow have been able to do— we must still face condition (v), which, as Boltzmann (1964, p. 41) emphasizes, not only "is necessary to the rigor of the proof" but must be assumed in all applications of Boltzmann's equation. No region of the universe that contains, say, a star and a sizable chunk of interstellar space around it complies with condition (v). It may well be that "after a very long time" all such regions and the universe as a whole will meet this condition of uniformity, but it would be utterly reckless to assume, for the sake of the argument,

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[90] Boltzmann (1872) denoted this quantity by *E*. The now standard designation *H* was introduced by Burbury (1890) and adopted by Boltzmann.

that this "very long time" has elapsed already. Nevertheless, in the subsequent, at times passionate debate about the validity and meaning of the *H*-theorem, the major participants generally remained silent about the restrictions that Boltzmann's premises imposed on his conclusions. It was as if a goblin hidden in their minds had made them deaf and blind to anything that might threaten the satisfaction of their yen for global truth.

The two main objections to Boltzmann's *H*-Theorem are the *reversibility* objection, soon raised by Joseph Loschmidt (1876),[91] and the *recurrence* objection, due to Ernst Zermelo (1896*a*, 1896*b*).[92] I shall only discuss the former, which can be explained as follows. Consider an isolated, finite classical mechanical system consisting of N point-particles that meet the assumptions of Boltzmann's proof. The system's dynamical state Σ_0 (at initial time $\tau = 0$) is fully characterized by $3N$ position coordinates $q_1(0),\dots, q_{3N}(0)$ and $3N$ momentum coordinates $p_1(0),\dots, p_{3N}(0)$. If the distribution $f(0)$ differs from f_{MB}, then, according to the *H*-theorem, the value of H for this system must decrease to a minimum $H_{\min}$ which it reaches when $f = f_{MB}$ and must remain constant thereafter. Suppose this happens at time $\tau = t$. The laws of mechanics determine exactly the position and momentum

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91 According to von Plato (1994), p. 85, Loschmidt's objection had already been stated by William Thomson (1874). However, all I can find in Thomson's text (as reproduced in Brush 2003, p. 351) is a clear statement of the solid ground on which the objection rests, viz. the time reversal invariance of the laws of "abstract dynamics" (Thomson's phrase), but I do not find an argument *contra* Boltzmann.

92 Zermelo's recurrence objection rests on a theorem by Poincaré (1890), which can be stated as follows: *In a system of mass-points under the influence of forces that depend only on position in space, any state of motion must recur infinitely many times, at least to any arbitrary degree of approximation, if the position and momentum coordinates cannot increase to infinity* (Zermelo 1896*a*, in Brush 2003, pp. 382-383). Since H depends on the distribution f, which in turn depends on the momentum coordinates, Zermelo argued that H must therefore return infinitely many times to its initial value, contrary to the original Boltzmann claim that H decreases steadily until it reaches a minimum which it retains. According to Mackey (1992, p. 45). "Zermelo was right in his assertion that the entropy of a system whose dynamics are governed by Hamilton's equations, or any set of ordinary differential equations for that matter, cannot change", but was wrong to base his argument on Poincaré's theorem, Zermelo's fallacy lying on "his implicit assumption that densities (on which the behavior of entropy depnds) will behave like points".

coordinates $q_1(t),\dots, q_{3N}(t), p_1(t),\dots, p_{3N}(t)$ that characterize the state Σ_t of the system at that time. Consider now a system whose state Σ_0' at $\tau = 0$ is characterized by the coordinates $q_i'(0) = q_i(t)$, $p_i'(0) = -p_i(t)$ $(1 \leq i \leq 3N)$. According to the laws of mechanics the evolution of this system from $\tau = 0$ to $\tau = t$ is exactly the reverse of that of the system we considered first. Therefore, its state Σ_t' at $\tau = t$ will be given by $q_i'(t) = q_i(0)$, $p_i'(t) = -p_i(0)$ $(1 \leq i \leq 3N)$. Clearly, for this system, the distribution $f'(0) = f_{MB}$ and the initial value of $H = H_{\min}$, whereas the distribution $f'(t) \neq f_{MB}$ and the final value of H will exceed its initial value. Thus, if the H-theorem holds for our first system, then it does not hold for the second one, although this is a *bona fide* classical system that satisfies the theorem's assumptions. Therefore, if P stands for "The H-theorem is true of any system that meets conditions (i)-(vii)", then, by the familiar tautology $(P \supset \neg P) \supset \neg P$, statement P is plainly false.

Boltzmann must have been cut to the quick by Loschmidt's objection for, although he explained it faithfully and clearly and, in the end, essentially granted it, he described it as "an interesting sophism" and set out, without more ado, "to locate the source of the fallacy in this argument" (1877*a*, in Brush 2003, p. 365). However, Boltzmann's line of defense depends entirely on the fact, apparently overlooked by Loschmidt, that some of the premises from which the H-theorem is proved are statements of probability. As a consequence of this fact, the H-theorem cannot be regarded as a universal law of nature, but only as an overwhelmingly probable statistical generalization. Thus, Boltzmann does not actually disclose a fallacy at the heart of Loschmidt's argument, but rather a colossal misunderstanding, for which Boltzmann himself was partly to blame, since he had not sufficiently emphasized the unorthodox meaning and reach of molecular-kinetic statements in his former publications.[93] To elucidate it, one usually distinguishes between the microstates and the macrostates of a mechanical system of N particles. The *microstate* of the system at time t is identified by the exact values of the $6N$ coordinates $p_i(t)$, $q_i(t)$ $(1 \leq i \leq 3N)$. The set of all such $6N$-tuples fills the

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93 Jan von Plato (1994), p. 79, believes that "Boltzmann had by 1872 already a full hand against his future critics", for he was sufficiently explicit about the statistical nature of his premises and conclusions. For a more balanced judgment concerning Boltzmann's position before and after 1876, see Uffink (2006), §4.2. No matter when Boltzmann got his full house, what Loschmidt could show against it looks to me like a straight flush.

system's *phase space* $\mathcal{S} \subseteq \mathbb{R}^{6N}$. A *macrostate* of the system is an open set $M \subset \mathcal{S}$ formed by microstates that share the values of certain physical quantities one may plausibly regard as macroscopic observables. Evidently, if N is the number of molecules in a mere cubic meter of gas (at normal pressure and temperature), it is absolutely impracticable to identify the microstate of our system. Therefore, molecular-kinetic theory cannot predict the evolution of microstates according to the laws of mechanics, but must rely on statistical reasoning concerning the macrostates. To get started, this kind of reasoning requires the definition of a probability measure μ on the phase space.[94] Boltzmann assumes that every conceivable microstate $\langle q_1,\ldots,q_{3N},p_1,\ldots,p_{3N}\rangle \in \mathcal{S}$ is equally probable; judging by his reasoning, it appears that he took this to mean that μ is uniformly distributed over $\mathcal{S}$. Hence, for any open set $M \subset \mathcal{S}$, $\mu(M)$ is proportional to the Euclidean volume of M. There are, of course, no a priori grounds for this assumption, but it can somehow be justified a posteriori by the predictive success of inferences based on it. If $M_{MB} \subset \mathcal{S}$ is the set of microstates characterized by the Maxwell-Boltzmann distribution f_{MB}, it is easy to show that $\mu(M_{MB})$ is very large, indeed very much larger than the measure of any other macrostate $M \subset \mathcal{S}$. Therefore, according to Boltzmann, if our system is initially in a macrostate M_I for which the distribution $f \neq f_{MB}$, then almost every microstate in M_I must eventually evolve into a microstate belonging to M_{MB}. This evolution will take more or less time depending on the microstate, but while the evolution lasts the function H will steadily decrease until it reaches its minimum $H_{\min}$, as f becomes equal to f_{MB}. Prompted by Loschmidt's challenge, Boltzmann wrote a classical paper "On the relation between the second principle of the mechanical theory of heat and the probability calculus with respect to the theorems concerning thermal equilibrium" (1877*b*), followed by "Further remarks on some problems of the mechanical theory of heat" (1878), where he gave the definition of the entropy S of a system in terms of the probability W of its mechanical state which is carved on Boltzmann's tombstone: $S = k \log W$.

From the overwhelming value of $\mu(M_{MB})$ Boltzmann infers that it is enormously likely that a point in a low probability macrostate is the starting point of an evolution leading to a microstate in M_{MB}. His inference rests on the notion that the length of

94 Since measure theory and the measure-theoretic approach to probability were still unborn in the 1870's, my manner of speaking here is surely anachronistic. Nevertheless, I expect it to be helpful.

time that a mechanical system spends in a macrostate M is proportional to its probability $\mu(M)$. Fortunately, the present discussion does not require that we go into the foundations and difficulties of this notion.[95] We can simply accept it and yet conclude that Boltzmann's defense is powerless against Loschmidt's objection. For ease of reference, I introduce a few symbols. I shall write (i) M_0 for the particular non-equilibrium macrostate I choose for consideration, (ii) $M_{0,t}^{\to MB}$ for the proper subset of M_0 formed by the starting points of phase space trajectories that leave M_0 at time $\tau = 0$ and reach M_{MB} at time $\tau = t$; and (iii) $M_{MB}^{\leftarrow 0,t}$ for the set of points at which the phase trajectories initiated in $M_{0,t}^{\to MB}$ reach M_{MB}. I shall denote by $\mathcal{R}$ both (iv) the transformation of the phase space $\mathcal{S} \subseteq \mathbb{R}^{6n}$ defined by $\langle q_1,\ldots,q_{3N},p_1,\ldots,p_{3N}\rangle \mapsto \langle q_1,\ldots,q_{3N},-p_1,\ldots,-p_{3N}\rangle$ and (v) the mapping induced by this transformation in the power set $\wp\mathcal{S}$. The use of the same symbol for designating two such mappings is of course standard; however, to avoid needless confusion, I write, as usual, $\mathcal{R}(x)$ for the value of mapping (iv) at a microstate $x \in \mathcal{S}$, but I write $\mathcal{R}M$ for the value of (v) at a set $M \subset \mathcal{S}$. Consider in particular the set $\mathcal{R}M_{MB}^{\leftarrow 0,t}$. This set is obtained by reversing —*à la* Loschmidt— the velocity of each particle in each microstate comprised in $M_{MB}^{\leftarrow 0,t}$. According to the laws of mechanics, the trajectories initiated in $\mathcal{R}M_{MB}^{\leftarrow 0,t}$ inexorably lead in a time interval of length t to states belonging to the non equilibrium macrostate $\mathcal{R}M_0$. During that time the function H increases steadily above $H_{\min}$. By a well-known theorem named after Liouville, $\mu(M_{MB}^{\leftarrow 0,t}) = \mu(M_{0,t}^{\to MB}) \leq \mu(M_0) << \mu(M_{MB})$. The mapping $\mathcal{R}\colon \wp\mathcal{S} \to \wp\mathcal{S}$ preserves the measure μ. Therefore $\mu(\mathcal{R}M_{MB}^{\leftarrow 0,t}) = \mu(M_{MB}^{\leftarrow 0,t}) = \mu(M_{0,t}^{\to MB})$. Thus, the probability that our mechanical system will evolve in time t from an equilibrium state in which $H = H_{\min}$ toward a

[95] In his earlier writings on the subject, Boltzmann apparently based the idea that the length of time spent by the system in a macrostate is proportional to the probability of this macrostate on the so-called *ergodic* hypothesis, according to which the trajectory of the system in phase space passes through every point of the hypersurface corresponding to the system's energy. Since this is mathematically impossible —as Rosenthal (1913) and Plancherel (1913) independently proved— it has been suggested (already by Paul and Tatiana Ehrenfest in 1912) that Boltzmann was actually thinking of the *quasi*-ergodic hypothesis, by which the system comes as close as you wish to every point of the energy hypersurface. This is not impossible, but it does not yield the desired consequences regarding the probability distribution. See Uffink 2006, §4.1.

non-equilibrium state in which $H > H_{min}$ is not a whit smaller than the probability that it will evolve in time t from the particular non-equilibrium macrostate M_I to the equilibrium state M_{MB}, while H shrinks to H_{min}. If Boltzmann's statistical reasoning is valid, it proves (*a*) that $H = H_{min}$ for overwhelmingly long periods of time and (*b*) that, when $H > H_{min}$, H tends with overwhelmingly great probability to return to its minimum value. Nevertheless, the time reversal invariance of the laws of mechanics makes the following conclusion inevitable: the probability of evolutions that lead in any given time t from an (admittedly improbable) non-equilibrium macrostate M_0 to equilibrium, as H decreases, is precisely equal to the probability of evolutions that lead in time t from a (likewise improbable) subset of the equilibrium macrostate M_{MB} to the non-equilibrium macrostate $\mathcal{R}M_0$, while H increases. Boltzmann's appeal to statistics and probability does not rescue the H-theorem from Loschmidt's attack.

Boltzmann in effect granted this when he brought up, towards the end of his reply to Loschmidt,

> a peculiar consequence of Loschmidt's theorem, namely that when we follow the state of the world into the infinitely distant past, we are actually just as correct in taking it to be very probable that we would reach a state in which all temperature differences have disappeared, as we would be in following the state of the world into the distant future.
>
> (Boltzmann 1877*a*; in Brush 2003, p. 367)

This amazing result was reasserted by Boltzmann in his later writings and became entrenched in the philosophical literature of the 20th century (see Reichenbach, 1956; Grünbaum 1973). I can only regard it as a piece of intellectual bravado, for which Boltzmann could not claim the faintest empirical support.[96] He probably

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96 A few lines further on Boltzmann adds: "Perhaps this reduction of the second law to the realm of probability makes its application to the entire universe appear dubious". This apparent concession to ordinary intelligence is countered at once by the following remark: "Yet the laws of probability theory are confirmed by all experiments carried out in the laboratory" (1877*a*; in Brush 2003, p. 367). This is true, but then to extend these laws from the lab to the entire universe one would have to define a statistical ensemble of which the universe itself is an instance, and, as far as I can see, any attempt to do so cannot fail to be arbitrary.

thought that none would ever be forthcoming, for he dared to offer the following explanation of the ostensible time-directedness of thermal phenomena:

> One can think of the world as a mechanical system of an enormously large number of constituents, and of an immensely long period of time, so that the dimensions of that part containing our own "fixed stars" are minute compared to the extension of the universe; and times that we call eons are likewise minute compared to such a period. Then in the universe, which is in thermal equilibrium throughout and therefore dead, there will occur here and there relatively small regions of the same size as our galaxy (we call them single worlds) which, during the relative short time of eons, fluctuate noticeably from thermal equilibrium, and indeed the state probability in such cases will be equally likely to increase or decrease. *For the universe, the two directions of time are indistinguishable, just as in space there is no up or down. However, just as at a particular place on the earth's surface we call "down" the direction toward the center of the earth, so will a living being in a particular time interval of such a single world distinguish the direction of time toward the less probable state from the opposite direction (the former toward the past, the latter toward the future). By virtue of this terminology, such small isolated regions of the universe will always find themselves "initially" in an improbable state.* This method seems to me to be the only way in which one can understand the second law —the heat death of each single world— without a unidirectional change of the entire universe from a definite initial state to a final state.
>
> (Boltzmann 1964, pp. 446-447; my italics)

Since the forward and backward time orders are not just inequivocally distinguished by an apposite conventional terminology but are also experienced (in German one would say *erlebt*) as unmistakably different, Boltzmann is telling us here that it is downright impossible for someone not just to *describe* but also to *observe* an actual decrease of entropy (or a corresponding increase of H). Should it ever happen that we are actually involved in such a process, so that, say, the entropy of our surroundings was smaller yesterday than the day before yesterday, we would *perceive* yesterday as being tomorrow, and the day before yesterday as being the day after tomorrow. This mind-boggling idea came up about the same time as the hypothesis put forward by Lorentz and FitzGerald to explain Michelson's failure to detect the relative motion of the Earth and the ether (§2). We have here two cases in which first-rate scientists sought to overcome a flagrant conflict between theory and experience by attributing to nature some kind of systematic elusiveness. But surely Boltzmann went a long step farther than his colleagues. The Lorentz-FitzGerald contraction hypothesis only extended the scope of known forces, ascribing to them a new, hitherto unsuspected effect, which should be anyway open to ordinary experimental control. But Boltzmann postulated a radical change of language and indeed of consciousness to ensure that the phenomena of entropy decrease, which turned out to be neither impossible nor unlikely according to his statistical reasoning, remained unobserved forever.

Yet Boltzmann did not surrender his good sense to this fancy. In his reply to Zermelo (1896*b*), which contains a passage that the last quotation repeats almost verbatim, this is preceded by a warning "against placing too much confidence in the extension of our thought pictures beyond the domain of experience". Nevertheless, he adds, "with all these reservations, it is still possible for those who wish to give in to their natural impulses to make up a special picture of the universe". He proposes two such pictures: either (α) "the entire universe finds itself at present in a very improbable state" or (β) it "is in thermal equilibrium as a whole and therefore dead" except in "relatively small regions of the size of our galaxy (which we call worlds) which, during the relatively short time of eons, deviate significantly from thermal equilibrium" (Boltzmann 1897, in Brush 2003, p. 416). He does admit, however, that "whether one wishes to indulge in such speculations is of course a matter of taste" (ibid., p. 417).

It is told that when Loschmidt, who was Boltzmann's colleague in Vienna, first told him that his gas would return from equilibrium to its initial non-equilibrium states if all molecular velocities are reversed, Boltzmann replied to him: "Well, you try to reverse them!" Brush (1976, p. 605), from whom I have the story, has good reasons to think it is apocryphal. But it does drive home the gist of Boltzmann's statistical approach to time asymmetric thermal phenomena. Since non-equilibrium states are inordinately improbable, it is extremely difficult to pick out in the shoreless ocean of equilibrium states the pitifully small subsets from which non-equilibrium states would be reached within a sensible length of time. Therefore, although according to the laws of mechanics it is perfectly possible for Boltzmann's function H to increase above H_{min} in a thermally isolated gas in equilibrium, for all practical purposes it is impossible to prepare an experiment in which this will happen. In several passages, Boltzmann fondly hints at this fact, with some rhetorical flourish. Yet this very fact raises a big question for him, namely: Why, if states in which $H > H_{min}$ are so enormously improbable, is it fairly easy to pinpoint physical systems that are actually in such states and to isolate them so that they evolve in a fairly short time to an equilibrium state in which $H = H_{min}$?

The currently fashionable reply to this question has been chiefly promoted by the great Oxford mathematician Roger Penrose (1979; 1989, ch. 7; 2005, ch. 27).[97] It runs as follows. In the light of astronomical evidence, the universe can be represented to a good approximation (in the large) by an expanding Friedmann-Lemaître-Robertson-Walker (FLRW) model and this entails that, for some as yet unknown reason or no reason at all, when expansion began some 13,000,000,000 years ago the state of the universe as a whole was an extremely improbable one, that is, a state of extremely low Boltzmann entropy. Since then the entropy of the universe has steadily increased, but it still has a very long way to go before reaching the maximally probable state of thermal death, from which the universe can then only move away through short-lived local fluctuations. Thus, one of the two pictures of the world which according to Boltzmann (1897) are available to people who wish to give in to their "natural" metaphysical impulses, namely, the one labelled (α) above, can be assigned a definite content allegedly supported by scientific cosmology. I was greatly confused when I first read Penrose's proposal, for I admired his work on General Relativity (Penrose 1965, 1968; Hawking and Penrose 1970) and trusted his judgment on GR matters, but, on the other hand, I was well aware of the stringent condition of homogeneity satisfied by FLRW models. *Only* if the distribution of energy on each hypersurface of simultaneity is absolutely uniform, do the Einstein field equations admit a Friedmann-Lemaître solution. I took this to mean that FLRW models *arise* and *remain* in a state of thermal equilibrium. Indeed, in the peculiar hybrid of GR gravitational theory with non-GR particle physics known as Big Bang cosmology, "the matter (including radiation) in the early stages appears to have been completely thermalized (at least so far as this is possible, compatibly with the expansion)", for "if it had not been so, one would not get correct answers for the helium abundance" (Penrose 1979, p. 611). Indeed, the perfect thermal equilibrium between parts of the Big Bang universe which lie outside each other's horizon and therefore have never had an opportunity of interacting among themselves was initially one of the motivations of inflationary

[97] Penrose's idea is unquestioningly accepted by both Price and Callender, the two parties to the debate on "the origin of time's arrow" in Hitchcock (2004).

cosmology.[98] Of course, the universe *is not* in a state of thermal equilibrium and can be regarded as an expanding FLRW universe only through substantial simplification and idealization. Nevertheless, Penrose's claim that the world began in a state of very low entropy, based on a simplified and idealized world model that presupposes perfect uniformity, seemed to me baffling (to say the least).

Penrose argues that in Big Bang cosmology the early universe is in a very low entropy state due to the somewhat anomalous behavior of gravity with regard to entropy:[99]

> In many cases in which gravity is involved, a system may behave as though it has a negative specific heat.[100] [...] *This is essentially an effect of the universally attractive nature of the gravitational interaction*. As a gravitating system 'relaxes' more and more, velocities increase and the sources clump together – instead of uniformly spreading throughout space in a more familiar high-entropy arrangement. With other types of force, their attractive aspects tend to saturate (such as with a system bound electromagnetically), but this is not the case with gravity. Only non-gravitational forces can prevent parts of a gravitationally bound system from collapsing further inwards as the system relaxes. Kinetic energy itself can halt collapse only temporarily. In the absence of significant non-gravitational forces, when dissipative effects come further into play, clumping becomes more and more marked as the entropy increases. Finally, maximum entropy is achieved with collapse to a black hole.
>
> (Penrose 1979, p. 612; my italics).

If gravity is naturally attractive and gravitational sources tend to clump together, the thoroughly uniform, clump-free universe assumed by relativistic cosmology (Einstein 1917; Friedmann 1922, 1924) may well be said to be in a very

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98 This is nicely explained by Guth (1997), pp. 180-186.

99 At first sight the appeal to gravity is perplexing, for, as I pointed out in footnote 83, thermodynamics cannot be rigorously applied to a material system exposed to powerful gravitational action. But surely Penrose is not talking here about thermodynamic entropy, but about "some analytic quantity, usually involving expressions such as $-p \ln p$, that appears in information theory, probability theory and statistical mechanical models" (Lieb and Yngvason 1998, p. 571).

100 Penrose cites the case of black holes, which get hotter as they emit Hawking radiation, and of satellites in orbit around the earth, which speed up, rather than slowing down, due to frictional effects in the atmosphere.

improbable state (not just initially, though, but throughout its entire history). Now, attraction was always the distinguishing character of gravity, e.g. in Aristotelian physics, and under Newton's dispensation attraction became universal. But Friedmann showed, to Einstein's initial dismay, that the gravitational interaction governed by the GR field equations can be either attractive or repulsive (even if the cosmological constant $\Lambda = 0$), depending on parameters of the model. Thanks to Friedmann's mathematical discovery one could contemplate a viable scientific cosmology, which then came to fruition thanks to the physical discoveries of Hubble and of Penzias and Wilson. But neither the mathematics of GR nor all the splendid data of modern telescopy and radio-telescopy assign a probability to the initial state of an expanding FLRW universe (as compared, say, with the single-clump universe predicted by some forms of Newtonian cosmology). We encounter here the difficulty I already mentioned in footnote 96. The world teems with events, processes and situations to which the concept of probability and statististical reasoning are fruitfully applied; but to estimate the probability of the universe as whole involves, I dare say, a category mistake. Even if I am wrong, to set up the required terms of comparison —to define the probability space in which the initial state of our present universe is a point (or would it take up a region?)— clearly demands a much greater exertion of the human fancy than it is reasonable to allow in science. For a thorough demystification of the low entropy Big Bang I refer the reader to Earman (2006), a perspicuous and compelling example of powerful HPS criticism. Its timely publication allows me to put here an end to our trek.[101]

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101 A fuller discussion of time-asymmetry in statistical mechanics would have to deal with work done in the wake of Krylov's scathing criticism of the traditional foundations of statistical mechanics (Krylov 1979). A good review with abundant references will be found in Uffink (2006), §6. From my amateurish philosophical standpoint, I feel attracted by Mackey (1992), who studies the conditions for entropy increase to a maximum in closed thermodynamic systems and concludes that this is possible only under the special condition called *mixing*, and that it is necessary only under the more special condition of *exactness*, which can never hold in an *invertible* dynamical system such as those governed by Hamilton's equations. But I do not presume to pass judgment over these mathematical results. However, I was glad to note that the very notions of *invertibility* and *non-invertibility* —as defined by Mackey (1992, pp. 23f.) for Markov operators— *presuppose* the choice of a preferred direction of time.

In this §4 I have dwelt at considerable length on a matter which, to an even greater degree than those I dealt with in §§ 1-3, may be said to be a philosophical question raised by physics rather than a problem in physics. I think it is clear that the treatment of this matter can advance through conceptual criticism, perhaps *only* through it. However, it does not appear that there are any narrowly scientific problems whose solution has been delayed by befuddled notions regarding time's arrow (except insofar as its discussion has absorbed mental efforts that might have been invested in such problems). Obviously, my choice of examples owes much to my professional bias. It would be interesting to see if a physicist can come up with a difficulty in physics proper which has been solved or which one may expect to solve by dissecting concepts.[102] Unless such examples are forthcoming, we should conclude that HPS work, which Hasok Chang has aptly described as *the continuation of science by other means*, effectively continues it *for other ends* than those that working scientists usually have in mind while they get on with their daily chores.

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102 It is clear to me that some of Einstein's most decisive contributions fall squarely under this description and so do some of Newton's, and Galileo's. In our era of specialism, should it be the job of HPS to carry on with this kind of work?

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