Reference and Analysis in the Study of Time

Emily R. Grosholz
Liberal Arts Research Professor of Philosophy
Member, Center for Fundamental Theory / Institute for Gravitation and the Cosmos
The Pennsylvania State University
Member, REHSEIS / SPHERE / UMR 7912 CNRS

In his presentation, Alexis de Saint Ours often appeals to the writings of Carlo Rovelli and Julian Barbour, examining their relational, empirical, referential accounts of time. In Barbour's essay, "Relational Aspects of Time and Space," Barbour urges us to pay attention to "the discrepency between theory, in which change of variety is referred to a uniform standard, and actual practice, in which change and variety are in fact referred, not to space and time, which remain invisible, but to other variety…Ultimately, the reason why perfect uniformity cannot serve as the practical basis of a quantitative science is that perfect uniformity is nothing. As Leibniz said, 'Things which are uniform and contain no variety are never anything but abstractions, like time, space, and the other entities of pure mathematics.' You cannot measure change of variety against uniformity, because that is trying to compare something with nothing, whereas it is only possible to compare one thing with another thing." (p. 253) Here, however, Barbour is too severe. The abstractions of pure mathematics are constantly brought into rational relation with the referential discourse of mechanics concerned with clocks, atoms and large astronomical systems, despite the fact that the uniformities of pure mathematics are not empirically grounded. The issue, I think, is *how* scientists bring these two kinds of discourse into relation; good science needs them both. So Barbour is wrong to demand that referential discourse do the work of analytic discourse.

The parameter time, *t*, has haunted physical theories since Newton defined it, at the beginning of the *Principia*, as "absolute, true, and mathematical time, [which] of itself, and from its own nature, flows equably without relation to anything external," immediately distinguishing it from "duration: relative, apparent, and common time, … some sensible and external (whether accurate or unequable) measure of duration by the means of motion, which is commonly used instead of true time; such as an hour, a day, a month, a year." Thus it seems that there are two ways to investigate time: a priori as a condition of the intelligibility of things, and a posteriori in terms of the empirically determinable periodicity of things. (Another feature of time is its unidirectionality, apparent in the irrevocability of things; notably, Newton locates its asymmetry in his description of a priori time.)

In science as in ordinary life, in order to think well we must be able to refer successfully, so that we can show publicly and clearly what we are talking about. And we must analyze well (here I invoke Leibniz's definition of analysis as the search for conditions of intelligibility): we must discover productive and explanatory conditions of intelligibility for the things we are thinking about. In order to evaluate whether our means of analysis are really productive and explanatory, we need to be able to denote—publicly and clearly—what we are considering. And in order to check whether our ways of referring are really public and clear, we must set the object of investigation in a more abstract discursive context where we can study it deeply and broadly. Sometimes one task is more difficult, sometimes the other, sometimes both. The tasks themselves are very different, so it is not surprising that they generate different kinds of discourse. In referential discourse, we do our best to honor the extra-discursive world as what it is, with the best empirical means at our disposal; in analytic discourse, we treat the world abstractly, and totalize, simplify, idealize or infinitize it in the many ways that discourse allows.

The advantage of analytic discourse is that it is great for systematization and explanation, but tends to unfocus the specificity of things, to make them ghostly. The advantage of referential discourse is that it does better justice to the rich, specific variety of things, but it often loses its way in the forest of observation because of all those trees. In sum, science does its work best when we refer and analyze in tandem.

The kinds of representations that make successful reference possible and those that make successful analysis possible are *not the same*, so that significant scientific work typically proceeds by means of heterogeneous discourses that must be rationally reconciled without one collapsing into the other. The growth of scientific knowledge often stems from the work of reconciliation, whose fine structure has not received the attention it deserves. It has escaped the notice of many philosophers of science, perhaps because they are so deeply influenced by logic, which must impose homogeneity on the arguments it formalizes, so that they neglect the opportunities offered by, and the constraints imposed by, discursive heterogeneity in historically central reasoning. Philosophers need to pay more attention to this aspect of scientific practice, how scientists bring disparate discourses (and modes of representation) into rational, and productive, relation.

But what about the scientific investigation of time? Referential discourses about time include Leibniz's account of time as following upon—as posterior to—the relations of existing things, an account which (given his characterization of monads) guarantees the causal connectedness, continuity and lawfulness of physical processes, as well as their temporal asymmetry. In Leibniz's universe, everything strives, headed towards the future. Conversely, there is Boltzmann's referential definition of time, used in his argument that the apparent asymmetry of physical processes in time, suggested by the principles of thermodynamics, can be explained away if the macroscopic description of the situation is replaced by a molecular description: heat phenomena (the unidirectionality of the increase of entropy) can be reduced to and redescribed as the collisions of large numbers of particles, governed by the laws of Newtonian mechanics. The second law of thermodynamics is a statistical regularity, not a universal and necessary law like the laws of Newtonian mechanics. We are misled by the fact that we apprehend events at the macroscopic level via the approximations provided by our sense organs. In the first case, the insistence on defining time referentially results in an account of time as asymmetrical; in the second case, an equally referential account leads to the conclusion that the asymmetry of time is only apparent.

Another example is the standardization of clocks in terms of ephermeris time (reckoned in terms of the movements of the sun, moon and planets, to avoid the unpredictable irregularities of the mean solar time scale), adjusted both with respect to standards developed in relation to the cesium atomic clock, and with respect to relativistic considerations. (Julian Barbour suggests that we could move from defining ephemeris time in terms of our solar system, up to the whole cosmos—at the least the whole observable cosmos.) Conversely, other modern cosmologists suggest that we use the curvature of the universe as an observable that can stand in for time. In the first case, again, the insistence on defining time referentially results in a definition of time in terms of a clock that is essentially periodic; in the second case, it results in a definition of time that is asymmetrical and unidirectional.

What if we treat time analytically, as a condition of intelligibility of everything? Unlike his opponent Leibniz, Newton invokes analytic time instead of referential time. In Newtonian mechanics time is geometrized first of all as a line. In the diagrams of the *Principia*, the line of inertial motion (a virtual motion un-deflected by a center of force, nowhere realized in the universe) represents both the spatial displacement of a particle upon which no forces act, and the 'equable' unrolling of time. Because Newton presupposes that time is uniform and linear, and given prior to the creation of bodies and forces, he can let it play the role of independent variable with respect to which other physical parameters are differentiated and integrated. By assigning an origin and axes to the geometry of space, Newton also (as we would say now) identifies the Euclidean line with the real numbers; this convention guarantees the directionality of the line. So it seems as if Newtonian time is unidirectional. However, all the laws of Newtonian mechanics are time reversal invariant. That is, if there is an initial event A that must lead by the laws of Newtonian mechanics to another event B, then it is also the case that the event B´ (in which all particles are in the same configuration as in event B, but the velocities of all the particles are reversed) must lead to A´ (in which all particles are in the same configuration as in event A, but the velocities of all the particles are reversed). So Newton's formal treatment of time is ambiguous.

By contrast, Sadi Carnot treated time analytically in his *Réflexions sur la puissance motrice du feu*, and formulated his second law of thermodynamics to assert that the entropy of an isolated system not in equilibrium will tend to increase over time. Thus in Newton's analytical treatment of time, the temporality of mechanical systems seems to have no intrinsic direction, but in Carnot's treatment it does.

Recall that Boltzmann claimed that his referential reduction of (macroscopic) heat phenomena to (microscopic) Newtonian systems explained away the illusion of the arrow of time. But Leibniz's referential reduction of Newtonian absolute time to relational time asserted and indeed explained the arrow of time. In the theorizing of contemporary cosmologists, the referential reduction of the parameter time to the measurement of the changing curvature of the universe, or to the ephemeris time of the great clock of the universe, is similarly non-commital: in the first case we get a unidirectional time, in the second case we do not. In sum, we find that scientists who wish to define time analytically do not agree about its fundamental nature, and neither do scientists who wish to define time referentially. The arguments of scientists who define time referentially (like Barbour and Rovelli) in order to clear up the confusions of scientists who wish to define it analytically do not in fact settle the disputes, and the conflicts among scientists who define time referentially are also not settled by their abstracter colleagues who deploy analytical definitions.

However, the philosopher of science need not counsel skepticism. Our best hope of understanding time may lie in looking at what happens when referential and analytical discourses fail to be wholly reconciled: to what extent are they unified (and how is that unification possible) and where and why does that unification fail? Thus we may learn something important about time by studying the debates between Leibniz and Newton, or the current attempts of scientists to integrate quantum mechanics and general relativity. Scientific language used in the study to elaborate and systematize abstract thought is, clearly, very different from language used by scientists working in the laboratory, field and observatory. Texts that announce important ideas, bringing two or more spheres of activity into intelligible relation, are therefore typically heterogeneous and multivalent, a fact that has been missed or misunderstood by philosophers who equate rationality with the kind of discursive homogeneity required by formal logic.

Philosophers of science need to ask new questions that bring the work of combination itself into focus. Is there a useful taxonomy of strategies of combination among heterogeneous scientific discourse about the nature of time? How does it contribute to the growth of knowledge? If the tasks of analysis and reference are often disparate, we may expect that, for example, the records kept by astronomers (even when their work is informed by theory) will differ from the theorizing of physicists and natural philosophers (even when they are concerned primarily with celestial systems). The modes of representation and the idioms of mathematical and scientific expression will differ; and the explication and organization required of natural language will differ from one task to the other. What we can then expect are composite texts in both kinds of endeavor; and the nature of this composition can suggest a preliminary taxonomy of 'strategies of integration.' We need a better account of these strategies. The kinds of representations that make successful reference possible and those that make successful analysis possible are *not the same*, so that significant scientific (and mathematical) work typically proceeds by means of heterogeneous discourses that must be rationally reconciled without one collapsing into the other. The growth of scientific knowledge often stems from the work of reconciliation, whose fine structure has not received the attention it deserves.

So let us return once more to Julian Barbour. In his paper, "The Development of Machian Themes in the Twentieth Century," Barbour identifies three main strands in Mach's thought: a criticism of Newton's concepts of absolute space and time; a conviction that the task of science is not to set up theories about the world; and the suggestion that the universe can only be understood properly if it is treated as a whole. Thus Mach urges scientists to reformulate mechanics on a relational basis and to stay 'grounded,' establishing empirical connections among directly observable phenomena. These convictions go along with, oddly, a rather mystical commitment to holism. This combination led to Mach's principle (as Einstein called it) that locally observed inertial properties of particles arise not from absolute space but from "the combined effect of all the dynamically significant masses in the universe."

As the great dialectic of science and the philosophy of science proceeds from the mid-19th century to the present, Mach (and Poincaré) play a significant role, and so too do Rovelli and Barbour. I suggest, however, that all three, like Leibniz, cannot keep from elaborating an analytic discourse that stands in problematic (but productive) relation to the referential discourse they focus on, although they try to obscure the intervention of that analytic discourse. Such an account would take me beyond the bounds of this response, but one feature of Barbour's discourse mostly clearly betrays its hybridity. That is his insistence that his theory must apply to "the complete universe." (Recall his conception of ephemeris time calculated on the basis of the whole universe.) This is precisely the kind of theorization that analytic discourse offers, along with abstraction and uniformization, as Kant warned us: reason (which is about discourse, while the understanding is about perceived reality) seeks the full series of conditions and pretends that we can think them altogether. Barbour is here invoking the system of cosmological ideas, which Kant goes after in the First Antinomy, in the Transcendental Dialectic of the *Critique of Pure Reason*. In sum, this aspect of Barbour's theory, its insistence on the unity and organization of the universe, does not come from the patient measurement of stars by astronomers nor the careful recording of the ticks of the cesium atom; it comes from the speculation of analytic discourse which seeks the conditions, indeed the ultimate conditions, of the things of this world. So he too, like all good scientists, while insisting on the importance of empirical methods and the irreducible diversity of things, must also bring reference and analysis into rational relation.

This paper is the first draft of a response, and so far without a list of references, which will be added later as it is developed in tandem with the paper to which it responds.