ABSTRACTS

Joint Commission: Contributed Papers 2

The (non-Newtonian) conception of time in Hume and Einstein: Similarities and Differences

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Einstein's overt acknowledgement to "Hume, whose Treatise of Human Nature I studied with passion and admiration shortly before discovering the [special] theory of relativity," has drawn interest among contemporary historians and philosophers of physics. John D. Norton has provided a possible reading on the constructive connection of Hume to Einstein. His main thesis is that Einstein was most influenced by the way Hume saw ideas and concepts to be grounded in sense impressions. If the concept of simultaneity is grounded in sensible impressions, such as in visual sensations of immediate light flashes in two mirrors, it follows (given the two postulates of STR) that different inertial reference frames can observe the timely order of two non-causally related spatially distant events, the two light flashes, in different order. The revision of the concept of simultaneity defied the absolute Newtonian character of time. In this paper, I will take a closer look at Hume's and Einstein's conceptions of time. First I argue that there are important similarities between their conceptions. Both Hume and Einstein understand time in relationist terms: the idea or concept of time refers to objects. There is no meaning in speaking of Newtonian "absolute duration," or "time in and of itself and of its own nature, without reference to anything external." Duration and simultaneity are not absolute, since they are dependent on the observer/reference-object relation. However, I argue that in Hume's philosophy of time the relation between an observer and a reference object is not the same as in Einstein's STR. To Einstein (in STR), time is an event which can be expressed in mathematical terms by the Lorentz transformation equations. Hume does not understand time as an event, but rather as an abstract idea of succession or change which is caused by discretely disposed indivisible moments.

What happened to phlogiston? Reconsidering the Chemical Revolution

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Major theory-shifts in science, such as the transition in the late 18th century from a phlogiston-based chemistry to something more like modern chemistry, raise a number of philosophical questions. Among these are the question of accounting for the shift, and whether it can be regarded as rational. Another is whether the discarded theoretical commitments provide fodder for a pessimistic meta-induction. This talk looks at the so-called Chemical Revolution with these questions in mind. The shift involved a shift in multiple theoretical presuppositions, in commitments about the basic substances that

make up the world, and also a shift in methodology. Philosophical attempts to account for the shift have tended to be holistic. However, the components are logically independent, and it is possible to accept certain aspects of Lavoisier's novel approach to chemistry while rejecting others. This is key to understanding the shift. At any given point in science, certain propositions are more firmly established on the basis of evidence, others, more speculative. Moreover, such judgments need not be made only in hindsight; they are present in attitudes of scientists at the time. I will argue that Lavoisier provided convincing evidence that (contrary to accepted versions of phlogiston theory), in combustion a component of the air combines with the combustible material, and this is what Priestley had referred to as "dephlogisticated air" and Lavoisier renamed "oxygen." One can accept this proposition without accepting all of Lavoisier's theoretical edifice, and, indeed, without accepting the whole of Lavoisier's theory of combustion. However, acceptance of this proposition eventually led to abandonment of phlogiston. Implications for scientific realism will be drawn. Of the entities posited by a mature science, not all are equal; some are more firmly established on the basis of evidence, others, more speculative. I will argue that phlogiston was always somewhat speculative.

On Richard Cantillon, Or How the Economic Science Has Acquired Its Method and Methodology

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William S. Jevons called Cantillon's Essai sur la Nature du Commerce én Général "the Cradle of Political Economy". Although Schumpeter rejected this metaphor, he did recognize that the unique Cantillon's feature is his systematic method of investigation and presentation of economic reality. It is now acknowledged that Cantillon's strong influence goes through the Physiocrats to Adam Smith. This paper argues that the impact of Cantillon's Essai was not confined to theory: it provided a template for economic thinking for many generations of economists and presented the first conscious attempt to formulate methodological principles of the new science. The paper is intended to systematize both declared and actual methodology of Richard Cantillon, his critical remarks addressed to earlier authors, the ways how he isolated economic phenomena and structured economic realm. Cantillon widely used thought experiments and verbal modeling, but never forgot to verify them with actual, or historical facts, or numerical examples. He built deterministic models, but did not fail to remind readers of uncertainties of the real world preventing 'exact calculations'. He preferred 'to be nearer enough to the truth' rather than to seek for 'not very necessary exactitude'. He was consciously abstract in order to come closer to understanding concrete phenomena. He appealed to the ideal of value-free science, and violated it, as did later plenty of his successors. It is suggested that Cantillon's most durable contribution to economic science comes from ontological assumptions underlying his theorizing. These were borrowed by later theoreticians to become essential, taken for granted preconceptions of economic profession. Some of them were dropped by the successors and later reintroduced into economic theory without important insights implicit in Cantillon's vision. It refers primarily to uncertainty which was incorporated into

Cantillon's ontology through entrepreneurial activities, later disappeared from economic theories for about two centuries.

Charles Darwin and Sir John F. W. Herschel: Nineteenth-Century Science and its Methodology

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James Lennox has argued that if it is indeed possible to say that Darwin was an innovator in his field, "it is as a philosopher and methodologist." Indeed, there is a bewildering variety of claims connecting Darwin to nineteenth-century philosophy of science – including to Herschel, Whewell, Lyell, German Romanticism, Comte, and others. I argue here that, whatever is to be made of the other connections, Herschel's influence on Darwin is undeniable. The form of this influence, however, is often misunderstood. While Jon Hodge has worked out a careful interpretation of both Darwin and Herschel over a series of some half-dozen articles, this interpretation misreads Herschel's use of the vera causa principle, as well as his discussion of the role of hypotheses in scientific theory construction. Darwin learned from Herschel precisely the way in which one should frame a scientific argument – first by proposing a speculative hypothesis, grounded on an extensive analogical basis, then by demonstrating the adequacy of that hypothesis to produce the desired effect, and lastly its ability to account for a wide variety of phenomena which it was not originally proposed to explain. This new reading of Darwin's relationship to Herschel adds to the usual collection of sources Herschel's own marginalia to Darwin's Origin, from the archives at the University of Texas. It goes farthest, I argue, toward explaining why Darwin wrote the Origin in the way that he did, as well as why Herschel's criticism of his theory as "the law of higgeldy-piggeldy" would have stung Darwin so deeply.