

A further reply on Thomas Kuhn

By William Whitlow
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The following contribution from William Whitlow extends a discussion that began with his article last fall, Thomas S. Kuhn, post-modernism and materialist dialectics, and continued with a response by Philip Guelpa, A friendly response to William Whitlow's comments on Thomas Kuhn.

Dear Philip,

Thank you for your response to my article on Thomas Kuhn, replying in turn to a Kuhn supporter, David Lemberg. I am pleased to discuss these issues further, and your opposition to postmodernism and Kuhn's attack on objectivity is to be welcomed.

I am puzzled that you think I am discarding "the baby of scientific revolutions with the Kuhnian bathwater." I was attempting to explain briefly the dialectical materialist approach to the natural sciences and contrasting this to the irrationalism of Thomas Kuhn and recommending some articles by Leon Trotsky to clarify these questions [1].

Dialectical philosophy, fully developed first by G.W.F. Hegel in an idealist form, was aptly named by the Russian revolutionary Alexander Herzen, the "Algebra of Revolution" [2]. Hegel and other thinkers of his day were driven in the direction of dialectics in part by the insurmountable problems thrown up by previous philosophers such as Kant and by the great revolutionary developments of the period in which they lived: the French and American revolutions and the Industrial Revolution all demanded a dialectical philosophical response from the most advanced thinkers of the day. They were confronted by revolutionary advances in natural science; electricity, chemistry, and geology were all developing rapidly. Science was moving into areas where the mechanical materialist philosophy associated with the French *philosophes* of the Enlightenment was no longer adequate. Ideas only hinted at by Diderot, Rousseau and Vico had to be rigorously explored. The dialectical thought of these Enlightenment figures would be developed in a materialist form by Marx and has become the supreme philosophy of revolution, both social and natural scientific.

Without wanting to belabour the point, one could cite countless examples in the writings of Marx, Engels, Lenin and Trotsky showing their profound interest in the scientific revolutions of their day, drawing lessons from these revolutions for their philosophical outlook, but also—and here one thinks especially of Engels in *Dialectics of Nature* and Lenin in *Materialism and Empiriocriticism*—criticising the views of philosophers and scientists who had drawn idealist and even mystical conclusions from the current scientific revolutions.

If you read the few articles by Trotsky I recommended, you will see references to the importance of the scientific revolutions associated with Charles Darwin in biology, Mendeleev's introduction of the Periodic Law in chemistry, the discovery of radioactivity and the transformations of chemical elements, and even developments in psychology associated with the very different approaches of Freud and Pavlov. In the last article he refers to the revolution that was taking place in physics at the time he was writing—now known as quantum mechanics—and looks forward to its application as the "mighty hidden energy" of the atom.

So if we want to discuss great revolutionary developments in the science of the twentieth century, such as those in geology and biology to which you refer, it can be better done within the dialectical tradition of

Hegel and Marx than within the entirely anti-Marxist neo-Kantian tradition represented by Kuhn. This is the only way to educate a new generation of scientists and those interested in science in the philosophy of Marxism. If, despite his ignorance of this rich history of Hegelian and Marxist philosophy, Thomas Kuhn had contributed something of importance to a Marxist conception, one could recommend a study of his writing. But as he is advocating a core view that is diametrically opposed to materialist dialectics, surely it is irresponsible, as you seem to want to do, to recommend him. Students of the social sciences and humanities will doubtless encounter his work on their college courses; it is not our task as revolutionaries to endorse the official curriculum of the institutions they attend. Our task is to open their eyes to a wider field of learning and human endeavour.

Let us look again at the philosophical conception of "paradigm shifts" in scientific knowledge that is at the centre of Kuhn's approach. As I pointed out, Kuhn is far from clear in what he really means, often using ambiguous formulations, which is why I referred to the very thorough exposition of his philosophy given by Paul Hoyningen-Huene [3]. He explains that Kuhn opposed the "naive realist interpretation of science" and "wished to reject the more refined realist philosophy of science which sees the scientific process as a progressive 'drawing closer to the truth' . . ." (I should stress that Hoyningen-Huene's book was read by Kuhn and he agreed with it). As I explained, this can only be seen as meaning opposition to the Marxist materialist conception.

I do not see how it is possible, especially given the vagueness of Kuhn's language, to select bits of his writings, "some of the general patterns which he describes" as you put it, with which we can agree at some descriptive level, and separate them from his overall philosophy. Kuhn was, after all, primarily in the business of philosophy, not empirical history. His writings are regularly used by postmodernist academics to justify their positions. He was working in the tradition of neo-Kantianism, which is a philosophical approach to history that is often deceptive—at the level of historical description it is often quite appealing—yet is directed, in many cases consciously as Lukács demonstrates, and the neo-Kantian themselves affirmed, against Marxism.

To write, as Kuhn does, about "growing contradiction between theory and research results, crisis, and revolutionary resolution" does not imply any agreement with dialectics in the Marxist sense (or Hegelian for that matter). Kuhn rejects any continuity between concepts and theories used before a scientific revolution and concepts and theories used after it. This is what he means by "incommensurability". It means that there can be no development or evolution in scientific knowledge, no approximation to reality.

The dialectical philosophy of Marxism considers all material beings to be undergoing continuous change and development through contradiction. Since thinking is not separate from matter but is rather the highest product of the material world, it follows that our concepts and theories must also undergo dialectical development.

According to this conception, when a phenomenon, concept or scientific theory goes through a revolutionary change, the opposites within it do not cancel out to nothing but lead to a higher stage of development: in

Hegel's terminology this is a "determinate negation". Hegel also used the German word "*aufheben*" for such processes. It cannot easily be translated into English, but roughly means to preserve and maintain yet at the same time put an end to. It is central to Hegel and Marx's revolutionary conception of the world, including, of course, the way in which societies change.

In line with this approach to dialectical development I referred to "Newton's theory being an approximation to Einstein's that is superseded but preserved at a higher level." This is not Kuhn's position. If you read Kuhn's *Structure* you will see he argues that the concepts used in the Theory of Relativity—energy, mass, space, time—are not in any sense a continuation of the previous version of the concepts used in the classical mechanics of Newton. This flies in the face of the view held by most scientists, including Einstein himself, who lean, albeit unconsciously, towards materialist dialectics.

I can only present here the barest outline of the Marxist view. It demands a serious study of the classic texts of Marxism, not to mention the key writings of Hegel. A dialectical standpoint gives a far superior standpoint from which to understand the world and scientific change compared to all the varieties of bourgeois philosophy, but it doesn't provide a master key that can just be applied in every case. Every science requires prolonged hard work to master its traditions, theoretical conquests and empirical material.

The dialectical development of science must be understood as an objective process, and, as in the case of Einstein's theory, emerges out of a series of contradictions arising in the empirical basis of physics—the observed constancy of the speed of light, etc.—as well as contradictions in theory—principally between Newton's mechanics and the theory of electricity and magnetism established by James Clerk Maxwell.

What is more, the Marxist materialist approach to the natural sciences—and here it differs from the earlier Hegelian dialectics—stresses that a theory can only develop in continuous evaluation of empirical facts and by testing out in practical applications. A major point of my original article on the NASA experiment GP-B was to demonstrate how this was taking place in relation to Einstein's theories.

A serious study of the empirical evidence is not the same as empiricism. Marxists are opposed to empiricism, the view that in some way the facts can directly give us scientific truth without any serious concern over the elaboration of theoretical concepts or concern for philosophical issues. Engels poked fun at the scientists who accepted the "facts" of ghostly apparitions and noted that "the empirical contempt of dialectics on the part of some of the most sober empiricists is punished by their being led into the most barren of all superstitions, into modern spiritualism" [4].

Here I must question your approach to factual data. You write that in relation to archaeology "a significant portion of what was previously considered data is unusable or at least must undergo considerable reworking," that "much of the 'data' that had been collected under the old paradigm was to a large extent unusable under the new, or at least had very limited utility" and that "scientific research is constrained by over-arching theoretical formulations, which greatly influence what research questions are valid and even what constitutes 'data'."

These kinds of statements, very much in the Kuhnian genre, could easily be interpreted to mean that there can be a selection of facts or data according to which theory is chosen—a completely subjective and relativist position. It would, for example, justify climate change deniers when they claim that scientists, because of their environmentalist theories ("paradigms"), have wrongly interpreted the global temperature data over the last decades, selecting some facts and missing out other facts that are counter to their ideas. According to the Kuhnian view it is the way that science always proceeds.

Furthermore if the discovery of some new facts or the updating of old facts is used to justify the introduction of a new "paradigm" and even

ignore previous facts and data as you suggest—that is not the development of scientific theory but can only be seen as empiricism. It was the situation in archaeology that dates had to be revised because of techniques based on a deeper understanding of the physics of radioactive decay, and a range of new data became available from new applications of physics, chemistry and biology to the material from excavations. But I do not see how even a large amount of new factual material can be used to simply junk the older theories and concepts unless one were the crassest kind of empiricist.

This leads us to the issue of "New Archaeology", or processual archaeology as it is now called. You argue that postmodernism has not affected the field of archaeology as much as anthropology and other areas of social science. It may well be the case that many archaeologists concentrate on empirical work, and that is how they earn their funding. But on theoretical issues, my study of the literature leads me to strongly doubt that the effect of postmodernism is any different to other areas. Post-processual archaeology, as it is termed, seems to include all the various trends that have hit the humanities generally—critical theory, hermeneutics, post-colonialism, poststructuralism, etc., etc. It also seems to me that you are far too uncritical of the theoretical approaches that archaeologists took up in the 1960s. You write that:

"Culture was now viewed as a dynamic, interacting system in which components both acted upon and were influenced by other components. While most archaeologists have not yet taken the next step to understand these as dialectical processes, the adoption of systems theory by many researchers demonstrated that they were searching in this direction."

Here I think you are wrong in according systems theory any scientific status. Whilst noting that it is a general approach used in business management, economics and finance, it was never successfully used in the social sciences, including archaeology, to understand, let alone predict social change and development. As one of its proponents in archaeology, Lewis Binford put it:

"once a proposition has been advanced, no matter by what means it was reached, the next task is to deduce a series of testable hypotheses that, if verified against independent empirical data, would tend to verify the proposition. ... In our search for explanations of differences and similarities in the archaeological record, our ultimate goal is the formulation of laws of cultural dynamics." [5]

Binford's philosophical approach was one of extreme empiricism, or positivism. It did not matter how the theories were obtained, they were tested statistically against the data and accepted or rejected on that basis. Not surprisingly no "laws of cultural dynamics" were obtained, just a variety of ad-hoc theories were advanced which have never been generally agreed by archaeologists or developed into serious scientific laws of social development.

Kuhn's "paradigm shift" approach has been used to justify this as science because all previous concepts and theories were unceremoniously junked. You mention the work of Australian archaeologist V. Gordon Childe, who was a Marxist, as though he influenced the systems theory of the 1960s. I know of no serious evidence for this. It would seem much more likely that processual archaeology was widely supported, especially in the United States, because it appeared to give a scientific alternative to Marxism. In fact the list of pre-1960 theories of culture history, which

you term “butterfly collecting”, would probably usually include Childe’s theories, not to mention, in a broader anthropological context, the work of Lewis Henry Morgan, whose view of social development through stages was advocated by Frederick Engels in *The Origin of the Family, Private Property and the State*.

Thus, one book includes Childe in culture history, even though it is quite favourable to him as in the following comment [6]:

“Childe’s great achievement was to embed the enormous amount of archaeological evidence that had emerged in Europe—particularly for prehistory—within a grand explanatory framework. His prodigious output of publications, and their impact upon a wide range of scholars beyond archaeology who shared an interest in Marxism, did much to establish prehistoric archaeology as a respectable academic discipline.”

Childe had studied the works of Marx and Hegel as a young man, and developed a historical materialist approach to the early history of the Middle East and Europe. He became a supporter of the Soviet Union in the 1930s but never accepted the crude versions of Marxism that were put out by the Stalinist bureaucracy.

He wrote, for example:

“. . . societies at each phase of social evolution rested on definite productive forces which shaped their lives, but which in due course compelled the emergence of new productive forces and a new cycle of social evolution.” [7]

There is no doubt that important contributions have been made since Childe’s day on updating and collating the empirical material [8]. But surely a scientific theory of early societies can only be developed (“negated”) by extending Childe’s creative application of historical materialism. Frankly, I can see no validity in your use of Kuhn to claim that the “New Archaeology”, also known as processual archaeology, has any scientific basis. It certainly should not be confused with Marxism.

[1] “ABC of Materialist Dialectics”, <http://www.marxists.org/archive/trotsky/1939/12/abc.htm>; “Culture and Socialism”, <http://www.wsws.org/articles/2008/oct2008/cult-o23.shtml>; “Dialectical Materialism and Science,” <http://www.marxists.org/archive/trotsky/1925/09/science.htm>; “Radio, Science, Technique and Society”, <https://www.marxists.org/archive/trotsky/1926/03/science.htm>

[2] <http://www.marxists.org/archive/lenin/works/1912/may/08c.htm>

[3] Paul Hoyningen-Huene, *Thomas Kuhn’s Philosophy of Science*, University of Chicago Press, Chicago and London, 1993.

[4] <http://www.marxists.org/archive/marx/works/1883/don/ch10.htm>

[5] Quoted in Kevin Greene, *Archaeology: An Introduction*, Fourth Edition, Routledge, London 2002, p 245.

[6] *ibid*, p 239.

[7] Vere Gordon Childe, *What Happened in History*, Penguin, London, 1964, p 8.

[8] See for example:

<http://www.wsws.org/articles/2003/apr2003/loot-a19.shtml>;

<http://www.wsws.org/articles/2008/oct2008/book-o09.shtml>

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