Chapter 5

Dialectics vs. positivism in the philosophy of science

A Whiggish history of science

Part three of North's polemic is devoted primarily to what he calls "Steiner's View of Science". It is rather curious that North would spend so much time on my views on a topic that did not appear in either of our polemics while totally ignoring the main issues that we had contested in *Marxism Without its Head or its Heart*. In fact, at one point in the midst of this section, North himself brings attention to the oddity of his endeavour,

> It may seem odd that the role of hermeticism in the Inquisition's execution of Bruno and that of alchemy in the physics of Newton should emerge as significant issues. But the attention being given here to Steiner's approach to the history of science is justified to the extent that it sheds significant light on the evolution of his own theoretical and political outlook.

It is odd indeed that North chooses to devote several pages to what he claims to be my views on Hermeticism and the execution of Bruno. But by now an attentive reader who has followed the previous installments of this polemic will recognize what North is up to – he has found what he thinks is more ammunition in his smear campaign against me, namely, a couple of statements I made in correspondence with Chris Talbot. North devotes the bulk of part three to an exercise in mud-slinging and guilt-by-association type arguments to prove that I am an idealist, an irrationalist and hostile to science. He provides a "scholarly" gloss to this enterprise by consigning much of his argument to the footnotes where he uses the method of selective quotation from various historical works to prove that I am sympathetic to some very retrogressive interpretations of the Scientific Revolution.

The history of science, especially the history of the emergence of modern science in the Renaissance is a very complicated field. It is an area in which virtually no opinion is uncontested and the terrain of argumentation can get very detailed and esoteric to all but the specialist in the field. Like much historical analysis, but even more so in this case, the devil is in the details. Specific questions such as, "Was Newton influenced by alchemy and if he was then how was this influence detectable in the elaboration of his theory of gravity?" depend on a minute examination of numerous texts, the proper dating of ancient manuscripts, research into correspondence with contemporaries and a deep knowledge of both physics and the tradition of alchemy. This is above all an empirical inquiry. To be sure empirical research cannot be divorced from philosophical questions but philosophical positions can never be a substitute for such research. That is not however the position of North and the Talbots. North, citing my opinion on this subject, writes,

> [I]t is curious that Steiner should inform Talbot that the question of the role of alchemy in the development of Newtonian physics "can only be adjudicated on the basis of the historical evidence and not on some a priori notion of how Newtonian science must have developed." As a matter of fact, Steiner ignores the historical evidence. But beyond that, the problem of the relation of religion to science is precisely the sort of question that requires a philosophically informed insight into the underlying issues.

North argues this in the context of his discussion of Frances Yates and Betty Jo Dobbs, two historians of science who have brought attention to the influence of alchemy and other occult traditions in the work of Bruno and Newton. In taking issue with my statement that Yates' and Dobbs' theses "can only be adjudicated on the basis of the historical evidence", North's claim that "the problem of the relation of religion to science is precisely the sort of question that requires a philosophically informed insight into the underlying issues" is just a roundabout way of providing a rationale for ignoring the historical evidence. This is evident when we come to his account of Newton:

> Moreover, in considering the question of the relation of alchemy to science, the details of Newton's personal interest in alchemy, not to mention his ardent belief in God, are of decidedly secondary importance. Newton was a man of his time, as Bruno was of his. They, as individuals, could not simply step outside the world in which they lived. Concepts and modes of thought inherited from the past exerted a residual influence upon even the greatest minds of their ages. But in the final analysis, as the development of chemistry required its liberation from alchemy, the elaboration of science and its appropriate methodology demanded a break with a religious worldview. Notwithstanding the contradictions in the intellectual development of one or another scientist, the fundamental and irreconcilable antagonism between science and religion asserted itself—often partially and ambiguously in individuals, but completely and irreconcilably in the historical process as a whole.

How does North know that "the details of Newton's personal interest in alchemy, not to mention in his ardent belief in God, are of decidedly secondary importance" when "considering the question of the relation of alchemy to science"? Such a judgment can only be made on the basis of an a priori assumption that refuses to look at the historical evidence. This is precisely the point made by Betty Jo Dobbs when arguing against the historian I. Bernard Cohen:

> …his [Cohen's] position seemed to be based on the a priori assumption that alchemy could never, by its very nature, make a contribution to science. To accept the premise that alchemy could not do so is to prejudge the historical question of whether it did so in Newton's case, which is after all the point at issue. [1]

As a matter of fact Dobbs's complaint about Cohen seems unjustified in this case as Cohen did not ignore the historical evidence. He examined it and came to a different conclusion than Dobbs. But her basic methodological point is completely valid – to prejudge the outcome of a historical inquiry by excluding all evidence from certain sources or traditions of thought that are deemed by their nature to be irrelevant is an

[1] Betty Jo Teeter Dobbs, *The Janus faces of genius: The role of alchemy in Newton's thought*, (Cambridge University Press, 1991), p.4.

illegitimate mode of argumentation. The viewpoint Dobbs is arguing against - and one which North undoubtedly holds - can be characterized as a Whiggish view of the history of science. [2] That is to say it is the view that science must have always looked much the same as it looks to us today.

North's account considers that the individuals involved in the launching of the new science "could not step outside the world in which they lived." In other words, he concedes that individuals embody the historical contradictions of their time. But when it comes to "the historical process as a whole" North denies the contradictory nature of progress from a religious outlook to a scientific one. According to him "the fundamental and irreconcilable antagonism between science and religion asserted itself … completely and irreconcilably." This is nothing but another variation of the same non-dialectical conception of progress we have met before in the history of philosophy. Recall that North and the Talbots, following in the footsteps of George Novack, saw no place in the history of philosophy for a progressive role for idealism. In their conceptualization materialist philosophy goes from triumph to triumph in an ever-ascending line of linear progress. Likewise, North's view of the emergence of science has no place for anything other than diminutive versions of what eventually developed as science. Whereas science did indeed have to establish its autonomous role and shake off the bonds of religion, it would be a mistake to look at the end product of science as an autonomous discipline free from religion and confuse that outcome with the process of its historical development.

Before science became science, it was something else and that something else included elements of what we would today consider irrational beliefs. Some of those irrational beliefs (I would prefer the term "pre-rational") impelled 16th century figures such as Bruno to look at the world in a more naturalistic way. That is the significance of what John Henry called the tradition of natural magic in the Renaissance. In his book, *The Scientific Revolution and the Origins of Modern Science* (which ironically was recommended to me by Chris Talbot) Henry states,

> Further important sources of the empiricism of the Scientific Revolution were to be found in the magical tradition, and these influences can be seen at work in a number of areas. They deserve separate consideration here, however, because they have generated considerable historiographical debate. A number of historians of science have refused to accept that something which they see as so irrational could have had any impact whatsoever upon the supremely rational pursuit of science. Their assessment seems to be

[2] The term "Whiggish history" was introduced by the British historian Herbert Butterfield in an essay written in 1931, *The Whig Interpretation of History*. While Butterfield was arguing against a particular group of political historians who interpreted past history in order to validate the Whig principles of their current political affiliation, he recognized the wider application of his idea. In his Preface he wrote of,

> …the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification, if not the glorification of the present.

It goes without saying that the history of science is particularly vulnerable to this kind of triumphalist but historically inaccurate misrepresentation.

> based on mere prejudice, or on failure to understand the richness and complexity of the magical tradition. [3]

Henry goes on to elaborate what some of those rational elements were that were contained in contradictory form in the magical tradition of the Renaissance,

> If we wish to understand the role of magic in the Scientific Revolution it is important to note the existence of so-called natural magic, as arguably, the dominant aspect of the magical tradition. Natural magic was based on the assumption that certain things had hidden, or occult, powers to affect other things and so accomplish inexplicable phenomena. Success as a natural magician depended upon a profound knowledge of bodies, and how they act upon one another, in order to bring about the desired outcome. Repeatedly we see Renaissance natural magicians insisting that their form of magic depended upon nothing more than knowledge of nature, so much so that one recent historian has suggested that we should designate this kind of thinking as 'Renaissance naturalism' to distinguish it from what he thinks of as real magic.
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> In a very real sense, however, the separation of the naturalistic elements from other aspects of magic was just what was accomplished during the Scientific Revolution. The history of magic since the eighteenth century has been the history of what was left to that tradition *after* major elements of natural magic had been absorbed into natural philosophy. [4]

While noting the contribution to the development of science of certain strands in the Renaissance which today would be deemed irrational we are not suggesting that any and all irrational beliefs led in the direction of science. Some paths emerging in the fertile culture of the Renaissance led nowhere, bore no fruit and were wholly retrogressive. But if there was a pre-scientific culture that gave impetus to the origins of modern science, then we would expect that at least some elements of that pre-scientific culture contained in embryo what would later become elements of a scientific culture. Otherwise we are led to the absurd conclusion that modern science was born all in one piece and had no continuity with its historical antecedents. That would be a completely un-dialectical viewpoint, one which sees either continuity that excludes breaks, or breaks that exclude continuity. North's view is of the latter type. He sees the break between the Scientific Revolution and the preceding culture, but he sees no continuity. He is looking at history backwards and sees either something that looks like us – only less developed - or something that is wholly alien. That is a perfect description of a Whiggish view of history. [5]

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[3] John Henry, *The Scientific Revolution and the Origin of Modern Science*, (St. Martins Press, 1997), p. 42.
[4] Ibid. p. 43
[5] The Whiggish view of history was discussed by Marx long before the term even came into existence. In Volume I of Capital, Marx noted that the classical political economists, while capable of analyzing the commodity form, never asked why it takes on the form that it does because to them this was simply given – "a self-evident nature-imposed necessity" - and all previous economic systems were looked upon as clearing the way for the present. Marx likens this view to that of the early Church historians who viewed the old pagan religions as a kind of rehearsal for the triumphant inauguration of Christianity. Here is how he put it:

> These formulas [that specifies how the measurement of labour by its duration is expressed in the magnitude of the value of the product] which bear the unmistakable stamp of belonging to a social

But real history is not like that. It is not as North says, just "individuals" who were caught up in concepts from the past. Those concepts from the past were inextricably linked to newly emerging ideas that defined the new science in a contradictory unity. The battle between science and religion in the 16th and 17th century was real enough, but it should not be oversimplified. If by "religion" is meant the Catholic Church and the Inquisition, these institutions were indeed the most significant bulwarks against the rise of the new science in this period. But there were also other currents of thought that germinated in the late Renaissance whom we may designate broadly as religious or animistic, but that were more or less antagonistic to the Church, some secretly and others openly. North does not seem to realize that the tradition of natural magic of the Renaissance was one of those traditions that were hostile to the orthodoxy of the Church and thereby might provide an alternative institutional foundation for the support of those who were drawn to the heretical views of Copernicus. The tradition of natural magic discussed by John Henry, particularly in its concept that man can actively influence and tame Nature by learning its secrets, was an important influence on the thinking of Giordano Bruno who played the role of a midwife to the new science. [6] That magic was not science itself is not in dispute.

> formation in which the process of production has mastery over man, instead of the opposite, appear to the political economist' bourgeois consciousness to be as much a self-evident and nature-imposed necessity as productive labour itself. Hence the pre-bourgeois forms of the social organization of production are treated by political economy in much the same way as the Fathers of the Church treated pre-Christian religions.

Capital, Volume I, Penguin Classics, p174-175

[6] Bruno's appropriation of natural magic is discussed in great detail by one of the authors North cites, Maurice A. Finocchiaro. North brings in his reference to Finocchairo in note #49 of his *Odyssey* piece to prove that Bruno's execution was symptomatic of the struggle between philosophy and religion. Finocchiaro was writing against Yates who opposed that thesis and claimed that "it was probably mainly as a magician that Bruno was burned." Finocchiaro is right against Yates. Yet North expends much energy arguing this point and trying to link my position to Yates in another one of his straw-man and guilt by association arguments. Finocchiaro, in his essay probes the complexity of Bruno's relationship to magic and shows, in contrast to Yates – and I might say to North as well – that of the various strands of the magical tradition in the Renaissance, Bruno appropriated the one that was later called "natural magic" while rejecting the others. Here is Finocchiaro's account:

> ..Bruno explained his views on the nature of the magical arts. Regarding the art of conjuring, he dismissed it with contempt. As regards the art of divination or judicial astrology, he admitted the intellectual curiosity of wanting to learn about it to see if it had any validity, but he bemoaned the fact that as yet he had not found the time to study it. He also dismissed something which he labeled 'mathematical or superstitious magic', without explaining what he meant by this. Next he commented on magic per se, or natural magic, understood as 'knowledge of the secrets of nature together with the ability to imitate nature in her operations and to do things which are popularly seen as wonders'. Quoting Saint Thomas Aquinas, Bruno stated that all knowledge can be good or bad depending on whether it is used by good or bad persons, and he went on to argue that therefore there is nothing intrinsically evil in natural magic; that, in his own eloquent words, 'it is like a sword, which is bad in the hand of an evildoer but can be good in the hand of someone who feels the fear of God.' Finally, he clarified that even for natural magic, his interest in it was theoretical rather than practical or pedagogical: 'I have never had the intention of preaching the said science … but only … that I should be informed of the character and theory of the science, because I never liked its practice.'

Bruno's last statement clearly shows that he considered natural magic as a kind of science. North, in his appropriation of this same essay by Finocchiaro, sticks to the latter's insistence that the issue in Bruno's

Nor is it in dispute that science had to shed its roots in alchemy before it could become what we today recognize as genuine science. But again, that is a consideration about the culmination of a historical process. If we focus instead on the process itself in its movement, we cannot simply read backwards into this history a diminutive version of the final product existing in every phase of its development. We have to see the contradictory path whereby science emerged out of that history.

In fact, North's views here are not as far removed from Frances Yates as he thinks. She too saw an absolute dichotomy between science and the magical tradition of the Renaissance. The difference is that where North puts a plus sign, she puts a minus sign. For Yates, when the magical tradition was finally replaced by modern science, we lost something significant. For North it is just the opposite – the only thing we lost was superstition and ignorance.

Now it is true that with the final emergence of modern science lots of ballast rooted in superstition and ignorance was discarded. What North misses however is that some elements of the occult tradition of the Renaissance made a positive contribution to this emergence of modern science out of ignorance and superstition. This is paradoxical yet true. The illustration of this contradiction however involves precisely the kind of detailed examination of historical sources that North disdains. Perhaps the most dramatic example of the debt of modern science to one of the pre-scientific traditions of the Renaissance can be found in Newton's theory of universal gravity. And as the following summary by the historian H. Floris Cohen indicates, we owe a debt to the late Betty Jo Teeter Dobbs for the discovery of this connection:

> …there is one case only, though indeed the most baffling of all, where a tangible link between 'esoteric' and 'hard-core' thinking has been suggested and underpinned with a wealth of factual evidence. This is the riddle of Isaac Newton's alchemy. It was one thing for Frances Yates to show that the chief contributions made by the Rosicrucians to the Hermetic tradition was a renewed interest in alchemy. She linked this in a vaguely suggestive way to the further history of scientific thought in the 17th century, and one pioneer student of Newton's alchemical papers, Betty Jo Teeter Dobbs, quickly pointed out the superficiality of her supposed connections. The fundamental defects he perceived in the mechanical philosophy may very well have derived from his alchemical pursuits. Here, so Dobbs as well as Westfall contended, was a probable source of inspiration for Newton's conception of the world as filled with forces, both attractive and repulsive, operating over varying distances, and acting upon different bodies. What, for Newton, made such a conception of things distinct from Renaissance operations with occult forces, however, is that these forces could, in principle, be measured, and that a scientific treatment of them was possible only to the extent that they had been measured in fact…

trial was the conflict between philosophy and religion, but avoids Finochiarro's discussion of Bruno's interest in the tradition of natural magic.

> Such a revised picture [of Newton] makes considerably more sense in the new, wider perspective provided by the Hermeticist current in the historiography of early modern science than it does in the 'regrettable remnants' tradition of old. [7]

This account from such a no-nonsense 'hard core' historian of science as Cohen should put paid to the narrative woven by North that any acknowledgement of the debt of modern science to some of the occult traditions of the Renaissance puts one in the camp of post-modernism and anti-science. North in fact, fits perfectly into the old tradition lampooned by Cohen that views any precursor of modern science as a 'regrettable remnant'. What that tradition misses is that it was precisely the path through some of these 'regrettable remnants' that were necessary for science to emerge.

In the end, neither Yates nor North are able to see the continuity as well as the break between the magical tradition of the Renaissance and the Scientific Revolution. And this is the crux of the matter when considering the history of science during the 17th century, when modern science was being born.

The Brouhaha over Frances Yates and Betty Jo Dobbs

I have already commented on the oddity of North's attention to Frances Yates and Betty Jo Dobbs. The combined ink devoted to bashing Yates and Dobbs by North and the Talbots is no less than 7 pages. That is more polemical material than North and Company have expended on any American political tendency in the past year. Now I would be the last person to criticize these writers for straying from so-called practical matters into a consideration of theory, except that in this case their venture into the esoteric history of Renaissance Science is little more than a pretext for branding me as a follower of the political and philosophical views of Yates and Dobbs. Yates in particular is the subject of a barrage of attacks for her "conservative idealism", neo-Platonism and attacks on science and the Enlightenment while the reader is meant to infer that I share Yates's views in these areas. The entire case against me rests on two brief statements I made in my reply to Chris Talbot where I introduced the works of Yates and Dobbs as an example of some new historical material that further demonstrated the complexity and contradictory nature of the Scientific Revolution. [8] Neither Yates nor Dobbs were even mentioned in my original lecture on Science and Dialectics. Here is what I said about Yates in my correspondence with Chris Talbot,

> ...There is however another influence—one that was little known until recent scholarship —I mean the influence of the Hermetic tradition and magical ideas. The story of the birth of 17th century science is incomplete without a discussion of the mystical sources that animated the great pioneers. In the case of Bruno, an excellent book that discusses the

[7] H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry*, (University of Chicago Press, 1994), p. 175.

[8] Elsewhere in their piece the Talbots accuse me of ignoring this complexity. But why let an inconvenient fact stand in the way of a good argument? See: http://permanent-revolution.org/polemics/downward_spiral_ch04.pdf p. 118.

> influence of the Hermetic tradition on the new science are "Giordano Bruno and the Hermetic Tradition" by Frances Yates. [9]

What needs to be said is that the citation of Yates' work to illustrate that some of the pioneers of the Scientific Revolution were animated by magical ideas does not mean that I follow Yates's conservative politics or her historiography. Yet the Talbots take my remarks to mean just that. According to them I "rely" on Yates and Dobbs and I have taken their position in a long-running controversy in the history of science. They write,

> History has its own history, and by relying on Yates and Dobbs, Steiner is taking a position in a long-running polemic that can be traced back to 1931 and the Second International Congress of the History of Science and Technology in London.

This is simply nonsense. I took no position in a long-running polemic among historians of the Renaissance as I am not and never claimed to be a historian of the Renaissance. For that matter neither are North or the Talbots though that does not stop them from making all sorts of dogmatic pronouncements about the implications of this historiography. What I did was point out in my correspondence with Chris Talbot that Yates and Dobbs make a good case for the contradictory nature of the development of science, that some of its chief practitioners represented a highly contradictory unity of beliefs in the occult with what we now recognize as modern science.

At one time Ann Talbot at least was in agreement with the methodology behind my approach. Here is what she wrote about Newton in 2000,

> It was from members of the Hartlib circle that Newton derived his knowledge of alchemy. Newton was familiar with the principles of chemistry. He was fostered by an apothecary while he was at Grantham grammar school and spent his boyhood experimenting with chemicals. But from the 1670s he devoted himself specifically to alchemy, under the influence of surviving members of the Hartlib circle, who included Robert Boyle. Newton corresponded with Boyle until Boyle's death in 1691 and may well have acquired his collection of alchemical texts from other members of the group. Newton continued his researches for two decades and at one point even believed that he had succeeded in producing gold. This passion for alchemy, which has proved such an embarrassment to later historians, shows Newton's connection to the revolutionary tradition of the 1640s.

Ironically, the Talbots make use of this same article in their Addendum to North's *Odyssey* piece, but they studiously avoid quoting anything from the section that discusses *"This passion for alchemy, which has proved such an embarrassment to later historians."* It looks as if the Talbots have joined the ranks of these "later historians" who are so embarrassed at any mention of the contradictory roots of modern science.[10]

Later in the same article, Ann Talbot adds,

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[9] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #2.

[10] Though this is a side issue in the context of the present discussion, one might well wonder about Ann Talbot's claim that the "passion for alchemy … shows Newton's connection to the revolutionary tradition of the 1640s." That seems too strained an interpretation of the facts and a dodging of the dialectic of Newton's history.

> Not only did historians downplay Newton's involvement with technology, but also they glossed over his religious ideas and interest in alchemy. As more of his papers became available for study it became increasingly difficult to square the Newton who was an icon of British empiricism with the Newton that emerged from the documents. [11]

The obvious question to ask then is why is it legitimate for Ann Talbot, writing in 2000, to point to the contradictory roots of modern science in Newton – once described in a famous quote by the economist John Maynard Keynes, an avid collector of Newton's alchemical writings, as being "not the first of the age of reason, but the last of the magicians" – whereas when I make the same point in 2003 I am branded an idealist and enemy of the Enlightenment?

Elsewhere in their polemic, even while denouncing me for following Yates' "conservative idealism", the Talbots have to admit that Yates' work was "pioneering".

> In the latter part of the 20th century, Yates's conservative idealism seems to have met up with postmodern trends in both the United States and Britain, which gave a fresh lease on life to her works at a time when the new developments in Renaissance and early modern scholarship might have been expected to render her, admittedly pioneering, work outmoded.

We get no such qualification in North's polemic.

Indeed the Talbots can hardly avoid pointing to the pioneering qualities of Yates' work as she did bring attention to certain influences on Giordano Bruno that were previously little known or understood. In their respective contributions to the *Odyssey* series both North and the Talbots fulminate over this. How is it possible, they ask, for such irrational beliefs as magic and Hermeticism to have any influence over the birth of modern science? They claim that anyone who thinks that the development of modern science owes anything to such sources must be a defender of irrationalism and a postmodernist. And that is exactly how they brand me. The fact that I bring up Yates and Dobbs just proves their case as far as they are concerned.

When it comes to the history of science, both North and the Talbots cannot distinguish form and content. Whereas the form of magic may be irrational, the magical traditions of the Renaissance contained in embryo some of the seeds that would later mature to fruition in the Scientific Revolution.

[11] http://www.wsws.org/articles/2000/sep2000/newt-s26.shtml

It is also true that Yates' scholarship has been challenged in recent years. [12] John Henry points out for instance that her claims for the influence of Hermeticism on Renaissance science were greatly exaggerated. But even if Yates got the story of Hermeticism all wrong, Hermeticism being just one small branch of the magical traditions that prevailed in the Renaissance, no one doubts the importance of the magical tradition as a source of the Scientific Revolution in its infancy. As we have seen, John Henry and other historians have made the very useful distinction between "natural magic" in this tradition and other aspects of magic which fell away as a source of the Scientific Revolution. The fact that Yates had a religious axe to grind in her presentation, and that this often entered in a polemical way into her writing, should not detract from the positive aspects of that

[12] North makes a huge fuss about the challenges to Yates' scholarship in several long footnotes. For instance his footnote #49 goes on for over a page and quotes several scholars who challenge Yates, including Maurice Finocchiaro, about whose discussion I have previously commented. There is also a very long discussion of a critique by Brian Vickers of Yates' book, *The Rosicrucian Enlightenment*. Vickers critique is indeed "devastating" as North writes, but is irrelevant to anything I said or wrote as I never commented on this book and my case for the contradictory nature of the Scientific Revolution does not rest on Yates' interpretation of Rosicrucianism. North's footnotes here are in any case highly selective as virtually every scholar whom he cites as a critic of Yates will also grant that at least some of her work did have some positive value. Attention should be drawn to the assessments of critics of Yates such as I. Bernard Cohen, John Henry and Maurice Finocchiaro, all of whom concede the importance of Yates's work in opening up a whole new dimension of scholarship about the Scientific Revolution. (In this connection, see note #6). I can also add to this list the historian H. Floris Cohen who is cited by North in his footnote #52 for being concerned that Yates' thesis opened the gates to a view of science "as just one among a variety of belief systems, each with its own standards of rationality or lack thereof." Thereby North hopes to reinforce his argument that because I find something of value in Yates' scholarship, that I am encouraging epistemological relativism and postmodernist attacks on science. But what North doesn't say is that the author cites a discussion by another historian from the very page containing the quote he produces, who, while opposing the relativism that some have read into Yates' work, acknowledges her contribution to our understanding of the birth of science in the Renaissance. Here is the quote (from the historian Paolo Rossi):

> …the study of the interconnection between hermeticism and modern science have greatly enlarged our historical horizon. However, the recognition of the 'hidden presences' within the hermetic tradition of modern science does not entitle us to reduce the latter to the former, and to forget that in the case of the history of science – at least from the age of Galileo and quite apart from what was happening in the world of magic – it is justifiable to speak of theories that are more or less rigorous, have greater or lesser explanatory and/or predictive power, and are verifiable to a greater or lesser degree." (H. Floris Cohen, Ibid. p. 180.)

Elsewhere, H. Floris Cohen, in summing up a very critical appraisal of Yates, has this to say about her work:

> Precisely because Yates' thesis is bound up to such an unusual extent with a peculiar, and also hard to define, conception of science did it seems important to sift out in somewhat nitpicking detail what in her thesis, if viewed from the perspective of more conventional conceptions to which I myself by and large subscribe, appears worth preserving. (H. Floris Cohen, Ibid. p. 296)

In fact, virtually all the scholars quoted by North, even when sharply critical of Yates, are also willing to acknowledge the value of aspects of her work to one degree or another. There is thus something of an intellectual fraud being perpetrated by North in amassing these footnotes in order to convey the impression that anyone who has anything positive to say about Yates' work must subscribe to her penchant for mysticism and is in sympathy with postmodernism.

scholarship. We should be able to separate out the wheat from the chaff. Otherwise, how can even the Talbots justify their acknowledgement that there was something "pioneering" in her work?

To summarize, my citation of the work of Yates and Dobbs was to illustrate the contradictory nature of the emergence of science. It had nothing to do with a supposed kinship on my part to magic and mysticism. And this was exactly the same approach that Ann Talbot exhibited in her discussion of Newton's investment with alchemy when she wrote about this topic some years ago. Even the strongest detractors of the work of Yates and Dobbs, historians that are favorably mentioned by Chris Talbot such as I.Bernard Cohen and John Henry, acknowledge that their work was "pioneering" and should not be ignored by any serious student of the Scientific Revolution. [13]

Were we to follow the logic of guilt-by-association type arguments practiced by the Talbots we could prove some very interesting propositions starting from the following premise:

Chris Talbot recommends the authors John Henry and I. Bernard Cohen.

As both Henry and Cohen have some positive things to say about the work of Yates and Dobbs despite their criticisms, therefore Chris Talbot, in recommending Henry and Cohen, "relies" on the work of Yates and Dobbs.

In addition, as both Yates and Dobbs have encouraged postmodernist attacks on the validity of science, therefore, because Chris Talbot relies on the work of Frances Yates and Betty Jo Dobbs, he has encouraged the postmodernist attack on science.

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[13] John Henry, while critical of Frances Yates' claim that Hermeticism played an important role in the thinking of Giordano Bruno, acknowledges the important role of what he calls "natural magic" in the citation we previously gave. He also recommends the writings of Betty Jo Dobbs on Newton and alchemy in his bibliography, where he says that her book, *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought*, is the "latest restatement of the role of alchemy in Newton's thought…superb on religious dimension." (Henry, Ibid. p. 102). I. Bernard Cohen, while critical of Betty Jo Dobbs, saw fit to include an essay by her on Newton's alchemy in an anthology he edited with Richard S. Westphal. (See *Newton: A Norton Critical Edition*, Norton, 1995). In the Introduction to an anthology of essays on Newton written by I. Bernard Cohen and George Edwin Smith, we find the following laudatory words about Betty Jo Dobbs,

> Two scholars in particular have made massive studies of Newton's alchemical writings: the late Betty Jo Dobbs and Karin Figala. Dobbs wrote two books on the subject, summarizing her findings and conjectures. Her conclusions are of real significance for any philosopher wishing to understand the mind of Newton.

The Cambridge Companion to Newton, edited by I. Bernard Cohen, George Edwin Smith, (Cambridge University Press, 2002), p. 26.

The poverty as well as the dishonesty of this manner of "reasoning" requires no further explanation.

Issues in the philosophy of science

Although North dipped into a few scholarly books searching for incriminating quotes he could use against me, his overall knowledge of issues in the history and philosophy of science appears to be very slight. For one thing, neither he nor the Talbots even cite the key reference book for the philosophy of science from a Marxist perspective, Helena Sheehan's *Marxism and the Philosophy of Science: A Critical History*. [14] It is also noteworthy that while I am denounced for disagreeing with Engels on basic philosophical issues, neither North or the Talbots have anything to say about the founding text of the Marxist philosophy of science, Engels' *Dialectics of Nature*, a defense of which was after all the feature of my lecture on science and dialectics.

There are a number of key issues in the philosophy of science that map out the terrain. Whatever philosophical approach one adopts is defined by the answers one provides to these key issues. As neither North nor the Talbots address any of these issues explicitly I will briefly summarize a number of them.

Perhaps the central issue of importance to a Marxist understanding of science is the relationship between science and philosophy. The difference in approach between Marxism and positivism is most clearly articulated in their differing interpretations of the relationship between these two antipodes. We can sum up the difference as follows:

For Marxism, science and philosophy are dialectically intertwined. This was perhaps the main lesson to be garnered from Engels' classic *Dialectics of Nature*. Of course saying that science and philosophy are dialectically connected does not answer the question of **how**. To do that requires much work, of the sort that Engels only began. On the other hand, for positivism and its cousins empiricism and pragmatism, science and philosophy go their own separate ways. The deleterious effects of that separation between science and philosophy was the central thesis of my lecture on *Dialectics and the Crisis of Science*. And it is this positivist approach that dominates much discourse in the philosophy of science and makes it difficult for Marxism to get a serious hearing. That separation in the modern imagination between science and philosophy has not always been the case. It is part of the legacy we have inherited from the 17th century Scientific Revolution, a topic that figured largely in my lecture as well. The historical roots of the separation of science and philosophy are a well known topic in the history of science. For instance, take the words of the historian H. Floris Cohen (who is himself sympathetic to positivism):

> From the 17th century on, science and philosophy went their largely separate ways, with philosophers continuing for a long time to produce total systems in which all things were to be assigned their fitting place. As scientists grew bolder over the centuries, some went so far as to inflate their own scientific findings and turn them into –allegedly - complete

[14] Helena Sheehan, *Marxism and the Philosophy of Science: A Critical History*, (Humanities Press, 1993)

> conceptions of the world and humanity, scientistic constructions whose history remains to be written. [15]

In addition, to the question of the relationship of philosophy to science, there are a number of other questions that map out the terrain in this area.

1. One key issue is the relationship of our perceptual experience to objective reality.

2. Another issue is the question whether science can be treated as an historically embedded social practice or an impartial, ahistorical and self-correcting search for truth.

3. Still another issue is the relationship between experiment and observation and the role of deduction and thought experiments in the scientific enterprise.

4. Another question is the relationship between facts and theories. Are facts completely independent of theories or is there a meaningful sense in which we can speak of facts as "theory-laden"? This question is closely related to the supposed dichotomy between facts and values.

5. There is also a question as to whether the path of discovery of scientific laws is the same as the proof of the validity of those laws.

Alongside these questions are a host of related issues but in general one's answer to these basic questions defines ones attitude toward the other subordinate questions. Furthermore the way these issues come to the fore in contemporary culture often mask over the real underlying philosophical questions. For instance, the dichotomy between science and philosophy announced itself back in the 1950s as a supposed separation between the culture of the sciences and that of the humanities. In the 1950's the British scientist C.P. Snow wrote a book, *Two Cultures and the Scientific Revolution,* in which he bemoaned the lack of communication between the culture of the humanities, which he called "literary culture" and the culture of the sciences. Snow identified scientific culture with progress from a vaguely left wing perspective whereas literary culture was identified with the forces of the Right and superstition.

Half a century later, North and the Talbots have adopted C.P. Snow's thesis with one modification, which for all intents is of little significance. Whereas Snow characterized the enemies of a progressive science in his time as right wing literary figures, North and the Talbots see the enemies of progressive science as the "radical intelligentsia" which they identify as a host of figures from anyone remotely influenced by the Frankfurt School to the most vociferous postmodernists. But regardless of their targets, North and the Talbots follow Snow in implicitly seeing an unbridgeable gulf between the sciences and the humanities. Both share the underlying assumption that there are two separate cultures and behind that assessment is an acceptance of the separation between philosophy and science. And that perspective is a repudiation of a central thesis of the

[15] H. Floris Cohen, op cit, p. 168.

Marxian philosophy of science as articulated by Engels in his *Dialectics of Nature*, namely that philosophy is essential for the work of the sciences.

The Talbots in particular are unmitigated technology boosters who view any philosophically informed criticism of the misuse of technology and the ethos of contemporary consumerism with extreme hostility. They claim that my discussion of the crisis of science,

> …is a pretty thin disguise for an attack on science. He [Steiner] expresses intense dissatisfaction with modern science, which he condemns as "reductionist," "atomistic" and "empirical."

They add that,

> Steiner's complaints against science are of a piece with the familiar anti-Enlightenment, anti-scientific litany of that school, which has become well established in the postmodern schools of thought that have developed among the radical intelligentsia hostile to Marxism.

And for good measure they assert that my discussion of the crisis of science is

> …in reality, an attack on objective scientific thought.

They follow up these remarks by citing as an example of my "attack on objective scientific thought" my critique of the reductionism of certain writers who conflate the neurophysiology of the brain with consciousness. Yet nowhere did I attack the scientific discipline of neurophysiology or suggest that discoveries in this area have no value. All I did was reassert something that has been a backbone of Marxist theory for a long time, that you cannot reduce consciousness to the biological or physiological mechanism of the brain. Indeed, it is a thought that was nicely articulated by Trotsky in his Notebooks, where he writes,

> The brain is the material substrate of consciousness. Does this mean that consciousness is simply a form of "manifestation" of the physiological processes in the brain? If this were the state of affairs, then one would have to ask: What is the need for consciousness? If consciousness has no *independent* function, which rises *above* physiological processes in the brain and nerves, then it is unnecessary, useless; it is harmful because it is a superfluous complication—and what a complication! [16]

My reply to Chris Talbot went into great detail on the different types of reductionism – an area that is a large topic in its own right. What I wish to emphasize in the context of the above remarks is that for the Talbots any philosophically informed methodological critique of the work of scientists or authors of science books is tantamount to an improper meddling by an outsider into the scientific community. This kind of crude reaction against philosophy is not unexpected from many scientists, particularly those who have been influenced by positivism. It is even to a certain extent understandable in light of the

[16] Excerpts from Trotsky's Notebooks can be found at http://www.permanent-revolution.org/archives/trotsky_notebooks.pdf The cited quotation is on page 31.

fact that some philosophers and critics really do exhibit an appalling ignorance and even hostility to science. But one would expect that a working scientist who has grown up for decades in the culture of Marxism would have a more enlightened attitude toward a philosophical critique of science. One would expect that, but in the case of Chris Talbot, who is himself a professional mathematician, one would be wrong.

Positivists, Social Constructivists and Marxists

Let us now examine a few other answers to the fundamental issues in the philosophy of science that are provided by different philosophical tendencies.

The first issue to consider is the relationship between perceptual experience and objective reality. This issue dominated the first major battle between Marxism and positivism a century ago. The version of positivism then in vogue, as articulated by the physicist Ernst Mach, asserted that all we know are our experiences as given to us in sense perceptions and there is no point in asking about an objective reality behind our perceptions. Mach's position, and its various refinements at the hands of others, were taken on by Lenin in his first work that addressed philosophical issues, his *Materialism and Empirio-Criticism*. Lenin correctly argued that the position of the Machists led inevitably to a philosophy of subjective idealism. The only consistently materialist position is that our perceptions provide us, however partial and however contradictory, a view of an objective world that exists independently of our perceptions. Even non-Marxists have recognized in retrospect the importance of this discussion in the context of the philosophy of science. For it represented one of the first attempts to argue against the view that new discoveries in science which had put into question our previous picture of the world refuted the objectivity and materiality of the world. Lenin correctly argued that the new discoveries in physics, far from refuting a materialist philosophy, only deepened it by contributing to our understanding of the nature of matter. New discoveries in science demonstrate that our conceptions of the objective world were inadequate, rather than the world being somehow inadequate to our conceptions.

While this was an important debate, there are a host of other issues in contention in the philosophy of science that were never raised. In particular there are many other issues that came to the fore in the later evolution of positivism. But this is where North and the Talbots' understanding of positivism stops. Other than repeating Lenin's critique of the Machists, one would be hard-pressed to find anything in their collective output that bears on the topic of positivism. There is not a single discussion of positivism in the entire annals of the WSWS and only a couple of passing references.

When the topic is mentioned, the only issue they ever take up explicitly is the relationship between our perceptual experience and objective reality. But that question only addresses one of many issues under contention in the philosophy of science. They have little to say about other issues: whether science is historically embedded, or the relationship between observation and thought experiments in the scientific enterprise, or the relationship of facts to theories or any of a number of other questions that have been at issue in the philosophy of science since 1908 when Lenin's polemic against the

Machists was published. It is not an exaggeration to say that those Marxists who think that the last word on positivism is provided by Lenin's debate with the Machists have been in a kind of intellectual deep freeze. And the results of this neglect of theoretical issues is that those who have turned their backs on these theoretical questions have unwittingly adopted much of the baggage of positivism while abjuring dialectics. This is the fate that has overtaken North and the Talbots.

Yet ironically, there exists a rich tradition of Marxist engagement in the philosophy of science that provides a unique set of answers to the issues that are today in contention. That tradition was inaugurated by Friedrich Engels and developed in the early years of the Soviet Union by scientists and philosophers who were inspired by his pioneering work. As the historian Helena Sheehan indicated,

> Many of the current debates are rooted in a persistent inability to reconcile the rationality of science with the metaphysical and sociohistorical character of science. In a strange way, the residues of positivism linger on and color the views of even the most radical or anti-positivists. And the flames of the crisis in the epistemological foundations of science burn more wildly than ever. There is no consensus, indeed, there is exceedingly sharp polarization regarding the relationship of science to philosophy or to history…
>
> From the beginning, the Marxist tradition bravely set itself the task of elaborating the philosophical implications of the sciences of its times with a view to working out a scientific *Weltanschauung* adequate for its epoch. Engels's antipositivist materialism was an extraordinary achievement. He did not shrink from the great basic questions that perplexed the philosophers of the ages, but he did insist that attempts at answers be grounded in the best empirical knowledge of the time. In so doing he not only laid the foundations of a scientifically grounded world view, but he set forth views on many issues, such as reductionism, the history of science, and the logic of scientific discovery, that not only anticipated certain contemporary theories, but are still in advance of them. [17]

One will search in vain in North or the Talbots' polemics or in the archives of the WSWS for that matter, for a discussion of any of these issues in the philosophy of science or any indication of the rich heritage of Marxism in this area. While in the current polemic North and the Talbots say little about their own position in the course of creating a fictional narrative of my position, one can nevertheless infer much of their position on these issues by turning to my correspondence with Chris Talbot.

Many of those issues were touched upon in my reply to Talbot's claim that my use of a quote from Einstein was encouraging postmodernist tendencies in science. Talbot claimed that I latched onto Einstein's statement in order to open the door to postmodernists who have used that quote themselves to prove that Einstein was a cultural relativist. [18] My response explained that neither I nor Einstein can be held responsible for

[17] Helena Sheehan, *Marxism and the Philosophy of Science: A Critical History*, (Humanities Press, 1993), p. 6-7.

[18] See Talbot comment #8:

> In wanting to deal a blow at the "experimentalist", empiricist school of thought I think you're in danger of ignoring approaches to the development of science that are completely opposed to Marxism. I mean the various postmodern perspectives and widespread anti-scientific moods that

the misuse of that quote by postmodernists. My intention in using that quote in my lecture was not to deny the role of experiment and observation in the physical sciences, but to highlight the equally important role of thought experiments and in doing so to challenge the myth of science as a purely experimental enterprise. Despite the fact that I made this point absolutely clear in my letter to Talbot, both North and the Talbots reiterate the charge that I use the quote from Einstein in order to deny that he took experiments seriously. North makes use of the same accusation made by Talbot in our correspondence. After citing my exchange with Chris Talbot on the Einstein quote, North writes, "Talbot then made a prescient warning" and then goes on repeat Talbot's "warning", namely that I am in danger of sharing my bed with postmodernists. The Talbots also resurrect this accusation in their contribution to the current polemic, saying that,

> Einstein was responding, with a certain amount of irony, to the excitement that Eddington's work had generated in the press, but to use this remark, as Steiner does, to suggest that Einstein did not take experimental results seriously is simply unfounded and intellectually irresponsible.

Both North and the Talbots completely ignore my response to Chris Talbot, as if it had never been written. Their blatant dishonesty notwithstanding, the issue raised in that discussion is worthy of further examination. I took the opportunity in responding to Chris Talbot's accusation, to go beyond a reply to the immediate charge. I tried to broaden the discussion into a consideration of some of the seminal issues in the philosophy of science. Here is what I wrote:

> You are misconstruing the intent of the Einstein quote. Obviously I am not quoting Einstein in order to support, either overtly or covertly, some version of a postmodernist interpretation of science. The fact that this quote has been twisted out of historical context and used to promote an anti-scientific agenda is quite beside the point. The opponents of science and rationality, whether of a fundamentalist religious persuasion, or a postmodernist irrationalist stamp, always latch onto statements made by scientists or philosophers that highlight the paradoxical or problematic nature of the scientific enterprise. I could probably provide you with numerous examples in which such people quoted not only Einstein, but Galileo, Darwin, Freud and many others. They are especially fond of quoting some of their polemical opponents in their more reflective moments.

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you attack very well in your article on Heidegger.

I've mentioned the idea that Galileo didn't really do his experiments. Another is the use of the Einstein quote: "But I knew the theory was correct." (The quote is taken from Ilse Rosenthal-Schneider, a student of Einstein's in 1919, from her reminiscences. It refers to Einstein's response in hearing that Eddington's eclipse expedition that measured the bending of light-rays by the sun confirmed the General Theory of Relativity). It is a favorite reference for those who want to prove that scientific knowledge is entirely relative, that it is just one more "narrative", and that its verification by observation and experiment are an empiricist myth opposed by the great scientist Einstein.
http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf

> What I like about the Einstein quote is not that it supports the "relative" nature of scientific knowledge, but that it illustrates the importance that Einstein attached to his own thought experiments in his conviction of the validity of his theory. See my previous discussion about the interplay of thought experiments with observation in relation to Galileo. [19]

I am hardly alone in pointing to the importance that thought experiments held for Einstein. James Robert Brown, a philosopher of science who has done much work in investigating the nature and significance of thought experiments in the sciences writes the following:

> When he was only sixteen Einstein wondered what it would be like to run so fast as to be able to catch up to the front of a beam of light. Perhaps it would be like running toward the shore from the end of a pier stretched out into the ocean with a wave coming in: there would be a hump in the water that is stationary with respect to the runner. However, it can't be like that since change is essential for a light wave; if either the electric of the magnetic field is static it will not give rise to the other and hence there will be no electromagnetic wave.

Brown then quotes Einstein,

> "If I pursue a beam of light with the velocity c (velocity of light in a vacuum), I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to Maxwell;s equations."

Brown then adds,

> Conceptual considerations such as those brought on by this bit of youthful cleverness played a much greater role in the genesis of special relativity than worries about the Michelson-Morley experiment. Einstein goes on to describe the role of his thought experiment in later developments,
>
> "From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to be happen according to the same laws as for an observer who, relative to the earth, was at rest. For how, otherwise should the first observer know, i.e., be able to determine, that he is in a state of fast uniform motion. One sees that in this paradox the germ of the special relativity theory is already contained." [20]

Brown's judgment of the importance of thought experiments may be open to challenge, but he makes a serious case, as does Einstein himself. Even Chris Talbot would be hard pressed to accuse either one of them of opening the sluice gates to postmodernism.

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[19] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #8.

[20] James Robert Brown, *The Laboratory of the Mind: Thought Experiments in the Natural Sciences*, (Routledge, 1993), p. 14-15.
The Einstein quotes are from Autobiographical Notes, by A. Schilpp (ed), *Albert Einstein: Philosopher and Scientist*, (La Salle, 1949), p. 53

In his zeal to paint Einstein in his own image, Chris Talbot, in his correspondence with me, quotes a historian summarizing Einstein's philosophical views in order to emphasize that Einstein placed great importance on a materialist philosophical position in opposition to the anti-realist position of Mach and other positivists:

> The historian of science Gerard Holton, for example, notes that Einstein, who in his earlier period sided with Mach against the materialist positions of the scientist Max Planck, wrote a laudatory introduction to Plancks's 1931 article, "Positivism and the Real External World." Holton summarises Einstein's later views thus:
>
> "that there exists an external, objective, physical reality which we may hope to grasp – not directly, empirically, or logically, or with the fullest certainty, but at least by an intuitive leap, one that is only guided by experience of the totality of sensible "facts"."
>
> (Thematic Origins of Scientific Thought, Harvard University Press, 1988, p263) [21]

I do not deny that Einstein adopted a firmly materialist position against the skeptical anti-realism of Mach. But at the same time, Einstein was very far from the kind of empiricist-minded philosopher that Talbot would like to depict. To do so he has to selectively present Einstein – in this case by quoting a commentator's summary. Talbot mentions the laudatory essay on Planck but he does not quote from it. Were he to do so he would find to his horror that one of Einstein's philosophical inspirers was not the Englishman Locke or the Scotsman Hume, but the arch-rationalist German philosopher, Gottfried Wilhelm Leibniz, the author of the principle of preestablished harmony. Here is an excerpt from Einstein's essay where his debt to Leibniz is openly acknowledged:

> It is obvious to every experienced researcher that the theoretical system of physics is dependent upon and controlled by the world of sense-perception, though there is no logical way whereby we can proceed from sensory perception to the principles that underlie the theoretical structure. Moreover, the conceptual synthesis which is a transcript of the empirical world may be reduced to a few fundamental laws on which the whole synthesis is logically built. In every important advance the physicist finds that the fundamental laws are simplified more and more as experimental research advances. He is astonished to notice how sublime order emerges from what appeared to be chaos. And this cannot be traced back to the workings of his own mind but is due to a quality that is inherent in the world of perception. Leibniz well expressed this quality by calling it a preëstablished harmony.
>
> Physicists sometimes reproach the philosophers who busy themselves with theories of knowledge, claiming that the latter do not appreciate this fact fully. And I think that this was at the basis of the controversy waged a few years ago between Ernst Mach and Max Planck. The latter probably felt that Mach did not fully appreciate the physicist's longing for perception of this preëstablished harmony. This longing has been the inexhaustible source of that patience and persistence with which we have seen Planck devoting himself to the most ordinary questions arising in connection with physical science, when he might have been tempted into other ways which led to more attractive results.

[21] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Talbot Comment #9.

Nor is Einstein the only physicist to have found inspiration in Leibniz. The contemporary physicist Lee Smolin has explained the importance of Leibniz's principle of internal relations for much of modern physics. The principle of internal relations was the philosophical key behind relativity theory and is the key to the philosophical distinction underlying relativity theory from Newtonian mechanics. That is to say, the mechanical physics codified by Newton saw objects in space whose relationship to each other was imposed externally, through the workings of a divine clockmaker who had to wind everything up so that the universe kept time according to the laws of nature. Leibniz instead maintained that you could not separate objects from their relations – that relations are an integral part of the property of things. This idea anticipated by two centuries Einstein's theory of relativity whereby the space-time continuum of the universe was shown to be a property of matter and not something imposed on it externally. [22] Furthermore, the principle of internal relations has importance for the dialectical philosophy of science. Bertell Ollman has correctly identified this idea of Leibniz as one of the cornerstones of dialectics that was later assimilated by Hegel and Marx.[23]

Why did I insist on the importance of thought experiments in the work of Einstein (and Galileo) and why was Chris Talbot so quick to dismiss it? The underlying issue is not just our respective judgments about an event in the history of science, but our approach to the scientific method. Chris Talbot is essentially a positivist with a materialist patina. He accepts that sense perceptions grant us access to an objective world, a position that differentiates him from the skeptical empiricism of a Hume or a Mach. However, he is in agreement with the positivists on a whole range of other issues and is therefore at odds with a dialectical approach to the sciences. The issue of thought experiments is important in that it illustrates the dialectic between induction and deduction in the sciences. I discussed this point in some detail in my correspondence with Talbot where I summarized one of the most famous thought experiments in the history of science, Galileo's proof that Aristotle's theory of the motion of free falling bodies cannot be correct. Here is an excerpt from that discussion:

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[22] Lee Smolin cannot find enough words to praise Leibniz for having gone beyond Newton conceptually if not yet scientifically. He says of this idea of Leibniz,

> The argument Leibniz makes for his relational point of view is one of the most important in the whole history of philosophizing about nature.

Lee Smolin, *The Life of the Cosmos*, (Oxford, 1997), p. 215.

[23] In his book, *Alienation,* Bertell Ollman stresses Marx's indebtedness to Leibniz as well as to Hegel and Spinoza for assimilating the idea of internal relations. He writes,

> …I believe I am justified in ascribing a philosophy of internal relations to Marx because it would have required a total break with the philosophical tradition in which he was nourished for this not to be so. Hegel, Leibniz and Spinoza had all sought for the meaning of things and/or of the terms which characterize them in their relations inside the whole (variously referred to as 'substance', 'nature', 'God', etc); and judging by his voluminous notebooks, there are thinkers the young Marx studied with the greatest care.

Bertell Ollman*, Alienation: Marx's Conception of Man in Capitalist Society*, (Cambridge, 1976), p. 29-30.

> The other side of the coin of glorifying the experimental enterprise is ignoring the role of deduction and thought experiments in the development of the new science. The role of thought experiments – a purely deductive enterprise – comes out very sharply in an argument Galileo formulates in his Discorsi to prove that all objects, whether heavy or light, fall at the same rate. Galileo ponders Aristotle's thesis that heavier objects fall faster than light ones. He asks us to imagine that a heavy cannonballs is physically attached to a light musket ball. What happens if they are released together? If we take the Aristotelian view then we are lead to the conclusion that the lighter ball will slow up the heavier ball and the speed of the combined heavy and light balls will be slower than the speed of the heavy ball falling alone. However, the opposite conclusion is also true. The combined system is heavier than the heavy ball falling alone. Therefore it should fall faster than the heavy ball alone. Starting with Aristotle's premise, we have reached the absurd conclusion that the heavy ball is both faster and slower than the combined balls. Therefore, we have proven, by reductio ad absurdum, that Aristotle's premise cannot be correct. It follows that heavy balls do not fall faster than light balls.
>
> This example from Galileo's published work clearly demonstrates that he was more than simply an "experimenter"…
>
> Many commentators have noted that such thought-experiments play an ever more prominent role in the development of physics. Contemporary thought-experiments in physics are likely to be interlaced with many complex mathematical expressions, which make them far more difficult to follow intuitively. Nevertheless they retain the essential qualities of thought experiments. Does this mean that physical experiments/observations intended to verify theories are irrelevant? Of course not! There is a dialectical interaction between experiment and observation on the one hand and deductions and thought experiments on the other hand. [24]

The dialectical interplay between induction and deduction is a point that Engels raised in his *Dialectics of Nature* and which I quoted in my correspondence with Chris Talbot:

> "Induction and deduction belong together as necessarily as synthesis and analysis. Instead of one-sidedly raising one to the heavens at the cost of the other, one should seek to apply each of them in its place, and that can only be done by bearing in mind that they belong together, **that each completes the other**."(my emphasis)
>
> "Haeckel's Nonsense. – Induction against deduction. As if it were not the case that deduction = inference, and therefore induction is also a deduction. This comes from polarization." [25]

Basing himself on a study of Hegel's *Logic* as well as the much-maligned *Philosophy of Nature*, Engels tried to apply dialectical categories to the work of the natural sciences and brilliantly anticipated many of the key issues that are in contention today. In particular, his insight about the interplay between induction and deduction challenges one of the pillars of positivism in the philosophy of science – that there lies an unbridgeable gap between the method of induction and that of deduction, that the one deals with "pure facts" and experiments and observations, while the other deals with pure concepts and

[24] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner Reply #3.
[25] See Steiner Reply #9. The quotes are from Engels, Dialectics of Nature, p.204, 225.

relations whose connection to facts can never be ascertained. Helena Sheehan points to Engels's anticipation of current debates in the philosophy of science and specifically to the discussion of the relation between theory and observation:

> There is one more point worth making about Engels's philosophy of science in light of the subsequent history of the philosophy of science. This is the fact that Engels held views on many matters, such as on the history of science and the logic of scientific discovery, that not only anticipated certain contemporary theories, but are in some respects still in advance of them.
>
> Engels's views on the logic of scientific discovery also included many points sometimes thought to be original with Popper, but without the one-sidedness of Popper's account. Engels, in taking exception to the narrowness of his positivist contemporaries, pointed out "how little induction can claim to be the sole or even the predominant form of scientific discovery", and considered the main form of development in the natural sciences to be the hypothesis. However, he wisely did not throw out induction altogether, and penetrated more deeply than Popper into the complexities of the process, in holding that "induction and deduction belong together as necessarily as synthesis and analysis." [26]

Finally, to emphasize the point once more, I even provided Chris Talbot with a quote from Galileo himself in which he clearly states that while he did conduct experiments, he vehemently denies that this was his principle guide in formulating his theories. He gives the credit for that to what he calls "natural reason", i.e. hypotheses largely based on thought experiments:

> I have been a better philosopher than you in two ways: For besides asserting something which is the opposite of what actually happens, you have added a lie by saying that it was an experimental observation; whereas I have made the experiment, and even before that, natural reason had firmly persuaded me that the effect had to happen the way it indeed does. [27]

With the refinement that positivist philosophy received at the hands of the logical positivists [28] the split between induction and deduction was transformed into a more general statement about the relationship between theory and observation. Here is how James Robert Brown characterized it in summing up some of the chief principles of logical positivism:

> *There is a sharp distinction between theory and observation; the latter is neutral, independent of any theory or background beliefs.* This has been one of the most

[26] Helena Sheehan, *Marxism and the Philosophy of Science: A Critical History*, (Humanities Press, 1993), p. 46-47.

[27] Maurice A. Finocchiaro , *The Galileo Affair: A Documentary History*, (University of California Press, 1989), p. 184.

[28] The logical positivists were a group of philosophers and scientists who originally came together in Vienna in the 1920's and attempted to reform philosophy by purging it of all elements they considered non-scientific, or in their vocabulary, non-verifiable. Largely Jewish and politically moderately leftist, they were concerned about the growing influence of irrational and mystical attacks on science promoted by nationalists and anti-Semites.

> contentious of empiricist doctrines. Thanks to Kuhn and others…this point is largely discredited today. [29]

When North and the Talbots mock my discussion of the interplay between observation and thought experiments in physics, they are actually mocking Engels and the entire tradition of dialectics in the natural sciences and they are at the same time taking sides in a long dispute between a dialectical understanding of the scientific method and a positivist approach.

A bogus attack on postmodernism

North concludes his remarks about my views on science with a bogus attack on postmodernism that he introduces solely in order to mask over the abandonment by the International Committee of any theoretical work on pragmatism, empiricism and positivism. We have underscored this point in our previous polemics, most notably in MWHH where we noted,

> And while postmodernism certainly exerts some influence, particularly among academics and middle class radicals, we are not the first ones to note that the grip of the 'POMOS' is definitely on the wane today.
>
> … North's claim that the only progeny of pragmatism are the most irrational schools of subjective idealism is but another evasion of the responsibility to examine Deweyism and its influence on the Marxist movement. [30]

North denies all evidence to the contrary and continues to insist that postmodernism is the main, if not the only ideological threat to Marxism. On this occasion he comes down on the side of Chris Talbot's position on this issue in the context of Talbot's critique of my lecture on science. In that exchange Talbot enunciated a position that minimized the significance of the struggle against empiricism and pragmatism. He wrote,

> The problem with this emphasis [i.e. on the struggle against empiricism and vulgar Marxism] is that it assumes that vulgar materialism and empiricism are the main philosophical opposition faced by Marxists… That may have been the case for the circles Meikle was writing for, twenty years ago, but it is certainly not true today. [31]

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[29] James Robert Brown, *Who Rules in Science: An Opinionated Guide to the Science Wars*, (Harvard, 2001), p. 56. I cited this very useful survey of the science wars in my reply to Talbot. It includes an excellent summary of the different views in contention in contemporary debates within the philosophy of science. Neither North nor the Talbots refer to this book or any of the current debates between positivists and social constructivists. They lump all types of social constructivists in with postmodernists when the postmodernists are in fact only one extreme in one of the main branches of social constructivism. I discussed some of these distinctions in my reply to Talbot, apparently to no avail.

[30] http://permanent-revolution.org/polemics/mwhh_ch04.pdf p. 93.

[31] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Talbot comment #21.

Talbot however had it all backwards. According to him, empiricism and vulgar Marxism were simply transitory fads that came and went, whereas postmodernism is the main threat to Marxism. Such a view not only ignores the real history of postmodernism, but more fundamentally, repudiates the long held position within the International Committee that empiricism and pragmatism have deep historical roots within American and British capitalism. In my response to Talbot I tried to show that the influence of bourgeois ideology within the wider culture has far deeper roots in the traditions associated with empiricism, pragmatism and positivism than postmodernism, which has proven to be a temporary fad among radical academics. In making my case I noted that while the influence of postmodernism is not negligible, it is well past its heyday and is a small player when compared to the more traditional forms of bourgeois ideology. As an indication of the relative influence of postmodernism, I cited some well-known empirical indices: these included the number of university positions devoted to postmodernist themes but were certainly not limited to them. I also noted the prevalence in the mainstream media of themes associated with reductionist views of human nature, themes that are in line with pragmatism and empiricism but are very distant from postmodernism. I also quoted an intellectual historian, Dominique Lecourt, who, writing about recent trends in French philosophy, noted that,

> 'The picture of French philosophy implicitly painted by Alan Sokal and Jean Bricmont's choice of targets in *Intellectual Impostures* [a famous book that exposed postmodernist pretensions in science] seems outdated.' [32]

I never did receive a response from Talbot but five years later North finally responded in his *Odyssey* series. How does North answer me? He simply asserts that empirical evidence has no significance in this case. Here are his words:

> Ironically, for all his ritualistic denunciations of empiricism, Steiner's belittling of the intellectual problem posed by postmodernism was based on the most crudely empirical and pragmatic considerations. Relying on his own rough and ready calculations, Steiner argued that empiricists outnumbered postmodernists.

Perhaps my "calculations" were "rough and ready", but the amazing thing is that North's objection to my observations about the relative weight of these intellectual trends in contemporary culture are based on no evidence whatsoever. He does not present a single fact or a single observation to back up his contention that postmodernism is a bigger threat to Marxism today than empiricism or pragmatism. Here is what he says,

> The importance of philosophical trends cannot be correctly assessed on the basis of this sort of scorekeeping. Whether empiricists or postmodernists occupy more university chairs is not the decisive question. Far more significant is the objective content of postmodern thought—that is, the response it gives to essential philosophical problems—and its relationship to critical issues of the contemporary epoch. Eclectically drawing upon various retrograde trends in bourgeois thought, including pragmatism, postmodernism has arisen largely as an attempt to destroy Marxism by striking at its most essential propositions—above all, the objectivity of cognition and the concept of objective truth. Postmodernism goes beyond traditional skepticism in that it not only questions and denies the possibility of

[32] http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #22.

> attaining truth; postmodernist thought denounces and subjects to ridicule all intellectual projects that aspire to objective truth. On this basis it has sought with some success to inculcate within the intellectual environment an outlook of boundless cynicism and demoralization. The involvement of so many ex-radicals (including Stalinists and former Trotskyists) in this reactionary intellectual enterprise has contributed to its destructive impact, as postmodernism is broadly identified as a variety of left and even neo-Marxist thought.

The first thing to be said is that my case does not rest on "crude scorekeeping", but even a little bit of "crude scorekeeping" on North's part would have been an improvement on what he actually wrote. In contrast to my "crude score-keeping" North adduces that postmodernism is more significant than empiricism and pragmatism because of "the response it gives to essential philosophical problems." According to North, postmodernism, "drawing upon various retrograde trends in bourgeois thought, including pragmatism, postmodernism has arisen largely as an attempt to destroy Marxism by striking at its most essential propositions—above all, the objectivity of cognition and the concept of objective truth." There are several things wrong with this statement. First of all, postmodernism historically emerges not so much as a conscious attempt to destroy Marxism, but rather as the aftershock of a group of radical intellectuals in France and elsewhere who were disappointed with the outcome of the May-June 1968 events in France, when the radicalization of the working class was derailed by Stalinist and reformist bureaucrats. Of course postmodernist thought repudiates Marxism, which it labels a "failed meta-narrative", but there is more of the element of disorientation and demoralization in the "theorizing" of the postmodernists than of a conscious political direction. In any case, virtually every writer of recent intellectual history has remarked that postmodernism has long since passed the high point of its influence. I will just add one more observer to this list, from a book published very recently,

> It is one of the supreme ironies of the contemporary period that postmodernism's demise has been most rapid and extensive in contemporary France, its putative philosophical birthplace. [33]

But an even more serious problem with North's statement is his contention that postmodernism requires our attention because of its response to two fundamental issues in Marxist philosophy, namely "the objectivity of cognition and the concept of objective truth." The claim that these two issues constitute the "most essential propositions" of Marxist philosophy is truly "crude scorekeeping": it ignores the dialectic as a "most essential proposition" of Marxism and thereby obscures the fundamental antagonism between Marxism and bourgeois philosophy. After all, Marxism is not the only philosophy that acknowledges "the objectivity of cognition and the concept of objective truth." Empiricism, positivism and certain forms of pragmatism do as well. Thus, North's largely vacuous fulminations against postmodernism really serve as a rationale for an accommodation to bourgeois ideology, and ultimately to liberalism. And they also serve, as we noted previously, to mask over the embarrassing fact that the International Committee has done no work on empiricism, pragmatism and positivism in over two

[33] Richard Wolin, *The Seduction of Unreason: The Intellectual Romance of Fascism from Nietzsche to Postmodernism*, (Princeton University Press, 2004), p. xiii.

decades. Finally, while there is much heat in North's pronouncements on postmodernism, there is little substance. We have noted on previous occasions that even when it comes to postmodernism, no serious theoretical work has been done. That was very much the case when we wrote *Marxism Without its Head or its Heart* back in 2007 and it remains the case today. The tirades against postmodernism are entirely bogus, meant to distract the reader from the poverty of theory at the heart of the International Committee under North's leadership.

To be continued

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