

Note: The following is the record of an exchange I had with Chris Talbot precipitated by a lecture I gave in Berlin in the summer of 2002 to members and supporters of the Socialist Equality Party of Germany (Partei für Soziale Gleichheit – PSG). In order to make this exchange more readable, I have interspersed my replies to the points Chris Talbot raised immediately following his comments. The interested reader may follow the links in the Table of Contents to the individual comments and replies or may read the entire piece to get a sense of the whole picture. Talbot originally sent me his comments in December, 2002 and my reply was sent in February, 2003. Note that this was private correspondence never meant for publication. I am publishing it now as part of my reply to the smear campaign launched by David North and Ann and Chris Talbot as they make extensive use of selective quotations both from my response here and from my original Berlin lecture. For my reply to North and the Talbots see [The Downward Spiral of the International Committee of the Fourth International](#) on the [Permanent-Revolution](#) web site.

Alex Steiner

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Reply to Chris Talbot

The only reasonable way to arrange my response to the numerous points you raised was to intersperse my comments with the text of your remarks. In what follows, my responses are highlighted.

Talbot Comment 1

Some points on “Dialectics and the Crisis of Science”.

1) I don’t think that the approach you are taking to the Scientific Revolution is correct historically, and it is not adequate for a Marxist evaluation of such a major question.

Steiner Reply 1

Whether my evaluation is adequate will have to be left up to those who heard or read the lecture. But keep in mind that this was a one-hour lecture given to an audience of non-specialists in which I tried to convey some very difficult philosophical and scientific concepts. What is adequate in this context cannot be judged against the same yardstick as what I would have prepared for publication. It is always a challenge to present such difficult material without significantly diluting its content or going to the other extreme of making it so technical that it goes over the heads of the audience. I should point out that the lecture was very enthusiastically received by those who were in attendance.

Talbot Comment 2

I agree that Galileo is a central figure but surely the approach that Koyré takes to him (written in the late 1930s) – however important a contribution to the history of science at the time – cannot be given such a central place today. For example in the Cambridge Companion to Galileo, a book you recommend in your reply to critics of your Heidegger article, we find the comment:

“Koyré’s fascinating view is difficult to criticize because . . . it concerns what Galileo should have done rather than what he actually did . . . it attempts to use Galileo as a mouthpiece through which to expound Koyré’s own view of science.” (p404) So I don’t think your suggestion (following I presume the approach of Koyré) that Bruno, Galileo, Newton and the other creators of the new science saw themselves as Platonists will stand up to serious study. I’m not saying that Platonist ideas (in many forms) did not play a role, but in the last few decades literally hundreds of books and thousands of papers have been written on Galileo as well as all the other giant figures of the Scientific Revolution that do not substantiate Koyré’s approach. (I found a useful guide to the vast literature in

The Scientific Revolution and the Origins of Modern Science, by John Henry, Macmillan, 1997).

Steiner Reply 2

You object to my statement that Galileo as well as Bruno and Newton saw themselves as Platonists, but then you qualify your objection <<I’m not saying that Platonist ideas (in many forms) did not play a role>> and then qualify the qualification by pointing to hundreds of books and thousands of papers written on Galileo. Indeed the assessment of Galileo remains embroiled in scholarly controversy to this very day. I could not get into the nuances of these discussions in the context of a popular lecture. In fact I did not say that Galileo was actually a Platonist, merely that he “saw himself as a Platonist”. There is a big difference between the two. We often find in history that the ideas of a figure from one period are appropriated, often in a very one-sided or even paradoxical manner, to serve the purpose of another historical period. This is a point that Marx made with unmatched eloquence in his 18 Brumaire when he explained that the language and images of the bourgeois revolution appropriated the symbols and words of the ancient world or in the case of the English Revolution, of the Old Testament. Once these historical transformations have been realized, then their real content emerges, as Marx says and “Locke supplanted Habbakuk”. The heroic and millennial legends with which the bourgeois revolution had to intoxicate itself to accomplish its tasks gave way to the sober – and uninspiring - philosophy of private property and possessive individualism. In similar manner, opponents of medieval scholastic dogma – a dogma that was ascribed to Aristotle and the wisdom of the ancients – painted themselves in the mantle of the other great philosopher of the ancient world, Plato. Does this mean that the self-avowed “Platonist” scientists and philosophers of the Renaissance subscribed to Plato’s doctrine of knowledge as recollection or the theory of forms? In the scientific controversies of the 17th century, these austere epistemological issues were rarely the real issue. I specifically explained that the Platonic idea that motivated these critics of the neo-Aristotelian orthodoxy was that reality could be conceptualized mathematically. The “Aristotelians” vigorously denied this – though they made an exception for the laws governing the Ptolemaic heavenly bodies. Thus the central issue of the mathematical treatment of nature was posed in terms of a supposed debate between Aristotelians and Platonists. And without any doubt, in this debate, Galileo was on the side of the Platonists. It is certainly true that these debates between 16th and 17th century self-avowed Aristotelians and Platonists were embedded in a complex web of scientific and cultural contexts whose significance is still being debated by historians. Indeed one of the essays in the Cambridge Companion to Galileo indicates that,

“...even before Koyre had characterized Platonism as mathematics, Ernst Cassirer had found over sixteen distinguishable types of Platonism in the sixteenth century.”

(Peter Machamer, “Galileo’s machines, his Mathematics and his Experiments”, p. 55)

This same author later expresses his view that “much of the debate about the nature of early modern science revolved around attributed the labels of Platonism and Aristotelianism to various practitioners” inadequately formulated the issues and he

adduces a third candidate from the ancient world as the logical precursor to Galileo, namely Archimedes. He also makes the point that some of the self-avowed Aristotelians in the 16th century were champions of the mathematical application of physical science,

“...Petrus Catena, in the mid sixteenth century, was busy in Venice publishing works that showed Aristotle’s important use of mathematics.” (p.55)

Machamer is therefore skeptical of the characterization of Galileo and others who believed in the power of mathematics as “Platonists”.

In just this one volume we find a divergence of views on the issue of Galileo’s philosophical inspiration. William Wallace, in a discussion of the early influences on Galileo during his time Pisa notes that, “Possibly the strongest influence on Galileo from his years in Pisa, however, came not from his professors there, but from the colleague he encountered when he started teaching there Jacopo Mazzoni...Unlike his Pisan colleagues in philosophy, Mazzoni was not a monolithic Aristotelian. He had Platonic sympathies, and in the summer of 1589 he had introduced a course in Plato’s thought at the university.” (Galileo’s Pisan Studies in Science and Philosophy, p. 39).

As a final exhibit from this little survey, we have the opinion of Rivkah Feldhay who writes of, “...the futility of any attempt to reduce Galileo’s options into the dichotomy of a Platonic or Aristotelian discourse...This does not mean that Platonism and Aristotelianism had no ideological role in Galileo’s politics of knowledge. It means, however, that the labels must be deconstructed, in order to understand their function as one cultural practice among others used by many sixteenth and seventeenth century intellectuals, among them Galileo.” (The Use and Abuse of Mathematical Entities, p.121)

Leaving aside the postmodern glaze the terms “politics of knowledge” and “cultural practice” suggest, Feldhay’s point is that the terms Platonist and Aristotelian had a certain polemical usage in the context of the historical struggles over science and philosophy in the 16 and 17 century and we should not take too seriously the association of these labels with their inspiration in ancient philosophy. This is pretty close to the view I presented.

If your point is simply that we can be more nuanced in discussing the self-avowed Platonism of Galileo, then I would certainly agree. However if you are suggesting that there is no case to be made at all for discussing Galileo and Platonism in the same breath, then I think recent scholarship – and not just the scholarship of Koyre – will differ with you. Clearly from the vignettes I sampled from the Cambridge Companion to Aristotle, this is still an area in which scholars differ.

The influence of neo-Platonic ideas - not the same as the original philosophy of Plato, but as attenuated through the work of Plotinus and the rediscovery of Platonism in the Renaissance by Ficino – is apparent in the works of Bruno and Kepler. 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 some of the great pioneers. In the case of Bruno, an excellent book that discusses the influence of the Hermetic tradition on the new science are “Giordano Bruno and the Hermetic Tradition” by Frances Yates. There the author notes that the ideas of the Roman atomist Lucretius were indeed important to Bruno, particularly his view of the infinite universe. However Bruno’s appropriation of Lucretius is very contradictory. Yates mentions that,

“Bruno found the conceptions of infinite space and innumerable worlds, inhabited like our own, in Lucretius *De natura rerum* ... But he absolutely transforms the Lucretian notions (themselves derived, of course, from the Epicurean philosophy) by imparting to the innumerable worlds magical animation, totally absent from Lucretius’ cold universe, and to the infinite and its contents the function of being an image of the infinite divinity – again a notion totally foreign to the agnosticism of Lucretius. Thus the godless universe of Lucretius, in which that pessimistic man took refuge from the terrors of religion, is transformed by Bruno into a vast extension of Hermetic gnosis, a new revelation of God as magician, informing innumerable worlds with magical animation...” (p. 246)

Bruno’s interest in magic and alchemy has been known for centuries, although historians downplayed them. In the case of Newton, little of his interest in the occult, magic and alchemy were known until fairly recently. Betty Jo Dobbs, in *The Janus Faces of Genius* and other works has done much to transform our understanding of Newton. It turns out that Newton spent more time on alchemical experiments than on physics and his physical science was conceived by him as an expression of his mystical religious beliefs. Dobbs and the historian you mentioned, I. Bernard Cohen, have had an ongoing dispute as to whether Newton’s alchemical interest was at all relevant to his scientific endeavours. It seems to me that a dispute such as this 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. While I do not presume to pass judgment on this ongoing dispute, I think that Dobbs theory is intriguing and should not be dismissed out of hand.

Talbot Comment 3

One does not have to be an empiricist, arguing that scientific ideas always follow on from experiments, to point out the key role that observation and experiment played in that period. The view that Galileo could not have carried out some of his experiments, though fashionable for a while, has been refuted by the work of Stillman Drake and by experimenters who have carefully reproduced the experiments (see for example the revised edition of *The Birth of a New Physics* by I. Bernard Cohen, especially the supplements).

Steiner Reply 3

The amazing Stillman Drake did indeed make an important contribution to our understanding of Galileo. (Drake’s professional life began as neither a scientist nor a historian. He went from law into a second career in his forties.) Drake discovered in 1973 some previously unknown manuscripts proving that Galileo did carry out a great number of experiments. I therefore think that I was mistaken in omitting a qualifying reference to Drake’s work in my account of Koyre’s interpretation of Galileo when I wrote,

“Furthermore it has been pointed out by some historians that Galileo did not have the technical ability to measure the elapsed time of falling bodies with sufficient precision to prove his case.”

I should point out that I did make reference to Drake’s differences with Koyre in my more comprehensive treatment of Galileo on another occasion, replying to a critic of my Heidegger article. Note that I never denied the role of experiment and observation in the birth of the new science. I specifically wrote in the sentences immediately preceding the one above,

. “Certainly experiment and observation play a role in the work of Galileo for instance. For Galileo however, experiments such as the dropping of weights from a high tower were meant to validate his theory. It was not the basis for the discovery of his theory.”

My object in this section was not to deny the role of experiments in the scientific enterprise, but to upend one of the myths surrounding that enterprise – i.e. the scientist as primarily an experimenter or observer. This myth is deeply intertwined with positivist and empiricist conceptions of the scientific enterprise. This interpretation of Galileo gained currency in the 18th century and dominated historians until Koyre’s work. An article in the Cambridge Companion to Galileo includes an excellent discussion of this and other myths surrounding Galileo. The author, Michael Segre, writes,

“Yet the view that modern science is essentially empirical and detached from prejudices prevailed, and with it the belief that Galileo, as one of the earliest modern scientists, grounded – and perhaps even founded – experimental science. Galileo became (and still is) the model for the empiricist scientist who, unlike the natural philosophers of his day, sought to answer questions not by reading philosophical works, but rather through direct contact with nature... Thus, by the end of the eighteenth century, the picture that emerged of Galileo was that of Galileo as freethinker, martyr of science, and founder of experimental science.” (The Never-ending Galileo Story, p. 398)

Although we may be sympathetic to the motives of those who parlayed this story in the age of Enlightenment, namely the struggle against clericalism and reactionary philosophical dogmas, this view of Galileo is nevertheless historically inaccurate and thus retains the status of a myth. It is first of all not true that Galileo inaugurated experimental methodology in the sciences. Aristotle and the ancients were quite cognizant of experiment and observation. It is also true that for the ancients experiment and

observation did not attain the rigorous standards that were achieved in the Renaissance and much of their empirical data was little more than second or third hand anecdotes. This fact accounts for some of the “bloopers” modern historians have found in Aristotle’s scientific corpus. But to dismiss Aristotle’s scientific work as “worthless” as some empirically minded contemporary critics have done is just ahistorical and silly. Aristotle’s scholastic interpreters on the other hand did indeed abjure the role of experiments. Their view was that the Aristotelian-Ptolemaic conception of the universe was a coherent and interconnected body of knowledge that did not admit of any errors. It was against this dogmatic attitude that Galileo and the other pioneers of the new science had to do battle.

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 cannonball is physically attached to a light musket ball. What happens if they are released together? If we take the Aristotelian view then we are led 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”. Finally, we have Galileo’s own description of his methodology in a letter written to a critic. In his Reply to Ingoli he wrote,

“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.**”(emphasis added, A.S.) (from *The Galileo Affair: A Documentary History*, edited by Maurice A. Finocchiaro, University of California Press, 1989, p. 184)

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. (See my later discussion of the dialectic of induction and deduction.)

Finally, whereas I accept your criticism that I overstated my thesis by not mentioning Stillman Drake’s work, I think that perhaps you overstate just a bit the significance of Drake’s research when you say that he “refuted” the findings of Koyre. Drake’s research certainly presented a necessary corrective to Koyre, but there are at least some scholars today who feel there remain big areas of controversy here. For instance, Michael Segre, in the article previously quoted, notes that an earlier critic of Koyre, Thomas Settle, reconstructed some of the inclined plane experiments ridiculed by Koyre.

“(Settle) concluded, in contradiction to Koyre, that the experiment could be performed with the means Galileo described. Settle’s experiment, however, neither refuted nor claimed to refute Koyre’s general methodological argument that since all experiments are premeditated, whether or not Galileo performed some experiments is not important for understanding his intentions.” (The Never-ending Galileo story, p. 405).

Segre then adds that some others drew different conclusions,

“Settle’s interesting result, however, was twisted by several later empirically oriented works. Typical of these is Stillman Drake’s complaint, in his classic and outstanding article on Galileo’s experimental confirmation of horizontal inertia: “<Koyre’s paper was reprinted years later in book form without so much as a note by the editors concerning Settle’s refutation of its thesis.>” (Segre, p. 405. Stillman Drake quote cited on p.416)

It appears then that Segre likewise believes that Drake is overstating his case when he claims that Koyre’s thesis has been “refuted”.

Finally, it is not even a settled matter whether Drake’s discovery proves that Galileo did in fact carry out experiments with the required degree of precision. I found the following abstract of a talk given on this subject by way of an Internet search. (Unfortunately I do not have the text of the talk, but it is possible to email the author whose address appears in the header.)

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In the early 1950's the influential historian Alexander Koyre asserted - contrary to commonly held thought at the time - that Galileo's actual experiments were sadly inadequate and that he relied primarily on armchair or thought experiments. This view was challenged in the early sixties when Thomas Settle demonstrated that Galileo could indeed have carried out with satisfactory precision the experiments with inclined planes that he describes in the Discorsi. Confirmation of Settle's argument came in 1973 when Stillman Drake perused Galileo's unpublished working papers in the Florence Biblioteca - this collection is known as Manuscript 72 or Codex 72

- and found irrefutable written records of Galileo's experiments. (Incredibly, no one appeared to have had a critical look at these pages before.) However, Drake and others overstate the case: the inclined plane experiments of Galileo are not as carefully crafted as is asserted. In particular, the agreement between experiment and theory (elementary physics and calculus) is not nearly as tight as is claimed. The purpose of my talk is to make this point via a mathematical analysis of folios 81, 114, and 116 of Codex 72.

I am not sure what this proves other than that there is still no consensus among scholars as to the significance of Drake's "refutation" of Koyre. In hindsight, I think it would be fair to say that Koyre introduced a necessary correction to the previously "experimental" interpretation of Galileo. In doing so, Koyre may have overstated his case and Stillman Drake provided a necessary correction to Koyre. In the process of correcting Koyre however, Stillman Drake overstated his own case. I think the proper balance between Koyre's work and that of Drake's can only be purchased from the standpoint of a dialectical understanding of the interplay of induction and deduction in the scientific enterprise.

Talbot Comment 4

Also although obviously I am opposed to "vulgar Marxism of the Stalinist camp", I cannot accept your downplaying the influence of Democritus and Epicurus. For example Pierre Gassendi, one of the most influential figures of the 17th century, based his work on Epicurus.

Steiner Reply 4

I should have added that such distorted accounts of a historical process are by no means confined to "vulgar Marxists of the Stalinist camp." I will later present an example to illustrate my point. (See my discussion of reductionism.)

Talbot Comment 5

Even Koyré admitted in his later writing that he had underestimated their importance. Koyré writes:

“. . . by a mathematical approach to nature, atomism in the works of Galileo, Boyle, Newton, etc. – became a scientifically valid conception and Lucretius and Epicurus appeared as the forerunners of modern science.” (Cambridge Companion to Galileo p183).

Steiner Reply 5

The Koyre quote is from an article by Pietro Redondi, From Galileo to Augustine, in which the latter states that, "Koyre himself later recognized that he had underestimated another kind of physics."

Redondi’s judgment has no basis in a reading of Koyre. Redondi’s use of the quote from Koyre is a classic example of ripping a partial statement out of its context in order to create an impression that is contrary to that of the author one is quoting. The full quote from Koyre is as follows:

“The atomism of the ancients, at least in the aspect presented to us by Epicurus and Lucretius – it may be that it was different with Democritus, but we know very little about Democritus – was not a scientific theory, and though some of its precepts, as for instance, that which enjoins us to explain the celestial phenomena on the pattern of the terrestrial ones, *seem* to lead to the unification of the world achieved by modern science, it has never been able to yield a foundation for the development of a physics; not even in modern times: indeed, its revival by Gassendi remained perfectly sterile. The explanation of this sterility lies, in my opinion, in the extreme sensualism of the Epicurean tradition; it is only when this sensualism was rejected by the founders of modern science and replaced by a mathematical approach to nature that atomism – in the works of Galileo, R. Boyle, Newton, etc. – became a scientifically valid conception, and Lucretius and Epicurus appeared as forerunners of modern science. It is possible, of course, and even probable, that, in linking mathematics with atomism, modern science revived the deepest intuitions and intentions of Democritus.” (From the Closed World to the Infinite Universe, p. 278,n 7)

Koyre reiterates that the birth of the new physics had little to do with the influence of the ancient atomists. Instead, it was motivated by the theme of the mathematization of nature, which in the Renaissance came to be identified with the philosophy of Plato or Pythagoras. The result however of the successful application of this methodological revolution in science was the elaboration of an atomistic conception of the universe, one that recalled the insights of the ancient atomists. But these insights -of the ancient atomists- by themselves could not have been used to develop the new science. They remained brilliant, but not yet scientific intuitions. In this judgment, Koyre is not so far removed from the well-known summary of this period provided by Friedrich Engels.

“It was necessary first to examine things before it was possible to examine processes. One had first to know what a particular thing was before one could observe the changes going on in connection with it. And such was the case with natural science. The old metaphysics which accepted things as finished objects arose from a natural science which investigates dead and living things as finished objects. But when this investigation progressed so far that it became possible to take the decisive step forward of transition to the systematic investigation of the changes which these things undergo in nature itself, then the last hour of the old metaphysics sounded in the realm of philosophy also. And in fact, while natural science up to the end of the last century was predominantly a collecting science, a science of finished things, in our century it is essentially a classifying science, a science of the processes, of the origin and development of these things and of the inteconnection which binds all these natural processes into one great whole.” (Ludwig Feuerbach, p45-46).

Engels was writing almost a century and a half prior to our time. He considered the height of the crisis of science then to have reached its peak in the 18th century. He discusses the non-dialectical outlook that dominated 18th century science and compares it unfavorably with the dialectical (though not yet scientific) intuitions of the ancient Greeks,

“High as the natural science of the first half of the eighteenth century stood above Greek antiquity in knowledge and even in the sifting of its material, it stood just as deeply below Greek antiquity in the theoretical mastery of this, in the general outlook on nature.” (Dialectics of Nature, p.7)

Engels felt that the advent of historical investigation in natural science began to challenge the anti-dialectical philosophical underpinnings of this science. He specifically mentions the investigation of the origin of the solar system (Kant-Laplace), the origin of the earth (Lyell) and the origin of man and the differentiation of species (Darwin) as key. By the latter part of the 19th century, Engels’ own period, a dialectical conception of nature begins to emerge in which science once more returns to the spirit of the ancient Greeks – only this time on this basis of real science instead of brilliant speculation – where we can talk of “the origin and development of these great things and of the interconnection which binds all these natural processes into one great whole.” Elsewhere Engels notes that this new dialectical outlook emerges out of the development of the sciences themselves, and that dialectics so to speak forced its way into the thinking of scientists who were nurtured in non-dialectical modes of thought.

“...the metaphysical outlook has become impossible in natural science owing to the very development of the latter...”

The return of dialectics takes places unconsciously, hence contradictory and slow. Dialectics as the science of the total connections.” (Dialectics of Nature, p. 269)

The conviction that science had overcome its own gaps and fragmentation was not uncommon in Engels time. But then came the new challenges posed by relativity, quantum mechanics and much else, developments that Engels could not have anticipated. Thus, a new crisis of science emerges. It was the theme of my lecture to explore this new crisis of science, specifically as to how it is elaborated in physics, and how through its own inner development, the new science that at first seems to fragment reality into incommensurable parts, once more gives way to the reintroduction of dialectics as “the science of the total connections” and how it does so “unconsciously, hence contradictory and slow.” I do not see from your remarks any agreement that we are dealing with a crisis of science today, not to mention the possibility of its inner resolution. (Of course such resolution is always provisional for the work of science is a never-ending spiral of progression.)

Talbot Comment 6

2) I also would question your approach to the development of Relativity and Quantum Mechanics. If we are considering the historical development of modern physics, and are

developing a Marxist approach to a central issue of our times, I can't recommend putting so much reliance on one work by a present day scientist, i.e. Kaku.

It is misleading to put so much emphasis on Riemann's geometry in the overthrow of Newtonian mechanics. Kaku puts great stress on the importance of geometry (ten dimensional) as the basis of string theory that is hoped will unite quantum mechanics and relativity. No doubt he feels that in a popular presentation it is good to reinstate Einstein's geometrical approach, after a number of decades in which his work on unified field theory was seen as misguided.

As someone who did their doctorate in Relativity and Riemannian Geometry, I don't want to belittle the contribution that Riemann made to mathematics, along with Gauss, Lobachevski and others in developing geometries that went beyond Euclid. Certainly the fact that Einstein was able to use Riemannian geometry to replace Newton's troublesome “action at a distance” gravitational force was one of the major scientific advances in the 20th century.

However for a serious historical understanding of the development of relativity and quantum mechanics such a concentration on Riemann is very one-sided. Throughout the latter part of the nineteenth century and into the twentieth, there were a whole range of practical and theoretical problems that demanded the attention of physicists and mathematicians and were eventually resolved (to some degree anyway) in Relativity and Quantum Mechanics. Kaku doesn't really consider these in the seven pages he devotes to Einstein's Special Theory in Chapter 4 and seven pages on Quantum Mechanics in Chapter 5 (before he moves on to Yang Mills). The problems that arose in physics – such as the incompatibility of electromagnetic theory (especially light) with Newtonian mechanics – were not known in Riemann's time and only confirmed by a series of experiments (of which Michelson-Morley is the most famous).

I don't think the question of a fuller and more reliable account of the history is a secondary issue here. The philosophical problems that Kaku refers to very briefly (the sections on the anthropic principle, Schrodinger's cat, and the many worlds theory and then again in the concluding chapter) arose quite sharply in the 1920s in the famous debates between Bohr and Einstein. The Copenhagen Interpretation, as it became known, that eventually became accepted despite Einstein's reservations, was heavily influenced by neo-Kantian and positivist views that had become widely accepted. Instead of a serious challenge being mounted by Marxists along the lines that had been advocated by Lenin and Trotsky, the crude approach of the Stalinists did great damage to the cause of materialist dialectics.

In my opinion, whilst not in anyway minimizing the achievements that have been made by string theory and the use of higher dimensional geometry, serious issues relating to the foundations of a unified theory of particle/fields and gravity still remain. (To be fair to Kaku he does refer to the problem of a lack of physical principles behind the entirely mathematical development of string theory).

Steiner Reply 6

I agree with much of your criticisms of Kaku’s book. It is a popular book written for laymen. The explanation of some key areas of the history of physics is too sketchy. The philosophical issues that have come to the fore with the introduction of relativity and quantum mechanics are dealt with very superficially. That being said, I think it is only fair to point out the strengths of Kaku’s book. First of all, it is written in a popular style but unlike other “popular science” books it provides a reasonable account of the scientific material, enough to convey key concepts without going into the technical-mathematical details. Perhaps because you come from a technical background you may fail to appreciate just how difficult it is for lay readers to absorb this kind of material and if someone manages to write about it coherently without completely oversimplifying or distorting the material, then that is a big plus in my opinion. Secondly, I was intrigued by the guiding thread of Kaku’s book, that it has been through the introduction of higher dimensions that previously disjointed areas of physics have become unified. Unlike most science writers, who see the development of science as a series of random, brilliant intuitions, Kaku sees a certain logic and order in scientific breakthroughs and relates them to the philosophical discipline of the search for unity. Furthermore, there is a great deal of material in Kaku’s book that relates the developments in physics to broader philosophical and cultural concerns. For instance, I was particularly intrigued by Kaku’s ability to trace the first intimations of a universe of higher dimensions, which has a mathematical origin in the work of Riemann, (and Lobachevski et al as you mention), to its adoption as a theme of popular culture and appropriation by the spiritualist phenomena of the 19th century, by popular literature, the Cubist art movement and other areas of culture, only to return to scientific speculation when general relativity secured a physical explanation for a non-Euclidean universe.

Talbot Comment 7

Apart from its weakness on the real historical development of modern physics, it should also be pointed out that Kaku’s approach to string theory itself is only one of many in this exciting area of the frontiers of physics. I would recommend a look at such books as Brian Greene’s “The Elegant Universe”, Lee Smolin’s “Three Roads to Quantum Gravity,” the books by Martin Rees for the connections with astronomy and cosmology, and (though more mathematical) the books by Roger Penrose. Smolin gives websites that contain much more material.

Steiner Reply 7

I am well aware that Kaku’s book contains one of many approaches to string theory. He says so himself and I provided this caveat in my talk.

I have looked at all the books you have recommended, though I have not yet absorbed them. Out of the selections you mentioned, I particularly liked Roger Penrose’s book and Lee Smolin’s book. In fact, I was so intrigued by Smolin’s book that I also purchased his earlier work, *The Life of the Cosmos*. Smolin presents what I consider to be a dialectical understanding of the origin of the cosmos based on the concept of self-organized wholes, one that challenges all the assumptions of what he calls “radical atomism”. Smolin is

also aware of the philosophical pedigree of the idea of self-organized wholes in the philosophy of Leibniz. He includes a quote from Leibniz in his prologue:

“This interconnection (or accommodation) of all created things to each other, brings it about that each simple substance has relations that express all the others, and consequently, that each simple substance is a perpetual, living mirror of the universe.”

Bertell Ollman, in his book “Alienation”, has rightly pointed out that Leibniz’s philosophy of “internal relations” is a direct precursor of dialectics as it was worked through systematically by Hegel and later transformed by Marx. Smolin is therefore making a very conscious break both in science and in philosophy away from atomism toward dialectics. Smolin in turn lead me to another author who has been credited with devising the theoretical underpinnings of “self-ordering wholes” out of complexity theory – Stuart Kauffman. I had been aware of Kauffman previously, but had not had the opportunity to study his work. With his book, *The Origins of Order*, Kauffman has, I believe revolutionized our understanding not only of biological evolution, but has provided the analytical tools to investigate and conceptualize all kinds of self-ordered wholes, how they come into being and how they maintain themselves in a state of dynamic equilibrium despite changes in the environment. While it may be premature to make any definite pronouncements about this new area of scientific research, I think it has the potential to be a fruitful wellspring for a dialectical reconceptualization of much current scientific theorizing.

Talbot Comment 8

3) 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 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.

Steiner Reply 8

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.

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.

Talbot Comment 9

The real history shows the opposite to be the case. Einstein, despite his earlier influence by Mach, by 1919 was increasingly opposed to the positivism of Mach and others, who claimed that only individual sensations or observations could be relied on in science. Mach had refused to accept Relativity Theory, regarding it as “inadmissibly speculative.” Einstein’s philosophical views, like his biography as a whole, are the subject of countless studies and debates. However there is no evidence that his position – despite the use of this quote by sceptical philosophers – moved away from materialism but rather the opposite. 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 Planck’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)

The same issue arises with your favorable reference to Thomas Kuhn. That all experimentation and observation is “theory laden” to begin with can easily be interpreted as support for complete relativism. Whilst Kuhn did some serious work on the history of science (such as his study “Black Body Radiation and the Quantum Discontinuity”) his more philosophical writings – particularly his theme in “The Structure of Scientific Revolutions” that one scientific theory replaces another in a “revolution” such that the two theories are “incommensurable” – have surely become central to postmodern attacks on science (see for example Alan Sokal’s “Intellectual Impostures”).

Steiner Reply 9

The mention of Thomas Kuhn in my lecture goes back to the original venue of this talk, which was at Kuhn’s academic home, the Massachusetts Institute of Technology. The use

of Kuhn, like that of MIT professor emeritus Noam Chomsky, is one of those “hooks” one uses to get the attention of an audience. The passing reference to Kuhn was specifically in connection with the idea that the experimental side of the scientific enterprise, the collection or observation of empirical data, is always “theory-laden”. (Incidentally, if memory serves me right I believe that I skipped the reference to Kuhn when I gave this talk at the Humboldt as it would have been less meaningful to that audience than to one at MIT. The Kuhn reference remained in my transcript.) You jump to the conclusion that my reference to Kuhn opens the sluice gates to relativism. In doing so, you are failing to distinguish between two very different interpretations of the significance of Kuhn’s work. These positions have recently been the subject of debate in the philosophy of science. I am referring to the “strong” programme in the sociology of knowledge (identified with the work of David Bloor) as opposed to the “weak” programme (identified with Robert Merton). The strong programme does indeed imply that all scientific knowledge is ultimately just a cultural artifice. It has no objective standing on its own. The weak programme in the sociology of knowledge on the other hand states that the scientific enterprise is always historically and socially embedded, but its products are ultimately objective truths about the state of the world. Both the strong and weak programmes of the sociology of knowledge oppose positivism, which does not recognize the historical dimension of the scientific enterprise at all. But that is where the similarity ends. The strong programme advocates a radical historicism that denies the objective nature of reality. The postmodernists are but one expression of the most radical currents within this stream. The weak programme on the other hand is concerned with such issues as the historical background that made it possible to formulate Newton’s laws of motion in the 17th century and why this was not possible at the time of Democritus. It concerns itself with investigating the historical conditions that made possible a particular line of inquiry. It does not presume however to attempt any connection between the historical genesis of particular scientific theories and their validity. The latter is precisely what the strong programme does attempt. That is what opens the door to such strange creatures as “feminist physics” and the derision of mainstream science as “totalitarian”. The weak programme in the sociology of knowledge is however completely consistent with a Marxist understanding of the development of science. The Marxist view of the scientific enterprise is clearly opposed to both the positivist approach and the cultural relativism of the strong programme in the sociology of knowledge. I would recommend a recent book that does an admirable job in summarizing these issues – *Who Rules in Science: An Opinionated Guide to the Wars*, by James Robert Brown. (Harvard University Press, 2001.)

As for Kuhn himself, there is no question that his book, *The Structure of Scientific Revolutions*, became a kind of bible to the strong programme of the sociology of knowledge. It is not clear however to which of the two camps of the sociology of knowledge Kuhn himself belonged. Some of his statements, for instance that different paradigms of scientific theories are incommensurable, can be taken to be supportive of cultural relativism. But he also backed off the more extreme interpretations of his theories. Again, Brown’s book gives a useful summary of these developments.

I should add that despite his problems, Kuhn should not be so easily dismissed. Kuhn’s book was a landmark event in the philosophy of science, coming as it did in a time when the ahistorical positivist approach to science, typified by Popper and Hempel, was then virtually unchallenged. And despite the excesses to be found in his work to which cultural relativists have latched, some of his pronouncements make perfect sense from a Marxist perspective. Of course Kuhn was no Marxist, but he did provide an account of scientific revolutions in which the dialectic of the transformation of quantity into quality plays a prominent role. The positivists tended to see the scientific enterprise merely as a cumulative process, whereas Kuhn insisted on the difference between “normal” science, wherein work is carried on according to an established paradigm, and those moments when a conceptual revolution in science emerges. Kuhn was wrong in insisting that the old paradigm was then incommensurable in terms of the new paradigm. We know for instance that with the new paradigm of relativity Newtonian mechanics and the Euclidean geometry it is based on are seen to be merely a special case, i.e. the case in which the curvature of space is precisely zero.

Additionally, Kuhn’s insight that observations are “theory laden” cannot be dismissed. In saying this, Kuhn was doing nothing more than insisting that the kind of data the scientific enterprise deals with is historically mediated. He was arguing against the positivists who always maintain that “facts”, given through sense perception, are immediately present at hand to us. If you recall, I made the very same point in reply to a critic of my Heidegger essay – that the immediate can never be divorced from conceptual mediation. If I may be allowed to quote from this piece, I wrote at the time:

<<But Hegel already pointed out a long time ago that there is no such thing as a purely immediate intuition, i.e. one that is uncontaminated with mediation.

“The antithesis between an independent immediacy of the content or of knowing, and, on the other side, an equally independent mediation that is irreconcilable with it, must be put aside, first of all because it is a mere presupposition and an arbitrary assurance.”

You argue that it is a mistake to regard our mathematical understanding of the natural world as something that is simply given outside of a historical context. True enough, but it is equally true that our ‘immediate’ intuitions are not given once and for all, but are culturally and historically determined. The ‘immediate’ intuition of a Trobriand Islander who has never seen an airplane, when first confronted with this phenomena, will not be the same as that of an inhabitant of the island of Manhattan. What is immediate is therefore relative. Every intuition is a combination of the relatively immediate with the relatively mediated. Truth as such does not lie with the certainty of the immediate, as Heidegger would have it, but with the most highly developed forms of mediation. >>

If we further consider the implications of the “theory laden” nature of scientific observations, we can see that this is an echo of something to which Engels points in his *Dialectics of Nature*, namely the dialectical interconnection between induction and

deduction in the scientific enterprise. Take the following samples from *Dialectics of Nature*:

“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, p204)

“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.” (p. 225.)

Note that Engels specifically targets one of the mechanical materialists of his time, Ernst Haeckel, as falling into the trap of not recognizing that the gathering of data (induction) is inseparable from the organization of data (deduction) and that one conditions the other. The cause of this problem for Haeckel, for whose scientific work Engels has some respect, is that he is caught in an anti-dialectical mode of conceptualization (“polarization”).

Where the social constructionists go wrong, as exemplified in the most radical wing of postmodernists that Sokal so effectively lampooned, is in taking the “theory laden” nature of observation and from that jumping to the conclusion that all knowledge is subjective, a result of nothing more than cultural prejudice or “will to power”. But Sokal’s piece, while it exposed the soft underbelly of postmodernist pretenders who write about science, fails to address any of the more complicated issues posed by the more sophisticated adherents of the strong programme of the sociology of knowledge. Sokal himself was aware of this and provided a caveat to the reader in an essay discussing the impact of his hoax.

Talbot Comment 10

It seems to me that in writing on both the Scientific Revolution and on Quantum Gravity/String Theory there is an excellent opportunity, indeed responsibility, for Marxists to deliver well-aimed blows at the postmoderns. In the history of science there are now whole sociological schools that treat science as the social production of knowledge, and see the truth of scientific results as an entirely relative question. In science itself, especially with regard to questions in Quantum Mechanics such as causality and the relationship between observer and the physical world, there are several competing viewpoints – including the view of a majority of physicists that if there are mathematical theories that predict the experimental results, philosophical questions are best ignored. In his book “Schrodinger’s Kittens”, John Gribbin reports that when Paul Davies organized a radio programme on Quantum Mechanics in the mid 1980s and asked eight top experts to contribute, there were virtually eight conflicting standpoints. Even though Gribbin is a well known proponent of science he concludes his book in completely postmodern vein:

“I stress, again, that all such interpretations are myths, crutches to help us imagine what is going on at the quantum level and to make testable predictions. They are not, any of them, uniquely ‘the truth’; rather, they are *all* real, even where they disagree with one another.”

Steiner Reply 10

I think Gribbin’s statement is a perfect example of the snares one gets into by thinking that philosophical questions can be avoided, or that they are irrelevant to scientists. I also think that it is not precise to characterize Gribben’s statement as an expression of postmodernist currents of thought. Rather Gribben’s statement has much more immediate kinship with a vaguely formulated form of positivism. When Gribbin defines reality as “the ability to make testable predictions” he is merely expressing a version of the beloved “verification theory of meaning” that has been the bedrock of logical positivism since the days of the Vienna circle.

Talbot Comment 11

4) It is of course perfectly valid for Marxists to explain the limitations of mechanical and empiricist philosophical conceptions, and to advocate a study of the great exponents of the dialectic -- Aristotle and Hegel. I note that you have recommended the book by Scott Meikle in this regard. Now from the standpoint of philosophy only, it is possible to see the truth in Meikle’s conception that “atomism and essentialism have been fighting it out since [the Greeks]” (p 9). However in the context of a broader discussion, it is surely not permissible for a Marxist to ignore questions of historical materialism, the role of productive forces, the class struggle, politics and so on. Here I think that the approach you take to the history and development of science are misleading. I mean in talking about the “philosophical heritage spawned by Newton” in which you include Locke, Hume, the French materialists, the positivists of the 19th century, the logical atomism of Russell and Whitehead, the early Wittgenstein, and the later turn to language analysis, and even, in the following paragraph, Thomas Hobbes.

In lumping together the great figures of the Enlightenment with latter day empiricists and positivists in this way it seems to me you can end up making big concessions to the postmoderns. I thought your approach to Heidegger – putting him squarely in his historical context and tying him to the fascists—was a very powerful one. Surely that is the issue here. Isn’t it essential to place these thinkers in their historical and social context?

Steiner Reply 11

It is true that I put Heidegger in historical context and showed his lifelong connections to fascism. However I hope I did more than that. I was trying also to show that his

philosophy was an expression of his fascism, without at the same time reducing an examination of his philosophy to the biographical details of his life. I therefore examined his ideas, and showed how they were consistent with and supported a certain world outlook that fascism likewise embraced. I also indicated the common historical roots, in the ideology of the Volk, in Lebensphilosophie, the nihilism of Nietzsche, all the different threads of what Lukacs called “the destruction of reason”; and I also tried to indicate how this irrationalist tendency emerged as one of the branches of the reaction against the Enlightenment.

I could have gone into much greater depth in discussing the historical basis behind the emergence of the 17th century revolution in science. But neither did I ignore it. Some practical choices are unavoidable in preparing material for a popular lecture. In discussing a topic as vast as the 17th century revolution in science as well as the 20th century revolution, certain decisions had to be made as to what gets highlighted within the scope of a one hour lecture. Given these constraints, I made the decision to focus on the content of the ideas of Newton, Hobbes, and the others and provided only the barest sketch of a historical background. That is perhaps unfortunate, but I do not see how it could have been avoided without consigning the discussion of the philosophical ideas to the most superficial of treatments. By the way, just because the names Newton, Hobbes, Locke, the French materialists and Russell and Wittgenstein come up in the same sentence does not mean that I “lump them together”. That sentence is just a very high level summary of a conclusion I draw about the fortunes of a philosophical theme over some three hundred years. I draw this conclusion only after I had previously discussed, sometimes with more, sometimes with less detail, the individual ideas of some of these figures such as Newton and Hobbes.

Talbot Comment 12

Don't we need for example to explain the ‘metaphysical’ approach of these great Enlightenment figures in terms of the lack of scientific understanding of their day, following Engels? In other words to stress that they predate the *historical* approach to nature that became possible in the 19th century -- with the developments in geology, the Kant-Laplace theory of the origins of the solar system, and above all Darwinian evolution. And even then it is necessary to point out that they were not the limited thinkers that later empiricists make out as Meikle correctly points out in relation to Locke.

Steiner Reply 12

You are correct to point out that the Enlightenment figures lacked a historical dimension in their theories. However, that is not the only reason why their thinking was called by Engels “metaphysical”. What Engels meant by this terms was that the mechanical thinkers were confined in their understanding to categories of rigid opposition. Identity

was simply identity and excluded difference. Necessity excluded chance. Quantitative changes excluded qualitative transformations. The introduction of a historical treatment into the sciences was very significant, but by itself that in no way guarantees that “metaphysical” forms of thinking have been transcended. Engels point in his discussion of the historical understanding introduced by the Kant-Laplace theory of the origin of the solar system, Lyell’s theory of the origin of the earth’s geography and Darwin’s theory of the origin of man, was that what was previously considered separate and unrelated branches of sciences had now been brought together. That was certainly a significant development, but it did not prevent the continued application of categories of rigid opposition in relation to all the branches of scientific inquiry.

A typical example of the problem of rigid opposition in the sciences is discussed by Engels in his *Dialectics of Nature*.

“The law of identity in the old metaphysical sense is the fundamental law of the old outlook: $a=a$. Each thing is equal to itself. Everything was permanent, the solar system, stars, organisms. This law has been refuted by natural science bit by bit in each separate case, but theoretically it still prevails and is still put forward by the supporters of the old in opposition to the new: a thing cannot simultaneously be itself and something else. ..For natural science in its comprehensive role, however, even in each single branch, abstract identity is totally insufficient, and although on the whole it has now been abolished in practice, it still dominates people’s minds, and most natural scientists imagine that identity and difference are irreconcilable opposites, instead of one-sided poles the truth of which lies only in their reciprocal action, in the inclusion of difference within identity.” (DN, p.183)

Talbot Comment 13

Surely it is necessary in that way to defend Enlightenment thought against the postmoderns – as the historical precursor of socialism and Marxism. (Spinoza is a particularly central character in this regard -- see the important recent book by Israel).

Steiner Reply 13

I agree that I should have included Spinoza in my discussion. However, I must reiterate once again that I was not attempting either a comprehensive presentation of the history of science nor any kind of comprehensive view of the history of philosophy

Talbot Comment 14

Again I would caution that postmoderns often use attacks on empiricism and mechanical materialism – taken out of their real historical context -- to smuggle in philosophical relativism and their thesis that there are no “grand narratives.”

Steiner Reply 14

Is it true that postmodernists “often use attacks on empiricism and mechanical materialism taken out of their historical context to smuggle in philosophical relativism”? In many cases the postmodernists don’t feel they have to “smuggle” anything in. They are quite open about their rejection of any notion of objective truth and their embrace of relativism. That is what the postmodern notion of the “difference” is all about. I trust by the way that you are not suggesting that I presented a view of empiricism and mechanical materialism “taken out of their historical context.” I specifically noted the progressive nature of the mechanical philosophy when it expressed the new understanding gained by the sciences as well as its role in opposing the reactionary institutions of feudalism and the Church. I also noted that what was progressive in the 17th and 18th century has become reactionary today. History and science have moved on, but the mechanical conceptions we inherited from the 17th century still hold sway.

An even more important point to note is the notion you implicitly put forward that every historical-philosophical discussion is obliged to defend the Enlightenment against the attacks of postmodernists. I certainly agree that Marxists should defend the Enlightenment against its irrationalist detractors. But don’t you think Marxists also have an obligation to defend dialectics against some of the progeny of the Enlightenment, the atomists and empiricists? Why does one need to undermine the other? Remember that when you say that the Enlightenment is the historical precursor of socialism and Marxism that it is just as true to say that it is also the precursor of liberalism and the bourgeoisie. The heritage of the Enlightenment is not an all or nothing proposition. Marxism comes into its own by way of a critique of the Enlightenment. Just take a look at the Thesis on Feuerbach! Practically each one of the theses is directed at some doctrine or other of the Enlightenment. But “critique” does not mean “rejection”. Marxism represents a genuine Hegelian *aufheben* of the Enlightenment. The Enlightenment is at once preserved and overcome.

As Marxists, we stake out a philosophical position that is independent of and opposed to both the subjective idealism of the postmodernists and the anti-dialectical and reductionist notions (about which I will say more presently) of the modern atomists. I have written two major pieces that take on the postmodernists, one being the Heidegger piece and the other a still unpublished essay called “The End of Irony”. I don’t think therefore that I can be accused of having neglected postmodernism. Why then question the legitimacy of my directing attention in the lecture on science and dialectics, at the problems posed by empiricism and atomism?

It is curious that a similar objection was raised in Berlin by a member of the audience. This comrade felt that the main ideological battleground today should be directed against the postmodernists and that Marxists were therefore obligated to engage in a kind of “united front” (his words) with the positivists and empiricists against the postmodernists. I trust you are not advocating anything so antithetical to the spirit of Marxism.

Talbot Comment 15

It really needs a lot of work putting in to show the way these ideas developed, especially in France, out of Stalinist and radical circles. To give just one example from a book I happen to have in hand, the feminist Margaret Wertheim, in “Pythagoras’ Trousers”, writes the following nonsense which is typical of this genre:

“By constructing a natural philosophy in which the universe was wholly controlled by a male deity, the mechanists created a world picture that reflected the kind of society they wished to justify – patriarchal and monarchist. . . .Constructed in opposition to magic, organicism, civil unrest, and rising female power, the mechanist world picture must be viewed not simply as a product of “science” but also as the by product of a conservative sociopolitical backlash.”

Steiner Reply 15

I do thank you for providing this very entertaining quote, but I think as I have indicated that to concentrate the bulk of your attention on the stupidity of the postmodernists without at the same time taking on the empiricists and positivists becomes a distraction from the crucial ideological work that Marxists have to carry out today.

Talbot Comment 16

5) I also found your approach to “reductionism” a troubling one. It should be pointed out that the approaches being taken by most physicists to quantum gravity, including Kaku, are fundamentally reductionist. Strings are not the same as atomic, corpuscular matter perhaps, but nevertheless the advocates of string theory are attempting to show the whole world is made up of these entities. I see nothing wrong with this.

Steiner Reply 16

I see nothing wrong with this either. The problem is that your use of the term “reductionism” differs from mine. String theory attempts to unify the physical universe in terms of a single explanatory theory – the so-called Theory of Everything. As such it brings together previously disparate theories about the nature of gravity and the other physical forces. String theory therefore sets out to accomplish precisely what I mentioned was the driving impulse of science from its inception – to provide a coherent picture of an interconnected whole. The crisis of science that I discussed involves the periodic emergence within science of new areas of knowledge that disrupt our understanding of a unitary whole. Thus, the initial effect of the revolution in science of the 17th century was

to disrupt the coherent picture of the Great Chain of Being that was the bedrock of the Aristotelian-Ptolemaic version of things. Recall John Donne’s words,

“Tis all in pieces, all coherence gone.”

Eventually this crisis is resolved by the triumph of Newtonianism which introduced a mechanical picture of the world and integrated all known physical phenomena. (I should add that the discovery of the historical dimension in the 19th century, as Engels pointed out, rounds out the picture and shows a deeper interconnection between previously separate areas of scientific investigation - namely the connection between living and non-living matter, the earth and the solar system, etc.)

But then comes the discoveries of the 20th century that knock Newton off his pedestal. Once more “coherence” is gone from our picture of the universe. String theory holds out the promise – and so far of course it is only that – to resolve the crisis of 20th and 21st century physics. The possibility that string theory may be able to integrate all known physical forces into a unitary organizing principle – what you call “reductionism”, is therefore a hopeful development.

1) Reduction of the biological into the physical – and reduction of the cultural-historical into the biological.

The type of reductionism referred to in my lecture is the conflation of the principles that define one level of scientific investigation with those of another level. The classic example is the attempt to explain biological phenomena in terms of the laws of physical mechanics. That is precisely what Descartes and some of his followers attempted. The result was instead of explaining animal and human organisms in terms such as “stasis” or “metabolism”, they were depicted instead as purely mechanical devices composed of levers and pulleys. This had the effect of removing the element of “life” from the investigation of a biological organism. Historically, this step in the history of science was doubtless unavoidable, for once mechanics emerged as a genuine science it was inevitable that it would be employed to explain all natural phenomena. But today we rightly view such attempts to “reduce” biological phenomena to mechanical designs as a retrogressive step. Another example of reductionism was the attempt of the social Darwinists in the 19th century to apply the biological principle of natural selection to human culture and history. This resulted, as is well known, in interpretations of history that were either overtly racist or an apology for class oppression. The reductionist error here is that whereas natural selection applies in the realm of biology it cannot be transplanted to the realm of history and culture without doing violence to the immanent laws of historical development. Parenthetically, Marxists have always been opponents of reductionism. Engels once again provides an excellent example:

“The investigation of chemical process is confronted by the organic world as a field of research, that is to say, a world in which chemical processes take place, although under different conditions, according to the same laws as in the inorganic world, for the explanation of which chemistry suffices. In the organic world, on the other hand, all chemical investigation lead in the last resort to a body – protein – which, while being the

result of ordinary chemical processes, is distinguished from all others by being a self-acting, permanent chemical process... When chemistry produces protein, the chemical process will reach out beyond itself, as in the case of the mechanical process above, that is, it will come into a more comprehensive realm, that of the organism.”(DN p. 324)

In general reductionism conflates the structures of one level of a hierarchical structure with those of another. I explained in my lecture that the development of science dialectically understood leads us to see the objective world as an interconnected matrix of wholes hierarchically organized such that each whole is a part of a larger whole. Thus for example, biology, which encompasses living matter is a whole that is also a part of a larger whole, the natural world. (which is the largest whole there is.) Modern positivism and empiricism inevitably leads to reductionist approaches in the sciences because it locates reality only in the smallest possible units – atoms, or atomic facts in the philosophy of Russell. When translated into the social realm, atomism leads to the philosophy of possessive individualism and something like Hobbes notion of a war of all against all. This means that wholes are always defined in terms of their parts and fails to recognize when a part is organized into a new whole, a newly emergent structure comes into being that cannot be derived just from the parts. This point was made by Stephen Jay Gould in the context of a critique of Richard Dawkins:

“A higher unit may form historically by aggregation of lower units. But so long as the higher unit develops emergent properties by nonadditive interaction among parts (lower units), the higher unit becomes, by definition, an independent agent in its own right, and not the passive “slave” of controlling constituents.” (The Structure of Evolutionary Theory, p. 618).

I don't think it should be necessary to elaborate the dangers that a reductionist program in science poses. The modern avatar of reductionism in biology is no longer the version of man as a machine that was popular in the 18th century. Today we must deal with a new form of biological determinism - one that has been heavily influenced by information theory and genetics. This is the view that has been propounded most prominently by Richard Dawkins, Daniel Dennett and Stephen Pinker and philosophically inspired by the pioneer of sociobiology, E. O. Wilson. It is from these quarters that the theory of the “selfish gene” emerges, as well as the notion that culture is transmitted in atomistic chunks of data – memes. The reductionist program in biology makes of man a product not of his historical development but of his genes or his “memes.” It says nothing about productive forces or social relations or the role of classes or political movements or cultural institutions and ideology. Instead we get the following:

“We are survival machines – robot vehicles blindly programmed to preserve the selfish molecules known as genes. This is a truth that fills me with astonishment. Though I have known it for years, I never seem to get used to it.” (Richard Dawkins, *The Selfish Gene*, Oxford University Press, 1976, p. ix)

Once again, we find an appropriate reply to this kind of thinking in the work of Engels, who noted the sleight of hand practiced by the social Darwinists in transposing a theory originating in biology (though misinterpreted to begin with) back to the social world.

“The whole Darwinian teaching of the struggle for existence is simply a transference from society to living nature of Hobbes’s doctrine of bellum omnium contra omnes and of the bourgeois-economic doctrine of competition together with Malthus’s theory of population. When this conjurer’s trick has been performed... the same theories are transferred back again from organic nature into history and it is now claimed that their validity as eternal laws of human society has been proved. The puerility of this procedure is so obvious that not a word need be said about it.” (Letter to P. Lavrov, n12-17 November 1875).

2) Reduction of consciousness and culture into the biological-physical-chemical:

Still another kind of reductionism that is today very prevalent, particularly in popular culture, is the attempt to make consciousness and cultural products disappear into a welter of genes, neurons and chemical transmitters. There are numerous variations of this type of reductionism. We can see versions of it in the IQ testing industry, within the behavioral school of psychology, among certain researchers in neuro-physiology, within the new sub-discipline in analytic philosophy called “philosophy of mind”, etc. The agenda of philosophers such as the Churchills, or Daniel Dennett, language researchers such as Stephen Pinker, or biologists such as Richard Dawkins, is to make consciousness disappear. We can no longer talk about our feeling of love for instance, without discussing neuro-transmitters and serotonin levels. And we can no longer discuss the aesthetic qualities of a painting without reference to the experience of our ancestors millions of years ago on the African savannah. It is true that there are connections between these phenomena. Only a subjective idealist of the solipsistic variety exemplified by Berkeley would deny that neuro-transmitters and serotonin levels are a necessary objective underpinning to our feeling of love for instance. But it is only a reductionist who maintains on the other hand that understanding serotonin levels leads you to somehow understand the feeling of love, that love cannot be understood on its own terms as a felt emotion. There are crude and sophisticated versions of this type of reductionism. A recent rather crude example comes from Stephen Pinker’s book, “The Blank Slate”. In this book, Pinker argues that the rise of modern art in the first decades of the 20th century was based on a denial of our biologically determined human nature. He says, “the new philosophy of modernism that would dominate the elite arts and human nature was carried over with a vengeance to postmodernism ... (which is) more Marxist and far more paranoid.. (and gave us) Andres Serrano’s Piss Christ (a crucifix in a jar of artists urine), Chris Ofili’s painting of the Virgin Mary smeared in elephant dung, (and similar fare)”. Had we paid attention to our “human nature” Pinker maintains, we would still be able to enjoy the “middlebrow realistic fiction or traditional education” that our grandparents did. To paraphrase Louis Menand’s review of Pinker’s book, the preference by middle brow culture for paintings of red barns and weeping clowns has been naturally selected. Quite a damning critique of the modern aesthetic, wouldn’t you say? For a

wonderful dismantling of Stephen Pinker, I must refer you to the recent review of his book in the New Yorker by Louis Menand.

<http://www.hereinstead.com/sys-tmpl/bmenadonpinker/>

The philosophical critique of this type of reductionism was once again beautifully stated by Engels:

“Among natural scientists motion is always as a matter of course taken as mechanical motion, change of place. This is a survival from the pre-chemical eighteenth century and makes a clear conception of the process much more difficult. Motion, as applied to matter, is change in general. From the same misunderstanding is derived also the craze to reduce everything to mechanical motion ...- which obliterates the specific character of the other forms of motion. This is not to say that each of the higher forms of motion is not always necessarily connection with real mechanical ..motion, just as the higher forms of motion simultaneously also produce other forms; chemical is not possible without change of temperature and electric changes, organic life without mechanical, molecular, chemical thermal, electric changes, etc. **But the presence of these subsidiary forms does not exhaust the essence of the main form in each case. One day we shall certainly “reduce” thought experimentally to molecular and chemical motions in the brain; but does that exhaust the essence of thought?”** (emphasis mine A.S.) (D.N. p.174-175)

3) Reduction of philosophical and scientific ideas into the cultural:

There is yet another form of reductionism, one that has often been practiced by, but by no means limited to vulgar Marxists. I refer to the practice that was most clearly evident in such Stalinist masterpieces as “Pragmatism, the Philosophy of Imperialism”. Aside from their crude historical distortions, what characterizes these works is the thesis sometimes implicit and sometimes explicit, that the validity of a philosophical idea is exhausted by its relation or use to the class struggle. But it is not only among Stalinist hacks that we find such notions. Ellen M. Wood, currently an editor of Monthly Review, is a very far from being a Stalinist hack. She is an excellent historian who has made some important contributions to our understanding of both ancient society and the birth of capitalism in such books as Peasant, Citizen, Slave. Yet in her early work, *Class Ideology and Ancient Political Theory: Socrates, Plato and Aristotle in Social Context*, Wood presents an elaborate argument for treating philosophical ideas solely in terms of their historical and social content. Indeed, in a section of her book titled “The Poverty of Philosophy”, Wood denies that there is any objectively scientific content whatsoever to a philosophical idea. She writes,

“No longer can it be maintained that philosophical analysis can be kept separate from historical analysis, or at best that the latter is a very junior partner of the former. Instead, the very contrary can be argued...” (p. 12)

Wood’s formulation is half right. It is also therefore half wrong. Philosophical analysis cannot be divorced from historical analysis. But if that is all it becomes, i.e. if philosophy becomes a junior partner to history, then we have ventured into the problematic of cultural relativism. Indeed, it is just as true to say that a historical analysis cannot proceed that is divorced from a philosophical foundation. What is a historical analysis after all? Are we simply talking about a collection of facts? Or do we venture, when we do history, to investigate a series of phenomena as to their essential movement, in their dialectic of becoming? If the latter is the case, then philosophy is essential. As Marx said, “All science would be superfluous if the appearance of things coincided with their essence”. Wood’s notion, that history can somehow proceed above “philosophy” is just another form of the empiricist assumption that takes their philosophy for granted as the only sensible one. I discussed this form of philosophical mystification in my talk.

Wood’s confusion, one that she shares with the vulgar Stalinists, is that she conflates the historical conditions for the emergence of ideas with the validity of those ideas. This is a well-known logical fallacy – the genealogical fallacy. Her interpretation of the philosophy of Ancient Greece is therefore entirely one-dimensional. She attempts to explain the ideas of the philosophers more or less solely in terms of their social and class position. It never occurs to her, for example, that Socrates’ assertion of the distinction between opinion and knowledge, while obviously rooted in the outlook of an aristocratic elite, may also contain profound objective content for the future development of the sciences. Thus, she has chapter headings such as the following:

“Socrates: Saint of Counter-Revolution”, and “Aristotle: Tactician of Conservatism”.

This way of looking at philosophy has been and continues to be a favorite target of those seeking to discredit Marxism as being inherently reductionist. A typical example is the following statement:

“In the Communist Manifesto and in Engels’s Anti-Duhring intellectual theories – especially law, political philosophy, and ethics – are reduced directly to class interests...” (Eugene Kamenka, *Marxism and the History of Philosophy*, in *The Historiography of the History of Philosophy*, Mouton and Co, 1965)

Kamenka is wrong in claiming that Marx and Engels “reduced” intellectual theories to class interests. However, this is precisely what many self-professed Marxists have done, giving undeserved credibility to the charge that Marxism is a form of “historical reductionism”. What Marx and Engels did was demonstrate that specific theories can only emerge within the context of a certain level of economic, technical and cultural development. Furthermore, Marx and Engels never thought they were done with an idea by tracing its historical genesis. They always explored the content of an idea and demonstrated its internal contradictions and mystifications. The same cannot be said for reductionist historians such as Wood.

The same methodology that Wood exemplifies in her treatment of philosophy has been employed by others in the field of the natural sciences. The procedure of the strong

programme in the sociology of knowledge is identical to the one employed by Wood. David Bloor and other adherents of the strong programme conflate the historical origin of scientific ideas with the validity of those ideas. In this way reductionism leaves the door open to cultural relativism.

Talbot Comment 17

But also why is there any need to support Lewontin in what seems to me to be very dogmatic attacks on his fellow scientists? (It is noteworthy in this regard that Lewontin’s co-thinker British scientist Stephen Rose – who originated in the same Stalinist and radical circles – is leading the campaign against Israeli academics, i.e. holding them responsible for the Zionist regime).

Steiner Reply 17

I am not sure what you mean when you say that Lewontin has launched “very dogmatic attacks on his fellow scientists.” I think Lewontin has done an excellent job in subjecting to a critique the ideological biases and distortions propounded by psycho-metricians for instance. Lewontin, Rose and Leon Kamin did much to expose the fraudulent work of the late Sir Cyril Burt. The story of Burt’s manufacture of phony empirical data to support his thesis of a genetic determination of IQ is well known. Aside from that Lewontin and Richard Levins have done a great deal of important work in developing the principles of a dialectical biology. Lewontin in particular has consistently taken on and exposed the unscientific ideas of the biological determinists for the last three decades.

Why is it “noteworthy” that Stephen Rose is advocating a reactionary boycott of Israeli academics. Of course we should oppose the politics of Rose. But are we not able to distinguish the scientific work of Rose from his backward politics? Is this not what you rhetorically asked me to do in the case of Wilson, Dawkins, and Dennett? Can’t we discuss Rose’s contribution to the exposure of what Stephen Jay Gould called “the mismeasure of man” on its own merits?

Talbot Comment 18

I don’t think the job of Marxists is to defend one approach to biology or physics as “dialectical” against other “reductionist” ones.

Steiner Reply 18

You seem to be advocating some kind of “hands off” approach on the part of Marxists when it comes to the sciences. If that is indeed your position, then I wonder what you think the point of a dialectical philosophy of nature is? In other words, is it the role of

Marxists to be merely passive observers of the different approaches in science, or does philosophy actually have a role to play? Engels for one, thought that philosophy was critical to the development of science. He wrote,

“Natural scientists believe that they free themselves from philosophy by ignoring it or abusing it. They cannot, however, make any headway without thought, and for thought they need thought determinations. But if they take these categories unreflectingly from the common consciousness of so-called educated persons, which is dominated by the relics of long obsolete philosophies, or from the little bit of philosophy compulsorily listened to at the university (which is not only fragmentary, but also a medley of views of people belonging to the most varied and usually the worst schools), or from uncritical and unsystematic reading philosophical writing of all kinds. Hence they are no less in bondage to philosophy, but unfortunately in most cases to the worst philosophy, and those who abuse philosophy most are slaves to precisely the worst vulgarized relics of the worst philosophers.” (DN 183-184)

Of course in saying that Marxists should play an active role, I do not for a moment suggest that scientists must conform to a new orthodoxy as was done by the Stalinists. Rather, I am suggesting that philosophers have an obligation first of all to become acquainted with the work of the sciences, and then to subject that work to a philosophical critique. Neither am I claiming that Marxists have some infallible insight into the truth. But I do think that Marxists, like other critical thinkers, have an obligation to put forward their views on the vital and controversial issues that are involved in the current disputes in science and philosophy. That is in fact what Engels was attempting in his *Dialectics of Nature*. Conversely, scientists also have an obligation – one that is almost never fulfilled – to assimilate the highest achievements of philosophy and try to integrate them into their scientific work. That was precisely what Engels was advocating in the above citation.

I should add that there are some notable exceptions among contemporary scientists. Lee Smolin, Stuart Kauffman and the late Stephen Jay Gould in particular come to mind as examples of scientists who have grappled at a deep level with philosophical issues.

Talbot Comment 19

I think that we should follow Trotsky in his writings on science. He insisted that both Freud and Pavlov were following scientific, materialist approaches to psychology but from different directions. Trotsky valued both their great contributions. When it came to philosophical or political issues of course, as when Pavlov attempted to analyse war as the result of human psychology, Trotsky was prepared to disagree. Shouldn't we follow the same approach to E.O Wilson, Daniel Dennett and Richard Dawkins – all of whom have made important contributions to science?

Steiner Reply 19

I agree with Trotsky that we can distinguish the scientific work of scientists from their ideological biases, particularly when they adopt a reductionist argument derived from one branch of science and impose it on another as Pavlov did. I am quite capable of distinguishing the scientific work of E. O. Wilson from his ideological pronouncements. Don't we as Marxist have an obligation to subject remarks such as the following to a critique?

“The members of human societies sometimes cooperate closely in insectan fashion, but more frequently they compete for the limited resources allocated to their role sector. The best and most entrepreneurial of the role-actors usually gain a disproportionate share of the rewards, while the least successful are displaced to other, less desirable positions.” (E. O. Wilson, *Sociobiology: The New Synthesis*, Harvard University Press, 1975, p.554)

Is Wilson being a scientist when he is illegitimately transposing the laws of insect behavior onto human culture, or is he being an ideologue? I would argue that the triumvirate of Wilson, Dawkins and Dennett, regardless of the merits of their scientific work, have played a role as leading polemicists in the reductionist program of biological determinism. Whatever scientific work Dawkins may have produced, when he makes statements such as the following,

“We are survival machines – robot vehicles blindly programmed to preserve the selfish molecules known as genes.” (Richard Dawkins, *The Selfish Gene*, p. ix.)

are we not obligated to reply that the militant atheist Dawkins is replacing one kind of mysticism with another, i.e. he replaces theological mysticism with the mysticism of “selfish genes” that direct our zombie-like bodies?

And what are we to make of Dennett, who has written such absurdities as “Your grandmother was a macro”, in his book, “Darwin's Dangerous Idea”?

Talbot Comment 20

From a philosophical standpoint I think it is necessary to view reductionism in the way that Trotsky does in “Dialectical Materialism and Science”. Whilst recognizing that it is not permissible to directly reduce chemistry to physics, or social questions to physiology, nevertheless it is essential to recognize that each branch of science rests on the laws of other sciences “in the so-called *final instance*” (Trotsky's italics). Without this it would be impossible to maintain the unity of the material world.

Steiner Reply 20

I agree with this last point and Trotsky's statement. However the problems of reductionism that I have indicated are not addressed by this comment.

Talbot Comment 21

6) My final point concerns this very question of materialism versus idealism. Steve Long tells me that you do not think Engels was correct in making this the fundamental issue in philosophy. Perhaps you are influenced by Scott Meikle in this regard. Meikle writes:

“The really fundamental distinction for Marxists is that between essentialism and atomism. The diamat of ‘official’ Marxism had as its basic distinction that between Idealism and Materialism; a reductive materialist account was then given of materialism which, because it gave no adequate place to whole entities with natures, readily led into atomism; hence enormous confusion.” (p 9)

Steiner Reply 21

I think it should be obvious by now that the person who most inspired me to delve into the crisis of science is none other than Friedrich Engels.

You should be aware that Engels made other statements summarizing the history of philosophy. For instance,

“Two philosophical tendencies, the metaphysical with fixed categories, and the dialectical (especially Aristotle and Hegel) with fluid categories...” (Dialectics of Nature, p. 153)

On this occasion at least, Engels did not formulate the basic question in the history of philosophy as that of the struggle between materialism and idealism.

I have always thought that Engels high level summaries should be used with caution. As a summary at a very high level for a popular presentation, Engels formulation may prove useful. The problem however has been that Engels statements, through no fault of his, have been misused, particularly by the Stalinists, to paint a picture of the history of philosophy that is completely false. The entire history of philosophy is examined as if the sole question at issue has been that of idealism vs. materialism. This first of all ignores the fact that there have been many other critical questions in dispute in the history of philosophy and they are not all reducible to the materialism vs. idealism paradigm. For example, there is the question of the One and the Many, a question first posed by Parmenides and that periodically turns up over and over again. There is also the question of free will vs. determinism. (Determinism is most often identified with a materialist position, but it is by no means clear that this logically follows. For instance, Marx noted in his Doctoral dissertation that the ancient materialist Epicurus left room in his philosophy for free will and this was an advance over the determinism of previous atomists.)

Furthermore, the history of philosophy is distorted in other ways by trying to fit it into this very narrow framework. The Stalinists used to present the history of philosophy as a progressive march toward materialism. They put a plus sign next to every philosopher designated as a materialist, and a minus sign next to those designated as idealists. George Novack followed this same methodology. I have noted from private discussions that the great majority of people who call themselves Marxists think exactly the same way. Engels, when he actually wrote about the real historical development of philosophy, never made the mistake of Novack and the Stalinists. He always recognized that the progressive development of philosophy is itself contradictory. I wrote about the dangers of this mechanical methodology some three decades ago (in my *Liberal Philosophy of George Novack*) and I see no reason to change my mind now. Such an approach is fundamentally ahistorical. It takes the categories of idealism and materialism and abstracts them out of their historical context. It fails to recognize that idealism may be a progressive development in one period, while materialism can just as surely be a retrogressive tendency under certain conditions in another period. It also fails to distinguish between philosophies that are qualitatively heterogeneous. Thus there are many people who do not recognize that the materialism developed by Marx is fundamentally different from the materialism of the 18th century philosophes. (Marx made this point very clearly in his *Thesis on Feuerbach*.) Ultimately, the development of philosophy, allied with the sciences, does lead to a fundamentally materialist outlook, but one that contains all the richness, of all previous philosophy, both idealist and materialist and various shades in between. If as Hegel said, Truth is the Whole, then we can draw no other conclusion.

As for Meikle, I am not sure why you bring him up. I did not mention him at all in this lecture. It is true that I have found some of his work extremely useful and I have recommended him elsewhere. Meikle did very important job in bringing out Marx's debt to Aristotle and he did an excellent job in showing that Marx's philosophy was incompatible with empiricism. In particular, Meikle brought to our attention the fact that Marx's project cannot be properly understood without grasping his conscious retrieval of final causality as an explanatory principle. In other words, when Marx is investigating what kind of thing capitalism is, it is critical to know the end toward which the inner contradictions of the capitalist mode of production lead. This is in keeping with Aristotle's dictum that "To know a thing is to know its end."

That does not mean that I am an uncritical admirer of Meikle. The statement of his that you quote, that the fundamental axis of ideological struggle is between atomism and essentialism is one with which I would agree only with many qualifications. I don't think for instance, that it can apply as a general statement of philosophical currents throughout all periods of history. I don't think for instance that the differences between essentialism and atomism held the same significance in the ancient world as they do today. I also think that Meikle does not sufficiently recognize that in certain periods of history atomism was a progressive development. His treatment therefore lacks a historical perspective. I do think however that this is an apt description of one of the main lines of ideological struggle – though by no means the only one - in the last three centuries, since the birth of the new science and the bourgeois revolution first announced itself.

Talbot Comment 22

The problem with this emphasis is that it assumes that vulgar materialism and empiricism are the main philosophical opposition faced by Marxists. (I should probably put Marxists in inverted commas since it appears that most of the people that Meikle is responding to in his book are not Marxists but various radicals and Stalinists. Meikle himself seems to be associated with a radical group around the journal *Critique*, led by Hillel Ticktin). That may have been the case for the circles Meikle was writing for, twenty years ago, but it is certainly not true today.

Steiner Reply 22

Here I think we come to a fundamental disagreement. The immediate object of Meikle’s criticism is perhaps of little concern today, but empiricist and atomistic modes of thinking have not disappeared. Far from it, I think they remain “the main philosophical opposition faced by Marxists.” How could it be otherwise, for empiricism and atomism are not simply intellectual fads of the 1980s but are the dominant modes of thinking that we have inherited thanks to the heritage of the still potent scientific revolution of the 17th century, the bourgeois ideology of possessive individualism and all the pressures for reductionist explanations of society and consciousness. If you are maintaining that postmodernism today represents a bigger threat than empiricism and positivism, then I think you are wrong both factually and historically. A survey of the situation on the universities today may help us get a handle on current intellectual trends. Particularly in North America and Britain, the stranglehold of analytic philosophy and positivism on philosophy departments remains firmly in place. It is true that the postmodernists have taken over some literature departments along with a proliferation of ethnic studies, cultural studies and other disciplines rooted in cultural relativism. And postmodernists do make a token presence in some philosophy and sociology departments. But the bulk of humanities studies are firmly in the camp of positivism and empiricism. When it comes to the sciences, I think it is fair to say that there are virtually no scientists at all who are postmodernists. Those that have any philosophical inclinations are basically positivists of one form or another. There are it is true a small number of philosophers of science that are sympathetic to postmodernism, following in the footsteps of Paul Feyrabend, but they are considered by their colleagues to be fringe elements out of the mainstream.

As for the intellectual climate in other countries, there is some evidence that the longstanding resistance on the part of continental philosophy to the Anglo-American variety of positivism and language analysis has been steadily eroding. In France for instance, despite all the splash made by Derrida, Irigaray, Baudrillard and others, it is generally agreed that the heyday of postmodernism is over. For at least the last decade the conservative New Philosophers such as Ferry and Renault have been much more in evidence in the mainstream press than the Derrida’s or Baudrillards. And what is the

message coming from Luc Ferry and Renault? It is the need for French philosophy to abandon its historical roots in Rousseau and the French Revolution and embrace the empiricism and constitutional liberalism of Locke and the English system of constitutional government. This observation is made in a recent book by Dominique Lecourt, a student of Althusser,

“...Anglo-Saxon political philosophy of a liberal inspiration has had a strong impact in France in the wake of historiographical work challenging the orthodox, marxisant interpretation of the French Revolution. Francois Furet drove Albert Soboul from the front of the stage. Tocqueville opened the door of our republic of letters to John Rawls, who was belatedly translated into French. Reading the recent attacks on French philosophy in the United States, one has the impression that this profound transformation is very poorly appreciated in the Anglo-Saxon countries. The picture of French philosophy implicitly painted by Alan Sokal and Jean Bricmont’s choice of targets in *Intellectual Impostures* seems outdated.” (Dominique Lecourt, *Mediocrity: French Philosophy Since 1968*, Verso, 2001, xiv)

In Germany there have been similar developments. Jurgen Habermas remains the preeminent public intellectual in Germany today. And just where has he lead German intellectual thought in the past 50 years? He went from an appreciation of Hegel and existentialism in his earliest period to a rapprochement with Kant. From there he moved toward the Anglo-American philosophical tradition embracing Peirce, G.H. Mead and even the empiricist sociologist Talcott Parsons. Axel Honneth has also been taking the remnants of the Frankfurt school in a similar direction. Analytic philosophy is becoming more and more a presence in German universities, to the detriment of any serious consideration of German idealism or Marx. This is not surprising. It is the cultural byproduct of the remodeled and reunified German state which is looking more and more to the traditions of liberalism and away from those of Marxism or even the mildly critical approach of the Frankfurt School. It is truly “The Berlin Republic” as the title of a recent book of Habermas proclaims.

What about the rest of the world. I know very little about contemporary intellectual trends in Russia, China or India for example. Russia has seen the revival of the most backwards forms of mysticism and spiritualism for reasons that are well-known. India and China, anxious to become major imperialist powers of their own, have been extolling the virtues of technology and Western culture, particularly American intellectual traditions. Of course the problem of revived religious fundamentalism remains acute in India and the Moslem world. But despite the presence of a few prominent cultural studies and feminist theorists among Indian expatriates, I don’t think postmodernism has had much of an impact in these countries.

If one walked into a meeting of the Modern Language Association and heard Professor Stanley Fish expound one might think that the lunatics have taken over the asylum and the end of Western civilization is indeed at hand. However, the impression is a distorted one because first of all this is just one outpost of the intellectual climate at the universities. A more comprehensive survey would, as I have indicated show other

intellectual trend to be far more significant today. That doesn't help the unfortunate students of literature who are stuck listening to Professor Fisch and for them we have the utmost sympathy.

But the intellectual climate at the universities is only one barometer of intellectual trends. Perhaps an even more important one is the state of popular culture. Just take a look at the topics highlighted in magazines and newspapers and you will find that the public is being inundated not with the intellectual fantasies of Jacques Derrida, but with the ethos of consumerism and a naïve worship of technology. There have been numerous accounts in the mass media propounding the views of biological determinism. For instance, at the time that the human genome project was completed, most accounts of its significance equated it with the discovery of human nature. There are hundreds of television nature shows - presenting a vulgarized adaptation of sociobiology - contend that the aggressive behavior exhibited by animals (often posed and encouraged for the camera to begin with) are correlates to the problems faced in human society. And every day hundreds of ads are run extolling the virtues of modern chemistry which allows you to reconstruct your consciousness simply by taking a pill. We have in fact become addicted to the quick and easy technological fix for virtually all our problems. I don't see much evidence of the postmodernists' antipathy to science in popular culture.

Finally, we may ask, what form does bourgeois consciousness most often assume among workers and youth today? In considering practical issues in everyday life as well as political activity, I would say that for every worker influenced directly or indirectly by Foucault, Lacan or Irigaray, I can show you a thousand who are more profoundly influenced by David Hume, John Dewey and their descendents.

Talbot Comment 23

From reading your Heidegger article I am sure you are well aware of the degeneration of thought that has taken place in modern bourgeois academic and media circles. The advocates of Heidegger, Nietzsche, etc. cannot be tackled without confronting their philosophical idealism and skepticism.

But even if it were just a question of opposing empiricism, surely we can agree with Meikle on the necessity to advocate a materialist ontology? He explains in Chapter 3 the problems associated with Rosdolsky's “quasi-Hegelianism”. A lack of clarity on the nature of Marx's dialectical categories, Meikle says, opens the way to empiricist interpreters of Marx like Jon Elster who think that Marx was using ‘sleight of hand’ in his logical exposition in *Capital*. Rosdolsky did not understand that Marx's categories were not just ‘logical’ in some sense but as Meikle insists, real essences in which “the ontological and the epistemic coalesce”. We are dealing not with a logical construct, “*but how things really are*” (Meikle's italics, page 80).

Steiner Reply 23

This is the part of Meikle that I have the most trouble with. As part of his argument with Rosdolsky, he completely dismisses epistemological concerns, glibly saying that,

“With the category of real essence, the ontological and the epistemic coalesce.”
(Essentialism in the Thought of Karl Marx, p 80)

Now I agree with his point that essences are real and it is only because they are so that we can cognize them at all, but what could it possibly mean to say that “the ontological and epistemic coalesce” other than to deny that there is any problem of knowledge at all. This formulation somehow skirts around the enterprise of science, that of separating essence from appearance. We don’t have to wait too long before Meikle draws the logical conclusion from this line of reasoning – i.e. he repudiates the correspondence theory of truth.

“The relation between the dialectical categories in which the real development is presented, and the reality of the development they present, is not well expressed as a ‘reflection.’ (It is certainly not a passive reflection – with this I agree – but what kind of process is it then? A.S.) The categories do not ‘reflect’. The specification of the essence of a mammal does not ‘reflect’ a mammal; it is (or tells you) what a mammal is.” (80)

The category suddenly attains magical powers and has the ability to “tell you” what a mammal is! If you find this an adequate explanation of the process of cognition (and I think cognition is a process, despite the unfortunate pedigree that term was given by Healy), then perhaps you can explain it to me. Where, in the formulation of this “coalescence” do you find what Hegel called “the labor of the negative?” Or let us once more recall Marx’s statement,

“All science would be superfluous if the appearance of things coincided with their essence.”

This illustrates a grave weakness in Meikle’s account. He practically ignores the contribution made by modern philosophy, most critically Hegel, in shaping the thought of Marx, not to mention Spinoza, Leibniz, the Left Hegelians and Feuerbach. In a more recent essay he writes that,

“...in Capital Marx works to a metaphysics and philosophy of science that antedated Hegel and Engels and that is in its essentials largely independent of Hegelianism and Engel’s version of it.” (Engels and the Enlightenment Reading of Marx, from Engels After Marx, ed. Manfred Steger and Terrell Carver, p.87)

He tends to see Marx as reconquering the lost heritage of Aristotle without recognizing that Marx is reappropriating Aristotle from the standpoint of modernity. And **that** involves the recognition of the key role of human freedom and the individual personality, something that was lacking in the philosophy of the ancients. Thus, Meikle leaves a number of significant gaps in his account of the relationship between Aristotle, Hegel and Marx. One glaring omission in particular stands out sharply - there is no discussion in

Meikle’s book of the relationship between Hegel’s logical categories and Aristotle’s. There is another noteworthy omission in Meikle’s treatment of dialectics. He has nothing at all to say about the subject of our discussion, the dialectics of nature, aside from a couple of cryptic remarks. That is in itself surprising in a treatment that purports to restore the Aristotelian dimension in Marx’s thought when one considers that it was Aristotle who practically invented natural philosophy!

Meikle’s subsequent development is not to be emulated. He followed up his book on Essentialism with another noteworthy book, “Aristotle’s Economic Thought”. Since then, I am afraid, Meikle has joined the anti-Engels cottage industry. He has recently written,

“Comparisons of Marx’s treatment of the “laws of motion” of market economy with a Newtonian treatment of the “laws of motion” of a physical system can at best deal in superficial resemblances. Marx’s ideas of what it is to give a scientific treatment of the capitalist entity are entirely misrepresented if his work is assimilated into the Enlightenment idea of science in that way, and the work of Engels encouraged just such an assimilation.” (Engels and the Enlightenment Reading of Marx, from Engels After Marx, ed. Manfred Steger and Terrell Carver, p.99)

Meikle is correct to note that an interpretation of the “laws of motion” of capital as if they were Newtonian “laws of motion” is a misreading of Marx, but he is wrong to pin the blame for such a misreading on Engels. He is also wrong in ascribing some variations of what he calls “Enlightenment metaphysics” to Hegel, as if Hegel was not himself both a child of the Enlightenment and one of its most profound critics. (See the interesting book by Lewis Hinchman, “Hegel’s Critique of the Enlightenment”)

Talbot Comment 24

I must say that I felt there was a similar lack of clarity in your formulation concerning the relation of the whole to the parts when you write of the determinations of reality “whose discovery is the work of the individual sciences and whose ultimate organization is the work of logic and philosophy.” Is this not sliding over into a viewpoint that sees the dialectical categories as philosophical constructs rather than as real essences?

Steiner Reply 24

Your formulation “philosophical constructs” loads the argument. It transforms the categories of “wholes” and “parts” into subjective creations of the imagination. They are in fact philosophical categories that have a real content precisely because they are generalizations at the highest level of ontological relations pertaining to nature, society and thought. It is not an idle “philosophical construct” to ask whether a whole is nothing but an accidental collection of parts or whether there is some kind of ordering principle that arises spontaneously and that determines the role that each part plays in constructing the whole. The latter theory for instance, has been given a precise scientific expression in

some of the recent work arising out of complexity theory by Stuart Kauffman and has a direct bearing on how we conceptualize evolution. Yet this very up-to-date scientific theory was anticipated by the venerable philosophers Aristotle, Leibniz, Kant and Hegel, not to mention Marx and the proletarian dialectician Joseph Dietzgen.

Talbot Comment 25

I find the problems of a Hegelian approach are put very clearly by Ilyenkov in his “Dialectical Logic.” He writes:

“The profound flaws in the Hegelian dialectic were directly linked with idealism, due to which the dialectic was readily transformed into ingenious, logically subtle apologies for everything that existed” (p229)

Steiner Reply 25

You should be aware that among Hegel scholars and historians, virtually no one believes this any more. Hegel’s reputation as being an apologist for “everything that existed” is based on a misinterpretation of a single sentence in the Preface to the Philosophy of Right. That sentence is usually translated in English to say, “All that is real is rational”. The German text has the following statement however:

“Was vernünftig ist, das ist wirklich; und was wirklich ist, das ist vernünftig.”

In the context of Hegel’s categories, *wirklich* should not be translated as “real”, or simply that which exists. Rather it should be translated, as it is in the Knox edition, as “actual”, which has the sense of that which deserves to exist. Properly understood, Hegel’s statement conveys an entirely different meaning than the oft-repeated legend. For a good unpacking of the history behind this legend, I would recommend the essay in the extremely useful anthology, *Hegel Myths and Legends*, “Hegel: The Real and the Rational” by M. W. Jackson.

The image of Hegel as the “official” philosopher of Prussian reaction had a great deal of cachet in the latter half of the 19th century and was revived in the post war period by Karl Popper and others. For a wonderful refutation of Popper’s thesis I recommend the classic essay by Walter Kaufmann, “The Hegel Myth and its Method”, *Hegel Myths and Legends*, edited by Jon Stewart, Northwest University Press, 1996.

Talbot Comment 26

“The fact is that the Hegelian conception of thought represented an uncritical description of the real position of things formed on the soil of a narrowly professional form of the

division of social labour, that is to say, on the division of mental work from physical labour, from immediately practical, sensuously objective activity.” (p229/230)

Steiner Reply 26

Again, I think Ilyenkov’s characterization of Hegel’s position as representing an “uncritical description of the real position of things” is too simplistic. At least such statements should be highly qualified. However, Ilyenkov himself is a very intriguing figure. Ilyenkov’s professional life was deformed under the yoke of Stalinism. He was a genuine thinker and suffered greatly because he was one of the few Soviet academics who took Marxist philosophy seriously. I found an interesting article about Ilyenkov on Internet and I am forwarding that to you as a separate attachment. The article indicates that because his freedom to write was severely circumscribed, he adopted an Aesopian language in order to convey his meaning.

Talbot Comment 27

In his last essay he explains the importance of the objective reality of the universal in Marx (a point which seems closely related to Meikle’s insistence on real essences). Hegel, he explains, could never have accepted Marx’s support for Benjamin Franklin’s aphorism that man is tool-making animal. Franklin’s and Marx’s definition was much too concrete for Hegel to be regarded as general or universal. Hegel’s insistence that the universal can exist only in the realm of pure thought then actually has the result of bringing him close to the metaphysical thinkers he was so opposed to. In other words the universal ends up as a formal abstraction.

Steiner Reply 27

I don’t think this is an adequate formulation of what Hegel thought. Hegel was an idealist it is true, but not in the one-dimensional sense that your account portrays. If we are to do justice to Marx’s real advance beyond Hegel, then we have to get beyond references to an “inversion thesis” as if that settles anything. As Robert Pippin has pointed out,

“Hegel is one of the most lionized and most vilified philosophers of history, at the same time that it is widely believed that no one really knows what he was talking about.” (Hegel’s Idealism, Cambridge University Press, p. 3)

As Pippin later clarifies, the thesis that Hegel was an idealist is hardly disputed. The trouble is that there is wide disagreement as to just what kind of idealist Hegel was. To be sure, there is a “textbook” version of Hegel that is usually taught in introductory survey courses to the effect that Hegel believed that reality was the Absolute Idea. This popular version of Hegel makes him practically indistinguishable from subjective idealists such

as Berkeley. If that is the case it is hard to understand what all the fuss was about as virtually no one takes Berkeley-type arguments seriously these days. Few Hegel scholars today subscribe to this popular version of Hegel.

The relationship of Marx to Hegel is a very complicated issue that I do not propose to address at this point. However I think it is high time that Marxists took advantage of the latest developments in Hegel and Marx scholarship and historiography. Believe it or not, we are in a position today to understand Hegel much better than his near contemporaries were. For one thing we have available far more of his work than they had, both published and unpublished. A very important manuscript of notes from an early version of Hegel's *Philosophy of Right*, now known as “the original *Philosophy of Right*”, was only discovered in the 1980's. These lectures, given at Heidelberg prior to the political freeze brought in by the censorious Carlsbad decrees of 1819, provide a much clearer picture of Hegel's political views than was previously possible. It has had a profound impact on Hegel studies as it shows that Hegel's political ideas, at least in his earlier period, were far more radical than anyone had guessed. This recent finding partially confirms the important interpretation of Hegel found in Lukacs' “*The Young Hegel*”, which was one of the first scholarly works to break away from the “Hegel the arch-conservative” thesis. And in 1997 H.S. Harris finally published his life's work, *Hegel's Ladder*, a massive two volume study and commentary on Hegel's *Phenomenology* that supercedes the previous scholarly work in this area. Our understanding of the intellectual climate of the late 18th century and early 19th century has in general undergone nothing less than a revolution in the last three of four decades thanks to a new level of historical inquiry that has put us in closer touch to the ideas of our ancestors than was previously possible. We are therefore in a much better position to place Hegel's, and Marx's writings in their historical context.

Talbot Comment 28

In my opinion there are very important questions here on how Marxists approach science and particularly modern physics. In an area where the division of labour between engineers, experimental physicists, and theoretical physicists or mathematicians has reached extreme proportions, there is a very strong tendency on the theoretical side for abstract mathematical concepts to be seen as the basis for all reality. Many mathematicians and theoretical physicists are neo-Platonists of one sort or another. So whilst I would agree with your conclusion that dialectical philosophy is indispensable for the comprehension and encouragement of science, I would have to add a proviso. Only if the distinction between the materialist ontology of Marxian dialectics, and the specific developments that Marx made in going beyond Hegel are made clear (the ‘inversion thesis’ as Meikle puts it in polemicising against Colletti, but also the development and application of scientific concepts to society – Meikle is weak on the issues of historical materialism).

Steiner Reply 28

As the subject of my lecture was not the differences that Marx had with Hegel, but the crisis of modern science, I did not get into this area. But where in my talk did I propose anything other than a materialist ontology? I have no doubt that we may disagree about the details of a materialist ontology, but I trust we both subscribe to the fundamental premise that there is an objectively real world. The problem for science and philosophy is to understand how it hangs together in its articulation and how we as a conscious part of that world can change it. And it is in that “how” that all the problems arise.

Talbot Comment 29

Otherwise there is always the danger that rather than developing an understanding of the “real essences” that underpin modern physics, the idealist proclivities of mathematical physicists are used to boost the latest “knowledge production” nonsense in the humanities, along the lines pointed out by Alan Sokal.

Chris Talbot Comment 8.12.02