

Chapter 7

A defense of positivism in the guise of a defense of science

*Note: The discussion that follows is not strictly speaking a reply to the **Odyssey** series of North and the Talbots. It is rather an extended commentary on a lecture given by Chris Talbot and published on the WSWS several months after his contribution to the **Odyssey** series. I thought it was appropriate to publish these comments in conjunction with my reply to the **Odyssey** series because Talbot's lecture further illuminates many of the issues that I discussed in some of the previous chapters of my reply. If I have done my job properly then it will become obvious to any unbiased reader that Talbot's lecture, while of no particular importance in its own right, warrants an extended commentary because it brings out very clearly how far the International Committee has departed from a dialectical approach in science and anywhere else for that matter.*

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How do you discuss Marxism and evolution without ever bringing in dialectics? This is the feat that Chris Talbot attempts to pull off in a lecture that was published in the summer of 2009 on the WSWS as a three part series, *Marx and Darwin: Two great revolutionary thinkers of the nineteenth century*.¹ The first two parts of this series presents an unremarkable but inoffensive summary of some well-trodden material – the revolutionary implications of Darwin's theory, its impact on Marx, the misappropriation of Darwin by the vulgar materialists of the 19th century, the culture wars of the 20th century over Darwinism, etc. We have to wait until part three till we get to something new – a discussion of the confrontation between Marxism, evolutionary psychology and the role of the “radical scientists”. I will confine my remarks to part three of this series.

Deference to the evolutionary psychologists

Talbot begins part three with a brief discussion of evolutionary psychology. This is a modern form of biological determinism that, though very suspect scientifically, has gotten enormous positive publicity since its inception in the early 1990's. It is in fact little more than a repackaging of an earlier form of biological determinism, sociobiology, that became something of a popular fad in the 1970s. This repackaging was necessary when sociobiology, founded by the naturalist E.O. Wilson, was discredited as a consequence of its early proponents embracing reactionary theories that sought to explain differences in

¹ The three part series, *Marx and Darwin: Two great revolutionary thinkers of the nineteenth century*, first appeared in the WSWS on June 17, 2009 with subsequent sections published on June 18 and June 19. The word “dialectics” does not appear once anywhere in this series.

<http://www.wsws.org/articles/2009/jun2009/dar1-j17.shtml>

<http://www.wsws.org/articles/2009/jun2009/dar2-j18.shtml>

<http://www.wsws.org/articles/2009/jun2009/dar3-j19.shtml>

IQ among different races by different genetic capacities. These theories clearly implied that it is a waste of societal resources to try to educate the poor and disadvantaged as their intellectual capacity is determined by their genetic heritage - a factor that social programs are powerless to influence.

In his account of evolutionary psychology, Talbot bends over backwards to look for mitigating explanations that would absolve the evolutionary psychologist for responsibility for the reactionary implications of their theories. He attributes the views of Steven Pinker, E.O. Wilson and other leading publicists of evolutionary psychology to “ignorance” about society. For instance, he says of the group of evolutionary psychologists,

...it is important to note the considerable amount of ignorance concerning history and society among scientists in the field of Evolutionary Psychology, which probably exceeds that of Buchner and his contemporaries.

Later, he says of E. O. Wilson, the founder of sociobiology and one of the leading exponents of evolutionary psychology,

He was not a fascist at all, but a good natural scientist with very little understanding of human society.

Now it is one thing to defend E. O. Wilson against the baseless charge of “fascism” that was hurled at him by some radicals in the 1970s but why this attitude of deference to the proponents of evolutionary psychology? Talbot depicts the founders and the leading publicists of evolutionary psychology as well-meaning but innocent scientists who are simply ill-informed. (Later on he even portrays some of them as sympathetic victims of an assault at the hands of violent radical protestors.) But Pinker and Wilson are not at all “ignorant”. They have taken a very public position in a very partisan debate about the nature of the scientific enterprise and about human nature.

Talbot’s depiction of them as merely ignorant is effectively saying that their philosophical approach is irrelevant - that they are merely mistaken when making pronouncements about society because that is a field outside their area of expertise. Talbot’s approach thus effectively removes any consideration of philosophical issues when it comes to discussing evolutionary psychology. Like the failure to even mention dialectics, this is another indication that Talbot has bought into the positivist conceit that philosophy has no relevance to science.

The nature of human nature

To be sure, Talbot does offer some arguments against evolutionary psychology. He reminds us that Marx and Engels opposed the views of the vulgar materialists of their time. He cites contemporaries of Marx and Darwin, who, using Darwin as a point of departure, claimed that human society can be explained by the laws of biology in general and natural selection in particular.

You wouldn't attempt to apply particle physics directly to analyze the molecular processes in the cell—why should you attempt to apply biological theories to society?

Later he points to the contemporary revival of these types of discredited theories,

Since the 1970s there has been a revival of attempts to apply biology directly to social questions. First there was sociobiology and later Evolutionary Psychology came on the scene.

So far, so good. But whereas Talbot's points are correct as far as they go, there is nothing particularly "Marxist" in his criticism of evolutionary psychology. This becomes evident soon enough when Talbot, in arguing against the proponent of evolutionary psychology, Steven Pinker, cites the work of David J. Buller, who he tells us "is not a Marxist",

...but we [meaning us Marxists] would agree with his [i.e. Buller's] conclusion that we should "abandon not only the quest for human nature but the very idea of human nature itself," in the sense of the fixed "psychological mechanisms" espoused by popular Evolutionary Psychology.

Buller is undoubtedly correct in dismissing the narrative of human nature woven by evolutionary psychologists such as Pinker as unscientific "just so stories".² However that leaves open the question of what would be a scientific explanation of human nature. As Buller is not a Marxist and is not concerned with the philosophical issues underlying this discussion, we can understand why he would prefer to dismiss the entire issue of human nature.

Yet the same cannot be said for Talbot who aspires to represent a tradition going back to Marx and Engels that should have a great deal to say about human nature. Pinker, in his book *The Blank Slate* and elsewhere argued that human nature is genetically determined and has been more or less set since the end of the Pleistocene age tens of thousands of years ago. Here is how he put it in an article written more recently,

Evolutionary psychology has challenged the blank slate in at least two ways. One is by documenting that beneath the undeniable fact of cross-cultural variation there is a bedrock of human universals: ways of thinking and feeling and behaving that can be seen in all of the cultures documented by ethnography. The anthropologist Donald Brown a few years ago compiled a list of them, and they number some 300, everything from Aesthetics, Affection, and Anthropomorphization, all the way to Vowel contrasts, Weapons, attempts to control the Weather, and a word for the color White.³

There is a huge problem with this thesis. While our genetic inheritance accounts for our biological "base line" as humans and are responsible for the range of our cognitive

² The term "just so stories" is based on the tales of Rudyard Kipling who wrote imaginative stories on themes such as "why did the leopard get its spots?" Stephen Jay Gould introduced the term as a polemical retort against certain ultra-Darwinists who explained every biological feature in terms of adaptation through natural selection even when there was evidence that some features are not adaptive but develop for other reasons.

³ Steven Pinker, *The Blank Slate*, *The General Psychologist*, Volume 41, No. 1 - Spring 2006, p.1. http://pinker.wjh.harvard.edu/articles/papers/The_Blank_Slate_General_Psychologist.pdf

possibilities, those traits by themselves tell us nothing about the manifold creations of human culture. Sociobiology and evolutionary psychology are extreme versions of reductionism. Marxism, on the other hand, has traditionally been profoundly anti-reductionist. For example, take the following quote from Engels,

One day we shall certainly 'reduce' thought experimentally to molecular and chemical motions in the brain; but does that exhaust the essence of thought? ⁴

While we cannot dispense with our genetic inheritance, it explains very little of what is interesting to us as creatures of culture. That fact however does not mean that we are adopting the position ascribed to us by Pinker – that we are proponents of the “blank slate” theory of human nature. It simply means that human nature is not the kind of thing that can be adequately grasped through the vulgar methodological prism of evolutionary psychology.

While Talbot acknowledges the problems inherent in the type reductionist arguments put forward by the evolutionary psychologists, the way he poses the problem of reductionism makes it appear as a matter of a scientist overstepping his area of expertise and venturing into an area about which he is “ignorant”. But this kind of common sense explanation of reductionism completely misses the philosophical essence of the reductionist outlook. Reductionism is situated in a manner of conceiving reality whose foundation is the mechanical materialism that first triumphed in the 17th century with the advent of the new physics of Galileo, Descartes and Newton. It posits a world that is composed of indivisible atomic entities. If we understand these atomic entities then in theory at least we are in position to build up a picture of the entire world and everything in it as all is ultimately composed of different combinations of these atoms.

By way of contrast, a Marxist, i.e. a dialectical materialist view of the world sees the world picture built up on the basis of atomism as a necessary moment in the development of science, but one which while useful within certain contexts even today, is being superseded (*aufheben*) by a more comprehensive outlook - one that can on the one hand explain the relative success of atomism within certain limits, while at the same time points to the inadequacy of atomism beyond those limits. The dialectical outlook provides a more faithful picture of the interconnection of different hierarchical levels of nature and society, each with its own specific laws of motion. Thus the dialectical scientist sees the realms of say neurophysiology and aesthetics as non-reducible to each other not because he may be a specialist in one and “ignorant” of the other, but because he sees that the laws of neurophysiology function in a realm of nature and biology that is connected to aesthetics only in a weak sense. The dialectical outlook is thus opposed both to a naïve holism which insists that the whole determines its parts, as well its opposite, an atomism that sees the parts as determining the whole and fails to recognize the emergence of new properties in qualitatively different levels of being.

⁴ Engels, *Dialectics of Nature*, p. 175.

Talbot not only avoids the philosophical implications of taking an anti-reductionist position against the evolutionary psychologists, he also avoids a direct confrontation with one of its leading publicists, Steven Pinker, when it comes to the latter's view on human nature. In response to Pinker's claim that Marxists look upon human nature as a "blank slate" Talbot tries to finesse the issue by citing Engels essay, *The Part Played by Labour in the Transition from Ape to Man*. He writes,

He [Engels] was perfectly clear about the biological basis of human behavior, but when society emerged, "a new element" had come into being.

But this is hardly a convincing answer to Pinker's claim that Marxists see human nature as a "blank slate". How does citing a "new element" answer Pinker? Of course Pinker would reject Engels brilliant analysis of the dialectic at work in man forming his own nature by evolving as a tool making animal, but he would not disagree that man's tool making capabilities provide him with a basis for relating to the world in a way fundamentally different from other animals, i.e. introducing a "new element".

Furthermore, how can the Engels essay be used to reply to Pinker? Pinker is claiming that certain universal psychological traits were crystallized in the Pleistocene period which ended about 10,000 years ago, i.e. just prior to the historical period of humanity, and that the last 10,000 of recorded history have added nothing fundamental to the makeup of the human psyche. The refutation of Pinker's claim would be a demonstration that the contribution to human nature of the historical period of humanity has far greater weight than the contributions of the Pleistocene period (without of course denying the contributions of that prehistoric period.) Yet Engels's essay deals with human evolution in the same Pleistocene period. Therefore, it cannot be used to make the case for the significance of human culture in shaping human psychology if you wish to argue that human psychology has largely evolved in the post-Pleistocene period.

The work of Pinker and other champions of Evolutionary Psychology is reductionist on two levels. First they reduce human psychology to biological explanations in terms of "hard-wired" instincts that humans have evolved during the Pleistocene era. Then they reduce sociological explanations to psychological ones. These reductionist explanations are not only scientifically worthless, but have profoundly reactionary implications. For example, the evolutionary psychologist David Barrash has argued that men are by nature predisposed to rape. This wiseacre claims that it is part of man's evolutionary heritage to have a predatory relationship toward women. Or take the argument made by E.O. Wilson that ascribes our supposed preference for "traditionalist" art to our genetic heritage,

The growing evidence of an overall structured and powerful human culture, channeling development of the mind, favours a more traditionalist view of the arts. The arts are not solely shaped by errant genius out of historical circumstances and idiosyncratic personal experience. The roots of their inspiration date back in deep history to the genetic origins of the human brain and are permanent. ⁵

⁵ E.O. Wilson, *Consilience: The Unity of Knowledge* (Little Brown, 1998) p. 218. Louis Menand had a nice comment about such arguments, indicating that they are little more than rationalizations for parochial

This crude and middle-brow “theory” of aesthetics is what you inevitably get when you reduce the complexity of human nature to some kind of easily quantifiable statistical average.

A viable reply to Pinker and his associates is not possible without a theory of human psychology. Pinker has such a theory, although a crude and primitive one. Talbot does not. This is a problem previously discussed in our polemics as the “empty place” within Marxism when it comes to understanding human psychology.⁶ Indeed, the vacuous nature of Talbot’s reply to Pinker was anticipated by Frank Brenner’s essay of more than a decade ago. Talbot thinks that a sufficient reply to Pinker consists in quoting Marx’s statement that,

“...the human essence is no abstraction inherent in each single individual. In its reality it is the ensemble of the social relations.”

This is supposed to refute Pinker’s contention that Marxists view human nature as a blank slate! Apropos of this statement, Brenner, commenting on the treatment of human nature found in orthodox Marxist texts, in particular one written by George Novack, made the following observation in his essay of 1996:

Why should there be any need for a specifically Marxist conception of human nature? Surely the issue was settled long ago when Marx defined the human essence as “the ensemble of the social relations,” which is basically all that Novack is saying here. But as important as Marx’s thesis is, it isn’t – and was never meant to be – the final word on an issue as complex as human nature; indeed, the great interest Marx and Engels took in Bachofen and Morgan’s work in anthropology and Engels’s essay on the labor theory of human origins attest to their own efforts to deepen their understanding of human nature.⁷

Talbot is unable to provide a convincing reply to Pinker because his reliance on “orthodoxy” leaves him without a philosophical leg to stand on. By dismissing the idea of “human nature” Talbot makes himself vulnerable to Pinker’s critique of Marxists as believers in the “blank slate” theory of human development.⁸

prejudices:

The other trouble with evolutionary psychology is that it is not really psychology. In general, the views that Pinker derives from “the new sciences of human nature” are mainstream Clinton-era views: incarceration is regrettable but necessary; sexism is unacceptable, but men and women will always have different attitudes toward sex; dialogue is preferable to threats of force in defusing ethnic and nationalist conflicts; most group stereotypes are roughly correct, but we should never judge an individual by group stereotypes; rectitude is all very well, but “noble guys tend to finish last”; and so on. People who share these beliefs probably didn’t need science to arrive at them, but the science is undoubtedly reassuring. (Louis Menand, “What Comes Naturally”, *The New Yorker*, Nov. 22, 2002)

⁶ Frank Brenner, Psychoanalysis and the “empty place” of psychology within Marxism, http://permanent-revolution.org/essays/marxism_pschoanalysis.pdf

⁷ Ibid. http://permanent-revolution.org/essays/marxism_pschoanalysis.pdf

⁸ Talbot may argue that he does not dismiss the concept of human nature, but “the very idea of human nature itself,” in the sense of the fixed “psychological mechanisms” espoused by popular Evolutionary Psychology. But he is stuck in the same cul-de-sac as others such as Novack because his idea of human nature is entirely without content and despite his protests, completely malleable. In other words, it really is

Contrary to Talbot, it is not just “vulgar Marxists” who have been guilty of seeing human nature as a blank slate. The same can be said for “orthodox” Marxists such as George Novack.⁹ Here is Novack on the subject of human nature:

Human nature – good, bad, or indifferent – is the product of society. The qualities of human beings are endlessly changeable, just as their potential capacities are boundless. Human nature is far more changeable than glass, which can flow like a stream, be drawn into threads, or become rigidly frozen. Human nature, hardened into one mold, can be shattered, remelted, and recast into very different, almost unrecognizable, forms. The whole panorama of social evolution testifies to this plasticity of humankind.¹⁰

Brenner, in the essay previously cited, made a cogent point here about Novack’s view,

...if human nature is, as it were, mere putty in the hands of society, what would there be to prevent a reactionary government from ‘molding’ its population in such a way as to maintain them in eternal subservience?¹¹

And one can just imagine the field day Steven Pinker would have with Novack’s definition. He need look no farther for evidence that Marxists are supporters of the “blank slate” hypothesis and are thus vulnerable to all his polemics against that outlook.

One reason for Pinker’s success and the rise of Evolutionary Psychology in the last two decades is because opponents of Pinker are reluctant to challenge him on the fundamental ground of human nature. Like Talbot, they are surreptitious supporters of the “blank slate” view because that is inevitably what you get when you deny the existence of essential relations. Pinker’s view is reactionary and wrong-headed, but he has the advantage in these debates because he at least does have a clearly articulated view of human nature.¹²

Trashing the radical scientists

little different than Pinker’s description of the “blank slate”.

⁹ Talbot’s attempt to place the blame for the “blank slate” theory of human nature on “vulgar Marxists” is but another example of a recurring phenomenon about which we have previously commented, the vulgar critique of vulgar materialism. See the essay by Frank Brenner, On the vulgar critique of vulgar materialism, http://permanent-revolution.org/polemics/vulgar_critique.pdf

¹⁰ George Novack, *Humanism and Socialism*, p. 127.

¹¹ Frank Brenner, Psychoanalysis and the “empty place” of psychology within Marxism, http://permanent-revolution.org/essays/marxism_pschoanalysis.pdf

¹² It is not accidental that the “blank slate” view of human nature was first introduced by the British empiricist John Locke. Locke’s notion of a *tabula rasa* was tied in to polemics against essentialism. I discuss the issue of essentialism and its connection to Marx’s view of human nature much more fully in my essay, *From Alienation to Revolution: A Defense of Marx’s Theory of Alienation*, http://permanent-revolution.org/essays/alienation_revolution.pdf. The 1986 book by Scott Meikle, *Essentialism in the Thought of Karl Marx*, is devoted to an exposition of this thesis.

While Talbot's problems are at first largely sins of omission, he compounds it in the next section when he discusses what he calls the "radical scientists." Here we get sins of commission. He gives this section the title, "Radical scientists generate confusion," but the lion's share of confusion generated in this section is that caused by Talbot himself.

First of all, to label the individuals named in this section, Stephen Jay Gould, Richard Lewontin and Steven Rose, "radical scientists", is something of a misnomer, particularly when that appellation is placed within the context of polemics emanating from the WSWS. As any regular reader of the WSWS should know, the word "radical" is without exception used as a term of opprobrium by the various polemicists who contribute to that online journal. It has the connotation of being unserious, superficial and unhistorical and espousing poorly thought out left wing verbiage. The subliminal suggestion therefore is that a "radical scientist" is one whose fuzzy left wing thinking somehow impinges on their approach to science. Talbot tries to demonstrate this very point. He writes,

Science is necessarily a controversial business and the radical scientists raised many important biological questions on which I do not intend to comment. What I want to raise here are the questions that relate to Marxism. I believe that Lewontin, Rose and Gould put forward a distorted viewpoint that is contrary to Marx and Engels' attitude to science. Their intervention has created a lot of confusion.

Talbot's charge demands an explanation. But the one he provides is little more than a rant against the politics – both real and imagined – of the "radical scientists". The only examination of their "attitude to science" and whether it is "contrary to Marx and Engels" consists of three quotes taken out of context. Completely ignored however is the important work these scientists have contributed both to science and to the dialectical philosophy of science.

To justify his trashing of Gould, Lewontin and Rose, Talbot relates a well-known episode from the culture wars of the 1970s:

Evolutionary psychology and its antecedent socio-biology were vigorously opposed by radical scientists, often calling themselves Marxists. Biologists like Richard Lewontin in the US and Steven Rose in the UK, as well as the US paleontologist Stephen Jay Gould, were part of an organization called Science for the People. In 1975 they sent a letter to the *New York Review of Books* accusing socio-biology of fascistic tendencies redolent of the Nazis. Demonstrations were held and lectures interrupted. It was a hysterical response. The leading sociobiologist E. O. Wilson was one of the victims of their campaign. He had water poured over his head in a famous protest at one of his lectures. He was not a fascist at all, but a good natural scientist with very little understanding of human society. His specialism was social insects.

Up till now the incident with Wilson having water poured over his head has had an iconic status in right wing circles. It has been used over and over again as an illustration of

the evils of “political correctness” and Wilson has been turned into a martyr of conservative pundits on that account.¹³

It is strange, to say the least, that Talbot, an avowed Marxist, should choose to recycle the story of this truly trivial incident, which has been repeatedly used by right-wingers to brand all left-wing critics of sociobiology as “inquisitorial” and even “terrorists.” Talbot thereby manages to avoid the serious critique of sociobiology that was being raised by Gould, Lewontin and Rose.

Talbot further denigrates the radical scientists through guilt by association. He says,

Their approach to the question of science arose out of the radical politics they espoused. They were influenced by a form of Maoism, and by the ideas of the Frankfurt School that we have been giving some attention to on the *World Socialist Web Site* after attacks on us from this direction. Gould moved away from his earlier radical politics, but Lewontin and Rose still hold such views today.

One would think, reading Talbot, that it is almost a sin for a scientist to have an interest in radical politics. Note the double standard: when it comes to a figure like Wilson, who espouses views with deeply reactionary implications, Talbot is remarkably deferential, but when it comes to scientists who have consciously sought to link the needs of science to opposition to capitalist oppression, Talbot is contemptuous of such efforts as indicative of nothing more than confusion.

I am hardly an uncritical booster of the radical scientists. It is perfectly proper to subject the work of Gould, Lewontin and Rose to a critical analysis. There are theoretical problems associated with their work. But those theoretical problems cannot begin to be

¹³ The following account, from the right wing British newspaper, *The Independent*, is typical of the propaganda written over the years about this incident:

Sociobiology turned out to be academic dynamite. Wilson traced animal behaviour back to its genetic origins, but then, in his final chapter, he turned his attention to humans, insisting that they too behaved according to genetic imperatives. He was at Harvard and it was the mid-Seventies, the era when it was still in some circles almost de rigueur to be a Marxist. Wilson's thesis was seen as at best a defence of the bourgeois status quo, or at worst a racist, fascist, sexist apologia. The Harvard Marxists believed all human behaviour could be socially explained and they fell on this heresy with inquisitorial fury. This was the beginning of militant political correctness. Wilson was abused, suppressed and, at one conference, had a jug of water poured over his head. A committee was formed to destroy sociobiology, headed by the Marxist population geneticist Richard Lewontin and the biologist Stephen Jay Gould. (Bryan Appleyard, *Living Proof: A naturalist is no stamp collector*, *The Independent*, Dec. 4, 1995. (<http://www.independent.co.uk/life-style/living-proof-a-naturalist-is-no-stamp-collector-1524079.html>)

Stephen Jay Gould, who was sharing the podium with Wilson when this incident happened, provided an account of it many years later in his last book, *The Hedgehog, the Fox and the Magister's Pox*. Although he was supposedly one of the ideological inspirers of the protestors, having joined a radical group that critiqued the direction of science, Science for the People, Gould was just as taken aback as anyone else by the protest. Gould regrets that he did nothing to try to stop the protestor from pouring water on Wilson.

addressed if one remains blind to the genuine contributions each of them has made both to science and to the philosophy of science.

In any case, Talbot isn't interested in either the contributions or the problems in the work of these scientists; his sole interest is to attack them for their radical politics. Notice how, in a quote I cited earlier, Talbot brackets off "the many important biological questions" raised by the work of Gould, Lewontin et al. While claiming to demonstrate that their "distorted viewpoint" is "contrary to Marx and Engels' attitude to science", Talbot does so without any reference to that science. And yet, as I'll soon show in examining their scientific contributions, what makes these scientists truly radical – in the best sense of that term – is not their progressive (if somewhat confused) political sympathies but rather *their conscious and, at times, brilliant employment of dialectics in their scientific work*. It is simply astounding that Talbot ignores this. How is it possible to understand "Marx and Engels' attitude to science" *without* a consideration of the role of dialectics in science? It is clear that what Talbot means by this phrase is not what Marx, Engels and Trotsky would have understood by it. Thus, the use of the names of Marx and Engels in this phrase is a case of false advertising: what Talbot really means is that the viewpoint of Gould and Lewontin is "contrary" to the prevailing viewpoint of mainstream science – which is to say, the viewpoint of empiricism and positivism. And this is certainly true, but what is remarkable is that an avowed Marxist like Talbot should side with mainstream science against those scientists who have turned to the dialectic.

Talbot will of course claim that this is a distortion of his position, since after all he made no mention of dialectics (or empiricism). But that is precisely the point – his failure even to mention these matters shows that *for him science and philosophy are completely separate realms*. That is why dialectics never enters into his understanding of the attitude to science either of the radical scientists or of Marx and Engels. But to conceive of science as being apart from philosophy is itself a philosophy – that of positivism – and with that philosophy goes a raft of theoretical and ideological assumptions that are all the more insidious because they are seen as arising 'naturally' out of science. Thus, Talbot's critique of the radical scientists is far more revealing about his own attitude to science than it is of theirs. It also provides another, striking, example of one of the central criticisms that Frank Brenner and I have made of the ICFI – which is that dialectics has become a dead letter within that movement.

A further point in this regard: Talbot may think he is defending science in this way but he isn't. You cannot defend science with an uncritical attitude to positivism and empiricism anymore than you can defend democratic rights while holding an uncritical attitude to private property and the bourgeois state. What you end up defending, in this way, is not science but its positivist distortion, what can be called, for lack of a better term, *scientism*. It is to the credit of the radical scientists that they have tried to highlight the differences between science and scientism, but this is precisely what Talbot the positivist dismisses out of hand as "confusion". That is how he ends up attacking the scientists whose work actually puts them closer to a genuinely Marxist attitude to science.

That attack, as I said earlier, includes a resort to guilt by association to discredit these scientists. But dragging in the Frankfurt School is such obvious nonsense that it underscores the intellectual dishonesty of Talbot's criticisms. Not a single one of these scientists has ever claimed any influence from the Frankfurt School or has employed any of the work of Horkheimer, Adorno or Marcuse. It is also well-known that Frankfurt School critical theory had no place for the dialectics of nature. This was one tenet of classical Marxism that was unanimously repudiated by the Frankfurt School.¹⁴ There is thus something bizarre in Talbot's attempt to find a connection between a group of radical scientists who were inspired by the dialectics of nature and the Frankfurt School which dismissed it outright. The only reason the Frankfurt School reference is there at all is thanks to a tortured attempt by Talbot to draw a "connection" between the radical scientists and the critique Frank Brenner and I have made of the International Committee. (The cryptic reference to "attacks launched on us from this direction" is obviously a reference to the polemics penned by Brenner and myself.)

To anyone who has not been following the polemics launched against us by David North and others, the reference to the Frankfurt School is bound to sound puzzling. What has happened in fact is that polemicists of the WSWS have made of the Frankfurt School a kind of catch-all bogeyman that explains all manner of evil in the world. North blamed the eclipse of a generation of materialist-minded social scientists on the Frankfurt School. He also tried to pin the blame for the rise of postmodernism and cultural studies on the Frankfurt School. That claim is particularly absurd given the well-known animosity between cultural studies and critical theory. More recently, David Walsh, in a series of lectures about art, claimed that Marcuse is in some way responsible for many of the false paths taken by contemporary art. Now Talbot weighs in and blames the Frankfurt School for the "confusion" sown by the radical scientists.

What is going on here? Are these people really talking about the Frankfurt School in any recognizable form? Or are we dealing with a coded language in which certain words and phrases are emptied of their cognitive content and replaced with emotional associations? And those emotional associations, while meaning little to most readers of the WSWS, have been carefully cultivated by North and other leaders of the IC among their members and close supporters. Thus when North or Talbot talks about "The Frankfurt School" they are not referring to the academic research institution that emerged in Germany after World War I and whose initial inspiration was the desire to adopt the methodology and categories of Marxism (and later Freud) in the study of the social sciences. Rather, their aim is to conjure up emotional visions of middle class radicalism gone wild, subjective idealist attacks on science and a repudiation of the historical role of the working class.

Having poisoned the well against the radical scientists with the accusation that they are in league with the Frankfurt School, Talbot next gives us a total of three quotes, two from Lewontin and one from Richard Levins, (although interestingly none from Gould or Rose) to illustrate their allegiance to subjective idealist views on science. To call Talbot's

¹⁴ The most detailed examination of and rejection of the dialectics of nature by anyone affiliated with the Frankfurt School was Alfred Schmidt's book, *Marx's Concept of Nature*.

examination of Lewontin's and Levins' statements here a hatchet job would be a more charitable description of his effort than it deserves.

For instance, he quotes Lewontin,

Despite its claims to be above society, science, like the Church before it, is a supremely social institution, reflecting and reinforcing the dominant values and views of society at each historical epoch.

Talbot lets this statement pass without comment, but it is obvious that he employs this citation to suggest that Lewontin is a radical social constructionist who questions the objective nature of the scientific enterprise. Had Lewontin said that "science is **solely** a mechanism for reinforcing the dominant values and view of society", he would have a good case against Lewontin. But Lewontin did not say that. Lewontin's rhetoric overstates the case, but he is basically correct to note that science is a social institution which must be differentiated from the ideal of science as an impartial search for truth. The institution of science cannot be other than socially embedded but it intersects with the ideal of science as an impartial search for truth in a dialectic that is often fraught with tension. This dialectic between the social and class imperatives of the scientific enterprise and its historical telos as the search for truth is found in every society and becomes especially heated in capitalist society where the scientific enterprise is both encouraged and distorted to serve the needs of capital. Insofar as the social function of scientific institutions impact the ideal of science, scientific theories, especially when one gets closer to the science of human society, are embedded with ideology.

Citing still another remark where he thinks Lewontin is stressing a subjectivist interpretation of science, Talbot writes,

Lewontin even goes so far as to say that the capitalist ideology of individuals competing with one another has predominated throughout science from the Scientific Revolution to the present day.

To illustrate his case he then provides the following quote from Lewontin,

"This atomized view of society is matched by a new view of nature, the reductionist view ...the individual bits and pieces, the atoms, molecules, cells and genes are the causes of the properties of the whole objects and must be separately studied . . ."

Lewontin is correct. The ideology that our human nature consists in genes or DNA is an example of the transference of atomism, which was initially dominant in the physical sciences, onto the field of biology and social sciences. Why Talbot should find this statement astonishing is a mystery unless he is prepared to reject completely the historicity of the scientific enterprise.¹⁵ Not only has Lewontin pointed out that atomistic

¹⁵ In this connection, note that in writing about the 'Hessen episode', Chris and Ann Talbot swing to the other side and stress the 'earthy core' of scientific concepts rooted in history. Here Chris Talbot attacks Richard Lewontin for his attempt to locate the source of scientific concepts in history. I have previously

prejudices have been interwoven in biology, but the physicist Lee Smolin has made a nice case for the same atomistic prejudices hampering the development of physics in his book *The Life of the Cosmos*. What Talbot seems unable to fathom is that identifying the social basis behind the atomistic model of many sciences does not at all mean what the postmodernists claim, that science is simply another narrative, like a novel, having no objective truth.

Here is where we get at the root of Talbot's philosophical problem. He is unable to fathom how the scientific enterprise can be both an objective and impartial search for truth and be socially embedded. To him it is either one or the other. **In other words, he does not recognize the dialectic at work in the scientific enterprise** - that scientific theories can both represent a moment of truth and are at the same time embedded within the prevalent ideological biases of class society. But to deny the dialectic between objectivity and the social nature of the scientific enterprise is another way of embracing the ideal of positivism. For the goal of positivism in the philosophy of science has always been to define and formalize the scientific enterprise as a purely objective and impartial search for truth in which the impact of social relations are "bracketed out" as external factors. Thereby positivism reinforces the status quo in a manner not unlike some of the ancient philosophers who identified the social institutions of their time, including slavery, as just the way things are "by nature".¹⁶

As the social roots of the atomistic model of the universe in the 17th century Scientific Revolution are too well known to be dismissed, Talbot immediately interjects a statement telling us that he is hedging his bets – that maybe there is something to be said for identifying the social causes that bias scientific theories in one direction instead of another. He writes,

Perhaps there is a grain of truth here in that the mechanical outlook from the first most successful branch of science, physics, did tend to predominate throughout science, at least up to the first part of the 19th century.

Perhaps indeed! Nevertheless, Talbot is not a man to be deterred by such qualifications and he presses on with the following assertion,

However, taken for the whole of science under capitalism I think that Lewontin's conception is false and it leads to a view, now very prevalent in the humanities, that objectivity in science is not possible.

This is quite a logical jump. It is true that there are currents in the humanities that deny the objectivity of science. But it does not follow that a critique of scientific institutions that unmask their power relations and of certain theories that are presented as "science" – such as evolutionary psychology – but are in fact ideologies, leads to the wholesale abandonment of objectivity in the sciences. One can only make this jump if one has

commented on the incoherence of Talbot's position in Chapter 6. See http://permanent-revolution.org/polemics/downward_spiral_ch06.pdf p. 172-173.

¹⁶ Aristotle famously argued that some men are slaves "by nature" although he also argued that it is possible to be in bondage and not be a slave "by nature".

already rejected the dialectic between the socially embedded nature of the scientific enterprise and its ability to arrive at a partial moment of objective truth. This is where Talbot's unstated positivism leads.

Talbot's positivist leaning becomes even more obvious as he proceeds to formulate an argument for the irrelevance of philosophy when it comes to the natural sciences. He writes,

Individual scientists hold all kinds of political and philosophical views, often reflecting their position in society as middle class academics. Many of them are pillars of the establishment.

He is making a case for the superfluity of dialectics in the natural sciences. The political or philosophical presumptions of scientists should be viewed as purely private matters, of little concern to social and political as well as purely scientific issues. This is about as clear a case as has been ever stated for the positivist presumption that science and philosophy are two very different realms that never intersect.

The contributions of the radical scientists

In his haste to discredit the radical scientists, Talbot leaves out even the most cursory account of their contributions both to science and to the philosophy of science. It is surprising that an essay purporting to provide a Marxist overview of Darwinism fails to give any account of the work of a group of contemporary evolutionary biologists who have consciously adopted dialectics as a method of doing scientific inquiry. Let us try to fill in this yawning gap in Talbot's account, however brief and inadequate such a summary will be, of the important work of these scientists.

The late Stephen Jay Gould is recognized by many as one of the giants of evolutionary biology in the 20th century. He is the co-author, with Niles Eldridge, of the theory of punctuated equilibrium, which many have recognized as inaugurating a new paradigm in evolutionary biology. While punctuated equilibrium is not without its critics, it has found more and more acceptance within the scientific community. And Gould, though not a Marxist politically, was one of the few contemporary scientists who openly argued for the importance of dialectics in guiding the work of the natural scientist. Here is his eloquent testimony about the importance of dialectics:

Dialectical thinking should be taken more seriously by Western scholars, not discarded because some nations of the second world [the former Soviet Bloc] have constructed a cardboard version as an official political doctrine. The issues that it raises are, in another form, the crucial questions of reductionism versus holism, now so much under discussion throughout biology (where reductionist accounts have reached their limits and further progress demands new approaches to process existing data, not only an accumulation of more information).

When presented as guidelines for a philosophy of change, not as dogmatic precepts true by fiat, the three classical laws of dialectics [formulated by Engels] embody a holistic vision that views change as interaction among components of complete systems, and sees the components themselves...as both products of and inputs to the system. Thus the law of "interpenetrating opposites" records the inextricable interdependence of components: the "transformation of quantity to quality" defends a systems-based view of change that translates incremental inputs into alterations of state; and the "negation of negation" describes the direction given to history because complex systems cannot revert exactly to previous states.¹⁷

The theory of punctuated equilibrium is a marvelous confirmation of Gould's employment of some of Engels's insights into the dialectics of nature. Gould and his colleague Niles Eldridge questioned one of the fundamental tenets of Darwinism, namely the thesis of gradualism. For Darwin, the thesis of gradual change was central to his explanation of how new species evolve. Take for instance, the following well known statement from the last chapter of *Origin*,

"As natural selection acts solely by accumulating slight, successive, favorable variations, it can produce no great or sudden modification; it can act only by very slow steps. Hence the canon of 'Natura non facit saltum' [Nature does not make sudden jumps]...is on this theory simply intelligible."

The theory of punctuated equilibrium provides a corrective to traditional Darwinian theory by showing that nature does sometimes make sudden jumps. The thesis of gradualism was not in fact born out by empirical observation. The fossil record does not show what one would expect if the traditional Darwinian account were strictly true. We do not see transitional forms occupying every possible gradation in the transition from one phenotype to another. There are lots of gaps and many transitional forms that we would expect to find are missing. Darwin explained this by pointing to the imperfection of the fossil record. But Gould and Eldridge rethought this age old thesis and came up with a better explanation for the gaps in the fossil record. They hypothesized that the gaps in the fossil record are an inevitable outcome of the fact that some species emerge in a relatively brief period of time, perhaps just a few thousand years, hardly a blip in the scale of geological time. As fossil remains are relatively rare, it is not very likely that a species which evolves that rapidly will leave behind much evidence in the fossil record. Much more common in the fossil record are the remains of species that have been relatively stable for millions of years. The so-called imperfection of the fossil record is therefore no mystery – it is exactly what we would expect if evolution consisted in long periods of relative stability interrupted ("punctuated") by brief periods of very rapid transition.

Now the recognition that in nature we can witness both gradual, almost imperceptible changes and sudden leaps, and that the gradual changes prepare the ground for a sudden transition, is of course one of the cornerstones of dialectics, the law of the transition of quantity into quality. As brilliant as Darwin was, he was nevertheless hampered by his non-dialectical gradualist approach – which he even identifies with rationality itself in the

¹⁷ Stephen Jay Gould, *An Urchin in the Storm*, "Nurturing Nature", (Norton, 1987), p 153-154.

above quote - and therefore missed this part of what the fossil record was really telling him. The thesis of gradualism which he shared with the geologist Charles Lyell, also led him to dismiss the possibility of occasional catastrophic environmental changes. Every geological and environmental change had to be gradual. (Lyell called this “uniformitarianism”.) But we now know that occasional catastrophes, either by way of comets hitting the earth or a volcanic eruption, have in the past had a profound effect on the ecology of the planet, resulting in mass extinctions of whole groups of plants and animals.

Gould and Eldridge were able to overcome the gradualist bias of most of their peers thanks in part to their interest in dialectics. It should also be noted that many of the insights that helped them achieve their scientific breakthrough were anticipated by Trotsky several decades earlier in his *Philosophical Notebooks*. For instance, Trotsky points to the fact that Darwin’s investment in gradualism hampered him philosophically and therefore hampered him scientifically as well:

This brilliant biologist [Darwin], while showing how small quantitative deviations accumulate and yield a completely new biological ‘quality,’ in this way explaining the origin of species, applied without being conscious of it, the methods of dialectical materialism in the area of organic life. The Hegelian law of the transition from quantity to quality found in Darwin a brilliant, although a philosophically unenlightened application.

We can also find in Trotsky the following anticipation of the role of catastrophes in shaping the planet and its ecology as well as several key aspects of the theory of punctuated equilibrium in the following essay written in the 1920s:

The Darwinian theory of the origin of species encompasses the entire span of development of the plant and animal kingdoms. The struggle for survival and the processes of natural and sexual selection proceed continuously and uninterruptedly. But if one could observe these processes with ample time at one’s disposal – a millennium, say, as the smallest unit of measure – one would undoubtedly discover with one’s eyes that there are long ages of relative equilibrium in the world of living things, when the laws of selection operate almost imperceptibly, and different species remain relatively stable, seeming the very embodiment of Plato’s ideal types. But there are also ages when the equilibrium between plants, animals, and their geophysical environment is disrupted, epochs of geobiological crisis, when laws of natural selection come to the fore in all their ferocity, and evolution passes over the corpses of entire plant and animal species. On this gigantic scale Darwinian theory stands out above all as the theory of critical epochs in plant and animal development.¹⁸

Of course Trotsky was not a working scientist so his intuition remained merely a brilliant anticipation of the work undertaken decades later by Gould and others, but it is hard not to be impressed by the power of the dialectic in reading these statements.

¹⁸ Trotsky, *Portraits, Personal and Political*, p. 30,
<http://www.marxists.org/archive/trotsky/profiles/kautsky.htm>

Another scientist whose ‘radicalism’ is not to Talbot’s liking is the biologist Richard Lewontin. Like Gould who was his friend and colleague for many years, Lewontin has made important and original contributions to both science and the philosophy of science. His main area of expertise is population genetics and he has made some important discoveries in this area. He was also a co-author with Stephen Jay Gould of an important paper that challenged the assumptions of the ultra-Darwinians and helped reinforce the arguments for punctuated, equilibrium, “The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme”.¹⁹ But outside the narrow community of scientists working in the field of population genetics Lewontin is much better known as a public intellectual of unabashed left wing persuasion who has taken on the biological determinists in numerous forums and debates. He is also one of the leading champions of dialectics in the natural sciences, having co-authored a book devoted to that theme, *The Dialectical Biologist*.

Lewontin had done some original work in ecology. In this realm of scientific investigation, he was aided by insights gained from his study of dialectics. In particular, Lewontin challenged the thesis of the preponderance of evolutionary biologists, that in the relationship between an organism and its environment, the organism is always and only reacting to and adapting to the environment. Lewontin saw that the relationship was that of a reciprocal interaction. An organism not only adapts to its environment, but changes it and by changing it changes the further course of its interaction with the environment. This is a case of reciprocal interaction in a dynamic system and needs to be analyzed with much more finely grained tools than had been the case previously.

The employment of dialectical analysis in ecological studies allows us to better grasp the huge environmental challenges facing us today, and for which there is no solution within the profit system. Lewontin was one of the first to try to work out the implications of these relationships by consciously turning to dialectics. In the Introduction to *Biology Under the Influence*, he writes with his co-author, Richard Levins,

We come to the project as participant observers. Both of us have been active in overlapping though somewhat different areas of population genetics, ecology, evolution, biogeography and mathematical modeling. As participants we have been engaged in the nuts and bolts of our sciences in lab and field and before the computer. In our scientific work we have attempted to apply the insights of dialectical materialism that emphasizes wholeness, connectedness, historical contingency, the integration of levels of analysis, and the dynamic nature of “things” as snapshots of processes...

¹⁹ Reprinted in the anthology, *The Richness of Life: The Essential Stephen Jay Gould*, edited by Steven Rose, (Norton, 2007), p. 423. The “ultra-Darwinians” are a group of scientists and philosophers, represented by Richard Dawkins and Daniel Dennet most prominently, who believe that natural selection is the only cause of evolution and speciation and the unit of selection is the gene, Gould’s point about the spandrel is that it is a structure that was originally created not for its present adaptive function, but for some other unrelated reason. At some point, that preexisting structure can take on an adaptive significance although it was not originally “selected” for it. This is a way of making the point that natural selection is only one of several mechanisms at work in evolution.

We also step outside of the specific scientific problems to become observers and examine the nature of science and the uses of mathematics and modeling. In this we step into what usually fits within the philosophy of science.²⁰

The fact of the matter is that Gould, Lewontin, Rose and a handful of others were distinguished from practically all their colleagues by their open embrace of dialectics as a method of understanding and doing science.²¹ Is their turn to dialectics to be condemned because it was initially inspired by the radicalism of the 1960s and 1970s, including in the case of Lewontin and Rose, a flirtation with Maoism? As I said earlier, what is at work here is a perverse double standard – while Talbot condemns these radical scientists, he would never dream of condemning the work of countless positivist-oriented scientists for their liberal or conservative politics.

As for Gould, he never was a Maoist. He once provided an explanation for his sometimes open, sometimes covert employment of dialectical categories in his scientific work. He relates that he learned the ABC's of dialectics as a child from his left-leaning family. While Gould did not embrace Marxism politically, he was always appreciative of the heuristic value of the Marxian dialectic and sought to employ it in his work.

Before leaving the subject of Talbot's treatment of Lewontin and the other radical scientists, it might be enlightening to look at some of the topics covered in Lewontin's most recent book, *Biology Under the Influence*, which he co-authored with Richard Levins. Talbot's one reference to this book provides us with another fragmentary quote, this one about science in Cuba:

In his most recent book, co-authored with Richard Levins, we find science described as “a commoditized expression of liberal European capitalist masculinist interests and ideologies.” The last section of the book is a paean to what is called “Cuban socialist science”, contrasted to the “bourgeois” science in the western world.

Admittedly, the language here is over the top, giving way to the trappings of feminist rhetoric. One can certainly challenge the assertion that there is something “masculinist” about capitalism. Furthermore, the fact that the author here, Richard Levins, has illusions about Cuban society, which he wrongly considers socialist, should not blind us to seriously consider the subject matter of the essay where this phrase occurs. Contrary to Talbot's suggestion, the theme of the essay has nothing to do with a so-called “Cuban socialist science” which is contrasted to “bourgeois science” in the same manner in which the Stalinist bureaucracy in the Soviet Union extolled the virtues of “Soviet science”, then under the influence of the pseudo-scientist Lysenko, to “bourgeois science”. The

²⁰ Richard Lewontin and Richard Levins, *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health*, (Monthly Review Press, 2007), p. 10.

²¹ The neurobiologist Steven Rose is another radical scientist named by Talbot. Rose has collaborated with Lewontin on a number of books challenging the myths of biological determinism. He was the co-author, along with Lewontin and Leon Kamin, of *Not in Our Genes*, one of the first and still one of the most important books challenging the pretensions of sociobiology and its reactionary and racist implications. One could also add the name of Richard Levins to this particular group of biologists who were influenced by dialectics. Levins was the co-author of two important books with Richard Lewontin, *The Dialectical Biologist*, and *Biology Under the Influence*.

essay in question, “How Cuba is going Ecological”, contrasts the model of an ecologically sustainable agricultural development that is promoted in Cuba to the more common model of industrialized agriculture that has been exported to the underdeveloped world by organizations like the International Monetary Fund. You can certainly take issue with Levins’s conclusions in this essay, but it is dishonest in the extreme to suggest that he is championing some new form of Lysenkoism. In fact, Richard Lewontin and Richard Levins wrote a critical examination of Lysenkoism in their earlier book, *The Dialectical Biologist*.²²

Talbot is selectively choosing what he considers the weakest essay in a book which is admittedly somewhat uneven in the quality of its contributions. More to the point, Talbot totally ignores the many fine essays in this book that make a genuine contribution both to biology and to the dialectical philosophy of science. For instance, Chapter 17, “Dialectics and Systems Theory” is a detailed examination of the commonalities and differences between dialectics and modern systems theory. Chapter 18, “Aspects of Wholes and Parts in Population Biology”, discusses how the dialectic of wholes and parts can be employed to gain insights in the study of population biology and that the work in this area can in turn deepen our understanding of the relationship between wholes and parts. Chapter 21, “Educating the Intuition to Cope with Complexity”, includes a discussion of Hegel’s dictum, “The Truth is the Whole” and provides a scientific application whereby we can actually follow the changing relationships of whole and parts in a dynamic environment. The preface to this volume also contains a straightforward repudiation of both scientific obscurantism and postmodernism:

... We have to be engaged on two fronts: 1) We stand against the obscurantist anti-science, which ranges from direct manipulation of the EPA and FDA by the government and the hype of the drug companies, to creationism and the mystification of mathematical chaos; 2) we also reject scientism, the claim that other people’s ideas are superstition while ours are uniquely objective knowledge verified by numbers. We reject the postmodern view that, still reeling from having discovered the fallibility of science, comes to deny the validity of knowledge, or overwhelmed by the uniqueness of the particular, refuses to see patterns even of uniqueness.²³

Enlisting Trotsky to lend authority to a dishonest argument

To lend the air of authority to his exorcism of philosophy from the natural sciences Talbot tries to enlist Trotsky. He gives a quote from a lecture Trotsky gave to the Mendeleev Society of Russian chemists in 1925.

²² Richard Levins and Richard Lewontin, *The Dialectical Biologist*, (Harvard University Press, 1985), “The Problem of Lysenkoism”, p. 163-196. There is however some big problems with this essay due to the authors’ investment in Maoism as a legitimate contribution to dialectical philosophy. Nevertheless the authors come out firmly against Lysenkoism and all manifestations of a state bureaucracy telling scientists what is legitimate science and what is not..

²³ Richard Lewontin and Richard Levins, *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health*, (Monthly Review Press, 2007), p. 9.

...the need to know nature is imposed upon men by their need to subordinate nature to themselves. Any digressions in this sphere from objective relationships, which are determined by the property of matter itself, are corrected by practical experience.

The point of providing this quote is to illustrate Talbot's claim that unlike the case of the social sciences, when natural scientists are wedded to a false philosophical method, there is a minimal price, if any, to be paid, because sooner or later, nature will hit them over the head and correct them. But this is an appalling misuse of Trotsky's speech to the Mendeleev Congress. While it may be true that there is to some degree a built-in corrective to poor methods in the natural sciences, it is hardly a matter of indifference that science could set off in a false direction for decades before being called to order by this built-in corrective. What can be said is that if there is a difference in the consequences of employing a false method in the natural sciences and the social sciences, that difference is strictly relative and not absolute.

To use this quote of Trotsky to bolster a positivist, anti-philosophical stand is a repudiation of everything Trotsky fought for on the question of dialectics. It takes the quote out of context, as if this is the only thing that "Trotsky stressed in the natural sciences". The context of Trotsky's remarks was a speech to a scientific congress the bulk of whose members were not committed to Marxism either politically or philosophically. One of Trotsky's aims was to assure the assembled scientists that they can still make a contribution to science, regardless of their philosophical inclinations, that they can rest assured that the Soviet state was prepared to guarantee the autonomy of science from politics.²⁴ But elsewhere in the same speech Trotsky tries to demonstrate to this audience the superiority of the dialectical method in the natural sciences and that the scientist's aversion to philosophical thought will eventually have a negative impact on their work.

Isaac Deutscher's summary of Trotsky's speech makes this point with great clarity. He notes that Trotsky used the example of the great Russian scientist Mendeleev to illustrate the conundrum of the scientist who works without a clear philosophical method:

Mendeleev's case then illustrates the predicament of the modern scientist: his lack of an integrated vision of the world and even of science. Of necessity science works empirically; and specialization and fragmentation of knowledge accompanies its progress. Yet the great the specialization and fragmentation, the more urgent is the need for a unifying conception of the world – otherwise the thinker's mind becomes constricted within his specialty and even within it his progress is impeded. Lack of philosophical insight and distrust of generalizing thought have been responsible for much avoidable scientific confusion and groping in the dark. Marxism offers the scientist an integrated vision of nature and human

²⁴ Alas, within a few short years after Trotsky's speech, the Stalinist bureaucracy consolidated its hold on Soviet society and it did indeed force scientists to toe the line on ideological matters. This had a devastating effect on the state of the sciences in the Soviet Union. It also resulted in sentences to the Gulag and the eventual murder of some of the Soviet Union's most prominent scientists, among them Nikolai Vavilov and Boris Hessen.

society, a vision which, far from being an arbitrary concoction of a figment of the metaphysical mind, accords intimately with the varied empirical experience of science.²⁵

Talbot's object in presenting Trotsky's quote is to defend the position that when it comes to the natural sciences philosophical method is irrelevant. This is to say the least not only a repudiation of the intent of Trotsky's speech, but also a clear repudiation of Engel's discussion of the subject in his *Dialectics of Nature*. In that brilliant work, Engels noted that although the nebular hypothesis of Kant was decades ahead of its time, the anti-philosophical attitude of contemporary scientists prevented them from recognizing its importance.

If the great majority of the natural scientists had a little less of the repugnance to [philosophical] thinking that Newton expressed in the warning: "Physics beware of Metaphysics!", they would have been compelled from this single brilliant discovery of Kant's to draw conclusions that would have spared them endless deviations and immeasurable amounts of time and labor wasted in false directions. For Kant's discovery contained the point of departure for all further progress [i.e. for overcoming the static and adopting the dynamic view of nature as a whole]... If at once further investigation had been resolutely pursued in this direction, natural science would now be considerably further advanced than it is. But what good could come of philosophy? Kant's work remained without immediate results, until many years later Laplace and Herschel expounded its contents and gave them a deeper foundation, thereby gradually bringing the "nebular hypothesis" into favour.²⁶

Spreading confusion about the Sociology of Scientific Knowledge

Having set up the straw man of Lewontin as a proponent of subjectivism in science and misusing a quote from Trotsky to promote the positivist view that the scientific enterprise can proceed unimpeded by any consideration of philosophical questions, Talbot next tries to tie Lewontin and others into recent subjective trends in sociology. He writes,

The approach of Lewontin et al has had its concomitant in the history of science. Here there have emerged schools of thought such as the Sociology of Scientific Knowledge (SSK) that have placed enormous weight on the social context in which science is carried out. This often has the result of making scientific knowledge appear to be entirely relative to particular classes or social groups, undermining all objectivity and challenging the materialist basis of scientific thought and the conception that science does reflect, to some degree of approximation, the world that exists outside human thoughts and sensations.

Aside from his dishonest attempt to consign Lewontin to the subjectivist school of sociology, Talbot does not present a coherent account of the current divisions within sociology. He is deliberately confusing two different positions in the Sociology of

²⁵ Isaac Deutscher, *The Prophet Unarmed: Trotsky 1921-1929*, (Verso, 2003), p. 145.

²⁶ Engels, *Dialectics of Nature*, (International Publishers, 1963), p. 8-9.

The editor of this work, the great Marxist scientist J.B. S. Haldane, writes in a footnote that modern science no longer regards the nebular hypothesis of Laplace as correct. "The hypothesis was however of immense importance because it first made it likely that the solar system has a history. It may be compared with the ideas of the ancients on biological evolution."

Scientific Knowledge – the strong programme which does indeed deny the objectivity of scientific knowledge, and the weak program, which defends the objectivity of scientific knowledge but also insists on the social role of scientific institutions and their influence on the history of science. Talbot should be aware of this because I made this point very clearly in some correspondence I had with him six years ago. He chooses to ignore this distinction as it would undermine his attempt to present what is essentially a positivist view of science. Here is part of what I wrote to Talbot on this topic:

These positions [in the sociology of scientific knowledge] 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.)

I would add that Friedrich Engels can be credited as having anticipated by several decades the projects of the weak programme in the sociology of knowledge. Take the following well-known observation from Engels correspondence:

“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).

Is Engels not providing a critique of science as a social construction here?

If Engels is the spiritual grandfather of the weak programme in the sociology of knowledge, the Marxist scientist Boris Hessen must be credited with being the father of this branch of sociology and the philosophy of science. For it was Hessen, whom Talbot has elsewhere praised, who presented a groundbreaking essay at a conference in London in 1931 that first brought attention to the social imperatives behind Newton's physics.²⁷

A miserable conclusion

The remainder of Talbot's essay is a miserable attempt to link the work of Gould, Lewontin and Rose to a few misguided souls who have embraced anti-science and postmodernism. For instance, he quotes the work of Daniel Gasman, a historian who traced the origins of Nazism back to the theories espoused by the German 19th century evolutionist Ernst Haeckel in his book, *The Scientific Origins of National Socialism*. He then says cryptically, "Stephen Jay Gould was heavily influenced by this book"

The reader is left to assume that Gould held the same one-dimensional view of Haeckel as Gasman. But that would be a big mistake. Gould in fact, while keenly critical of the racist conclusions to which Haeckel's version of biological determinism led was also sensitive enough to appreciate his work as a naturalist. As to how "heavily influenced" Gould was by Gasman's account of Haeckel, consider the following discussion in which Gould cites Gasman:

Men of large vision often display outsized foibles as well. No character in the early days of Darwinism can match Haeckel for enigmatic contrast of the admirable and dubious. No one could equal his energy and the volume of his output – most of high quality, including volumes of technical taxonomic description (concentrating on microscopic radiolarians, and on jellyfishes and their allies), not only theoretical effusions. But no major figure took so much consistent liberty in imposing his theoretical beliefs upon nature's observable factuality.

I won't even discuss Haeckel's misuse of Darwinian notions in the service of a strident German nationalism based on claims of cultural, and even biological, superiority – a set of ideas that became enormously popular and did provide later fodder for Nazi propagandists (obviously not Haeckel's direct fault, although scholars must bear some responsibility for exaggerated but not distorted, uses of their arguments – see D. Gasman, *The Scientific Origins of National Socialism: Social Darwinism in Ernst Haeckel and the German Monist League*.²⁸

As any unbiased reader can see, Gould's description of Haeckel is eminently fair to the man and bears no resemblance to the caricature of Haeckel as the godfather of Nazism

²⁷ I have discussed the role of Hessen much more fully in my earlier reply to Ann and Chris Talbot. See Chapter 6, http://permanent-revolution.org/polemics/downward_spiral_ch06.pdf, *Science at the Crossroads*.

²⁸ Stephen Jay Gould, *I Have Landed*, "Abscheulich! (Atrocious)", (Three Rivers Press, 2003), p. 307-308.

that Talbot tries to pin on Gould. Talbot continues in this vein by conjuring up another writer who takes the next step and blames the rise of Nazism on Darwin. There is little point in looking any further at Talbot's attempt to discredit the radical scientists by associating them with the works of several figures who have taken the thesis of science as a social construction to absurd lengths.

The obverse of Talbot's vigorous denigration of the efforts of the radical scientists is his facility in bending over backwards to excuse the sins of the vulgar materialists. We have already seen some examples of this, but Talbot ends his essay by providing yet one more such example:

By placing all emphasis on Haeckel's social and political views and making him partly responsible for Nazism, there is no hope of making an objective assessment of the scientific contribution of this important scientist or of biology in general in that period. Many of the biologists of the late 19th and early 20th century were in favour of eugenics and many held views on race that we would find abhorrent.

This statement is noteworthy for a couple of reasons. First, anyone who has been following the polemics between ourselves and North will recognize that guilt by association is one of the hallmarks of North's polemical style. It is therefore no small irony that Talbot takes time out to make the point that Darwin or Haeckel should not be blamed for the sins of others who were inspired by them or their misguided political pronouncements, while at the same time employing the exact same guilt by association type arguments against the radical scientists. (Recall the dig about "Cuban science" in an attempt to cast a shadow on the theoretical work of Lewontin and Levins.)

In addition to its unintended irony the above statement also highlights once more another aspect of Talbot's treatment of Haeckel. He maintains that Haeckel's brand of vulgar materialism, while it may have influenced Haeckel's social and political views, had no impact on his work as a scientist. This is once more an attempt to break the connection between scientific work and philosophical method as we have already seen in Talbot's misuse of a quote from Trotsky's speech to the Mendeleev Congress. And it puts Talbot squarely in opposition to Engels. In his *Dialectics of Nature*, Engels made several observations about Haeckel, noting that his empirical methods had a direct impact of his work as a scientist. For instance,

Haeckel's nonsense: induction against deduction. As if it were not the case that deduction=conclusion, and therefore induction is also a deduction. This comes from polarisation. Haeckel's *Schopfungsgeschichte*, pp. 76-77. The conclusion polarised into induction and deduction!

It is also characteristic of the thinking capacity of our natural scientists that Haeckel fanatically champions induction at the very moment when the *results* of induction – the classifications – are everywhere put in question (*Limulus* a spider, *Ascidia* a vertebrate or *chordate*, the *Dipnoi*, however, being fishes, in opposition to all original definitions of amphibia) and daily new facts are being discovered which overthrow the *entire* previous classification by induction. What a beautiful confirmation of Hegel's thesis that the

inductive conclusion is essentially a problematic one! Indeed, owing to the theory of evolution, even the whole classification of organisms has been taken away from induction and brought back to “deduction,” to descent – one species being literally *deduced* from another by descent – and it is impossible to prove the theory of evolution by induction alone, since it is quite anti-inductive. The concepts with which induction operates: species, genus, class, have been rendered fluid by the theory of evolution and so have become *relative*: but one cannot use relative concepts for induction.

Consequences of the neglect of Marxist philosophy

Summing up, in his discussion of evolutionary psychology and the radical scientists, Talbot presents a version of the philosophy of science that bears little resemblance to Marxism. On the contrary Talbot’s philosophical outlook can better be described as a form of positivism in all but name. All the major tenets of positivism are there except the label.

- The scientific enterprise is an ahistorical search for truth.
- The articulation of the social and historical roots of science undermines the objective nature of the scientific enterprise. (Talbot implicitly accepts this framework. He either emphasizes the historical roots of science, as he does in his remarks on Hessen²⁹, or he emphasizes the objective nature of the scientific enterprise as in his discussion of Lewontin. What he cannot do is put the two together.)
- There is no connection between philosophical method and doing “good science”.

In light of this, one must wonder about the conclusion of Talbot’s piece:

Biology has made enormous strides in the last decade and there has been some growth of interest in Darwin, despite the government’s educational policies. But I think that a renewed interest in the vast work of Marx and Engels is also essential, and the application of Marxist theory to build a socialist movement is most urgent, given the huge social issues we face—massive social inequality, poverty for much of the world, the growing impact from global warming, and now a massive recession with a future of unemployment and economic stagnation.

Leaving aside the ritualized and empty call to “build a socialist movement”, why in the world would anyone who has read Talbot’s piece and who is interested in science in general or Darwin in particular turn to “the vast work of Marx and Engels”? And why is

²⁹ See Chapter 6 of the current series, http://permanent-revolution.org/polemics/downward_spiral_ch06.pdf, *Science at the Crossroads*.

this “essential” after Talbot has spent the entire essay arguing that philosophical method is irrelevant when it comes to science? Talbot’s closing rhetorical flourish only highlights the fatuous nature of his entire piece.

When it comes to science, Talbot’s philosophy can be characterized by one word – complacency. Talbot has elsewhere taken strong exception to certain remarks I made highlighting the crisis of contemporary science. Writing with Ann Talbot, he has maintained that there is no crisis in science and anyone who claims otherwise is in the camp of anti-science. At the same time as Talbot maintains this position, along comes Lee Smolin, one of the world’s leading theoretical physicists, who takes the position that physics faces a severe crisis precisely because the current generation of physicists have turned away from foundational philosophical issues.³⁰

It is now well over a century since Engels argued that a dialectical philosophical outlook is indispensable in assisting the working scientist in overcoming various conceptual dead ends. And it is over 80 years since Trotsky’s speech to the Mendeleev Congress where he made the same point.

But none of this has any impact on Talbot. He remains unphased, a firm believer in the positivist notion of science as an impartial search for truth free of the taint of class bias or ideology and free to advance without the cumbersome albatross of the dialectic. By his own circuitous route, he has followed in the footsteps of an earlier generation who were drawn to Marxism but felt that the dialectic was a mystical encumbrance that got in the way of genuine science. I am thinking specifically of Max Eastman and James Burnham. The only difference is that Talbot will not openly attack the dialectic. But it is also very clear that he sees no use for it whatsoever. This was an attitude that infuriated Trotsky when he first encountered it in Max Eastman. Trotsky’s characterization of this attitude in his Philosophical Notebooks is apropos here:

Those who repudiate “dialectics” consider it to be simply superfluous, a useless playing with thought. Positive science is enough!³¹

It needs to be said that this examination of Talbot’s essay reveals far more than the philosophical deficiencies of Chris Talbot. There is no reason to believe that Talbot represents a particularly poor example of theoretical training among the senior cadres of the International Committee. And given his commission to write and lecture on behalf of the International Committee on such an important topic as Marxism and Darwinism he is clearly considered one of the leading comrades when it comes to theoretical issues. What Talbot’s piece reveals is therefore the end point to which more than two decades of

³⁰ Smolin discusses the crisis of science in his most recent book, *The Trouble with Physics*. I discuss the implications of Talbot’s denial of the crisis of science much more fully in my reply to the essay he co-authored with Ann Talbot, *Marxism and Science: An addendum to “The Frankfurt School vs. Marxism”*, By Ann Talbot and Chris Talbot, 28 October, 2008, <http://wsws.org/articles/2008/oct2008/scie-o28.shtml>. I replied to Talbot in an earlier installment of this series. See Chapter 4, http://permanent-revolution.org/polemics/downward_spiral_ch04.pdf *Denying the crisis of science*.

³¹ Quoted in my essay on Trotsky’s Philosophical Notebooks, *Foreshadowing In Defense of Marxism*, http://permanent-revolution.org/essays/trotsky_notebooks.pdf

theoretical ossification in the International Committee have led. This is the outcome on the theoretical front of the neglect of a confrontation with positivism and empiricism. Those who think this neglect of theoretical issues has no political repercussions are only fooling themselves. The history of the Trotskyist movement shows otherwise.³²

To be continued.

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³² See Chapter four of *Marxism Without its Head or its Heart* for an extended discussion of the significance of the struggle documented in *In Defense of Marxism*. http://www.permanent-revolution.org/polemics/mwhh_ch04.pdf