Chapter 4

The Talbots weigh in: An olive branch to liberalism and a series of blatant misrepresentations on science

David North's *Odyssey* series was followed by an auxiliary piece published on Oct 28 under the byline of Ann Talbot and Chris Talbot, titled *"Marxism and Science: An Addendum to 'The Frankfurt School vs. Marxism'"*. [1] I have already commented in the previous chapters on the motives and methods behind North's orchestration of a smear campaign against me, the major purpose of which was to distract the readers' attention away from his failure to answer us on any of the theoretical and political issues raised by me and Frank Brenner. [2] I will here address the Talbots' remarks.

Because their essay is replete with numerous distortions and outright lies, it is necessary to spend a good deal of time deconstructing their arguments. I have only tried to address what I consider the major distortions of their piece. No one has the time to wade through all their rhetoric. In many cases, their arguments are barely coherent, relying on what they assume are the ingrained prejudices and stereotypes of their readers in order to make their points. Wherever possible, I have tried to reconstruct their arguments to make them at least somewhat coherent. If I restate one of their arguments it is only to present it in the best light. It is a poor polemicist who argues against the worst possible interpretation of his opponent. I have therefore probably put a better light on the Talbots' case than they deserve.

The *raison d'être* behind the Talbots' addendum to North's effort is to reinforce his effort to smear me. Thus, although the Talbots make a number of pronouncements on history, science and philosophy, this serves as little more than a "theoretical" veneer for their dishonorable enterprise. I will reply to all their statements in this area as their piece does raise questions that transcend the sleazy purpose behind it. However, readers should keep this polemical context in mind in what follows.

A tale of two dinners

The Talbots introduce their subject with the following narrative of how we came to meet and exchange views:

> The history of science is a complex subject and we assumed that Steiner had stumbled into an area of which he knew little and needed to do more work to develop an adequate analysis. We met informally for dinner while Steiner was on holiday in the UK and discussed some of the issues

[1] 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

[2] See Chapters 1-3 of this series, *Concocting a smear campaign*:
http://permanent-revolution.org/polemics/downward_spiral_ch01.pdf
http://permanent-revolution.org/polemics/downward_spiral_ch02.pdf
http://permanent-revolution.org/polemics/downward_spiral_ch03.pdf

> arising from the lecture in friendly terms. Later, Chris Talbot wrote to him suggesting some reading. Our patience was rewarded with the reply that North has already discussed. It was a 37-page diatribe reasserting and expanding on the points he made in the original lecture and article.

The above text is the third paragraph of the Talbots' polemic and really the first one of any substance. One would think that the Talbots would be especially careful to at least get some elementary facts straight at the very beginning of their polemic. One would think that, but one would be wrong.

To understand what is going on here a little background is called for. In the summer of 2002 I gave two lectures in Berlin to members and supporters of the Socialist Equality Party of Germany. One of these lectures was on the topic of science and dialectics. I subsequently mailed out a rough transcript of these lectures to a few interested parties. One of these comrades forwarded my lecture notes to Chris Talbot and told me that Chris would follow up with some comments about my lecture.

In December of 2002 I did indeed receive a critique of my lecture from Chris Talbot. Although at first reading I considered Talbot's critique to have been written in bad faith because he seemed to be almost deliberately misinterpreting the statements in my lecture notes, I decided to give him the benefit of the doubt and worked on a serious and respectful reply to the points he raised. That reply involved a good deal of research and took some time. I finally mailed him my reply on Feb. 2, 2003. We continued to correspond until our dinner in Manchester when I finally met Chris Talbot and his partner Ann at the home of a mutual friend on June 25, 2003. We had a polite and friendly discussion over dinner. Although certain disagreements on philosophical issues came up during our discussion there was nothing extraordinary or even particularly memorable in our exchange. We agreed to continue corresponding. I tried to encourage Chris Talbot to explore some of the issues raised during our discussion and sent him a letter when I returned home recommending some reading material on topics in which he had expressed an interest.

How does the Talbots' version of these events square with the facts? The first thing to be noted about the Talbots' characterization of our encounter is that Chris Talbot did not write to me "**later** [i.e. following the dinner – A.S.]… suggesting some reading." Talbot's criticism of my lecture (along with his reading suggestions) was sent to me **seven months prior** to our dinner conversation. My response to his critique was sent to him a good **five months prior** to our personal encounter. If the Talbots were truly outraged by what they now describe as my "diatribe" that tried their patience, they had the opportunity to tell me so in person during our dinner conversation. But nothing of the sort happened. While it was clear to me that Chris Talbot did not agree with large parts of my response to him, neither he nor Ann Talbot expressed anything resembling a sense of outrage at my "diatribe".

The Talbots narrative is based on a false chronology of these events. The sequence of events they present has time going backwards, much like the Mad Hatter's Tea Party, where Alice was asked to take more tea before any had been served. [3] Their anger at my supposedly ungrateful

[3] `Take some more tea,' the March Hare said to Alice, very earnestly.
`I've had nothing yet,' Alice replied in an offended tone, `so I can't take more.'

"diatribe" only makes sense if that diatribe was penned **after** the friendly dinner party. But in reality, my "diatribe" was written long **before** the dinner party. (And it was indeed a friendly affair despite our philosophical differences. Lots of tea and wine was consumed by all parties.) One can only conclude that the anger the Talbots profess to feel against my "diatribe" is itself fictitious, manufactured in order to fit the requirements of today's polemic. [4]

This falsification of the chronology is rather interesting considering that in North's *Odyssey* piece he makes quite a big deal of the fact that I got wrong the date of my application for party membership by one year. I replied to North's charge - that I was deliberately misstating the year of my application for membership - in Chapter 1 of the current series, noting that no conspiracy was involved. I had simply been relying on my memory and in this case it proved fallible. The same however cannot be said of the Talbots. It is not plausible that the Talbots made an honest

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`You mean you can't take *less*,' said the Hatter: `it's very easy to take *more* than nothing.'

Lewis Carroll, *Alice's Adventures in Wonderland*

[4] The record of my correspondence with Chris Talbot bears out my version of this chronology. On Feb 16, 2003, I received the following note from Chris Talbot acknowledging receipt of my reply to his criticisms of my lecture:

> Dear Alex
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> Thank you very much for the detailed reply to my criticisms. I have rather a busy schedule at the moment with my job and political commitments -- demonstrations with 2 million people through London does not make things any easier! -- so my apologies for not responding very quickly. I will definitely get back to you within a few weeks.
>
> Best regards
>
> Chris

On March 31, 2003 I received the following letter from Chris in response to a note I sent several days earlier informing him that I would be in Manchester visiting with a mutual friend:

> Dear Alex
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> Sorry about the delay in replying I've been away. I live in Huddersfield, about 30 miles east of Manchester so there should be no problem in meeting up. Let me know nearer the date. I'm still studying the material on Hegel.
>
> Best regards
>
> Chris

Finally, on May 29, 2003, I received the following acknowledgement from Chris Talbot that he could meet me on June 25.

> Dear Alex
>
> I hope to meet you in Manchester on June 25. I will make arrangements with J.
>
> Regards
> Chris

mistake about the sequence of events leading up to our dinner. If that were the case, then the Talbots would have to explain why they now consider my reply to Chris Talbot a "diatribe" that requires the most vigorous denunciation when they already had read it long before our "friendly" dinner? They cannot have it both ways. Either the Talbots are deliberately lying about the sequence of events, conveniently rearranging them – quoting North - "to fit the needs of [their] present political narrative" or they are lying about the nature of the dinner we had together. If we were to accept their chronology of these events then it would not have been possible for us to have a friendly dinner.

To what purpose have the Talbots rewritten this history of our interaction? Why would they paint me as a hyper-argumentative and unreasonable person who lashes out at my critics after my errors are "patiently" corrected? Why do they adopt the mantle of stern headmasters who go out of their way to correct a wayward student whose ungrateful and arrogant response demands a severe rebuke? Clearly, all this has a great deal to do with David North's earlier launching of a smear campaign against me. What the Talbots are doing here is adding their bit of bile to the narrative woven by North's piece in which I am depicted as "emotionally volatile" and unreasonable. The fact that the Talbots' account of our encounter has no basis in reality is all beside the point.

Furthermore, what the Talbots now call a "diatribe" – i.e. the kind of writing one would expect from someone who is "emotionally volatile" - was a carefully researched reply to Chris Talbot's letter in which I tried to engage him on the issues he had brought up. I did not just "reassert" my earlier views, as the Talbot's claim, but in some cases modified them where I thought a valid criticism had been made and in other cases expanded on them in order to answer an objection that I thought was based on a misunderstanding.

It is noteworthy that in their current polemic the Talbots never once cite any of the arguments I raised in this 37 page "diatribe" with the exception of a couple of quotes they cherry pick out of my reply in order to paint me as an enemy of science and the Enlightenment. They studiously avoid all the main points I raised in response to Chris Talbot's original misreading of my lecture. In the process of replying to Chris Talbot I cited over 30 books and essays and provided extensive quotations from many of these sources. When I wrote this reply back in the winter of 2002-2003, I put considerable effort into trying to clarify our differences and seeing if we can find some agreement or at least a better understanding of where we disagree and why.

In the interests of providing readers with a comprehensive picture of the context behind the Talbots' piece, I am here publishing both my original lecture notes and my lengthy reply to Chris Talbot's criticism of it. [5] It should be borne in mind that neither of these pieces was meant for publication. There is thus a certain unpolished roughness to them. The reply to Chris Talbot's criticism in particular assumes a certain depth of understanding of the history of science and philosophy. It is thus sometimes challenging reading for those with little background in these areas. That being said, some readers may find the effort worthwhile. Those readers can judge for

[5] A transcript of my Berlin lecture, "*Dialectics and the crisis of science*" can be found here: http://permanent-revolution.org/polemics/dialectics_science.pdf

My extensive reply to Chris Talbot's criticism of my Berlin lecture can be found here: http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf

themselves whether in fact my reply to Chris Talbot was little more than a "diatribe" or a serious attempt to engage someone in a discussion vital to the future of Marxism.

Defending the Enlightenment against Marxism

The document moves on and then speaks of our critique of Ann Talbot's discussion of the Enlightenment that originally appeared in MWHH. Commenting on our review of some of Ann Talbot's articles for the WSWS they write:

> He regards them as examples of the uncritical attitude of the WSWS towards Enlightenment values and democratic rights and compares them to the comments of Christopher Hitchens and Sam Harris, or even the Bush administration, which claims to be exporting democratic values to Iraq.

This is another misrepresentation of our views. [6] Frank Brenner and I did cite Talbot's articles as examples of an uncritical attitude toward the Enlightenment, but we certainly did not "compare them to the comments of Christopher Hitchens and Sam Harris, or even the Bush administration." What we did was warn against an uncritical adoption of the Enlightenment, as this was bound to blur the lines between socialism and liberalism. I pointed out in my initial discussion on the subject in my 2004 essay, *The Dialectical Path of Cognition*, that,

> The reevaluation of the Enlightenment has served a definite political function. It has served to justify a philosophical blurring of the lines between socialism and liberalism. Indeed, about the only criticism made of liberalism in recent years is that liberalism is cowardly and has betrayed its own principles. I might add that the door has been left open, mostly through a lack of criticism, to the suggestion that liberalism and socialism are not necessarily irreconcilable opposites.

Brenner and I returned to this topic and expanded on these comments in 2007 when we were writing *Marxism Without its Head or its Heart*. In Chapter 7 of that document we stated:

> We can add that in recent years, the traditional liberal defense of the Enlightenment has been complemented by a distinct form of right wing Enlightenment boosterism. Proponents of this intellectual trend include such figures as Sam Harris and Christopher Hitchens. Harris, in his book, *Letters to a Christian Nation*, defends a version of Western Enlightenment culture that is distinctly chauvinist and supportive of "humane" imperialism. Hitchens, as is well known, is a former leftist who has become an enthusiastic supporter of the Bush Administration and its "War on Terror". But Hitchens puts his own spin on his support for the Bush Administration. He claims that the Left has abandoned the Enlightenment (and indeed there is some truth to that statement when applied to most petty bourgeois radical groups) whereas the Bush Administration is defending those very values by exporting the principles of Western democracy to Iraq. In the face of such gangrenous claims to the legacy of the Enlightenment, the task of Marxists, one would think, would be to stake out an understanding of the Enlightenment such that it is clearly differentiated from both the liberal and right wing narratives. Conversely, an oversimplified and schematized version of the Enlightenment can only lend credence to the liberal and right wing accounts.

[6] The Talbots follow David North's lead in ascribing solely to me the views expressed in *Marxism Without its Head or its Heart* and *Objectivism or Marxism*. Both these works are the product of a collaboration between me and Frank Brenner. This flouting of the most basic standards of scholarship is not accidental. It is one more cynical ploy in the smear campaign. If Frank Brenner is brought into the picture then that would blunt the impact of the efforts by North and the Talbots to paint me as peculiarly emotional and volatile.

It should be clear to anyone who can read that we were warning against an oversimplified narrative of the relationship between Marxism and the Enlightenment rather than "comparing" Ann Talbot to the likes of Hitchens and Harris. When both liberals and right wing pundits claim for themselves the mantle of the Enlightenment, is it not of critical importance for Marxists to clarify exactly how they stand in relation to this ambivalent legacy?

Casting a democratic noose around the necks of the proletariat

The Talbots return to the Enlightenment at the very end of their document. Here I will confine my remarks to their claims for a panoply of political issues that relate to a historical assessment of the Enlightenment and the bourgeois revolution. The essence of what the Talbots claim in this section is that because I reject their uncritical enthusiasm for the Enlightenment, that I therefore find myself in the camp of various reactionary trends that dismiss the struggle for equality and democratic rights.

To make this assertion, they quote the following paragraph from MWHH (which again they attribute solely to me instead of to Steiner and Brenner), where we write that in Marx's early work, *On the Jewish Question*,

> [Marx] takes on one of the themes beloved of the Enlightenment, that the solution of the problems faced in trying to create a liberal nation state is to offer national minorities, such as the Jews in much of Europe, equal political rights under the law. Marx considered that remedy inadequate because it did not address the problem posed by their isolation from world culture though the institutionalization of ghettoized minorities.

The Talbots rejoinder to this statement embroils them in a lengthy defense of what can only be characterized as the bourgeois ideals of democracy. Here is what they write in response to our statement that Marx criticized a purely democratic political solution as inadequate,

> If this were true, then no Marxist could campaign for the political rights of any oppressed minority, or even of the members of the working class who continue to hold religious ideas. There would be no point in demanding the right to vote, the right of assembly, free speech or habeas corpus in case those who benefited from those rights had not thrown off their religious ideas. One could immediately forget about building a socialist movement in America.

Suddenly, our bringing up the fact that Marx considered democratic rights to be inadequate means, according to the Talbots, that we reject any struggle over democratic rights. This is something like saying that anyone who warns drivers not to drive below the minimum speed on a freeway is urging those drivers to speed along above the maximum. The remark about America evinces more confusion. First of all, why single out America in this way? You couldn't build a socialist movement in England or France either without democratic rights. (There is the whiff here of the crude anti-Americanism so prevalent in the western European petty bourgeois left.) But the crux of the issue is that important as democratic rights are, they are also inadequate to building a socialist movement, in America or any place else. It is precisely this distinction – essential to a Marxist perspective – that the Talbots are trying to obscure. The subtext of their

remarks is that the struggle for democratic rights pretty much encompasses the entire content of the struggle for socialism. But that is not how Trotsky saw this issue.

In the Transitional Program, democratic demands, such as the right to vote, are clearly demarcated from transitional demands. Trotsky considered the raising of democratic demands to be most important in fascist countries or in backward countries that had not yet experienced a bourgeois revolution. Yet even in fascist countries, where democratic demands are most vital, Trotsky never submerged transitional demands - whose content was the struggle for proletarian revolution - within democratic demands. The identification of the struggle for socialism with democratic demands was precisely the program of the Stalinist People's Fronts that thrived in the 1930's. Trotsky insisted that,

> A merciless exposure of the theory and practice of the "People's Front" is therefore the first condition for a revolutionary struggle against fascism.

He went on to explain that this exposure of the People's Front propaganda about democracy,

> … does not mean that the Fourth International rejects democratic slogans as a means of mobilizing the masses against fascism. On the contrary, such slogans at certain moments can play a serious role. But the formulae of democracy (freedom of press, the right to unionize, etc.) mean for us only incidental or episodic slogans in the independent movement of the proletariat and not a democratic noose fastened to the neck of the proletariat by the bourgeoisie's agents (Spain!). As soon as the movement assumes something of a mass character, the democratic slogans will be intertwined with the transitional ones; factory committees, it may be supposed, will appear before the old routinists rush from their chancelleries to organize trade unions; soviets will cover Germany before a new Constituent Assembly will gather in Weimar. The same applies to Italy and the rest of the totalitarian and semi-totalitarian countries.

The Talbots on the other hand don't even mention transitional demands. They prove to be ardent advocates of the same type of confusion of democratic demands with the struggle for socialism that characterized the Stalinist Peoples Fronts. To this end, they cite Marx and Engels's support for democratic movements in 1848.

> Marx was consistent in his defence of democratic rights. Universal suffrage was the second demand in the pamphlet that the Communist League issued in 1848, as revolution broke out in Germany. The first was for a republic. The separation of church and state was a central demand. Marx edited the *Neue Rheinische Zeitung*, which carried on its masthead the slogan *Organ of Democracy*. He and Engels worked closely with British Chartists such as Julian Harney and Ernest Jones in their campaign for universal suffrage. They also defended the Irish Fenians in their struggle against British rule. All these are democratic questions.

It goes without saying that the relationship between the democratic struggle and the struggle for socialism changed in the 80 years between 1848, when Marx and Engels were writing, and the Stalinist People's Fronts of the 1930's. We have also now seen another 80 years of tragic experiences with the fatal illusions of democracy leading to the defeat of working class revolutions since the 1930's. One need only mention in this context the defeats of numerous working class uprisings from Spain in the 1930's to Chile in the 1970's. Yet none of this leaves any impression on the Talbots. To them it might as well still be 1848. They continue to insist

that the only problem with raising democratic demands is the "high-flown abstractions" of their rhetoric. They write,

> Marx and Engels were critical of the high-flown abstractions of the French revolutionary slogan Liberty, Equality, Fraternity, particularly when it was used hypocritically by the bourgeoisie as they were shooting down workers in 1848. But they always took to task socialists who did not treat democratic questions seriously. For them, democratic rights were a concrete matter.

By insisting that the bourgeois slogans of Liberty, Equality and Fraternity provide an adequate framework for socialist politics, provided they are made more "concrete", the Talbots have essentially returned to the muddled and reactionary conceptions that characterized the People's Fronts of the 1930's. They are clearly a million miles from Trotsky's insistence that,

> … the formulae of democracy (freedom of press, the right to unionize, etc.) mean for us only incidental or episodic slogans in the independent movement of the proletariat and not a democratic noose fastened to the neck of the proletariat by the bourgeoisie's agents.

They have at the same time accepted a fundamentally idealist notion of the relationship between the slogans of the bourgeois revolution and its economic base. Yet they phrase their argument as if they are the ones who defend historical materialism against me. Here is their argument,

> As far as Steiner is concerned, the democratic rights that Marx and Engels defended are merely the product of bourgeois reason. Steiner argues as though it was the Enlightenment demand for equality, the very idea of equality, that gave rise to the unequal social and political relations of bourgeois society, rather than it proving impossible to realize equality under the conditions of a capitalist economy. Ever the idealist, Steiner consistently imagines ideas as the determinants of history and never economics.

Now "Steiner" never argued anywhere that the Enlightenment demand for equality "gave rise to the unequal social and political relations of bourgeois society." What Steiner and Brenner did argue is that the Enlightenment demand for equality expressed both what was progressive and what was illusory in the bourgeois revolution. The notion of equality that arises in the Enlightenment is a contradictory one. It has both the seeds of anticipation of a society free from hierarchical relations and also conceals the unequal relations that in reality pervade capitalism. Its ultimate derivation is the equality of buyers and sellers in the market place. As such it is the very essence of mystification. And this mystification takes place not only in the realm of thought, but in the actual social relations of capitalist society itself. Formally, every seller of commodities is equal to every other. But at the same time, some sellers of commodities are more equal than others, or to be more specific the buyers of labour power – capitalists - are more equal than the sellers - workers. [7] This is a contradiction but it is not the result of a trick. It is inherent

[7] This point was beautifully expressed by the Russian Marxist I.I. Rubin,

> The theory of value, which takes as its starting point the equality of exchanged commodities, is indispensable for the explanation of capitalist society with its inequality, because production relations between capitalists and workers take the form of relations between formally equal, independent commodity producers. All attempts to separate the theory of value from the theory of the capitalist economy are incorrect…

I.I. Rubin, *Essays on Marx's Theory of Valu*e, Chapter 10, http://www.marxists.org/archive/rubin/value/ch10.htm#a1

in the social relations of capitalism itself. And this mystification is reflected in the very notion of equality that is so highly prized by the bourgeoisie and their apologists. It cannot be otherwise for the idea of equality cannot be divorced from its basis in the relations of production that define capitalism.

Yet as far as the Talbots are concerned, the only problem with the bourgeois notion of Equality is that it cannot be realized "under the conditions of a capitalist economy". But in saying this they have done precisely what they accuse me of doing. They have divorced the notion of Equality from its roots in bourgeois society and transformed it into an ahistorical abstraction. They have also removed all the contradictions from this concept. For them there is nothing problematic in the bourgeois concept of "Equality". The only problem they see is with the practical application of this concept. But such an approach makes a huge concession to bourgeois ideology, for it forgets that formal bourgeois equality is indissolubly linked to the inequality of class society. On the other hand, the birth of revolutionary class consciousness first emerges when the fundamental inequality hidden within the formal equality of the commodity relation, the spontaneously reproduced consciousness of bourgeois society, is recognized for what it is. And alas we have in the literature of Marxism a withering critique of the limitations of the bourgeois notion of equality from the pen of Karl Marx himself. In the *Critique of the Gotha Programme*, Marx, in commenting on certain formulations placed into the program of the German Social Democratic Party by the Lasalleans, noted that the ideology of "rights", "fairness" and "equality" can never be higher than the society from which it springs. He wrote,

> Hence, *equal right* here is still in principle -- *bourgeois right*, although principle and practice are no longer at loggerheads, while the exchange of equivalents in commodity exchange exists only on the average and not in the individual case.

Marx made the point that this ideological baggage applies even to a society of cooperative producers, i.e. a society that has already gone beyond capitalism. Such a society in the initial period of its birth still retains vestiges of the capitalist society from which it sprang and these do not disappear in one day.

> In spite of this advance, this equal right is still constantly stigmatized by a bourgeois limitation. The right of the producers is *proportional* to the labor they supply; the equality consists in the fact that measurement is made with an *equal standard*, labor.

Marx emphasized that the ultimate goal of communism is not equality, but the full flowering of human capacities.

> But one man is superior to another physically, or mentally, and supplies more labor in the same time, or can labor for a longer time; and labor, to serve as a measure, must be defined by its duration or intensity, otherwise it ceases to be a standard of measurement. This *equal* right is an unequal right for unequal labor. It recognizes no class differences, because everyone is only a worker like everyone else; but it tacitly recognizes unequal individual endowment, and thus productive capacity, as a natural privilege. It is, therefore, a right of inequality, in its content, like every right. Right, by its very nature, can consist only in the application of an equal standard; but unequal individuals (and they would not be different individuals if they were not unequal) are measurable only by an equal standard insofar as they are brought under an equal point of view, are taken from one definite side only -- for instance, in the present case, are regarded *only as workers* and nothing more is seen in them, everything else being ignored. Further, one worker is married,

> another is not; one has more children than another, and so on and so forth. Thus, with an equal performance of labor, and hence an equal in the social consumption fund, one will in fact receive more than another, one will be richer than another, and so on. To avoid all these defects, right, instead of being equal, would have to be unequal.
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> But these defects are inevitable in the first phase of communist society as it is when it has just emerged after prolonged birth pangs from capitalist society. Right can never be higher than the economic structure of society and its cultural development conditioned thereby.
>
> In a higher phase of communist society, after the enslaving subordination of the individual to the division of labor, and therewith also the antithesis between mental and physical labor, has vanished; after labor has become not only a means of life but life's prime want; after the productive forces have also increased with the all-around development of the individual, and all the springs of co-operative wealth flow more abundantly -- only then can the narrow horizon of bourgeois right be crossed in its entirety and society inscribe on its banners: From each according to his ability, to each according to his needs! [8]

Marx never disdained the struggled for equality – as the Talbots accuse me of doing – but he always recognized the limitations bound up with bourgeois right and bourgeois equality and tried to stress to his followers the need to be clear about the difference between the goals of the bourgeois revolution and those of the communist movement. The Talbots, following in the footsteps of the Lasalleans against whom Marx was writing, obscure those differences.

Revolutionary history turned upside down

There is one final point that the Talbots raise in their discussion of the Enlightenment that needs to be addressed. Citing my essay *The Dialectical Path of Cognition*, where I wrote the following critique of Ann Talbot's obituary of the historian Christopher Hill,

> "Icons of bourgeois liberalism such as Hobbes, Locke and Jefferson have been elevated at the expense of the historical ancestors of modern communism, Winstanley, Münzer or Babeuf. I thought it somewhat peculiar that a recent obituary of the historian Christopher Hill criticized him for not recognizing the importance of Locke while at the same time barely mentioning Hill's most important book, *The World Turned Upside Down*, where Hill drew a memorable portrait of those personages involved in the English Revolution who went beyond the bounds of bourgeois property relations. ("These the times… this the man": an appraisal of historian Christopher Hill, by Ann Talbot, http://wsws.org/articles/2003/mar2003/hill-m25.shtml) In that book you will find the following spirited defense of one of the great precursors of the communist movement while at the same time providing a critique of the misanthropic bourgeois philosopher Thomas Hobbes.",

the Talbots respond by claiming that I was being unfair to Ann Talbot. She was after all writing an obituary about a 17th century historian and should not have been asked to discuss figures from the 16th or 18th centuries. They write,

> Ann Talbot's obituary of Hill was an article about a historian of the 17th century. There was no reason why it should have included a discussion of Thomas Münzer (ca. 1488-1525) or Gracchus Babeuf (1760-1797). Certainly, Christopher Hill was too good a historian to do so. Steiner's injunction that they should have been included indicates that he regards them as figures from a

[8] Marx, *Critique of the Gotha Programme*, http://www.marxists.org/archive/marx/works/1875/gotha/ch01.htm

> heroic pantheon rather than flesh and blood beings that lived and struggled in definite historical periods. This is very much in keeping with his utopian conceptions of socialism.

This defense of Ann Talbot's obituary is little more than double talk. I brought up Winstanley, Münzer and Babeuf as examples of the type of figures one would expect a Marxist historian writing about the bourgeois revolutions to discuss. I then cited Ann Talbot's obituary as an example of the failure to do that but never said that I expected her to specifically discuss Münzer or Babeuf. Yet the Talbots carry on the pretense that I did just that. At the same time, they are silent about Ann Talbot's failure to comment on Winstanley. But it was obviously the lack of a discussion of Winstanley that I had in mind when I cited Ann Talbot's obituary of Hill, as one of Hill's most important books, *The World Turned Upside Down*, deals with Winstanley and other figures who represented an attempt to go beyond the limits of the bourgeois revolution in England.

Obviously recognizing that this argument is not very credible, the Talbots add another reason for the lack of commentary about Hill's work on Winstanley. They have discovered that the ideals that figures like Babeuf or Winstanley fought for could not have been realized in their time. Proving what perspicacious historians they are, they then use this discovery to chastise me for regarding these men as "figures from a heroic pantheon rather than flesh and blood beings that lived and struggled in definite historical periods." And the clincher to this argument is that such illusions on my part are proof of my "utopian concepts of socialism". (The Talbots fail to provide the reasoning behind this conclusion but a likely reconstruction is that I am incapable of grounding political practice in an assessment of objective reality and therefore am prone to lionize the heroic but futile gestures of people such as Winstanley).

The obvious rejoinder to this invective is that we can celebrate the truly heroic role of figures like Winstanley or Babeuf without mythologizing them. The Talbots' argument here betrays the mindset of a cautious accountant rather than a revolutionary. They worry that any positive mention of Babeuf or Winstanley will inevitably lead them down the road of the dreaded "utopianism". Granted that the attempts of the Winstanleys and the Babeufs were doomed to failure because they could not transcend the limitations of their times, why is it not legitimate to seek inspiration in their struggles?

The Talbots quote the remarks of Marx and Engels about Babeuf from the *Communist Manifesto* and make a pointed reference to the statement found there that the writings of these early proto-communists were often "crude and reactionary." Here is the quote:

> We do not here refer to that literature which, in every great modern revolution, has always given voice to the demands of the proletariat, such as the writings of Babeuf and others.
>
> The first direct attempts of the proletariat to attain its own ends, made in times of universal excitement, when feudal society was being overthrown, necessarily failed, owing to the then undeveloped state of the proletariat, as well as to the absence of the economic conditions for its emancipation, conditions that had yet to be produced, and could be produced by the impending bourgeois epoch alone. The revolutionary literature that accompanied these first movements of the proletariat had necessarily a reactionary character. It inculcated universal asceticism and social levelling in its crudest form

The Talbots then go on to note that,

> [Marx and Engles] assessment of Babeuf's uprising and other similar early proletarian movements is a thoroughly historical materialist one. They stress that the conditions had not yet been produced by the development of capitalist society for such a movement to succeed and that its literature was necessarily reactionary and crude. Marxism is not based on crude, levelling ideas. It is based on the highest achievements of the Enlightenment.

But the Talbots have completely misunderstood the target of Marx's statement. They ignore the caveat at the beginning of this section:

> We do not here refer to that literature which, in every great modern revolution, has always given voice to the demands of the proletariat, such as the writings of Babeuf and others.

Marx and Engels harsh remarks, in which the writings of certain unnamed early communists are called "crude and reactionary", are taken by the Talbots to be a reference to the Babeuvists. But according to the eminent historian Hal Draper in his masterful annotated edition of the Communist Manifesto, the reference is not to the Babeuvists but to other rival sects. Draper indicates that Marx and Engels distinguished between Babeuf and his followers, whom they saw as precursors to the modern communist movement founded by Marx and Engels, from other groups whom they saw as alternative and rival sects requiring a polemical rejoinder. Here is Draper's comment on this section of the Manifesto,

> The present paragraph seems to say that [the Babeuvists literature] is not discussed because it expressed the demands of the proletariat. That is, the implication is the following: *Babeuf represented a form (if only a primitive one) of proletarian communism, i.e., of our own tendency, and here we are discussing only alternative tendencies.* [9]

Through the use of an Internet search engine, the Talbots were able to quickly locate a quote from Marx located near the word "Babeuvists". But as this example highlights, all the Internet search engines in the world are no substitute for thinking and an understanding of historical context. This may seem to be a small point, but I raise it only to highlight the manner in which the Talbots will jump on anything in order to twist an argument. In this case, they are trying to get off the hook of our charge that Ann Talbot, in failing to discuss Winstanley in her obituary of Hill, was carrying on what has become a mode of adaptation to liberal iconography in the pages of the WSWS.

In any case, the Talbots' quotes from Marx and Engels, even when not misattributing the reference, are very selective. They do not quote the words of Friedrich Engels on Thomas Münzer, who writing years later about the Peasant War in Germany, waxes eloquently about his creative genius. Here is a sample from Engels' remarks on Münzer,

> Münzer's political doctrine followed his revolutionary religious conceptions very closely, and as his theology reached far beyond the current conceptions of his time, so his political doctrine went beyond existing social and political conditions. As Münzer's philosophy of religion touched upon atheism, so his political programme touched upon communism, and there is more than one

[9] Hal Draper, *The Adventures of the Communist Manifesto*, (Center for Socialist History, 1998), p. 301.

> communist sect of modern times which, on the eve of the February Revolution, did not possess a theoretical equipment as rich as that of Münzer of the Sixteenth Century. His programme, less a compilation of the demands of the then existing plebeians than a genius's anticipation of the conditions for the emancipation of the proletarian element that had just begun to develop among the plebeians, demanded the immediate establishment of the kingdom of God, of the prophesied millennium on earth. This was to be accomplished by the return of the church to its origins and the abolition of all institutions that were in conflict with what Münzer conceived as original Christianity, which, in fact, was the idea of a very modern church. By the kingdom of God, Münzer understood nothing else than a state of society without class differences, without private property, and without Superimposed state powers opposed to the members of society. All existing authorities, as far as they did not submit and join the revolution, he taught, must be overthrown, all work and all property must be shared in common, and complete equality must be introduced. In his conception, a union of the people was to be organised to realise this programme, not only throughout Germany, but throughout entire Christendom. Princes and nobles were to be invited to join, and should they refuse, the union was to overthrow or kill them, with arms in hand, at the first opportunity. [10]

Doubtless Engels' tribute to Münzer would have elicited a sharp rebuke from the ever-vigilant Talbots', always on the lookout for the heresy of "utopianism". Perhaps sensing that this second sailing of their Apology is no better than the first, the Talbots' present yet one more reason for ignoring Winstanley. Here it is:

> But Hill was labouring under the burden of the Stalinist Popular Front theory of People's History, which sought to find national revolutionary heroes in every country. In an obituary, a writer must emphasise the best in the subject's life while not ignoring what was weakest. Hill's best work by far was his biography of Oliver Cromwell, who was a difficult figure for Hill to write about, but one who embodied the revolutionary impulse of his class.

At last we are presented with what is presumably the real reason for Ann Talbot's avoidance of a discussion of Winstanley. It is that Hill's book, the *World Turned Upside Down*, was influenced by the Stalinist theories of People's History and therefore is not as worthy of our consideration as Hill's book about Cromwell (*God's Englishmen*.) Furthermore, we are told that it is the duty of the composer of an obituary to present only what is positive about her subject and leave the remainder interred in the grave along with other reminders of a less than perfect life. And that is finally the reason why Hill's *The World Turned Upside Do*wn is not even mentioned.

This explanation makes no sense. First of all, Winstanley should be of interest to Marxists even more than Cromwell as it was the former who represented in the most radical form a precursor of the communist movement. Hill's book, *The World Turned Upside Down*, is perhaps the best work from a Marxist perspective on Winstanley and other remarkable and long forgotten figures of the English Revolution. The Talbots' wish to justify their dismissal of this book by labeling it as a product of the People's History school of historiography and therefore tainted with Stalinist Popular Frontism. They produce no arguments however for justifying this dismissal of Hill's book. Why is the Cromwell biography freer of the taint of a People's History than Hill's *The World Turned Upside Down*? The latter was actually written after the Cromwell biography and thus if anything would have reflected even less of an influence of Popular Frontism than the

[10] Friedrich Engels, *The Peasant War in Germany*, http://www.marxists.org/archive/marx/works/1850/peasant-war-germany/ch02.htm

Cromwell biography. Finally, even if we grant that the Talbots are correct in their judgment that the Cromwell biography is far superior to *The World Turned Upside Down*, why should the latter be ignored? Since when is it the task of Marxists to prettify the portrait of a historian who was influenced by Stalinism? The Talbots' claim that their duty is to "emphasise the best in a subject's life while not ignoring what was weakest", but according to their own admission they have indeed ignored what they thought was the weakest in the oeuvre of Christopher Hill.

While none of the Talbots arguments make any sense at face value, the thing to keep in mind is that the Talbots consistently reject any presentation of history that challenges too sharply the shibboleths of liberalism. The iconography of liberalism does not know what to do with historical figures that go beyond the limits of bourgeois constitutionalism. Locke or Cromwell is much easier to handle than Winstanley or Babeuf. And that is the real point of the Talbots' aversion to the precursors of communism.

Steiner's "views of modern physics": A case built on a series of lies

A key portion of the Talbots' document is devoted to an attack on what they claim are "Steiner's views of modern physics". In this section we find the following statement:

> North has already pointed out that Steiner's approach to science is highly idealist and fundamentally opposed to materialism, and has shown how Steiner's idealism is reflected in his attraction to the theories of Dame Francis Yates and Betty Jo Teeter Dobbs.

I will shortly address the substance of the argument about Francis Yates and Betty Jo Dobbs. However, it is rather curious that my supposed "attraction" to the theories of these two historians is based on a couple of remarks I made indicating that I thought their work on Bruno and Newton was a fruitful area of inquiry. On the other hand, no mention is made of the fact that I cited the work of Friedrich Engels more than a dozen times. Apparently I am not "attracted" to the work of Engels who figures much more prominently in my discussion of the philosophy of science.

Moving into the body of their polemic, the Talbots then begin to take a few pot shots at my lecture. Noting that I used a number of examples from Michio Kaku's book, *Hyperspace*, to illustrate my lecture, they write derisively,

> But not even Kaku, a serious String Theorist himself, would pretend that his little book could be the basis for developing a new philosophy of science.

My attempt to develop "a new philosophy of science" exists only in the heads of the Talbots. What I was actually doing was reviewing the methodological principles of an old philosophy of science, the one articulated by Friedrich Engels in his *Dialectics of Nature*. I was trying to demonstrate to my audience how the insights derived from a dialectical understanding of the development of science could prove fruitful in a historical survey of the development of physics.

Continuing their attempt to discredit my lecture, the Talbots follow up with this gem:

> Yet this is the only book that Steiner cites. No scientist could take him seriously and no Marxist could take him seriously.

Even this apparently innocuous factual statement is false. I cited not only Kaku's book, but a book by Stephen Hawking, as well as Engel's *Dialectics of Nature* and Alexander Koyre's *Galilean Studies*. And those were just the formal citations in footnotes. In addition I referred to works as disparate as those of Noam Chomsky, Thomas Kuhn, and Aristotle.

The Smoking Gun

Following along the Talbot's narrative, we soon come to the crème de la crème of their indictment of me as an idealist philosopher of science. They announce they have found the smoking gun in my discussion of Riemann and his discovery of non-Euclidean geometry. They write,

> According to Steiner, Riemann's multidimensional geometry led directly to Einstein's theory of relativity. But this is simply not true. We have here another of those instances that North refers to when Steiner, for all his repeated invocations of "dialectics," demonstrates that he is a common or garden empiricist at heart. Here are two facts: 1854, Riemannian geometry; 1905, Einstein's Theory of Relativity; one fact precedes the other, therefore we have a causative relationship: Riemannian geometry gives rise to the Theory of Relativity.

This indictment of me, upon which their entire case is based, rests on the following quote from my lecture:

> The physical interpretation of the fourth dimension finally begins in 1905 with Einstein's theory of special relativity.

Now to properly interpret this sentence, it is necessary to read a few other sentences, both those that come before it as well as those that come after it. But the Talbots provide no such context.

The Talbots claim is that because I wrote that Einstein's theory of special relativity followed chronologically Riemman's geometrical discoveries, that I maintain that the Riemannian geometry was the direct cause of Einstein's theory of relativity. It is notable that while the Talbots come up with a quote where I emphasize the importance of Riemann's discovery, they provide no quote to demonstrate that I claimed that to be the cause of Einstein's theory. They cannot do so for I never made any such statement. What I actually wrote was,

> The physical interpretation of the fourth dimension finally begins in 1905 with Einstein's theory of special relativity. The outlines of this theory are well known. I just want to mention its significance for the beginning of the overthrow of the Newtonian world outlook. With the special theory of relativity, space and time now become tightly intertwined. They are no longer separable and absolute as they were in Newton's physics. This means that motion in space affects time and vice versa.

Reading this, it should be clear that nowhere did I deny that there were earlier attempts to provide a physical interpretation of an 'n' dimensional universe. Not only did I not deny the earlier attempts, but I explicitly discussed some of these earlier attempts to provide a physical explanation for Riemann's non-Euclidean geometry in my lecture. For instance, I said,

> Riemann himself was far from what we call today a pure mathematician. He firmly believed that his geometry was applicable to the physical world.

Also, I added,

> Riemann himself believed that the geometry of four dimensions was a description of physical reality, however he was not able to demonstrate it. There were other physicists who thought this may be a fruitful topic for exploration, but they soon gave up as it seemed there was no conceivable way to test if we actually live in a three dimensional or a four dimensional space.

It should be obvious that reading the transcript of my lecture in context, when I said that "The physical interpretation of the fourth dimension finally begins in 1905 with Einstein's theory of special relativity", I was talking about the first **successful** physical explanation of a non-Euclidean universe. The Talbots make exactly the same point when they write,

> In other words, he [Riemann] was working on the same problem that Einstein was later to solve. [11]

That there were previous, less than successful, attempts to discover a physical interpretation of Riemann's geometrical discovery was indeed discussed in my lecture. Given the time constraints of a one hour lecture I could not get into this material in any depth. But I certainly noted it. And where in anything I said in that lecture was there any suggestion that I thought that the Riemannian geometry was the **cause** of the special theory of relativity? Yet it is on the basis of this deliberate misrepresentation of my lecture that the Talbots expend the next several paragraphs in a straw man argument against me. For instance, they assert,

> This is an entirely idealist theory and, what is more, it is false and unfounded in reality. The relationship between Riemann's geometry and Einstein's theory of relativity is immeasurably more complex than that, and of an intellectual complexity that far outstrips Steiner's impoverished and schematic conception. Reading Steiner's account of the development of modern physics is like being put on a diet of bread and water when there is a banquet set out in front of you.

One can only hope that such an egregious example of dishonesty in summarizing the arguments of a polemical opponent or a rival theoretician is a rare exception and not the norm within the scientific and academic community at the University of Huddersfield where Chris Talbot lectures.

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[11] That Einstein was the first to solve the problem of the physical interpretation of Riemann's n-dimensional geometry has been a common narrative for a long time among historians of science. For instance, take the following account found in a textbook first published in 1927:

> Although experimenters had utilized the most refined apparatus for detecting a possible non-Euclideanism of space and had failed in their efforts, it was reserved for theoretical investigator Einstein, by a stupendous effort of rational thought, based on a few flimsy empirical clues, to unravel the mystery and to lead Riemann's ideas to victory.

A. d'Abro, *The Evolution of Scientific Thought from Newton to Einstein*, (Dover, 1950), p. 58

Denying the role of experiments in physics

The next step in the Talbots' misrepresentation of my views is to claim that I deny the role of experimentation in the Scientific Revolution, that I champion an idealist theory of science based on pure mathematical intuition. They introduce this falsification with the following statement,

> We cannot, therefore, divorce physics from experimentalism and root its advances in an ideal world of pure mathematical thought, as Steiner would wish to do.

This is another willful misrepresentation of my views. My remarks about scientific experiments, far from denying the role of experiments and observation, tried to explain that experiment and observation were dialectically intertwined with the mathematical conceptualization of nature throughout the Scientific Revolution of the 17th century. What I denied was not the role of experiments in the scientific enterprise, but the myth, based on an empiricist understanding of this history, that experimentation was the sole driving force behind the new science. This is clear from the following excerpt from my lecture:

> I have not yet mentioned the role of hypothesis and experimentation in the new science. This is not because experimentation did not play an important role in the development of the new science. However, the place of experimental procedure in the birth of modern science, like the role of the ancient atomists, has taken on a mythic status. To properly appreciate the role of experimentation in the scientific revolution, it is necessary to disentangle fact from myth...
>
> The myth of "pure" experimentation, or "pure" observation as the bedrock of modern science is one that survives to the present day. It is one of the tenets of modern empiricist philosophy that I shall describe presently. We have noted that the creators of the new science, men such as Bruno, Galileo, Newton, did not think of themselves as experimenters who then derived generalizations based on their experiments. They thought of themselves as Platonists who sought to discover the mathematical laws that governed nature. 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. [12]

I not only stated that the Scientific Revolution was characterized by the interaction between experimental methods and mathematical analysis, but I cited several recent historians of science to back up this claim in my long reply to Chris Talbot's letter criticizing my lecture. Just in case Mr. Talbot missed it, I anticipated and answered in advance his misrepresentation of my views in our previous correspondence where I wrote the following,

> My object in this section [of my lecture] 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,

[12] Alex Steiner, Dialectics and the crisis of science, http://permanent-revolution.org/polemics/dialectics_science.pdf p. 11.

> "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) [13]

The quote from Segre points to another side of the Talbots' polemic. In addition to branding me as an idealist and a Platonist, their misrepresentation of my position serves another function – they are able to dodge the issue I was addressing and which is clearly articulated by Segre – namely the myth of Galileo "the empiricist scientist" who abjured philosophical questions. The Talbots do not wish to engage with this issue because they themselves are highly invested in the same myth of "the empiricist scientist" as we will see. They make no mention of this part of our correspondence. They have nothing to say about the article by Michael Segre that I cited. Nor do they have anything to say about the following quote from Galileo himself where Galileo insisted that he was not merely an experimenter but a theoretician guided by "natural reason", a quote that I also provided in my reply to his correspondence,

> "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) [14]

Yet the myth of Galileo the pure experimenter is today widely recognized to be just that – a myth – among historians of science. Even the venerable I. Bernard Cohen, whom Chris Talbot lavishes with praise elsewhere in his critique of my lecture, has this to say about this topic,

> Most scientists and historians of the late nineteenth and early twentieth centuries uncritically assumed that since Galileo was the "father" of modern physics (if not of modern science), he was equally the inventor and initiator of the experimental method. It followed that he must have made all of his discoveries by experiment. So prevalent was this view that the translators of Galileo's Two New Sciences, Henry Crew and Alfonso de Salvio, added the words "by experiment" to Galileo's text, so that his introduction to the subject of motion would not merely refer to the principles Galileo himself said he "found" (*comperio*, "I find"), but would make him say that these were new principles which "I have discovered by experiment." [15]

[13] Talbot – Steiner exchange, http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #3.
[14] Talbot – Steiner exchange, http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #3.
[15] I. Bernard Cohen, *The Birth of a New Physics*, (Norton, 1985), p. 201 n.

An Idealist Philosophy of Mathematics?

My supposed denial of the role of experimentation in the Scientific Revolution then leads the Talbots to the next phase of their campaign to prove that I am an idealist. They now detour into a discussion of the history of mathematics in order to prove their point.

> Viewed in an idealist manner, the history of mathematics might seem to refute a materialist analysis of the development of human thought. Mathematics appears to exist in an ideal world.

This is a typical example of the method – frequently used by David North and other leaders of the IC - of shadow boxing against an unnamed polemical opponent. In this case the polemical opponent is obviously me but as I did not discuss the history of mathematics in my lecture - I discussed instead the history of physics - and they cannot cite a specific quote from my lecture, they are reduced to railing against an unnamed person who views the history of mathematics "in an idealist manner". What I did emphasize in my lecture was that the development of the new science of Copernicus, Galileo, and Newton, was characterized by a belief that the material world can be rationally understood as the interrelated pieces of a whole and that quantitative mathematical relations define the behavior of the various parts of the universe and its interrelationships. In contrast to Aristotelian physics there is no incommensurable gulf between the heavens and the earth. The laws of gravity encompass and relate the one to the other. There is nothing particularly unique or new about this interpretation. What has been in contention in the history and philosophy of science was something else, namely the exact nature of the interrelationship between the mathematization of nature and experimental procedures. Some historians of science would give more weight to the role of experimentation while others would give more weight to the role of mathematical concepts and thought experiments. These are legitimate areas of dispute among historians of science today. There are virtually no historians of science that are pure neo-Platonists and completely abjure the role of experiments in the Scientific Revolution of the 17th century. Similarly there are no historians of science, even those who emphasize the role of experiments such as I. Bernard Cohen, who completely discount the role that thought experiments and the mathematization of nature played in the birth of the New Science. [16] The Talbots misrepresent this discussion and claim that I am on the side of a supposed neo-Platonist tendency in my understanding of the Scientific Revolution.

[16] Cohen was a supporter of Stillman Drake, who discovered in previously unexamined manuscripts that Galileo did indeed carry out some experiments. Drake's work corrected the earlier work of Alexander Koyre who emphasized Galileo's thought experiments and overturned the earlier view of Galileo as a modern experimental scientist. I summarized this discussion in my reply to Chris Talbot, where I wrote,

> [T]here 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 – Steiner exchange, http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner Reply #4)

Even I. Bernard Cohen, a champion of Stillman Drake, a staunch supporter of the experimental view and a favorite of Chris Talbot, more or less expresses the same sentiment. He writes,

In order to substantiate their charge of idealism against me, the Talbots launch into a discussion of the history of mathematics that leads them to champion the empiricist Babylonians as against the idealist Pythagoreans. Here is what they write,

> Yet if we pursue the history of mathematics with a little more tenacity and trace the discipline to its beginnings in the ancient civilisations of Egypt and Babylon, we find that mathematics develops in response to the material problems that confronted human beings in their attempt to make a living in the Nile valley or Iraq. Only at a later date did mathematics take on the abstract, generalised character with which we now associate it. This happened among the ancient Greeks in the course of the sixth century BC, when there was an attempt to understand the world scientifically, which is associated with the name of Thales of Miletus.
>
> Thales is said to have introduced geometry from Egypt and used it to predict a solar eclipse in 585 BC. Thales and his successors were essentially materialists, but at the same time there developed an idealist school of philosophy among the followers of Pythagoras who regarded numbers as the real basis of a transitory, material world. In the course of the fifth century BC as philosophy developed in Athens it became sharply polarised between idealists and materialists.

It turns out that I do not have to write anything new to answer this. I had already done it in 1972 when I criticized the work of George Novack, who had been for decades the philosophical spokesperson of the Socialist Workers Party. As part of my critique of Novack's understanding of the history of philosophy, I wrote at that time,

> [A] narrow empirical thinking characterized much of the non-religious thought of the other great civilizations of that time, such as the Babylonian. For that reason the Babylonians, though their culture was in many ways superior to the early Greeks, could never develop mathematics and science beyond the level of certain practical formulas that were useful in land measurement and other commercial acti vities.
>
> The next great impetus to philosophy was provided by the Pythagoreans, whom Novack only mentions in passing since they were complete idealists. The Pythagoreans developed philosophy in a contradictory way. They reverted from the natural principles of the Milesians to religious and mystical conceptions derived from the Orphean mystery cults. At the same time we find in the Pythagoreans the first conception that reason should be the guide to man's practical life. They carried this out in practice by forming quasi-Utopian commu nities based on their teachings. They were the first to suggest that theory had something to do with practical activity.
>
> Their most important contribution, however, lay in their being the first to develop the demonstrative proof in mathe matics. Particularly in their proof of incommensurable magni - tudes, they showed that abstract logical reasoning can come closer to the truth than any impressions based on intuitive understanding. They thus further developed the distinction between appearance and essence that the Milesians had begun.

It is therefore reasonable to conclude that Drake is essentially correct, even if with the zeal of the discoverer he may somewhat overstress his own image of Galileo as a modern experimental physicist and downplay both the role of intellection and the debt of Galileo to any of the late medieval concepts and rules concerning motion. (Ibid. p. 204)

> Their importance to the later development of Greek philosophy is witnessed by the fact that many later philosophers continually refer to the proof of the incommen surability of magnitudes as a model of the reasoning process that finally arrives at the truth. [17]

As my lecture did not deal with the history of mathematics, I left the Babylonians out of my talk. However, when I did write about this topic in 1972 I acknowledged the historical and material origins of mathematics in the practical issues faced by the Babylonians. But I also pointed out their limitations and the great advance that the Pythagoreans made over the work of the Babylonians, namely the use of demonstrative proofs. Whereas in 1972 I tried to present a dialectical understanding of the history of philosophy in which I emphasized the contributions of both the materialists and the idealists the Talbots' defense of the empiricism of the Babylonians and the Milesians leads them to denigrate the revolutionary achievement of the Greeks – the discovery of reason. It is this discovery that set Greek culture apart from its neighbors, as brilliant as some of them were. We do not find anything like a syllogism or a mathematical proof in the Egyptian or Babylonian civilizations. But for mathematics to become a genuine scientific discipline it had to be liberated from the constraints of empirical examples and demonstrate its universality. This is what the Greeks achieved. And the discovery of rational proof does not just lead to the birth of mathematics but to the development of logic which is a necessary step for the emergence of genuine science that depict the laws of nature and go beyond the collection and classification of facts. as important as that initial step was in the history of science and mathematics. [18]

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[17] Alex Steiner, *The Liberal Philosophy of George Novack*, (Labor Publications, 1972), p. 39-40.

[18] The most militant champions of the Egyptian origin of mathematics are currently certain Afrocentric historians such as Martin Bernal, whose two volume *Black Athena* spares no effort to belittle the contributions of the Greeks to the development of science and mathematics. It is ironic that the Talbots come to similar conclusions as Bernal because they do not wish to give credit to the idealist Pythagoreans and Platonists for advancing mathematical science. For a good critique of the thesis of the Egyptian or Babylonian origin of mathematics, see *Black Athena Revisited*, edited by Mary R. Lefkowitz and Guy MacLean Rogers (Published by UNC Press, 1996). This volume includes an essay, *Black Athena, Afrocentrism, and the History of Science*, by Robert Palter. In comparing the development of Egyptian to Greek mathematics, Palter writes,

> The first approach to the concept of irrational numbers was in the Pythagorean concept of the incommensurability of certain line segments (the side and the diagonal of a square)…
>
> Did any of the Egyptian results on volumes of solids, or the methods used to obtain such results influence the Greek geometrical tradition? On the specific matter of the volume of a square pyramid, Archimedes tells us that Eudoxus of Cnidus (first half of the fourth century B.C.E.) was the first to prove the result… But Eudoxus was a student of one of the most original Greek mathematicians of his time, Archytas of Tarentum (first half of the fourth century B.C.E.), and twice visited Athens, where he was closely associated with Plato. Eudoxus was thus, it may be assumed, thoroughly steeped in the Greek mathematical and philosophical tradition, and his mathematical proofs must have been of the rigorous axiomatic sort characteristic of that tradition. It is difficult to exaggerate the difference between such proofs and anything found in Egyptian mathematics. (p. 232)

We need not get involved in the controversy over the degree to which Greek mathematics borrowed from an earlier Egyptian discipline in order to appreciate Palter's point here – the development of rigorous mathematical demonstration that is found in Greek mathematics represented an enormous advance over the empirical methods of the Egyptians. Palter's point would also apply to a comparison of Greek mathematics with Babylonian mathematics. I raise this issue not to suggest that the Talbots share the Afrocentric historians' antipathy to Greek culture, but to illustrate the knots one gets into when one disdains the dialectic and adopts a vulgarized view of the history of ideas. Bernal is unwilling to give credit to Eudoxus and other Greek mathematicians because they were

Novack Redux

After setting up their straw-man argument, the Talbots then assert that I "divorce physics from experimentalism and root its advances in an ideal world of pure mathematical thought." Then they devote the next few pages railing against the "anti-materialist and unhistorical view" that I have supposedly adopted. Their account is studded with a comic-book vulgarization of the history of philosophy wherein all the good guys are materialists and all the bad guys are idealists. For instance, we get to hear about a famous battle between the gods and the giants from Plato's *Sophist* - the gods being the idealists whom Plato favors, and the giants the materialists whom Plato reviles. Here is what they say on this subject,

> In the course of the fifth century BC as philosophy developed in Athens it became sharply polarised between idealists and materialists. Plato characterised this struggle as a battle of gods and giants and, as is made clear in *The Sophist*, he was entirely on the side of the gods and opposed to the giants, the materialists, who wanted to drag everything down to the level of body.

The Talbots' views on the history of philosophy echo to a remarkable degree a popular presentation by George Novack in his book, *The Origins of Materialism*. Here is Novack's version of the same discussion,

> Plato was an even more implacable opponent of the materialists…
> Plato directly alludes to his irreconcilable struggle against the materialists in setting forth the doctrine of the Ideal Forms which constituted his theory of knowledge and depended upon the acknowledgement of the immortal soul. In *The Sophist* he wrote – Why this dispute is about reality is a sort of Battle of Gods and Giants. One side drags everything down to earth, literally laying hands on rocks and trees, arguing that only what can be felt and touched is real, defining reality as body, and if anyone says that something without body is real, they treat him with contempt and will not listen to another word.

Novack then explains the meaning of this myth,

> As George Thomson points out in Volume II of his *Studies in Ancient Greek Society*, the Giants are the materialists and the Gods are the idealists. [19]

Ironically, I wrote a critique of Novack's book for the Workers League in 1971 on exactly the same topic. Commenting on Novack's general approach to the history of philosophy in ancient Greece, I wrote,

> All previous attempts to explain nature and society and man's relation to it had to be imbued with elements of speculation, i.e., with non-scientific, idealist explanations such as deux ex machine, chance, etc. Within these limits however, great advances were made which paved the way for a consistently materialist outlook. Certain aspects of the world, certain areas of inquiry, could be developed scientifically, although the totality of phenomena could not previous to Marx. It is important to remember furthermore, that this development was not only the product of the materialist thinkers, but of the idealists as well, or rather it was out of man's struggle against

Europeans, whereas the Talbots are unwilling to give them credit because they were idealists.

[19] George Novack, *The Origins of Materialism*, (Merit Publishers, 1965), p. 222

> nature, reflected philosophically at crucial points in idealism and materialism, that scientific thought developed.
>
> Another mechanical notion that Novack develops is the tendency to see the materialists as automatically reflecting a progressive social movement and the idealists as automatically reflecting a reactionary ruling class in Ancient Greece. Things were not that simple. Rather, each of the successive materialists, as well as the idealists emerged out of a conflict with the prevailing notions of their time and while reflecting the class struggle, did not do so in any direct way.
>
> To understand this, we must trace the actual history of thought in its contradictory development, and not burden it with ahistorical notions of what is progressive and what is reactionary. Novack's attempt to look at history according to this mechanical view leads him to separate out the opposites in a phenomenon that could not have emerged in its real history in any other way. [20]

It is not accidental that the Talbots' understanding of the history of philosophy converges with Novack's undialectical and ahistorical account. Had they been writing in 1971 they could just as easily have been the target of the critique of Novack that I wrote for the International Committee. And it is on the basis of this return to the mechanical materialist method of Novack that the Talbots (and North) seek to brand me as an idealist. Readers should keep this in mind when considering David North's accusation that it is Frank Brenner and me, and not the current leadership of the International Committee, who have repudiated the heritage of the International Committee.

The Talbots next foray into the history of philosophy is to introduce us to Alfred North Whitehead whom they depict as a pugilistic opponent of the great materialists in the history of philosophy. [21] Whitehead was indeed an arch idealist. You might say he was the opposite of Novack. Whereas Novack painted all materialists in the history of philosophy with a plus sign, Whitehead painted them with a minus sign. But as I have never commented anywhere in my writings on Whitehead, I fail to see the relevance of this discussion, except to set up another guilt-by-association type argument. (I am presumably guilty because I do not follow the method of Novack and the Talbots in branding all materialists as positive and all idealists as negative in the history of philosophy.)

Nor did I ever suggest that the history of philosophy is a series of footnotes to Plato as Whitehead did. We will comment further on the history of philosophy as explicated by North and the Talbots on another occasion. The point that needs to be made here is that the Talbots launch into their harangue on the basis of a blatant misrepresentation of my views and then spend several pages declaiming against it. Furthermore, the framework for their discussion of the history of philosophy is indistinguishable from the non-dialectical and ahistorical views of George Novack, views that were sharply criticized in the publications of the International Committee in the early 1970's.

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[20] Alex Steiner, *The Liberal Philosophy of George Novack*, (Labor Publications, 1972), p. 35

[21] Alfred North Whitehead was an American idealist philosopher of the first half of the 20th century who was the author of the well-known statement that "all philosophy consists of a series of footnotes to Plato."

Back to the universe of n-dimensions

My supposed adoption of a neo-Platonist theory of mathematics allows the Talbots to return to their earlier argument that I ascribed the cause of the theory of special relativity to be the discovery of non-Euclidean geometry fifty years earlier. And basing themselves on this falsehood, they claim that I deny the richness and "dialectical complexity" of the relationship between mathematics and physics. They write,

> The richness and, in a genuine sense, dialectical complexity of the relationship between mathematics and physics is denied in Steiner's conception of a simple mechanical process in which one development in mathematics triggers a new direction in physics. Physics is certainly a mathematical discipline, but it is not possible to tie a development in humanity's understanding of the physical nature of the universe to an isolated advance in mathematics. Steiner is entirely blind to the manifold interconnections of social thought that produce a new development in physics or any other area of human endeavour.

Again, such a misrepresentation of my views cannot possibly be ascribed to a simple misunderstanding. This is willful dishonesty. Not only do I not deny the "dialectical complexity" of the relationship between mathematics and physics, but I spent a good deal of time providing an account of that relationship. For instance, in my correspondence with Chris Talbot, I wrote,

> 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 *18th 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. [22]

[22] Talbot – Steiner exchange http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf, Steiner reply #2.

I also cited several sources who have written on this topic in order to illustrate the problems involved when trying to disentangle the web of influences that were the background of the Scientific Revolution.

Dragging in the permanent revolution to score a point

After branding me a follower of Yates and Dobbs (about which I will comment in the next chapter) the Talbots next claim that because I am blind to the interconnections between social processes and scientific developments, I miss the connection between Einstein's theory of relativity and Trotsky's theory of permanent revolution. They cite a lecture by David North wherein great significance is attached to the fact that the first Russian Revolution of 1905 coincided with the publication of Einstein's theory of special relativity. [23] Now I can certainly agree that the development of capitalism in the latter part of the 19th and early part of the 20th century gave impetus to both discoveries. And North's lecture on the subject is a serviceable enough discussion of Trotsky's theory of permanent revolution. However, the Talbots inadverdently put their finger on a problem in North's presentation. In trying to conceptualize the similarities between Einstein's discovery and Trotsky's, the Talbots, quoting North, write,

> As Einstein's relativity theory—another gift of 1905 to mankind—fundamentally and irrevocably altered the conceptual framework within which man viewed the universe and provided a means of tackling problems for which no answers could be found within the straitjacket of classical Newtonian physics, Trotsky's theory of Permanent Revolution fundamentally shifted the analytical perspective from which revolutionary processes were viewed.

This formulation sees the theory of permanent revolution as a new analytical development within Marxism, much like the conceptual breakthrough that Einstein's theory represented compared to Newton. But that is not entirely the case. Rather, Trotsky's theory of permanent revolution represented a restoration of the theoretical depths of classical Marxism in opposition to the vulgarization of Marxism at the hands of the theoreticians of the Second International. To be sure, there is much that is new in Trotsky's theory, but the germs of Trotsky's theory of permanent revolution were already present in Marx's discussion of permanent revolution two generations earlier as well as in Marx's discussion of the nature of the Russian revolution shortly before his death in 1883. [24] The problem was that this work was "forgotten" when Trotsky was

[23] *Toward a reconsideration of Trotsky's legacy and his place in the history of the 20th century*, A lecture by David North, 29 June 2001,
http://www.wsws.org/articles/2001/jun2001/dn-j29.shtml

[24] I discussed Marx's rejection of the notion of a linear historical progression upon which the theorists of the Second International based their view that the Russian Revolution was fated to be a bourgeois revolution in my essay, *The Dialectical Path of Cognition and Revolutionizing Practice*,
http://permanent-revolution.org/polemics/dialectical_path.pdf p. 24-25. The following excerpt provides the essence of my argument,

> Plekhanov's notion of a linear progression of history, far from being based in the works of Marx, was squarely in opposition to Marx. It is well known that Plekhanov's close collaborator of many years, Vera Zasulitch, wrote a letter to Marx in 1881 asking him for his opinion about the fate of the Russian peasant commune. This was a critical issue for revolutionaries in Russia at the time as there was a fierce debate between the Narodniks who believed that a transition to socialism might be possible based on the ancient collectivist bonds of the peasant commune, and those who inspired by a superficial acquaintance with Marxism, thought that Russia would necessarily have to go through a stage of capitalist development before

developing his theory of permanent revolution. All the more impressive was his achievement. Einstein, on the other hand, was not dealing with a vulgarization of physics that he was restoring to an earlier more fruitful phase. He genuinely stood on the shoulders of the best physics of his day and completely revolutionized it. North and the Talbots fail to see this difference because they have become apologists for the vulgarized Marxism of the Second International.

The Talbots raise the issue of North's lecture for the sole purpose of comparing it favorably to my own lecture. They write,

> North's lecture and Symonds' article offer a far more profound analysis of what gave rise to the Theory of Relativity than Steiner can provide and they express a genuinely dialectical and materialist historical consciousness. They reveal the intellectual depth of the WSWS and the

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> socialism would be possible. Plekhanov and Zasulitch, were at that time leaders of a breakaway group from the Narodniks and were becoming interested in Marxism. Marx's reply showed that his Marxism was not constrained by linear schematic theories of inevitable developments.

If Marx's contribution to this discussion is taken seriously, then North's statement in his essay, which indicates that Plekhanov's formal schema was the "accepted wisdom" within Social Democracy, must be amended. Here is what North wrote,

> Russia, at the turn of the 20th century, Plekhanov maintained, still had before it the task of achieving its bourgeois democratic revolution—by which he meant the overthrow of the tsarist regime and the creation of the political and economic preconditions for a future, distant, social revolution. In all probability, Russia had before it many decades of bourgeois parliamentary development before its economic and social structure could sustain a socialist transformation. This organic conception of Russia's development constituted the accepted wisdom that prevailed among broad layers of the Russian social-democratic movement during the first years of the 20th century.

This linear and undialectical notion of the Russian Revolution may have been the "accepted wisdom" of the Russian social-democratic movement, but it was a step backwards from the wisdom of Marx, who, as a direct result of his study of Russian developments, saw fit to add the following to the Preface of the Second Russian edition of the Communist Manifesto,

> The Communist Manifesto set out to announce the inevitably approaching dissolution of modern bourgeois property. In Russia, however, we find that the fast-blossoming capitalist swindle and newly-developing bourgeois landed property stand face to face with peasant communal ownership of the greater part of the land. This poses the question: Can the Russian *obschchina*, a form, albeit heavily eroded, of the primitive communal ownership of the land, pass directly into the higher, communist form of communal ownership? Or must it first go through the same process of dissolution which marks the West's historical development?
>
> Today there is only one possible answer. If the Russian revolution becomes the signal for proletarian revolution in the West, so that the two complement each other, then Russia's peasant communal land-ownership may serve as the point of departure for a communist development.

North has never commented on this discussion and its implications for assessing the theoretical vitality of Plekhanov and the Second International in general. (Indeed he has never commented on anything of substance in my essay, *The Dialectical Path of Cognition*.) Assessed in the light of this evidence, it means that Trotsky's theory of permanent revolution was not such a completely new idea in the annals of Marxism as North claims, or to be more precise it was new only against the background of the vulgarization of Marxism that characterized much of the theoretical work of the Second International. That should not in any way detract from Trotsky's great theoretical achievement – which North correctly stresses in his essay - but it should forewarn us about the dangers of commenting on history selectively.

> seriousness with which its writers approach questions of science and culture in general. No writer for the WSWS would presume to base a theoretical article or lecture on one popular book.

To claim that North's lecture sheds much light on the theory of relativity is absurd. The lecture simply does not get into the theory of relativity at all, except to point out that it was not entirely coincidental that this analytical breakthrough in physics took place at the same time as Trotsky developed his theory of permanent revolution. This is indeed an intriguing insight, but it hardly constitutes a serious theoretical statement about Einstein's theory of relativity.

The unfavorable comparison of my lecture to North's and Symonds's [25] essays are simply another diversion. The Talbots are comparing apples to oranges. I did not set out to do what North or Symonds did and they did not try to do what I was doing. North's lecture touched on the sociological and economic background that gave rise to the theory of relativity but did not discuss the theory itself or the way any of the key concepts in physical science were transformed. And it had the problems that I noted. As for the article by Symonds, I would be the first to acknowledge that it was a very good review article about Einstein's discovery. However, to call it a "theoretical" article is a bit of a stretch. [26] There is certainly room for review articles that

[25] *One hundred years since Albert Einstein's annus mirabilis,* Peter Symonds, 11 July, 2005, http://www.wsws.org/articles/2005/jul2005/ein1-j11.shtml

[26] The dishonesty of comparing Symonds' essay to my lecture can be illustrated by noting that Symonds' essay is missing an examination of the 17th century Scientific Revolution which is dealt with in a few brief paragraphs and a quote from Engels. My lecture on the other hand, while dealing with the 17th century Scientific Revolution in much more detail, painted the story of the transition from Newton to Einstein with a much broader brush than Symonds'. That should not condemn either piece as the focus in each case was different. While Symonds' article does go into some of the scientific and historical issues in more detail than I did in my lecture, it is missing a serious philosophical engagement of this material. The only philosophical point the author raises is the challenge posed by Mach and later interpretations of quantum mechanics to a materialist world outlook. That is of course a legitimate and necessary task for a Marxist review to undertake, but it is hardly sufficient if the focus of the material is on theoretical issues. There have been many other forms of positivism and other challenges to materialism since Mach. In addition the challenges to Marxist materialism are not confined to restatements of philosophical idealism, but in the decades since Mach have more frequently taken the form of a repudiation of dialectics. The model for all such attacks on dialectics in the last few decades was the American radical and former supporter of Trotsky, Max Eastman. Eastman pioneered the use – or misuse – of "Science" as the arch-enemy of dialectics back in the 1920s. Eastman was decades ahead of such anti-dialecticians as Karl Popper who have had so much influence on contemporary philosophy of science. At the same time as Eastman was propagandizing against dialectics in the U.S., there was an intense philosophical struggle in the Soviet Union that involved both philosophers and scientists about the relationship of science to dialectics. That struggle came to end when the Stalinist bureaucracy consolidated its rule and began dictating to scientists what is permissible in the name of a fraudulent version of "dialectics". One of the victims of this Stalinist degeneration of Marxism was relativity theory. Einstein's theory of relativity was for a period denounced by the ideological watchdogs of Stalinism for its idealist and bourgeois deviations. The upshot of this dual attack on the Marxian dialectic as it pertains to science is that today few scientists know anything about dialectics or think that philosophical questions are of any relevance to scientific investigation. Any suggestion to the contrary conjures up visions of Stalinism or of Eastman or Popper's vision of dialectics as a form of mysticism and totalitarianism. Among those few scientists who have expressed an interest in dialectics understood in the broadest sense would be the late Stephen Jay Gould and Richard Lewontin in biology and almost alone in physics, Lee Smolin. (We should also mention that Einstein went against the grain of the great majority of his empiricist-minded contemporaries in considering that philosophical issues are of relevance to scientific investigation.) None of these issues are raised in Symonds' piece. His essay does at times flirt with some philosophical ideas, but they are not followed up. For instance, he rightly says that "Relativity theory represented a sharp and fundamental break with Newtonian mechanics, as well as its continuation." Apparently the author prefers not to use the philosophical term "aufheben" to illustrate this point as that word, suggesting as it does the strange universe of dialectics, is forbidden in the WSWS. (See North's various fulminations against this term in his polemics with myself and Frank Brenner.)

explain scientific ideas to laymen in a Marxist publication. However, in the context of a publication that has avoided dealing with philosophical issues for two decades, it is somewhat disingenuous to pass off such articles as "theoretical".

There is a burning need for Marxists to undertake a deep philosophical engagement with science. That is what I tried to do in my lecture. I saw a huge gap in the educational work of the International Committee and I hoped – not from my lecture alone or course, but by dint of my example - that other comrades would be impressed with the importance of turning to the question of dialectics in the discussion of the natural sciences. Rather than finding reasons to trash my lecture, I wonder why Chris Talbot, who has a background in science and mathematics, never gave a lecture himself on the subject of dialectics and science [27]

Denying the crisis of science

I come now to what is perhaps the most insidious falsification of my position by the Talbots. However, before examining the substance of their charge that I see a perpetual "crisis of science" it is worth examining their style. There is a noticeable elevation in their rhetoric in this section of their screed to the point where one begins to wonder what has overtaken them. Within the space of a few paragraphs I am accused of conducting an "incontinent rant", of going into a "frenzy", of having a "fit of apoplexy", of having "a hysterical response", of an "antipathy to science" and "contempt for the working class".

All these terms of abuse because I gave a lecture on dialectics and science with which the Talbots disagree?

What exercises the Talbots is their contention that I denigrate science – that I claim that science has been in a crisis for 300 years and will continue to be so until it returns to Aristotelian scholastic philosophy. Yes, they actually say that! Here is precisely what they say:

> Steiner is contending that science has been in a crisis for over 300 years. Galileo died in 1642. If Steiner is right in his identification of a crisis of science that began in the 17th century, then it would mean that all science from Galileo onwards is flawed and in some way suspect because it lacks the metaphysical foundations that Aristotle thought essential to it.
>
> Newton's work and that of the Enlightenment scientists would be part of the same crisis. More recent science would be equally dubious, since modern scientists have, according to Steiner, inherited from the Enlightenment the same unfortunate tendency to belittle Aristotle and lack any regard for his First Philosophy. If the crisis of science goes back to the Renaissance and continues unabated to this day, then it would mean that both Lenin's efforts to provide a philosophical

But it is an apt designation of something that represents simultaneously a break and a continuation. Again, such considerations are not meant to detract from Symonds' article. In its own terms it was a very good review of an important scientific topic while philosophical issues were a minor and secondary concern. But these considerations make evident the fraudulent nature of the Talbots' raising this article to serve as a model counter-example to my lecture.

[27] Chris Talbot finally did give a lecture in June 2009, some seven years after my lecture, on the subject of Marx and Darwin. http://www.wsws.org/articles/2009/jun2009/dar1-j17.shtml I will comment on this lecture in a subsequent chapter.

> resolution of the early 20th century crisis and Einstein's attempt to put physics on a new footing were equally fruitless.
>
> Steiner's "crisis of science" is a pretty thin disguise for an attack on science. He expresses intense dissatisfaction with modern science, which he condemns as "reductionist," "atomistic" and "empirical." For any one with even a passing acquaintance with the writings of the Frankfurt School, particularly Adorno, Horkheimer and Marcuse, it is clear that Steiner's complaints against science are of a piece with the familiar anti-Enlightenment, anti-scientific litany of that school, which has become well established in the postmodern schools of thought that have developed among the radical intelligentsia hostile to Marxism. Steiner's claim that there is a "crisis of science" because it has strayed from its original objectives is, in reality, an attack on objective scientific thought.

The reader who has followed this saga thus far should not be surprised to learn that the Talbots will use any opportunity to distort my words whenever they can. Here is a perfect example. First they take my remarks from the beginning of my lecture about the crisis of science, remarks of a very general sort where I try to give the audience a first approximation of the subject matter of the talk, and they conflate those general statements with my more specific remarks about the history of science once I get into the material. Summarizing my remarks I made the point that what we call science today was originally closely intertwined with philosophy when the first efforts to understand the natural world were made in Ancient Greece. I said that,

> In its beginnings, there was no sharp distinction between science and philosophy. In fact, the sharp distinction between the two that we now take for granted was only introduced in the last two or three centuries. Up to the renaissance the close and unbreakable relationship between science and philosophy was a given. Remnants of this close connection between philosophy and science survive up to the present day. It is why in many European universities for instance, the department of physics is called the department of natural philosophy. We begin to get a glimpse of the nature of the crisis once we realized how strange that formulation, "natural philosophy", sounds to us today.

Being that Aristotle practically invented most of the disciplines that even today we recognize as branches of the natural sciences, I discussed how Aristotle organized the sciences as a hierarchy of the investigation of the natural world and that what he called First Philosophy was conceived as the most general of all the sciences. As is well known, Aristotle's philosophy was dogmatically rigidified by the Church during the Middle Ages and became an encumbrance on the development of science. The Scientific Revolution that emerges in the 17th century is therefore among other things a break from this Church dogma and the Aristotelian philosophy with which it was closely identified. Of course the emergence of the new science of motion was a great advance in human history, but it also brought about what I considered an unfortunate side effect. That is to say, the Aristotelian philosophy that it overturned was replaced by an empiricist philosophy that was no longer conscious of its philosophical foundations. Through a long period of gestation, this eventually resulted in the dominant attitude that we find among scientists today, that philosophy is something that is at best irrelevant to their venture and at worst an encumbrance. I consider this attitude to be at the heart of the contemporary crisis of science. Progress in many areas of fundamental physics is today encumbered by the conceptual restrictions imposed by an empiricist and pragmatic mindset. In making this judgment, I am in

good company. A leading theoretical physicist, Lee Smolin, who ironically was recommended to me by Chris Talbot, recently wrote the following about the state of contemporary physics:

> How is it possible that string theory, which has been pursued by more than a thousand of the brightest and best-educated scientists, working in the best conditions, is in danger of failing? … What I believe is failing is not so much a particular theory but a style of doing science that was well-suited to the problems we faced in the middle part of the twentieth century but is ill suited to the kinds fundamental problems we face now…This style is pragmatic and hard-nosed and favors virtuosity in calculating over reflection on hard conceptual problems. This is profoundly different from the way that Albert Einstein, Niels Bohr, Werner Heisenberg, Erwin Schrodinger, and the other early-twentieth-century revolutionaries did science. Their work arose from deep thought on the most basic questions surrounding space, time and matter, and they saw what they did as part of a broader philosophical tradition in which they were at home.
>
> [T]he lesson of the last thirty years is that the problems we're up against today cannot be solved by this pragmatic way of doing science. To continue the progress of science, we have to again confront deep questions about space and time, and quantum theory and cosmology. We again need the kinds of people who can invent new solutions to long-standing foundational problems. As we shall see, the direction in which progress is being made – which are taking theory back into contact with experiment – are led by people who have an easier time inventing new ideas than following popular trends and for the most part do science in the reflective and foundational style of the early-twentieth-century pioneers. [28]

I tried to demonstrate that the problems modern physics are faced with today can only be overcome if working scientists begin to rethink their disdainful attitude toward philosophy. Of course I was not advocating literally a return to Aristotle's notion of First Philosophy. Anyone with an elementary school education who heard my lecture would understand that I was using Aristotle's idea of First Philosophy in a very generic sense to illustrate that the conceptual apparatus of the sciences had become too narrowly encumbered as a result of its unconscious acceptance of an empiricist metaphysics and that this crisis could only be resolved by a conscious turn toward philosophy on the part of scientists. What was left unsaid until the very end of my lecture was my belief that a turn specifically toward a dialectical philosophy of nature, as pioneered by Engels, would prove most salutary for scientists who are currently involved in the investigation of fundamental questions about the nature of the universe.

To someone who has never considered philosophy of any use, such sentiments must have been perceived as a grave threat. Perhaps this accounts for the intense vitriol that characterizes the Talbots' piece. But whatever the reason, the Talbots deliberately distort my remarks to claim that I am advocating a literal return to Aristotelian philosophy and science.

The Talbots also have a wooden ear for the multivalent concepts incorporated in the term "crisis of science" in my talk. I began my talk by discussing the "crisis of science" in the most general terms as the kind of turn away from fundamental philosophical issues that Lee Smolin nicely identified. But as I got into the material, I also made use of the expression to discuss the phenomenon in the history of science whereby a leading paradigm comes up against its limits as a result of new discoveries that cannot be made to fit into the paradigm. The Scientific Revolution of the 17th century was an example of the first "crisis of science" in this sense. The

[28] Lee Smolin, *The Trouble With Physics*, (First Mariner Books, 2007), p. xxii-xxiii.

discoveries of Copernicus, Kepler, and Galileo challenged the well-ordered view of the cosmos of the Aristotelian-Ptolemaic outlook. That challenge precipitates a crisis on many levels – on the level of fundamental theories about the natural world of course, but also on a philosophical and cultural level. When Newton comes along and is able to unify all the known laws of physics into one coherent system of universal gravitation, that crisis has been resolved. The old paradigm finally gives way to a new one. In this case the paradigm of the geocentric and well-ordered cosmos of Ptolemy and Aristotle is replaced by the mechanical universe of Newton. The new science of mechanics throws off the slough of the Aristotelian philosophy with which it was at odds and finds its adequate expression in the new mechanical philosophy as articulated by Descartes and others.

There have been in the history of science since the 17th century at least two other major "crises of science" and doubtless several lesser ones. The period when the Newtonian view of the universe began to be questioned from the latter half of the 19th century represented the second great crisis of science. That crisis lasted until the discovery of Einstein's theory of relativity and quantum mechanics in the early twentieth century replaced the Newtonian model and resolved it. Lastly, we are today in a new crisis of science as there are now many unanswered questions that cannot be accounted for by Einstein's theory of relativity or quantum mechanics. The contemporary crisis of science is in fact the topic of Lee Smolin's most recent book, *The Trouble With Physics*.

In their polemic, the Talbots collapse my description of the general "crisis of science" with these specific events in the history of science. That is the only way they can claim that I think science has been "flawed" since 1642 when Galileo died. The Talbots' are also clueless as to what is meant by a "crisis of science". It does not mean, as they assert, that science is "in some way suspect". On the contrary, the "crisis of science" is a symptom of the vitality of science not of its decay. Science can only be in a "crisis", when, as a result of the progress of scientific knowledge, new discoveries have pushed it beyond the limitations of its old paradigms. Until a new paradigm is developed that can encompass and explain the new discoveries, the crisis of science continues. Once the new paradigm is adopted, the crisis is resolved. At least for a time, until the next wave of discoveries begin to challenge the new paradigm. As scientific discovery is an infinite process, we can expect there to be further crises of science in the future as well as their resolution.

I mentioned in my lecture that Newton resolved the crisis of science that began in the 16th century with Copernicus. I said,

> The geocentric view of the universe, so comforting to many, was rudely displaced by the heliocentric outlook of Copernicus. The spirit of self-confidence has been evicted from its natural home and thrust into an infinite and empty void. Donne's literary expressions not coincidentally hearken back to the religious sentiment of a primal loss of innocence and subsequent banishment from the Garden of Eden. What this points to is an incongruity between the new developments of science that look to the future to philosophical and cultural attitudes inherited from the past. To put this in other words, the philosophy lagged behind the new science and was not catch up to it until the Enlightenment of the 18th century when a new spirit of optimism begins to emerge based on a firm conviction in the order and design of the universe, as codified in Newton laws of motion. Following the triumph of Newton, science and philosophy exhibit a renewed confidence in man's ability to discover the design of the universe and in this sense the crisis of 17th century science was

> for the moment resolved. We will see shortly that the crisis of contemporary science is in many ways analogous to this crisis of the 17th century. [29]

This excerpt from my lecture, where I noted that Newton resolved the crisis of science launched by Copernicus, puts the lie to the Talbots' claim that I bear an antipathy to science and think that it has not made any fundamental progress in 300 years. In my correspondence with Chris Talbot I further elaborated this view of the history of science. I noted that it was none other than Friedrich Engels who identified another crisis of science, the one that begins in the 18th century when new discoveries as to the origin of the solar system, the origin of the earth and the origin of man first question and finally resolve the unhistorical dimension of mechanical philosophy and science that was bequeathed to us by the Newtonian world view. I wrote,

> He [Engels] 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

[29] Alex Steiner, Dialectics and the crisis of science, http://permanent-revolution.org/polemics/dialectics_science.pdf p. 7.

> 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.) [30]

I will explore in a subsequent section the theoretical implications behind the Talbots' denial that there exists any crisis of science. But before ending these remarks, it is important to note that the Talbots segue into a discussion of my antipathy to science based on their mischaracterization of my remarks on the "crisis of science". And it is in their remarks in this section that they literally lose all sense of rationality. I am suddenly denounced for all manner of crimes. For instance, they quote my remarks about the way in which popular culture inculcates a reductionist view of human nature,

> 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 socio-biology—contend (sic) 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. [31]

To which the Talbots reply,

> This incontinent [!] rant against science and the mass of the population who expect to receive medical treatment for their ill-health and to use new technology in their daily lives, and who like to watch nature documentaries on television, reveals both Steiner's antipathy to science and his contempt for the working class.

Every word of this is nonsense. All I did was reiterate a very common truism, or at least a truism that should be common among socialists. It is that our popular culture encourages the most degrading views of human nature. This has been the case for a long time, ever since the days of Herbert Spencer and social Darwinism when the popular depiction of evolution saw Nature as "Raw in Tooth and Claw". The 21st century version of this degrading view of human nature is a popularized and vulgarized version of the theories of the sociobiologists. Why such a statement should be controversial among self-professed Marxists is hard to fathom. I also find it rather puzzling why any self-professed Marxist should recoil in horror at my statement about the mass media pushing quick technological fixes on the public. Have we not seen millions of people pacified by tranquilizers and anti-psychotic drugs and warehoused in inhuman institutions as a

[30] Talbot – Steiner exchange http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #5.
[31] Talbot – Steiner exchange http://permanent-revolution.org/polemics/talbot_steiner_exchange.pdf Steiner reply #22.

cheap alternative to providing them with genuine care? The most widely prescribed drug in America is the anti-depressant Prozac. By the Talbots' standards, this should be seen, not as a stark indicator of social alienation under capitalism, but rather as a triumph of biochemistry! That is the asinine way that the Talbots 'defend' science.

To be continued

Link to Chapter 1

Link to Chapter 2

Link to Chapter 3