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It summarises the debate to this point on the temporal and simultaneous approaches to value and on the alleged inconsistencies in Marx's approach.

VALUE AND MARX: WHY IT MATTERS

By Alan Freeman

Five years ago a book was published, edited by myself and Guglielmo Carchedi. In this book I explained what I thought the 'Naïve reader' understood by Marx. 'To such a reader,' I wrote, 'perhaps idealistic, discontent with oppression or injustice, wanting to change the world and desiring for this reason to understand how it works,'

Marx says, in summary: there are people who own property for its own sake, and people who do not. The latter create wealth, without which the former would not exist. The wealthy maintain this injustice with oppression, deceit, corruption and force. They fight over the spoils, visiting on the world its ills and suffering. And the object of their desire periodically escapes control, wreaking havoc on guilty and innocent with tragic or comic indifference. However, the process gives those who create wealth, if they consciously organise to do so, the opportunity to overturn this order and found a better one.

The conventional view of Marx's economics, including that of the great bulk of Marxist economists, is that such a naïve view cannot be true. The writers in our book showed that it can be. We made a straightforward declaration. "Marx's own supporters have announced the failure of his project," we wrote, "the premise of *Capital* itself: 'to reveal the economic law of motion of modern society'".

This has had an incalculable impact on the perception of Marx by the nonspecialist, the militant, the partisan and the merely honest disinterested observer of his work. The received view among intellectuals is that whatever Marx's towering political and social insights, his economics is wrong. The contributions to this book demonstrate these charges to be manifestly and profoundly false. Not only are the accusations of inconsistency unfounded, but it is not necessary to 'revise' or 'correct' Marx to show this...In this respect it differs from all other attempts to defend Marx's theory from the critics by modifying or 'correcting' this theory. None of the contributors claim Marx is immune from error or that further development of his thinking can be avoided; nevertheless he did not make the mistakes he has been accused of.

The decisive weakness of the recent discussion on value, as so far conducted in the Italian-language journals, is that in failing to refer to this debate, with honourable exceptions, it has failed to present the case for Marx. It is living in the past; it is rehearsing and rehashing a debate that is twenty years old, without recognising the advances that have appeared in those twenty years and which radically overturn the received ideas which Sraffa, Coletti and Napoleoni took for granted.

Our case is straightforward: what modern research has shown is that Marx's theory is not wrong. There is no logical inconsistency. His account of transformation is completely coherent within itself, his law of the tendency of the rate of profit to fall is, in terms of the concept of value that we have shown he possesses, without logical error. The so-called 'errors' in Marx arise not from his own theory, but from a specific and erroneous interpretation of it that originates with von Bortkiewicz, was introduced to the Western world by Sweezy, made mathematically rigorous by Seton, Morishima, and finally Sraffa. This theory suffers from one fatal flaw: it is not Marx's.

Of course, like any scholarly case, we do not assert this without proof. The articles that appeared in our book, and the articles that we are submitting for the debate in Proteo, will present our case, already accessible in English in a growing number of publications. In the writings of pioneers of this view such as Paolo Giussani and Guglielmo Carchedi himself, it is already available in Italian though it has been largely (and scandalously) ignored by all too many contributors to the present discussion.

We do not ask that the present participants in the debate accept our argument, which is of course highly controversial, without argument or discussion. We do ask they recognise that this view exists. To the extent that they do not, we would argue that the outcome of the discussion cannot be treated as scientific and has failed to present the case in support of Marx adequately to the Italian public.

The purpose of this short introduction is to try and explain, as I see it, why this matters.

First of all, what is the substance of the matter? Cavallaro, in my view, correctly identifies this. 'in terzo luogo', he writes of the traditional view of Marx's value concept, 'scontando la diversità di composizione organica del capitale nei diversi settori della produzione, si deve determinare il saggio di profitto

come rapporto tra il plusvalore totale e la somma di capital costante e capitale variabile, e, una volta dato quest' ultimo, provvedere a rettificare i prezzi dell'*output*...agli *input* si debbono applicare gli stessi prezzi dell'*output*; prezzi relativi e saggio di profitto vengono ora determinati simultaneamente a la Sraffa.'

The difficulty is very simple: Marx never determined prices or values in this way and nor is it conceivable that he could have done. The supposition that input and output prices had to be equal (otherwise known as, and mathematically identical to, the assumption of economic equilibrium) was wholly imposed by later writers. Bortkiewicz himself, who introduced this assertion, did not attribute it to Marx, presenting it instead as a necessary correction to Marx in order to bring him in line with the views of Walras, the founder of modern neoclassical economics. As Gattei (1982) testifies, Bortkiewicz's first letter to Walras on 9 November 1887 ends with the following words: 'Your writings, sir, have awakened in me a lively interest in the application of mathematics to political economy, and has pointed out to me the road to travel in my researches into the methodology of economic science.' This letter is reproduced in Jaffé (1965 Vol II p230). Of Marx, Bortkiewicz went on to write:

Alfred Marshall said once of Ricardo: 'He does not state clearly, and in some cases he perhaps did not fully and clearly perceive how, in the problem of normal value, the various elements govern one another *mutually*, not *successively*, in a long chain of causation'. This description applies even more to Marx ... [who] held firmly to the view that the elements concerned must be regarded as a kind of causal chain, in which each link is determined, in its composition and its magnitude, only by the preceding links ... Modern economics is beginning to free itself gradually from the successivist prejudice, the chief merit being due to the mathematical school led by Léon Walras.

Bortkiewicz was clear on a matter which subsequent thinking has obscured: his point was not to interpret Marx's ideas but to change them. His intention was to replace Marx's own, non-equilibrium concept with a Walrasian equilibrium concept.

However the idea that has conquered the modern literature, on the inception of Paul Sweezy who declared Marx to be a writer in the framework of General Equilibrium, is that this equilibrium concept of value is Marx's *own*; and this is the origin of all the confusions surrounding his alleged errors.

It is our case that all such errors, and all such inconsistencies, arise not from Marx but from attempting to interpret Marx as an equilibrium economist. The Gordian knot has to be cut; we have to cease trying to understand Marx – the most ardent chronicler of capitalism's inherent *failure* to ever attain equilibrium – as a theorist that began by supposing the opposite of this obvious state of affairs.

Once this is done, the inconsistencies vanish and a new and utterly different enquiry must begin, namely, instead of what is *wrong* with Marx, we can begin at last to enquire what is *right* about Marx. For, of course, it by no means proves, simply because Marx was logically coherent, that what he wrote was *empirically true*. It is the job of scientific investigation, matching theory to fact and evidence, to find this out. The point is that if this is allowed to happen, Marx could no longer be *excluded* from the investigation; the 'prehistory' of Marxist economics could come to an end and he could be accepted as a legitimate theorist whose ideas constitute a perfectly valid alternative to the dogmatic and fundamentalist ideas that constitute today's orthodoxy.

Yet this does not happen. Even the Marxists, debating earnestly what they can salvage from the received academic view of Marx's 'errors', leave out of their account the very arguments and evidence that would at least lead us to consider the possibility that he in fact committed no such errors. Why? This is what I will devote the rest of this short introduction to.

I was recently invited to Rome by a department of La Sapienza that concerns itself with economic statistics, a topic which has always been at the centre of my preoccupations and for which I am now responsible to the government of London although of course, as with my main presentation, the views I express here are my own and neither the GLA or any part of it has any responsibility for them.

I concentrate on a point which good statisticians take very seriously: the importance of analytical concepts. I will then relate this to the role, in economic thinking, of the assumption of equilibrium.

To take first the question of concepts. Carchedi has stated elsewhere that the most important, but missing, requirement in politico-economic analysis is the conceptual framework that is used to approach it. This is not an uncontroversial idea, since positive economics makes the standard assumption that its conceptual framework is 'given'; one does not find in its literature any real notion that this framework needs to be interrogated critically, once it has been stated. It is simply the assumed common discourse of economic science.

It is not widely understood or recognised that a change in analytical framework leads to a change in conclusions. Above all, I want to argue, it leads to a change in the causal explanations we offer for what we observe. To put it another way, if one adopts a different set of concepts, one has a different theory.

Let us take the simplest possible economic concept, that of output. Has the output of, say, Turkey grown in the last ten years? And how has this growth compared with that of the USA? If one measures it in money, indubitably it has grown faster. In 1991 it was 638 billion billion Turkish Lire and in 1999, 838 thousand billion billion, a growth of 12900%. If one measures it in dollars, however, it has grown from \$125

billion to \$153 billion, a growth of 22%. Thus we have a simple proof that nominal output is an inadequate concept of output, because it is not well-defined; it depends on the currency. This proof arises without any need for conceptual reflection about the nature of inflation, from the very presentation of the data themselves, by the statistics themselves as we receive them from the statisticians.

So it appears that, behind the many different 'nominal' measurements of output, must lie something more definitive, more stable. Economists therefore have developed the notion of 'real' output, accepting thereby the sometimes heretical idea that essence is different from appearance. 'Real' output is an attempt to express the idea that behind price lies something else independent of price, and that we may conceive of this as a certain *quantity* of output, as a physical magnitude.

However this is itself equally fraught and again, the demonstration may be made without resort to conceptual reflection, from the data themselves. If for example one measures the output of Turkey in 'real' dollars one finds that it has actually expanded by 2.3% over the past ten years. But if one measures it in 'real' Lire it has grown by 31%. And there is a strong case for measuring Turkey's output in real Euros, which would lead to yet another figure. So again, which is the 'really' real measure of output?

When I raise these problems with my fellow economists, a common reaction is to treat it as a problem of measurement. There is, it is assumed, such a thing as a single and coherent concept of 'output' and the only difficulty is to get a good estimate of it.

This does not stand up to examination. The price of a pizza is not just a different way of measuring its size; it expresses a different property of the pizza. Equally, the 'real' dollar value of Turkish output expresses something different from the 'real' Lire value, it expresses in a certain sense the purchasing power of Turkish output on the world market, as opposed to the domestic market. These are not different measures of the same concept, they are a single measure of two different concepts, and both in turn differ from yet a third concept, the nominal price of this output. Yet economic theory is happy to proceed as if there was one, and only one thing, 'real output' which can, against all the statistical evidence, be uniquely quantified so that economic laws of motion may be uniquely expressed in terms of it.

Moreover this is not a purely quantitative issue; it has qualitative consequences. If we are asked the question 'has Turkey grown faster than the USA in the last ten years?' we will answer 'yes' if we employ one concept of output, and 'no' if we employ another.

Most significant of all, it leads to different causal explanations, that is, different theories. If one wishes to explain why or whether the economy of Turkey has grown, a causal connection between growth and investment is a reasonable thing to look for. But in that case, in what terms should this causal connection be expressed? Do we seek to explain Turkey's high growth rate in real Lire, or its lower growth rate in real dollars? And what do we mean by 'investment'? Do we mean dollar investment, real dollar investment, real Lire investment, investment at historical cost, current cost? What *is* the capital stock of Turkey, in comparison with the capital stock of the USA? Statisticians argue about this day in and day out; economists formulate supposedly rigorous theorems in which the problem is treated as if it did not exist.

Standard economic theory argues that capital is one of the two central factors of production. Yet, when we examine this simple idea, which is daily incorporated in hundreds of econometric models and lies at the heart of modern growth theory, it leads to conclusions which, when examined more closely, depend critically on the

way that the data entering these models is conceived. The very concept of 'capital' itself is a great deal more fraught than appears at first sight.

Moreover, most of these same econometric models incorporate a theoretical construction known as the production function. In the production function, besides capital as a factor of production we find labour. Labour and capital are presumed to substitute for each other. But if they substitute for each other, they have something in common, and this something must be quantifiable. It is an obvious step, and is indeed an objective of the economists in measuring such ideas as 'multifactor productivity' to try and express both inputs in the same units, even if only to get an idea of their relative impact.

We have seen that there are great difficulties in expressing the notion of capital uniquely in terms of its 'real size'. These problems get bigger, not smaller, if we try to measure labour in the same framework, in terms of the cost of purchasing it.

But labour has a measure of its own, which is not subject to the same difficulties as capital: time. Time is a universal, perfectly quantifiable feature of every production process with insignificant (relativistic) differences between one person's time and another's. Nothing could be closer than the Ricardian ideal of an invariant measure. Why, then, not express capital in terms of the natural measure of labour? Even in terms of neoclassical theory, this seems an obvious line of enquiry.

A discipline that refuses to investigate a theoretical possibility surely cannot be considered scientific, since it has left out of the account a possible explanation and has not conducted itself as a science should, by testing all possible explanations against observation. A failure to investigate a serious theoretical possibility would be a serious dent in the claim of economics to be a science at all, on which its very claim to be 'positive' must surely rest.

Nevertheless, neoclassical economics does reject this line of enquiry, to such an extraordinary extent that with only marginal exceptions it refuses even to teach it, publish it, to provide students of the subject with access to it, and on many occasions, to provide employment to those who pursue it. Such systematic exclusion, which amounts in total to a form of censorship, rivals in a certain sense the level of exclusion which the Church devoted to the Copernican heresy.

On what grounds does economic theory draw back from an obvious line of enquiry? When we investigate the point, we find two arguments are given. The first line of defence is often to assert that the measurement of output in terms of labour time is out of date or discredited.

But what does the date of a theory have to do with its truth? Galileo's theory of the universe was invented in BC 250 by Aristarchus of Samos, and was referred to in Copernicus' day as the 'Greek Copernicus'. Was his theory wrong because it was 1800 years old? In the theory of light, atomistic and wave theories alternate with some regularity and modern theory finds that light must be conceived of as a combination of both. It would have been a rash physicist indeed, on the turn of this century and on the eve of modern quantum theory, to abandon the 200-year-old particle theory as 'outdated'.

If economics had, in its modern state, arrived at a condition where it adequately explained all the phenomena we now see there might be some justification for dismissing theories on grounds of age. But as is widely acknowledged, and accepted by the economists themselves, it neither explains nor predicts the most elementary events, such as the current recession. Economists, it is said jokingly, have 20/20

hindsight; they foresee the past perfectly. But the most sensible experts will not even venture to guess how deep or long the present recession will be, and most of those that did so have already been proved wrong.

And indeed finally, the very same economists which discount labour-time theories on grounds of age have no problems with even older but empirically much more problematic theories such as comparative advantage, or the hidden hand.

We turn now to the second argument, that to conceive of output and capital in terms of labour time has been discredited. As we have seen, this argument is itself logically erroneous, because it depends on the idea that to make such a measurement one must use the equilibrium approach of Sraffa. But as the articles that will appear in this journal will show, and as many others already published also establish, if one makes the measurement using the non-equilibrium approach of Marx, one arrives at completely coherent results.

So what has actually been established by all this research, then? In fact, the following proposition: that *if* one defines the value of a product by presupposing that its value does not change during the course of production, *then* one encounters insoluble contradictions. Moreover, one finds that the magnitude of output, so defined, is identical, except for a numéraire (a universal coefficient) to a magnitude given entirely by the physical consumption and production of outputs. On this basis, it is argued that the measurement of output in terms of labour time is discredited and redundant.

Well and good. A scientist would conclude as follows: *either*

(1) it is not possible to measure output in terms of labour time (or, if one does so, it is merely a redundant reformulation of output in terms of use-value, that is, physical or 'real' product;

or

(2) it is not possible adequately to conceptualise the notion of determining output by labour time by writing down a set of simultaneous equations that presuppose the economy reproduces itself perfectly, and that prices and values remain constant during production.

Prima facie the first idea lacks plausibility. After all, we all know that an hour of labour time produces greatly more, or greatly less, depending on the technology used. It would be quite odd, then, if it turned out that the number of labour hours in a thing was always proportional to the size of the thing. A statistician encountering such a result would be to go back and check his facts, because the facts themselves show that the theory cannot be true. *Prima facie*, the most obvious conclusion is that this method of determining output by labour time is a mistaken method, that it does not do what it claims to do.

A growing body of research, largely ignored in the present Italian, debate, has proven this point, and has instead investigated the second, neglected line of enquiry, which leads to a different and coherent determination of the magnitude of output by labour time, employing what is now known as the Temporal Single System Interpretation (TSS, or TSSI). Although there are many grounds for caution, statistical work is beginning to suggest that this determination might offer, or confirm, very different and neglected causal explanations for some of the most important observable phenomena of modern economics.

Most notably, and I will conclude on this point, it suggests that the prolonged phases of world decline in the rate of output growth (however measured), such as the one

through which we are now living, may be explained as a consequence of the process of growth itself, as a limit which accumulation places upon itself. It suggests that crisis, and the failure of the market, is not a result of external interference with the market, or poor regulation of the market, but of the operation of the market.

In my view, the very fact that this line has been rejected and indeed suppressed, as it has done, is the clearest historical evidence that economics is not a science. This conduct is not true to the idea of science, to the idea of the free clash of opposing explanations of observed reality, and moreover, it does not correspond to what the other sciences, no matter how imperfect, actually do in practice.

The answer, in my view, must lie in the actual mechanisms by which this profession is organised and funded. Economics is closer to the making of policy than any other social science, and closer also to the operation of levers which trip mechanisms which set the world market along this or that track; not least the IMF, the WTO, the treasuries of the great powers, and so on.

Gramsci once said that progress arises from an alliance between those who think because they suffer and those who suffer because they think; what is unfortunate is that too many of those that are paid to think, end up trying to prove that no-one else is entitled to do so. In my view, this is the function of the equilibrium paradigm; it is to convince those that think because they suffer that there is no possibility of ending their suffering. This is because, if one adopts the equilibrium paradigm, the very possibility that our circumstances might change has been expunged from the way one is permitted to think.

One of the great deceptions that arises from the equilibrium paradigm, the equilibrium theoretical and conceptual framework, is this: *non c'è nulla da far*. The great 'globalisation' machine is the result of automatic and instoppable mechanisms, a part of the natural order of things every bit as unassailable, and unaccessible, as the great Divine Order of the mediaeval universe which Galileo and Copernicus brought down to earth where ordinary mortals could become part of it.

The real world, and the real market, as Professor Mazzetti has pointed out elsewhere, is however not in equilibrium does not reproduce itself perfectly, is constantly changing its prices, constantly failing to realise its output. The *possibility* of crisis is always immanent in such a system. Once one theorises the underlying key variables of this system (output, investment, capital) in terms of labour time, one finds an explanation for the fact that this possibility is not merely latent, but actually erupts into the world in periodic recessions, long phases of decline with great political turbulence and high unemployment, and not least, the steady secular polarisation of the world into a small group of rich nations and a much larger group of poor nations.

Equilibrium theory purges this possibility of crisis from the theory. The decisive reason, in my view, why equilibrium theory is in almost every branch of economic theory preferred over non-equilibrium theory, is that in an equilibrium framework, it is actually impossible to theorise crisis. Instead, crisis always becomes the result of exogenous factors; of bad government, bad monetary policy, technology policy, system of regulation, trade unions, communists, terrorists, oil sheikhs – anything, in fact, except the system itself.

Eppur si muove The system does in fact produce crisis. We are now living through what I think is capitalism's 28th periodic recession and its fourth great prolonged wave of declining accumulation. Such events have recurred with the regularity of comets, under every conceivable combination of monetary policy, regime of regulation, political governance. To attribute such regular events, whose form is more

or less repeated in each distinct case, to ephemeral or transitory historical causes, seems to me utterly unscientific. Of course such external causes interact with, and deeply impact on the course of, these crisis but I think we must at least consider the possibility that their ultimate determinant is the market itself, and this is the idea which is intolerable and unacceptable to those whose power and wealth derives from this market.

Why is it unacceptable? Because, once it is clear that the system produces its own crises, the entire perspective changes. What actually happens is this: the market system, and above all the market in capital, sets its own limits on itself. The issue is conceived upside-down by even the most trenchant opponents of globalisation, because actually, they accept the theoretical view that globalisation is an automatic and natural process, and limit their objectives (literally in the case of the Tobin tax) to 'throwing a bit of sand in the works'. I have nothing against throwing sand in the works if it improves the human condition, but the problem is in my view actually much more serious, because the entire vehicle periodically goes off the tracks with or without the sand. In this case, the problem is entirely different; it is to escape with the minimum loss of life. There is no requirement to stop it or to advance it; this debate is a false one. The problem is what to do about the frightening results which arise when it stops of its own accord.

What is to be done? It is precisely at moments of failure that human consciousness becomes a factor. In a fast car on a straight motorway, the driver need attend only to the accelerator and on American cars, even this can be placed on automatic. But once the car begins to veer, the driver has to steer. Then even small actions count, and what becomes important is not how big you are, but how much you know. The architects of globalisation perforce use a theory that obscures what is going on. The victims of globalisation need a theory that clarifies what is going on; that is what the new research offers.

Alan Freeman