CRISIS, MARXISM, AND ECONOMIC LAWS: A RESPONSE TO GARY MONGIOVI

Alan Freeman

Gary Mongiovi accurately cites my thesis, presented in Freeman (2010a) (henceforth referred to as *Positivist Marxism* and abbreviated to *PM*), that "capitalism's inner laws express themselves in … different ways during booms and during crises" and that "When the business cycle is in an upswing … the tensions and contradictions that will eventually interrupt the process of capital accumulation are camouflaged by the commodity form, and so appear as natural laws of motion." He also expresses general agreement with this thesis, but complains that the way I present it is "marred by a gratuitous methodological argument" which is "misleading and therefore stand[s] in the way of productive discussion." The methodological argument in question is that no theory based on the method of static equilibrium can provide an explanation of endogenous capitalist crisis; this is the main issue I address in this response. *PM* applies this argument in order to examine a theoretical current that dominates Western, academic, Marxism. This current interprets Marx's theory as a variant of general equilibrium. In this response I refer to it as simultaneist Marxism (SM).

The structure of my response is as follows. I first show that *PM's* methodological argument explains why contemporary Marxists overwhelmingly reject the very thesis with which Mongiovi agrees. This methodological argument is thus not gratuitous. I then show that Mongiovi fails to refute it

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and indeed, concedes it. It is thus valid. I show that modern Marxism's persistent opposition to the thesis in question has severely damaged its ability to understand, or react to, the current recession. The methodological argument in question thus has material consequences.

I next consider the place of SM within Marxist theory in general and respond to Mongiovi's request to "dislodge the debate [between SM and the alternative TSS interpretation] from the impasse in which it currently stands." The principal roadblock, I argue, is that the "equilibrium models" for which Mongiovi requests acceptance are disguised definitions. This causes the practitioners informed by these models to suppress, unscientifically, all other definitions of economic magnitudes – and with them, Marx's own definitions – as illegitimate and not fit to be included in the normal scholarly process. SM can overcome this problem at any time by two measures which, I demonstrate, it has yet to take: by representing opposing paradigms and interpretations accurately, and by recognizing their legitimacy.

This concludes the summary of my case. To present it, I extend Mongiovi's summary by emphasizing that *PM*'s primary purpose was an attempt to understand the thinking behind Marx's phrase "law of motion" of capitalism. *PM* argued that Marx's theory of value and of history offers us a unified concept of this idea, in which the blind, endogenous operations of capitalism dominate during the descent into crisis, but recoveries – long booms – are contingent on conscious, human, political intervention.

Most languages, including German in which Marx formulated the concept of "law of motion", use the word "law" to denote both external constraints over which humans have no control – such as the "law of gravity" – and outcomes of collective human will, as in "laws of the land". This ambiguity is not merely semantic. The very notion of a "social law of motion" implies that human agency has brought this law into existence. The central question *PM* sought to answer was why Marxism itself, for most of its history, has resisted understanding the term "law of motion" consistently with Marx's conception of necessity, determination, and freedom, interpreting it instead as a mechanical and inevitable process over which humans have no collective control.

Thus *PM*'s main aim was not to repeat a theory of crisis that I have presented elsewhere (Freeman, 2003), but to explain Marxism's resistance to it. *PM* argued that this resistance originally arose from the impact of French positivism within the labor movement. In our time, however, *PM* argued that this resistance has taken the form of a theoretical obstacle: the misinterpretation by SM of Marx's theory of value. To establish the damage this has done, I showed that the possibility of endogenous mechanisms of crisis – blind, apparently natural laws that govern capitalism's descent from booms – cannot be deduced from SM's premises. SM's misinterpretation of Marx has thus blinded Marxism to the principal mechanisms of the present crisis. Mongiovi's main charge thus falls, because *PM*'s main conclusions follow from its methodological argument. He is entitled to claim my case is wrong, but has not established it is gratuitous.

Does he prove my case is wrong? I now turn to the arguments he uses. He states that my "criticisms are grounded in needless misunderstandings" of the equilibrium paradigm. However he may himself have misunderstood *PM*'s purpose. *PM*'s criticisms of the equilibrium paradigm are confined to five pages and its conclusions regarding them rest on one point only: that it is not possible, within the equilibrium paradigm, to deduce an endogenous mechanism of crisis. Mongiovi does not actually refute this critical argument. Indeed, he rather concedes my case. He cites my assertion that within an equilibrium model,

> it is impossible to formulate the idea that capitalism generates contradictions from within itself; that the rate of profit may fall, that the market may increase inequality between nations, that capitalism may fail to generate sufficient demand to call forth the continuation of accumulation, or even that circulation may be interrupted by hoarding money. (pp. 216–217)

He then responds: "The long-period framework with which [the equilibrium model] is concerned is not geared to address the issues mentioned by Freeman in the quoted passage." Misunderstandings are always possible, but this appears to me to state fairly clearly that the long-period framework – which includes SM – *cannot* address these issues.

Mongiovi goes on to make a different claim which I do not dispute:

> The Okishio Theorem does not claim that the profit rate cannot fall; it claims that if the profit rate does exhibit a tendency to fall, the mechanism must be different from the one laid out by Marx. (Mongiovi, Chapter 10, this volume, p. 271–284)

But the "mechanism laid out by Marx" is, precisely, an endogenous mechanism: it arises from the blind operation of an economy governed by the commodity form, the simple accumulation of value itself. I of course do not dispute that *other* factors may cause the rate of profit to fall or that SM may recognize them. The point is that these are *exogenous*, that is to say they arise from elsewhere than the blind workings of the commodity form itself: a particular form of competition, or an irrational capitalist predisposition not to invest in cost saving, or simply a profit squeeze, which is an outcome of the conscious struggle between capitalists and workers. Mongiovi could easily have refuted the argument he considers gratuitous, by exhibiting any example of an endogenous mechanism of crisis consistent with the simultaneous method. He does not.

Mongiovi compounds his problems by recruiting to his cause the many economists who acknowledge the exogenous mechanisms of *recovery*: "A good many mainstream economists would acknowledge that the Second World War was instrumental in rescuing the global economy" (Mongiovi, Chapter 10, this volume, p. 271–284) Since a unified law of motion of capitalism posits endogenous mechanisms of crisis but exogenous ones for recovery, these examples make no dent in my case. I freely concede that equilibrium analysis allows for the impact of exogenous factors on the economy. The issue is whether these economists also explained what, in the first place, produced the need for a rescue: whether they identified the *endogenous* mechanisms of decline that made the Second World War "necessary."

His third line of defense is to cite individual economists, of two classes: those who, *although* they subscribe to equilibrium reasoning, *nevertheless* recognize one or another mechanism of crisis that is incompatible with the equilibrium paradigm, and those whom he pressgangs into the equilibrium camp with no serious evidence that they belong there. Thus we learn that "neoclassical economists like Knut Wicksell and Dennis Robertson wrote insightfully about hoarding and monetary instability" or that "Left-leaning economists who make use of equilibrium analysis manage to avoid the error [that capitalism is a reliable deliverer of prosperity]" and that "neither Adam Smith nor David Ricardo, early proponents of the long-period equilibrium method, had any difficulty accommodating a falling profit rate in their analyses of capitalist dynamics."

Individual economists hold a great variety of views, some of them useful. This proves nothing about the theory they hold. Many clerics who hold creeds that abominate abortion, homosexuality, or indeed, equal rights, are sincere social liberals, have powered movements like Jubilee 2000, or have founded progressive movements like Liberation Theology. This does not entitle us to deduce that the bible is a rational guide to progressive ethical conduct. The issue is the relation of thought to *theory*, not the stance of particular individuals on particular questions.

Mongiovi charges me with "advising a carpenter to discard his screwdriver because it is no good for hammering nails" (Mongiovi, Chapter 10, this volume, p. 271–284). But actually, I am pointing out the lamentable consequences of driving screws with a hammer. I prove that equilibrium *theory* cannot deduce endogenous mechanisms of crisis. If, therefore, this theory is used to explain crisis, or more generally a unified law of motion of which crisis is only one part, it will screw up.

The issue is thus neither what individual practitioners of equilibrium believe, nor whether one may attach the label of equilibrium practitioners to individuals who would probably be quite startled at this posthumous enlistment, were they alive to protest. The issue is what is *consistent* with simultaneist theory. Mongiovi sidesteps this by stating that "economists must draw on a more eclectic box of tools" (Mongiovi, Chapter 10, this volume, p. 271–284). The issue remains: which theory leads to which conclusion? Even if we concede the virtues of eclecticism, how should the company of eclectics *choose* when to employ a hammer, a screwdriver or a buzzsaw? If this is not a relevant question, why have theory at all? The purpose of theory is to reach conclusions and make judgments about what we can reliably accept as true, and hence act on. If economists do not use their theories for this purpose, why teach or study theory at all? And indeed, why employ economists?

In summary, not only does Mongiovi fail to establish that my claim is gratuitous, he also fails to show it is false. SM does indeed exhibit the theoretical weakness he denies. I turn to the more substantive question that arises from this weakness: *in fact and in the practice of economists,* does SM and its domination of Marxist economics impede the progress of knowledge? And if so, how?

PM's purpose, as already noted, was not to propose a new theory of crisis but to establish why contemporary Marxism is predisposed to misunderstand Marx's concept of "law of motion" as describing a mechanical, deterministic or fatalistic process, against which no human action can prevail. Marxism's predilection for determinism is no discovery of mine (see for example Lukács, 2000). The problem, therefore, is to explain it.

The most common reason offered by unthinking critics is that Marx's thinking was itself deterministic. This reading conflicts with everything we know about Marx's views on free will and contingency (see Freeman, 1999; Wells, 2006). *PM* set out to clarify how honest scholars should reconcile the concept of "law of motion" with Marx's implacable and lifelong opposition to positivism and determinism.

PM's opening section argued that in Marxism's early history, its predilection to interpret this deterministically arose within the workers' movement itself. But we need to explain its survival into modern times when Marxism, with the important exception of China, and a scattering of small countries, is confined to the universities. As we progress through a crisis comparable only to the world-changing recessions of 1871 and 1929, we need to understand Marxism's suicidal and obdurate resistance to the obvious thesis that a profound event of this nature was generated by economic mechanisms inherent to capitalism. Attempts to attribute a recession now entering its fourth year to contingent and external mechanisms such as bad governance, misguided monetary policy, or "financialisation" are at ever-increasing odds with the facts.

This has practical policy conclusions to which the theoretical discussion is only a prelude, albeit a somewhat necessary one. The road to "recovery" can only pass through a large-scale *supersession* of the commodity form, that is to say, an irruption of the state into the sphere of production. Past irruptions of this nature have taken either the negative shape of imperialism, fascism and war, or the positive form of revolutionary developmental states.[1] The present crisis once more presents humanity with a choice: whether to initiate the recovery by democratic and revolutionary means, that is to say by working class means, or, by inaction leave it to the capitalist class to try it by autocratic and reactionary means, with great destructive consequences. In order even to make such choices in a conscious, democratic fashion, humanity demands intellectuals with the moral fiber to distinguish truth from lies or fantasies.

It brings me no pleasure that my very concrete 2008 prediction that, in the absence of state-led investment on a large scale, no stable recovery from the crash could be expected (see Freeman, 2009a, 2009b) is confirmed with every passing day, nor that the recession has already discredited such absurd alternate prognoses, advanced in the name of Marxism, as the idea that this was just another "blip" or a "bankers' crisis" that would shortly correct itself. To get humanity out of this mess, entire states and classes need to understand these things, not just isolated individuals or currents. The point of my article was to explain why Western, academic Marxism, in its present state, has proven such a hindrance to this much needed outcome. This is a failure of *theory*, not of individuals.

I now turn to the discussion itself, and the means it offers to overcome the above theoretical failure. Mongiovi wishes to "dislodge the debate from the impasse at which it presently stands" and believes that to so this "we need to do a better job of responding to each other's criticisms" so that "we might try to narrow the distance between our two positions" (Mongiovi, Chapter 10, this volume, p. 271–284).

I entirely agree that better responses to criticism are called for, though I suggest that it unwise, unhelpful and unnecessary to view the purpose of discussion as "narrowing distance" between opposed ideas. In my work on the role of pluralism in economics (Freeman, 2009c), I proposed, rather, that the duty of economists in discussion is to establish a scholarly and respectful process which is cognizant of difference, in order to judge between ideas which lead to different conclusions. The key to achieving this is to accord legitimacy to opposed positions. The most desirable outcome of discussion is thus *accuracy*, above all in the representation of opposed views.

Mongiovi's response therefore remains disappointing. He states I have "yet to engage his arguments." In Freeman (2002), which I presented at a conference he took part in, I provided an exhaustive response to the paper (Mongiovi, 2002) for which he claims a response is wanting. Freeman (2006) exhaustively refutes a claim, which he now repeats without justification, that prices "gravitate around" their long-run equilibrium. TSSI scholarship has produced a never-ending stream of responses to SM, which include copious refutations of their many claims. To take only one example, Kim (2010) accurately summarizes the long debate around one of these, the Fundamental Marxian Theorem, and also shows how easy constructive engagement is, despite deep difference, when it conforms to the basic scholarly norms of respect for difference and accuracy in representation.

The shoe is thus, somewhat, on the other foot. TSSI scholars have examined, clarified, responded to and refuted simultaneist claims in many forums for over fifteen years. What brought the "debate" to the "impasse" from which Mongiovi now aspires to "dislodge" it is the destructive pretence that TSSI has no legitimate claim to existence, combined with the systematic misrepresentation of TSSI sanctioned by the gatekeepers of discussion (see Freeman & Wells, 2010, 2011) within Mongiovi's camp. Any attempt to bridge this gap from the SM side is welcome, but the ball is firmly in the SM court.

Our response to Kim (Kliman & Freeman, 2011) deals with most of the points that Mongiovi raises concerning the differences between the simultaneist and temporal methods, as regards determination, prediction, and the function of economic models in the two paradigms. I choose not to repeat these arguments in this brief reply. Instead, I invite Mongiovi, or any member of his camp, to join the debate in *Marxism 21* in which Kim's excellent article is to be found, or to respond to my comprehensive critique of the equilibrium interpretation of Marx (Freeman, 2010b) in the *World Review of Political Economy*.

I can respond in at least one positive respect to Mongiovi's concern that my "stark dichotomy rules out of play a large class of serviceable models" by informing him that his anxiety is tangential to the issue at stake. I have no problem in conceding that hypotheses using equilibrium reasoning have many uses, and further that the simultaneous equation method in its entirety is a wholly legitimate approach to explaining and even predicting all and any economic phenomena. A legitimate method, as spelt out in Kliman and

Freeman (2011) is one that cannot be ruled out when scholarly tests are applied to judge which of its hypotheses are valid. A hypothesis is legitimate if expressed in a form that permits such scholarly scrutiny. To this end it must represent the alternatives accurately. A legitimate hypothesis must *therefore* express, in a form that permits scholarly scrutiny, the interpretations it makes of any theory – be it the theory of Marx or Menger, Keynes or Kalecki, Smith or Sraffa – on which its conclusions depend.

This is just normal science. The problem is that equilibrium practitioners do not themselves practice normal science. Therein lies the only question mark over the legitimacy of their hypotheses. These do not concede, to theories or interpretations which differ from theirs, the elementary rights outlined above. In consequence, they rarely satisfy the criterion of being expressed in a form that permits scholarly scrutiny. To invert the entire argument with which Mongiovi enters the lists on this occasion, the legitimacy of any hypothesis depends not on the method it uses as such, but on the manner in which it treats alternative methods, most of all, alternative interpretations of the terms it uses.

The consequences of failing to abide by this precept are most evident in Brenner's famous NLR essay (1998) in which he relies on three keynote simultaneist authorities to claim that:

> the ultimate result of [capitalist] innovation … *certainly cannot be to* reduce the rate of profit.

He does not say, as he might well have done if better advised, that

> the ultimate result of capitalist innovation … cannot possibly be to reduce the rate of profit *as defined using the simultaneous method*. However, TSSI scholarship has plausibly shown that if Marx is interpreted temporally, the rate of profit *as defined by Marx* does in fact fall, which accords with the empirical facts.

Although his simultaneist advisors have driven an outstanding Marxist historian completely to discard Marx's simple and superior explanation, as a good historian Brenner has to explain the empirical fact that the rate of profit does actually fall exactly as Marx predicted (Desai, 2010). He ends up constructing his own tortuous version of this same tendency. This is a waste. A new generation, to confront the gigantic problems posed by the present economic situation, needs to be free to appeal not to the expertise of a tiny layer of western experts, but to the knowledge embedded in ideas that informed the revolutionary sacrifices of two preceding centuries, above all that arising from previous great crises of similar scale. This knowledge comes down to us in the crystallized form of their theories. To shut these generations off from the *theoretical advances* that arose from these huge past experiences is an intellectual crime, in my view, of the first order.

The mode of reasoning which rejects *Marx's* conclusions on the sole grounds that the *simultaneist interpretation of Marx* finds them false is universal in simultaneist writings. Steedman's (1977, p. 14) canonical work on this question exhibits this in laboratory-pure form:

> On the basis of certain, common, reasonable assumptions it may be shown that
>
> (1) The conditions of production and the real wage paid to workers, both specified in terms of physical quantities of commodities, suffice to determine the rate of profit (and less importantly, all prices of production)
> (2) The quantities of labor embodied in the various commodities, which can themselves be determined once the conditions of production are known, thus play no *essential role* in the determination of the rate of profit (or of the prices of production)
> (3) Marx's solution of the 'transformation problem' is incorrect, not only with respect to prices of production but, more importantly, with respect to the rate of profit. The rate of profit in a competitive economy is not equal, in general, to [S/(C + V)], where S, C and V are aggregate surplus value, constant capital and variable capital respectively …
> (4) The social allocation of labor power can be determined without reference to any value magnitude;
> (5) The relationship between surplus labor and the existence of profits can be established quite independently of Marx's concept of value;
> (6) There is no *a priori* basis for establishing any expectation concerning the long-run movement of the rate of profit.

At no point in this SM catechism, nor in his list of "common, reasonable assumptions" does Steedman mention the simple fact that the magnitude of every single quantity described in this list is defined by the simultaneous equation method and that therefore, the entire case rests on the utterly unsubstantiated assumption that Marx employed this method in order to derive his conclusions.

In my previously-cited response whose existence Mongiovi denies, I gave (Freeman 2002:20) a long list of cases in which this mode of reasoning is applied. In introducing these cases I stated that

> This debate will therefore succeed, not if you recognise our interpretation of Marx to be "correct" but if you concede it is possible. If you accept that it is legitimate to interpret Marx in the way we suggest, in so doing you reject the century-old dogmatic grounds for excluding Marx from the normal terrain of scientific discourse. This is not an onerous challenge. It does not oblige the supporters of Sraffa to abandon either their approach to economics or their interpretation of Marx. It merely obliges them to abandon the view that this approach, and this interpretation, is the only possible one. Our experience is that our opponents nevertheless find such concessions hard, and this is why we consider their approach dogmatic. Sraffians and post-Sraffians alike will no longer have the right

> to speak as if their reading of Marx was the only one. They will have to stop once and for all presenting as a universal truth a result that is contingent upon an interpretation.

This challenge appeared nine years ago in a direct response to Mongiovi. He gives no reason for continuing to evade the point there made. I offer a new and simpler challenge: let him exhibit a single text on Marx by any SM or indeed post-Sraffian writer which concedes that her or his conclusions about Marx are based on a particular interpretation, and recognizes that an alternative, legitimate interpretation yields different results. This text could well serve as an exemplar for the editorial standards required of all journals which discuss or assess Marx's ideas.[2]

In the relatively small part of *PM* to which Mongiovi takes exception, I sought a theoretical explanation for the absolute and obstinate refusal of the SM current to engage with this simple issue of interpretation, itself well understood outside of economics. My explanation, given at greater length elsewhere (Freeman, 2007), is that the simultaneist paradigm is not a "model" but an ontology. It supplies not merely numerical values but definitions for all the endogenous variables of the capitalist economy – prices, quantities, the rate of profit – you name it, there is an equilibrium definition of it. This imposes a Platonic conceptual mindset which takes these definitions to be the "true" reality from which mere observation is just a temporary deviation. It then decrees this to be the only possible definition of the quantities in question. Hence, it concludes, this must be what Marx meant, because there is no other way to think.

The origin of this entire approach, *PM* set out to show, is an utter confusion of prediction with definition. A *calculation* of price, or profit, or any other magnitude of the capitalist economy, does not constitute a *definition* of that magnitude. If the practitioners of equilibrium genuinely concerned themselves only with producing testable hypotheses or "predictions" in the normal sense of the word, there would be no difficulty. But the simultaneist does not in fact predict the rate of profit: the real function of simultaneism is to declare it the only rate possible whether or not it is empirically true, just as its positivist forbears decreed the "iron laws of society" the only conceivable fate of humanity. The easiest way to refute *PM's* conclusion that the simultaneist Marxists are the true heirs of positivism is to behave differently from the positivists. Mongiovi and his camp must merely recognize that there is a valid, alternative, non-simultaneist interpretation of Marx's value theory which consistently allows us to deduce Marx's understanding of crisis from his theory of value. This is all that it would take to "dislodge the debate from the impasse". It is the fact that SM practitioners will not do this, willfully and in full knowledge of the choices before them – and their consequences – which leads to *PM*'s conclusions. I would be only too happy to stand refuted by a change in their practice.

NOTES

1. The reasoning that supports this conclusion fully answers Mongiovi when he asks how it helps us to know that "units of money that move around when resources are allocated in specific ways must be understood to represent quantities of labor." The syllogism is simple. The total value produced in society is limited by the amount of labor employed by capital. Society cannot escape this constraint through inflation, because the value represented by a given quantum of money simply decreases *pro rata*. Nor, as SM wrongly predicts, can it escape by raising productivity – that is, merely increasing the amount of use-value in which the same magnitude of labor is embodied. In a crisis, capital withdraws from value-producing investment, and ceases to employ the labor, or produce the use-values, required to maintain social reproduction. Therefore a social order must be found that employs labor to produce these or alternative use-values without constraining labor to produce exchangeable value. This is what the investor-state *does*: it produces for the public good, instead of for sale, use-values which capital consents to sell only in return for private profit. This is how public health *works:* it provides as a universal right what private providers produce only when they can sell it. The issue facing society is to extend these principles to all world citizens on a scale so far only seen in the advanced countries under conditions of War, fascism, or both.

2. Mongiovi's vision of the economist as eclectic is thus doubly wide of the mark: one recalls Henry Ford's famous dictum that you can have any color you like, as long as it's black. For the practitioners of equilibrium, you can employ any model you want, as long as its simultaneist. Eclecticism would be a somewhat more attractive principle, if the company of eclectics could bring itself to ease the conditions of entry.

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REFERENCES

Brenner, B. (1998). The economics of global turbulence. *New Left Review* (I/229).

Desai, R. (2010). Consumption demand in Marx and in the current crisis. In: P. Zarembka (Ed.), *The national question and the question of crisis, Research in Political Economy* (Vol. 26, pp. 101–141). Bingley: Emerald Group.

Freeman, A. (1999). *The limits of Ricardian value: Law, contingency and motion in economics*. University Library of Munich, Germany. Retrieved from ideas.repec.org/p/pra/mprapa/2574.html

Freeman, A. (2002). Marx after Marx after Sraffa. In *METU proceedings*. Middle East Technical University conference, Ankara. Retrieved from mpra.ub.uni-muenchen.de/2620/1/MPRA_paper_2620.pdf

Freeman, A. (2003). When things go wrong: The political economy of market breakdown. In: A. Westra & A. Zuege (Eds.), *Value and the world economy today* (pp. 91–119). Basingstoke; New York, NY: Palgrave.

Freeman, A. (2006). An invasive metaphor: The concept of centre of gravity in economics. In Association for Heterodox Economics, London. Retrieved from mpra.ub.uni-muenchen.de/6812/1/MPRA_paper_6812.pdf

Freeman, A. (2007). Heavens above. What equilibrium means for economics. In: V. Mosini (Ed.), *Equilibrium in economics: Scope and limits*. London: Routledge.

Freeman, A. (2009a). Investing in civilization. In: W. Anthony & J. Guard (Eds.), *Bailouts and bankruptcies*. Winnipeg: Fernwood Press. Retrieved from ideas.repec.org/p/pra/mprapa/26807.html.

Freeman, A. (2009b). *How much is enough?* University Library of Munich, Germany. Retrieved from ideas.repec.org/p/pra/mprapa/13262.html

Freeman, A. (2009c). The economists of tomorrow: The case for a pluralist subject benchmark statement for economics. *International Review of Economics Education*, *8*(2), 23–40.

Freeman, A. (2010a). Marxism without Marx: A note towards a critique. *Capital & Class*, *34*(1), 84–97.

Freeman, A. (2010b). Trends in value theory since 1881. *World Review of Political Economy*, *1*(4), 567–607.

Freeman, A., & Wells, J. (2010). *Open letter to the RRPE editorial board*. Retrieved from www.iwgvt.org/rrpe

Freeman, A., & Wells, J. (2011). *Why standards matter in heterodox economic publishing: The case of the review of radical political economics*. Presented to the Association for Heterodox Economics conference, Nottingham, July 2011. Retrieved from ntu.ac.uk/nbs/news_events/conferences/thirteenth_workshops.html

Kim, C. (2010). The recent controversy on Marx's value theory: A critical assessment. *Marxism*, *21*(7), 2.

Kliman, A., & Freeman, A., (2011). A welcome step in a useful direction: A response to Changkeun Kim. *Marxism 21,* Vol 22. pp. 167–214. Retrieved from http://nongae.gsnu.ac.kr/~issmarx/eng/article/22/Freeman&Kliman22.pdf

Lukács, G. (2000). *Tailism and the dialectics*. London: Verso. (German original Lukács, G. (1996). Chvostisms tendus und Dialektik, Budapest, Aron Verlag).

Mongiovi, G. (2002). Vulgar economy in Marxian garb: A critique of temporal single system Marxism. *Review of Radical Political Economics*, *34*(4), 393–416.

Steedman, I. (1977). *Marx after Sraffa*. Verso Books.

Wells, J. (2006). The dogs that didn't bark. Marx and Engels and statistical fatalism. In *Dissent in Science Seminar*. London School of Economics. Retrieved from http://staffnet.kingston.ac.uk/~ku32530/dogs.pdf