

Beyond talking the talk – towards a critical pluralist practice

Andrew Kliman

Pace University

and

Alan Freeman

The University of Greenwich

Critical pluralism – an introduction

This paper argues that pluralism in economics requires formal rules of conduct. These should provide for professional standards for research, presentation and editorial judgement which will constitute formal guarantees of pluralism in research.

The guiding principle of this reform is what we term *critical pluralism*. This is rooted in the view that progress towards truth arises at only if empirical reality is tested against a *multiplicity* of theoretical explanations of that reality. Such an approach renders it essential *properly to identify* the theoretical explanations being tested in order objectively to discriminate between them. This in turn requires a systematic *hermeneutics* in order to form evidence-based judgements about the views of the authors who formulate these theories. Finally this is possible only if it includes *historical enquiry* to lay bare the full range of theories past and present which the professional economist should be obliged to test against reality. The primary duty of a scientific economist is, in summary, to *engage* the range of theories and paradigms which purport or seek to explain what she is investigating, in contrast to the current practice of *competing* to displace or refute explanations and interpretations which fall outside the theoretical bias of the researcher.

What we propose is very different from prevailing informal standards which, in our view, exercise a suppressive function. Nevertheless in our experience, when most economists, whether heterodox or orthodox, come to realise what we are really proposing, their initial reaction is usually hostile. This is at first sight surprising, particularly since heterodox views suffer the most from the suppressive effects of the current procedures extant in economics. It is doubly surprising given that the kind of measures we propose – for example, the right of reply, the right of appeal, safeguards against misrepresentation – are regarded in all the rest of society as indispensable guarantees of objectivity and justice.

The fact that this reaction is so hostile tells us something about the profession of economics. It tells us it is in some very deep sense, profoundly at odds with current human values. Thus what we propose is, in essence, a reform programme. It is a programme to bring economics into line with principles to which the rest of society aspires.

This reaction also tells us the problem does not simply lie with orthodoxy, and will not be overcome by the mere existence of heterodoxy. It requires a different model of pluralism, something different from the present model which consists, we believe, of an 'evolutionary struggle between competing orthodoxies' in which the practices

which produce orthodoxy in the first place are merely reproduced among its rivals. This is the principal factor which arranges both for the survival of these practices and the dominance of orthodoxy.

Since the current practices of economics are rooted in its constitution as a distinct discipline, we believe it is incapable of self-reform. This reform programme is therefore not addressed to economists. It is addressed to their victims. It argues that those economists who seek the reform of their discipline need to appeal *outside their profession* to remove the freedom from external accountability to which economics alone among the sciences arrogantly lays claim. When a bridge falls down through bad design, the engineer is held to account. When a patient dies through poor treatment, society is rightly entitled to question the competence of the doctor. Yet when wrong economic policies bring about the demise of a country, the economists blame its people. Our proposed reforms are intended to impose on them a long-overdue responsibility for the consequences of their own actions.

It may be thought that such a programme is normative and ethical, rather than positive or scientific. We will argue that, to the contrary, without such formal guarantees of pluralism, economics will continue to function not as a science but as a religion. Our guiding principle is to identify those practices which society must impose on its economists, should it require these economists to provide them with scientifically valid information.

An alternative to Orthodox Heterodoxy

As an example and as a basis for discussion, we will present the scholarship guidelines arrived at during ten successive miniconferences of the International Working Group on Value Theory, which took place at the Eastern Economic Association's annual spring conferences in the USA each year from 1994 to 2004. These guidelines do not claim to be a complete programme of reform: they are a work in progress which we want to share with others interested in participating in a radical reconstruction of economic practice.

We also dissect, and present for discussion and for evidence, concrete experiences which we have had in the attempt to present and develop, collectively, a point of view within economics which is what we will term *paradigmatically incompatible* with the dominant orthodoxy both in economics as a whole, and within our own chosen field of enquiry, value theory and the economics of Marx.

It may be thought that this paper, this experience, and our conclusions about practice, relate only to a narrow specialism – the value theory of Karl Marx – and is therefore an internal discussion to Marxist theory. We believe to the contrary that the experiences we have had relate to economics as a whole. This is not simply because we want economics to treat Marx seriously, which we do. But in this respect we are no different from any other heterodox school of thought that wishes its ideas to be taken seriously, be they Post-Keynesians, Institutionalists, Schumpeterians, Austrians or any other.

The problem we wish to address is the way that heterodox schools of thought *themselves* deal with difference and with a plurality of views. If we ask any economist, for example, whether Keynesianism as a whole, or Marxism as a whole,

behaves any differently towards differences in its own ranks than orthodox economics, then if that economist is honest she would have to conclude that her own 'camp' behaves no differently than the currently dominant view. One needs only read the work of, for example, Paul Davidson, to understand that 'Keynesianism' not only constitutes a multiplicity of 'Keynesianisms' but that there exist dominant and suppressed *views of Keynes*; and that moreover many practitioners of the dominant view of Keynes behave, in relation to 'heterodox' views, with an intolerance probably equal to if not greater than the intolerance of neoliberalism towards Keynesianism of any variety.

Indeed, it goes further: if one examines more closely the general approach which is now labelled 'marginalism' and which is itself generally identified with orthodoxy, one finds that this school has in fact marginalised the first marginalists, in that the Austrian approach, which emphatically rejects general equilibrium, and yet which contained in its ranks the founding fathers of the approach, not least Böhm-Bawerk himself. It is not unfair to say, therefore, that resistance to pluralism is a general practice in the whole of economics, quite regardless of whether it is orthodox or heterodox.

Thus there is a paradox which cannot be addressed merely by taking issue with orthodoxy: heterodoxies are just as lacking in pluralistic practice as the orthodoxies against which they set themselves. The general intolerance of economics towards heterodoxy, difference and pluralism is, alarmingly, *universal*: it is not confined to the bad guys.

This is why we think our contribution is of wider relevance than the specialism of Marxist value theory. We experienced the suppressive practices of economics in two senses:

- (1) Economics as a whole treats the views of Marx as a heresy beyond consideration. This complete exclusion of a major founder thinker is without parallel in any other subject, including the remainder of the social sciences. It is the prime evidence of the unscientific nature of economics. It is as if, for example, Physics were to exclude from its thinking anything written by or traceable to Isaac Newton. Newton may be wrong or he may be right, but there is no way that physics could possibly progress towards truth without giving due place to his historical contribution and ideas. So it is with Marx and economics.
- (2) However beyond this, there is *within Marxism* complete and ruthless hostility to the consideration of a variety of *interpretations* of Marx. We have worked with an international community of some 20 scholars who have concluded that the normal 'orthodox' presentation of Marx's views is not only a wrong construction of his actual theory, but constitutes one of the principal obstacles to obliging economics to treat Marx seriously, in that it inevitably leads to inconsistencies which are then laid at Marx's door. However the reaction of the *marxists themselves* to this heterodoxy has been every bit as ferociously suppressive as the reaction of neoliberalism towards its critics

The conclusion we draw is that heterodox economics is the victim of a *false model of pluralism*; it conceives of it as a struggle of 'school against school' – Keynes *versus*

Marx *versus* Sraffa *versus* neoliberalism. But the schools themselves are the greatest enemies of pluralism, in that the school itself becomes the vehicle through which difference is suppressed. Thus the false camaraderie of the Marxists becomes the major obstacle to expressing a different reading of Marx – since Marxists are an endangered and beleaguereed species, the expression of difference among Marxists becomes a dangerous practice, a threat to the existence of all. The supreme irony is that only by reading Marx differently it is possible to challenge his illegitimate suppression.

But this irony only reflects the wider problem: heterodoxy cannot be limited to a battle to substitute one received truth for another. It requires instead a different way of doing things.

What we will term ‘orthodox heterodoxy’ is rooted in a false and reductionist syllogism: since, it is argued, there can only be one truth, therefore, there is not need to examine many theories. All we must do is find the ‘one theory’ which works best, and then apply it. From this standpoint, which is in fact shared by many heterodox economists, the only thing wrong with orthodoxy is that it has not found the truth. Therefore, the only necessary step to reform economics is to substitute the correct, heterodox single truth for the false, orthodox single truth.

This is the fundamental justification for the wrong practice in most heterodox economics itself, which by and large conducts itself as a multiplicity of orthodoxies, giving rise to what Francis Bacon denounced as ‘vermiculate knowledge’ – the fractioning of theory into interminably many sects and fractions, each dedicated to promoting its own particular version of the Truth, be this Marxism, Schumpeterianism, New Keynesianism, Post-Keynesianism, Institutionalism, New Institutionalism, Austrian Economics – the list is endless.

The limits of monotheoretical research

Behind this lies an almost universal view of what an economics researcher ‘does’ which becomes clear only when we examine the practice of the heterodox. Essentially, economics ‘research’ is held to consist of applying *one particular theory or idea* to the study of some aspect of empirical reality. In consequence, the recipients of economic advice are told to adopt a policy on the grounds that it is ‘scientifically’ or ‘technically’ correct. They are never told that it is *one particular view* and are never given the option to adopt different policies, far less to require of the policy advisor that she offer a *range of alternative options based on a range of alternative theories*.

It is only after the event that error is admitted into the process. Thus, right up to the minute before the collapse of the Argentinian peso it was argued with implacable authority that a hard currency peg was the only possible option available to Argentina’s bankers. It took until two years after the collapse until the IMF finally got around to considering it may have made the odd mistake, and indeed, the first time this statement saw the light of day was not from the IMF’s own economists but from the IMF’s independent audit body.

It is still being argued, with equal conviction of the impossibility of error,¹ that the only way forward for Argentina is the very policies of financial market liberalisation which made possible the mass outflow of capital that provoked the crash. Indeed immediately after the crash, with completely unconscious irony, Rudiger Dornbusch seriously proposed that the solution to Argentina's problems was to place the country's economy under the direction of a committee of internationally-renowned economists.

If economics had conducted itself responsibly or scientifically, it would *at the time of the hard currency peg itself* have offered, to the Argentinian public and policy-makers, the information that an entire range of very well-supported theory predicted very high risks attached both to a hard currency peg and to the abandonment of capital controls – not least, to doing both at once! – and would have *left the decision between options* to the democratic processes of Argentine society. A responsible profession would *now* be laying before the Argentine financial authorities the alternative options of capital controls, of state credit, of maintaining the Argentine National Bank in state hands, and so on, instead of recommending a continuation of the very policies which, in combination with the hard currency peg, brought the country to its knees.

As is well known, orthodoxy does not do this. Every new piece of advice, no matter how disastrous the previous, is always presented as the only possible view, as grounded in 'scientific analysis' and never, ever, as the expression of one particular theory (with all the bias that this implies) among many others.

All heterodox economists recognise this 'monotheoretical practice' to a greater or lesser degree precisely because they are heterodox, that is, they can see that orthodoxy contains mistakes. However they do not in general translate this insight into the errors of others into a reflection on their own practice.

In orthodox or mainstream economics monotheoretical practice is concealed because researchers do not need to put a name to the theory they are using. An economist at the International Monetary Fund is not required, when telling a country what to do, that this is the neoliberal view of what the country should do. She, or more usually he, just says 'this is what economics tells us you should do'. Nevertheless such judgements contain a concealed and unstated bias, since by the word 'economics' the researcher or advisor actually means *one particular theory* – the current dominant paradigm.

Heterodox economists cannot so conceal their bias. In order to say that one differs from the mainstream and in order to make common cause for a different theory and a different policy, one has to put a name and a description to what one proposes. One has to call it 'New Keynesianism' or 'Evolutionary economics' or 'National Systems theory'. *But in so doing one has already announced a new orthodoxy.*

Moreover the notion that the function of economic research is to apply and develop a single theory is utterly rooted in the way that it is organised. A struggling heterodox PhD student, or grant applicant, has enough trouble mastering one author

¹ And, most importantly, as a condition of further extensions of Argentina's debt to the IMF

or body of theory. Surely it is an impossible burden to demand that the researcher should be conversant with, and give consideration to, not only the theory but its critics and alternatives. How many times, at heterodox conferences, have we heard a presenter, when asked why she or he considered no alternative approach, simply state that she had no time, was not interested in it, would 'leave the other approaches to those who were involved in them', and so on.

Backwards and in high heels – the material origins of monotheoretic practice

It is at this point where one begins to realise why opposition to a consistent pluralism is so deep-rooted. A core element of the IGWVT guidelines, for example, states that presenters have to engage with, and study, as an integral part of the way they explain one particular theory, the views of its critics and the possible alternatives to it. Faced with this demand, a substantial body of contributors simply gave up and went back to doing what they had always done. Pluralism is, frankly, a lot of work. If it were imposed as an editorial criterion we make a rough estimate that around 90 per cent of currently published articles would be rejected on these grounds alone – never mind what would happen to research grants. Most international financial institutions would become extinct.

Thus to be a consistent pluralist is a daunting research option. It involves twice or three times as much work. It reminds one of Ginger Rogers' famous statement that "I did everything Fred did, but backwards and in high heels". The necessity of such practice is in fact revealed by the very different practice of *business* economists, who have to advise people with large amounts of money to spend what is their best course of action. In fact the very best working economists, and the very best economic units – notably (but not always, as the spectacular failure of the Long-Term Hedge Fund shows) the best financial economists, particularly when they do find themselves accountable for their judgements to employers that spend money instead of advising governments, *do* recognise the necessity of a multitheoretical approach, *do* triangulate from evidence, *do* examine a variety of explanations for what they hope to explain or predict. It is a perfectly possible thing to do. But it is hard work and it is time-consuming, and as a consequence it is not seen outside of the closed world of the business decision-makers. Above all in research, in publication, and most notably in the practice of *political economy* – giving advice to countries and governments – pluralism is virtually absent.

Thus profound *material, career choices*, from the very start of a researcher's career, impels her or him at every step to a narrow theoretical specialisation. Monotheoretical research practice is an intrinsic organisational factor in what is considered 'good' in economics.

To realise how deeply ingrained is this attitude of thought one need only consider some of the more common arguments offered against it. How many times have we heard that a presenter is 'too busy' to consider the ideas of her critics? Or that a journal rejects a submission because 'this journal does not publish this kind of material – it should be published somewhere else'. This 'natural' framework of thought informs us that the most efficient use of research time is to engage in narrow specialisation to the exclusion of all alternatives, and that the legitimate

purpose of publication is to promote one particular set of views, consistent with the particular theoretical prejudices of the editorial board and its reviewers. Pluralism will be taken care of, it is argued, on the basis of a kind of liberal market model: each school is free to publish its own material, present its own work, in its own conferences, its own schools and in its own journals.

But this is precisely to render economics a kind of microcosm of the liberal market. Schools of thought are thrown into a discreetly savage competition with each other: they compete for publication, for space at conferences, to control journals, for research grants, to win institutions, indeed to capture the ears of entire governments. At every stage however the 'judges' are practicing monotheorists, deaf to difference, resulting in a mafioso battle of organised clans and tribes of partisans masquerading as disinterested schools of thought. The infantile potty-training of every would-be economic researcher consists in learning how to politely dump her enemies in the shit.

In consequence, judgements on what is considered bad or good are almost inevitably made on a basis which drives the judges towards subjectivism. Why on earth should any rational person promote in her university a professor who disagrees with her? What kind of career decision is that? Or, consider those journals which do cover a range of subject matter. To whom is an article submitted for consideration? To the 'peers' of the author concerned. However the concept of 'peer' is defined to be 'other researchers that adopt the same label' – a kind of 'render under Caesar that which is Caesar's' of the economics profession. What advantage can any referee secure from consenting to the publication of an article which threatens to undermine her own research specialism?

Recognising these material pressures, a genuine pluralism would insist on controls. It would submit any article to a range of referees both within and without the specialism of the author, and in those referees it would include those who share not only the author's 'label' but the particular paradigm within which that author works. It would expect the referees to judge the consistency and merit of the article not on the basis of whether or not they agree with its content or accept its approach but on the basis of whether they can prove on the basis of evidence that the article complies with scholarly norms – use of evidence, consistent logic given the premises, and so on.

And if the referees failed in their duty to adhere to these standards of objectivity, it would provide an overriding accountability – just as do courts of justice – in the shape of formal appeal procedures in which it is legitimate to examine whether the referee has or has not done her or his job.

Finally, the very fact that pluralistic practice is considered 'too difficult' constitutes the most damning verdict on what the profession of economics regards as good practice. To be sure, consistent pluralism would make good economics very hard to do. This is not, unfortunately, an excuse. It is very hard to be a good doctor but society does not unleash quacks and charlatans to practice on the sick. It is hard to be a good chemist but we do not let alchemists wander at large poisoning people and blowing things up. It is hard to be a good astronomer but we do not hand over command of space flights to the writers of Sunday horoscopes. Society demands

standards of its professionals because it wants them to do their job, and it has every right to do so.

Bad economists are a more serious social danger than bad chemists, engineers or doctors. They do not just damage individuals but entire countries and populations. It simply isn't a good enough excuse to say that it is too difficult to do it right. At the very least if this approach is going to be adopted, the public is entitled to a few health warnings.

Economics and paradigm shift

The central argument of the paper turns on the way that the economics profession reacts when it encounters phenomena which it is unable to explain, and which can in fact only be explained on the basis of a 'paradigm shift'.

It is for this reason that, we must insist, what is at stake is not personal injury or rights, but the actual content of the output of economics.

As explained above, we argue that the 'traditional' organisation of this profession, the routine common-sense practices it considers 'normal and acceptable' when judging whether to publish an article, accept a job application, promote a researcher, fund a project, or grant a PhD, are when taken in their totality a systematic instrument for suppressing a plurality of views and imposing conformity.

This much is widely recognised. We doubt there is a single participant in this broad conference who does not have some experience of the suppressive mode of functioning of economics.

However, we want to go further. Until now, the standard reaction in heterodox economics has been, in our view, to 'play the game' – that is, to challenge the *results* of orthodox economics but not to challenge its *practices*. Pluralism, from the standpoint of this reaction, is then a luxury – something to aspire to, or to lament the lack of – but not something to be implemented. Pluralism is to economics what salt is to cooking. It is generally held to improve the taste, everyone wants it when it's on offer, but it contains no nutrition, it is probably injurious to health, and the true master chef has no need of it.

The central thesis of this article is that pluralism is not the condiment but the main course. *Because* economics is not pluralist, it is not scientific. Thus at stake is not just whether economics is 'nasty' or treats people badly, but whether its content is correct. We believe the suppressive operation of economics is functional – its acts to prevent the emergence of modes of enquiry, or theoretical frameworks, within which the difficulties encountered by market economies appear *endogenous* – that is, cannot be produced by the market itself. Economics contains within it a systematic bias, which is the greater the closer it is to the political seat of power, towards theories within which the problems faced by market economies are *exogenous* – are the result of external interference with the market.

The organisation of economics, we have argued, constitutes an unconscious reproduction of the model of the liberal market. Each school sets out its stall, marshals its supporters, and competes for fame and fortune, and may the best school win.

Indeed this model is generally speaking celebrated. It is held to embody liberal virtue, since anyone has the right to speak in principle.

The problem is that everyone may indeed have the right to speak, but what is missing is the right to be heard. More precisely, this model contains no guarantees that economics will abandon bad theories and adopt good ones. A good theory may be spoken, it is true: but there is no mechanism for ensuring it is selected. To the contrary, we will argue, the mechanisms operate to ensure that a good theory is *not* selected, if it contains conclusions uncomfortable to the dominant classes in society – such as the idea that the social system which provides them with their livelihood may have its intrinsic limits.

Those most conscious of the operation of the economic ‘market in ideas’ celebrate it, generally speaking on the basis of a kind of evolutionary model of the progress of knowledge. Since economic ideas are, like the brutish beasts of the field, in constant war with each other, on Darwinian principles the best must surely emerge.

This might be true if the ‘selection of the economic fittest’ was contingent on success in the natural world. When the competitive evolutionary model is applied in, say, engineering, there are objective constraints which limit the selection of bad ideas – the bridges and buildings fall down.

But the selection of economic ideas is not determined by success in the natural world. To the contrary, when one observes such phenomena as the collapse of Argentina, the effects of neoliberal globalisation, or perhaps the most classical example of the Irish famine when shiploads of unsaleable potatoes stood idle in the ports of Dublin even as the people starved, one has to conclude that material success in advancing human wellbeing is the last thing which decides what theories or policies are adopted.

The success of economic ideas is determined by success in the social world: by conformity with perpetuating the dominance of those classes and people who draw their living from the survival of the present system. It will not, therefore, and cannot, adopt or promote ideas that threaten this system and these classes. In consequence the competition of economic ideas is far closer to the competition of token currencies which, as we know from Gresham, is an evolutionary system in which Bad drives out Good.

The fact that economic ideas are engaged in evolutionary competition is no guarantee of truth – to the contrary, it is the major factor inhibiting its emergence. Evolution, to coin a phrase, can go down as well as up.

It is for this reason that we argue that without pluralism, economics cannot be considered scientific.

Let us step backwards and ask a simple question: what is it that has allowed natural science to succeed? What is the essential foundation of its achievements? The answer cannot be that it tests observed reality against a *full range of theoretical alternatives available to explain that observation*. It may do this in a tortuous and meandering way, as Kuhn has astutely observed. Nevertheless, despite the obstacles to ‘paradigm shift’ in science, when push comes to shove, new paradigms triumph

over old because they permits us better to understand what we see, and thereby to change it.

Natural Science does not 'go backwards' in its thinking because the ultimate criterion of success is external – the actual domination of humans over nature. It is precisely because we have sent spacecraft to Mars that we will never return to thinking that it revolves around the earth. Thus there are very powerful natural process which secure the triumph of superior ideas in the natural sciences and indeed, science adapts its practice to this fact. The 'model' of science which is certainly preached and generally if grudgingly practiced is intrinsically pluralistic and open to new ideas. This is not because scientists have a special attachment to pluralism – to the contrary, they would probably rather behave like economists – but because they are uncomfortably aware that if a rival theoretician is likely to build better rockets and better computers, they are likely to be listened to, and so there is an evolutionary disadvantage to an failure to consider such rival ideas before it is too late. Therefore no matter how much they hate rival ideas, scientists are driven to try and test them. Indeed, the most bitter struggles in the natural scientist arise precisely when rival schools test *each other's* theories in order to disprove them.

There is no such compulsion on economists. There is no evolutionary advantage to listening. No economist can benefit by testing a rival idea. Because of its special status within the social sciences, economics is 'naturally antipluralistic' with informal rules of conduct in practice accepted uncritically by the generality of economists whether orthodox or heterodox. These unwritten rules, the 'accepted common sense norms' of economics, are driven by a *social* and not a *natural* process of selection. This process is *ideological risk aversion* - the exclusion and suppression of theories from which endogenous market failure can be deduced.

What is the alternative? We define critical pluralism as a practice of active engagement, in which each practitioner is obliged by editorial and professional constraints to consider and engage, with explanations alternative to their own, for the body of evidence they are examining, including alternative interpretations of the textual evidence they present, and paradigmatically varying ontological premises and assignments of meaning to the terms that they use.

The notion of paradigm difference is crucial to this understanding. The notion of a paradigm, as is well known, was introduced by Thomas Kuhn in his famous study of the way that the natural sciences evolved. Kuhn, significantly, never considered any social sciences. His observations were moreover sociological rather than prescriptive. Kuhn did not set out to say what was good or bad science: he simply set out to explain what they do.

In relation to the social sciences and particularly economics, paradigm differences are clearly visible. Moreover, we would argue, there are crucial paradigm differences which *cross schools of thought*. In particular we have elsewhere explained that there is a critical paradigm difference between what we term the temporal and the equilibrium approach, which straddles the whole of economics. Thus Post-Keynesianism, particularly in its Kaleckian variant, is clearly a temporal interpretation of Keynes, which strongly contests the 'orthodox' ISLM interpretation – clearly an equilibrium reading. Equally the Austrian School

TBA complete this section

Appendix 1: Rules for Pluralistic Scholarly Engagement: The IWGVT Scholarship Guidelines

The present paper offers the Scholarship Guidelines of the International Working Group on Value Theory (IWGVT) as a model for other associations of heterodox economists to adopt and modify as appropriate. The authors founded the IWGVT in 1993 and have served as its co-organisers since that time. In this section, we wish to explain the circumstances that led us to develop and implement the Scholarship Guidelines in our annual conferences and elsewhere. We shall first briefly outline how a challenge to orthodox Marxist value theory emerged and why, as a result, the adoption of pluralistic, critical norms to guide the debate within value theory became an immediate concern. We shall then discuss the particular circumstances that led us to develop and implement the Scholarship Guidelines as a crucial component of our effort to organise the debate in accordance these pluralistic, critical norms.ⁱ

The Temporal Single-System Interpretation: a challenge to orthodox Marxist value theory

The Temporal Single-System Interpretation (TSSI) of Marx's value theory, which arose in the early 1980s, is controversial because it challenges a prior consensus within Marxist scholarship. In Duncan Foley's (1997:493) words, it 'endorses Marx's treatment of the transformation problem', that is, the account of the transformation of commodity values into prices of production given in Chapter 9 of *Capital*, Vol. III. It also offers the first refutation of Okishio's (1961) famous theorem, which had supposedly disproved Marx's claim that cost-reducing technical change tends to lower the rate of profit. In both cases, it confirms the logical coherence of Marx's theoretical results – which generations of earlier writers had purportedly proved to be internally inconsistent – without 'correcting' or replacing Marx's own presentation of his own views.

The TSSI's proponents do not seek a new orthodoxy. We do not assert that Marx made no mistakes, nor that other value theories and critical modifications of his ideas are illegitimate. We do insist, however, that allegations of error be substantiated. We have thus returned to Marx's texts, not in order to embrace them as infallible, but in order to ascertain whether he did indeed commit the errors that have long been attributed to him. We have found, to the contrary, that the apparent errors have arisen from misreadings of his texts.

By any objective standard, the significance of these findings is enormous. In the current historical context, they have an implication extending beyond the specialist study of value theory: they remove the only serious justification offered for the near-total exclusion of Marx's own ideas by mainstream economics. If the charges of internal inconsistency cannot be sustained, no rational basis for excluding Marx remains. Thus although the grounds for this censorship are allegedly logical, they are in fact ideological.

Given that the findings of TSSI research call mainstream economics into question in so fundamental a way, it might have been expected that Marxist economists would welcome them. Not so: TSSI authors first challenged the alleged proofs of inconsistency in Marx's value theory in the early 1980s. Since that time, mainstream

Marxian (and Sraffian) economics have consistently greeted TSSI research with scepticism, incredulity, and opposition.

Critical evaluation is of course welcome; the problem is that no such response was forthcoming. The interpretation was ignored and excluded by Marxists just as economics ignores and excludes Marx.ⁱⁱ

Yet the TSSI nevertheless started to become known, especially since the publication of *Marx and Non-equilibrium Economics* in 1996. Subsequently, some of its Marxist and Sraffian critics entered into a debate of sorts with its proponents. It was, however, a rather curious debate, since the critics either avoided, or indeed emphatically denied the need for, any serious re-evaluation of the question of internal inconsistency. They neither disproved the TSSI refutations of the alleged proofs that Marx's theory is inconsistent, nor acknowledged that the proofs are false. Inasmuch as these alleged proofs constitute the sole justification for the near-total exclusion of Marx's own work within economics, the critics' avoidance of the issue served to perpetuate that exclusion.

Moreover, articles based on alternative interpretations continue to be rejected – even by journals of radical political economics – on the grounds that their theoretical framework and results differ from those of the received Bortkiewicz-Sweezy-Steedman interpretation. Attempts to challenge such editorial standards have been met with great hostility. As has sadly been the case in the past, therefore, the Marxists themselves have played as substantial a role in the suppression of Marx's own ideas as have their non-Marxist opponents.

Confronting Dogmatic Exclusion with Pluralistic Engagement

In the course of their re-evaluation of Marx's legacy, TSSI authors were obliged also to re-appraise the conduct of Marxist scholarship. They were driven to a realisation that they could not respond to Marx's critics as these critics behaved towards them or, indeed, towards one another. They were thus drawn into a battle on two fronts. They had to seek recognition of, and debate around, their own discoveries. And they had to examine – and, as far as possible given their limited numbers and influence, critically reshape – the practices that led to the suppression of Marx's theory.

This examination involved more than a critique of the practice of others. TSSI authors were forced to ask themselves how they could react to their predecessors and opponents, to the existing body of theory, in such a way as to remove from the discourse the very possibility of establishing a new dogma. As part of the attempt to forge a new, non-dogmatic kind of discourse, proponents of the TSSI began to restructure their own conferences – the annual mini-conferences of the IWGVT.

In a rare and entirely welcome spirit of pluralism and support for heterodoxy, the Eastern Economic Association hosted the IWGVT mini-conferences for eleven straight years, beginning in 1994. A loose association of researchers sympathetic to the TSSI, the IWGVT was originally established to provide a framework for a small group of like-minded people to present, assess and discuss their work with one another.

It soon became clear, however, that the IWGVT occupied a terrain different from that which its founders intended. Its mini-conferences quickly became large and

diverse. At the 1996 conference, for instance, eighteen papers were submitted, but only a few of them were written by TSSI authors. The remainder came from people holding of a great variety of other views, who often had little in common with the IWGVT, but who nonetheless wished to discuss Marx, or Marxism, or their approaches to value at its mini-conference.

The suppression of Marx by mainstream economics had created an uneasy association by default. Scholars were flocking to a conference that had been organised to promote a research programme different from their own – a research programme in which a good many of them were uninterested and to which some of them evinced outright hostility – because in effect there was nowhere else to go.

The mini-conference organisers had to decide what to do. They could have fallen back on standard practice and tacitly excluded contributions that did not address their concerns. Or, in recognition of their wider responsibilities to scholarship, they could have stuck with the *status quo* – continued to organise quite large conferences in which the great majority of participants not only disagreed with their views, but also declined to engage their research. Neither of these options were attractive, however, so they searched for an alternative.

At the 1995 conference, a seminal discussion took place at which the conference participants, including both advocates and critics of the TSSI approach, asked themselves whether and how to organise discussion between *paradigmatically distinct* theories of value, and interpretations of Marx, in such a way as to rule out dogmatic exclusion. The watchword of the conferences became *engagement*. It was not enough, TSSI authors argued, to follow the established procedures of ‘positive’ economics, setting out each theory on its market stall and leaving the reader to shop around. It was necessary also to read, and respond to, the alternatives to one’s theory.

The alternatives are paradigmatically distinct because they do not share a common ontology. On the surface, different value theories may seem to refer to the same things, but they assign divergent and antagonistic meanings to the most basic terms – value, profit, price, output, consumption and investment. When a proponent of simultaneism speaks of the profit rate, she does not mean the same thing as a temporalist. When a dualist speaks of value, she does not mean the same as proponents of the New Interpretation or single-system interpretations.

An analogy, explored by Freeman (2004), is the cosmological debate of the sixteenth century. Galileo and his detractors could not resolve how to settle whether the earth moved because actually, they shared no common view of what the word ‘earth’ actually meant.

In the absence of a means to appeal against it, prior authority rules by inertia. Not only were established practitioners deeply suspicious of dialogue with newer interpretations, they had unknowingly fallen into an intensely dogmatic practice. Proponents of the standard interpretation assigned a meaning – their own meaning – to the words ‘value’ and ‘price’, and then judged all assertions about value and price as if this meaning were the only one possible. Texts and research projects were judged unacceptable on *a priori* ‘logical’ grounds when they were in fact fully coherent, but did not conform to the methodological and ontological

presuppositions of their judges. The result was what Dow (1985, 1996) has termed a 'closed system'. Free scientific enquiry – which demands constant critical examination and transformation of the meaning of concepts – was replaced by a system of purely deductive logic with a fixed and unalterable ontology, which would not and could not grant the legitimacy of other ways of thinking. It had ossified and become incapable of advance.

The alternative proposed by the IWGVT organisers was a set of standards termed the "IWGVT Scholarship Guidelines" adopted in 1997 and reproduced here. The basic purpose of the guidelines was to try to create conditions in which alternative theories and interpretations engage with one another. A second purpose was to secure recognition that every theory and interpretation carries with it its own conceptual framework, and therefore that a theory or interpretation can be tested properly only if the conceptual framework employed in the test is its own, rather than that of the person running the test.

From this point of view, the first function of debate is not to settle differences, but, by means of engagement, to understand what each alternative is trying to say in its own right, to draw out the implications, and thus see where the differences lie without any prior judgement on which theory or interpretation is necessarily true. At *this* point, when the differences are clear, criteria for deciding between the alternatives can be applied.

This does not reduce to relativism. Rival theories may construct their facts in different ways, but the 'raw material' that is being observed is always common property. We may construct different aggregates, averages, or indicators from a set of tax returns or recorded commodity prices or wage rates, but we are not entitled to alter the tax returns, or simply to declare that a commodity was sold for a price other than the money actually paid for it. As regards interpretation, texts are shared and determinate 'raw materials' to which all interpreters are obliged to refer. The genuine possibility arises, therefore, to test a variety of interpretations and theories against each other, in terms of their ability to explain what all must accept as empirically given.

This may seem simple and obvious, and indeed it is. Yet judging the validity of theories in terms of their empirical success, rather than in terms of their conformity with the accepted conceptual framework and methodological norms, represents a marked departure from the common practice of economics, including Marxian economics. The invitation to engage in a pluralistic but critical dialogue was met by Marxist economists with various degrees of scepticism, ranging from bewilderment to rejection.

In retrospect, it was exceptionally optimistic to hope that critics of the TSSI could be persuaded to adopt scholarship guidelines that worked against them, even though the same guidelines were clearly to the advantage of Marxists in the wider battle against censorship.

Our experience leads us to conclude that, in order to secure the implementation of rules of pluralistic scholarly engagement, a strategy of persuasion alone is insufficient, those who benefit from different, suppressive norms of conduct will not be persuaded to follow them voluntarily. Pluralistic rules must be implemented

despite, and in the face of, continuing resistance from some members of the scholarly community, even a scholarly community of heterodox economists (such as Marxist economists). To secure their implementation, it is important to enter into dialogue and work closely with those particular members and groups within the heterodox community who do favour, and/or whose interests are served by, pluralistic norms.

Appendix 2: Capital and Class and the right of reply

Tuesday, 29 March 2005

To: The editorial board, *C&C*

Cc: Executive Committee, the Annual General Meeting

Dear colleagues

Thank you for Gerry's letter (reproduced below) informing me of your decision not to publish the response from Andrew Kliman and myself to Simon Mohun's critique of our work that was published in *Capital and Class* (C&C) 81, dated September 2003. I request formally that you reconsider this decision.

Since as co-author of a disputed piece I have a personal interest in your response to this request it would be inappropriate for me to remain on the Executive Committee. With effect from Saturday 19th March 2005 I resign from it. Although I will campaign hard to change your minds, I have decided I cannot reconsider my resignation, an outcome I deeply regret.

Contrary to the reasons stated for the article's rejection, I think *C&C* should recognise the right of reply and should publish articles that address Marx's economic theory. Contrary to the precedent set by this decision, I think *C&C* should adhere to pluralistic principles and should present in its pages the full range of theories and interpretations relating to the statement of intent that it carries on its inside front cover, which states that "We are committed to developing a materialist critique of capitalism in the Marxist tradition within the Labour Movement and other movements of liberation."

Your response introduces editorial principles I cannot accept. I have accordingly prepared a resolution to the AGM (reproduced below) which outlines pluralist principles of conduct, including the right of reply, a commitment to publish material on Marx's value theory including a variety of interpretations of that theory, and the use of transparent and accountable editorial procedures.

You are the editors. I don't underestimate how much of your commitment this involves: I respect it. I urge others to respect it and to moderate their discourse accordingly. You enjoy however the despotic freedom of the volunteer. CSE no longer has an active base. If you withdraw your own commitment, there will be no journal. Therefore, things will not be changed by a vote but only if the arguments persuade you. This is why I want to spell out the issues clearly, as I see them; I wish you to take a view unobstructed by formalities.

The sharpness of my response in no way reflects my opinion of your capabilities or ideals. You have in three difficult years transformed and, I would say, rescued the journal. This is why I worked hard to support you, to re-found the journal's relation to its activist base, to provide us with a presence at the European Social Forum, and to set in motion what promises to be a very successful CSE conference in 2006. I was a founder of your journal, the last who remains active in it. I am convinced of its modern potential. I believe it can re-connect with a new emerging radical movement

in line with the statement that it presently carries on its inside cover. However, I do not believe it can do so on the basis of the editorial policy outlined in your response to myself and Andrew Kliman. I therefore sincerely hope you will reconsider it.

If, having considered my arguments you maintain the editorial approach which you have unilaterally introduced, I think the resulting journal will be something alien to that which it was when I took part in its foundation. If on the other hand you throw in your lot with opposition, difference, discussion and controversy, then I see no reason *C&C* should not become a modern beacon, firing hopes for the future with achievements of the past.

The issue is not our personal treatment but the underlying principles. I think your decision is incompatible with the principles on *C&C*'s inside front cover. *C&C* is unique. There is nothing else like it. It provides a voice for the unheard. A voice for resistance, for the suppressed, for truths that can, through your support, be unafraid to speak their name. If we cease to offer that – if we ourselves become safe, respectable, afraid of controversy and an instrument of suppression – then radicalism will pass us by.

C&C should make a clear decision: if it cannot discuss Marx's ideas then it should not claim to represent, or solicit articles in, the tradition of Marx. If it cannot embrace pluralism and the right of reply, then it cannot develop a critique within any movement of liberation. It should therefore change the declaration on its inside front cover and should inform its subscribers and readers of its decision.

Facts

Gerry, in a letter included at the end of this response, apologises on your behalf for your late response. Apology accepted. Nevertheless for the record, I do want to establish certain facts not to accuse anyone of personal misconduct but so that matters of substance may be addressed independent of disputed issues of evidence.

In issue 81 of *C&C* dated September 2003 there appeared two articles on value theory. These were

- (1) Guglielmo Carchedi's 'Note on Chris Arthur's dialectics of Negativity'
- (2) Simon Mohun's 'On the TSSI and the Exploitation Theory of Profit'

Chris Arthur submitted a response to Carchedi which was published in issue 82, that is, the next issue. This is the most rapid response possible, around two years faster than the journal's normal turnaround rate for refereed articles.

Kliman and Freeman submitted a reply to Mohun in February 2004 conforming to the guidelines published for the 'polemics' section in which Arthur's reply was published. In September 2004 Kliman, having had no response, wrote requesting information. You contacted Freeman on 2/9/4 saying the article had been lost and asking for information. Freeman re-sent the article and your secretary's confirmation we had received seven months earlier.

Freeman requested an update verbally in January and, finally, we received your letter in March 2005 rejecting our article. The article was not sent to external

referees which is normal practice when the editorial board is not competent to assess the content of an article.

Gerry states on your behalf that he believes a response was sent to us in September 2004. This does not seem possible, since on 2 September you did not, according to your request to Freeman, possess the article in question. Thus Arthur's response to Carchedi was published with a three-month delay, whereas Freeman and Kliman did not even receive a reply until thirteen months had elapsed.

Arthur's letter to the EB stated:

Dear Eds

In the current issue there appears a polemic against me. I fear I shall be forced to exercise my right of reply. This is primarily because he attributes to me views I do not hold, as any wide-awake referee should have pointed out to him.

Any info. on relevant deadlines would be welcome.
Chris Arthur

You published Chris Arthur's response to Carchedi in your *Polemics* section. Given this, establishing *de facto* that a right of reply was indeed editorial policy, Kliman and Freeman spend considerable time preparing a very precise reply to Mohun refuting his misrepresentation of our positions and conforming to C&C guidelines for the *Polemics* section as outlined on the inside rear cover. You rejected this response to Mohun, published in the same issue as Carchedi, on the grounds that

“The focus of the piece is far too narrowly specialised. The potential audience is extremely small.

“The piece is impenetrable to anyone other than the small group of people working in this field.

“The piece inevitably deploys a specialist jargon throughout which assumes a specialist understanding of the existing literature.

“In the second half the paper is liberal in its use of algebraic formulations which is against the well-established editorial policy of C&C.”

You further state that the article is 'too polemical' (for the *Polemics* section?) and finally say:

C&C does not have an unconditional right to reply policy. Submissions are evaluated according to their merit in the view of the editorial board.

Substance

This deals with the facts. I now turn to the substance.

Within value theory there are two fundamental positions: the equilibrium interpretation of Marx which is defended by Mohun and Arthur and opposed by Carchedi, Freeman and Kliman; and the temporal interpretation which is defended by Carchedi, Freeman and Kliman and is opposed by Mohun and is not supported by Arthur.

Within the equilibrium interpretation which is the universal mainstream interpretation, Marx contains insoluble contradictions and his economic theory has to be rejected. An inevitable and logical conclusion is that it is academically and scientifically illegitimate to conduct research and debate in Marx's framework. Within the temporal interpretation these contradictions do not exist and it is therefore not only legitimate but scientifically necessary to conduct research and debate in Marx's framework. At stake, therefore, is whether or not there is a scholarly basis for the publication of works in Marx's framework.

Between publishing Arthur's reply to Carchedi, and rejecting Freeman/Kliman's reply to Mohun, the EB changed its policy. Moreover, the treatment you accorded Freeman/Kliman, when compared with the treatment you accorded Arthur, clearly and violently discriminates in favour of a particular point of view, namely that defended by Mohun, which is additionally the dominant interpretation of Marx as an equilibrium theorist.

Finally the dominant interpretation claims that Marx's own theory of value cannot logically hold and is unsustainable. The TSS interpretation supported by Kliman, Carchedi and Freeman establishes that the dominant interpretation of Marx is false, and hence establishes that Marx's own theory of value can logically hold and is sustainable.

It is important to understand that this is an issue of ideas, not people. Pluralism requires that there should be a fair and open debate between the conflicting paradigmatic approaches; the right of reply is necessary precisely as a guarantee of this and is therefore not a personal right but a scholarly duty. If you do not apply it, you present only one side of a debate and cannot arrive at truth, which depends on evaluating the entire range of theories dealing with a body of evidence and the entire body of evidence with which they deal.

The debate has hitherto been conducted in mathematical terms, of which later. We went out of our way to reduce the algebraic formulations and as we pointed out in our covering letter, our submission contains 3 equations compared with 26 in the piece to which it responded.

The policy of carrying material with algebraic content is clearly established to the extent that the XC and EB together agreed, when appointing a new copy-editor, that the ability to proofread mathematical text was a requirement of the post. Moreover C&C did carry Mohun's article, which contains eight times as much mathematics as the Kliman-Freeman response

The "well-established editorial policy of *C&C*" to reject material containing algebraic formulations was, in short, established for the first time in your response to our piece.

The charge of specialist jargon is equally misleading. Every discourse contains specialist language. The editorial board is entitled to reject submissions which contain unnecessary jargon and to request a simpler exposition, but you did not do this. To the contrary you write that the piece "inevitably" deploys specialist jargon which asserts that it is impossible to avoid such jargon; your editorial decision is,

simply, not to carry material on value theory because it cannot be expressed in what you consider to be an accessible language.

Thus this is not as you assert an assessment of the merit of this particular article. It is a new policy, that articles on value theory will 'inevitably' be inaccessible to *C&C* readership from which it follows that, no matter what the merit of any article you receive on value theory, you will on the above grounds refuse to publish it.

Moreover the issue is not whether we carry articles containing specialist terms, but which specialisms we choose to prioritise. Chris Arthur's reply to Carchedi is expressed in the language of the Hegelian dialectic. I am very pleased about this. I have great interest in Hegelian dialectic and particularly Chris Arthur's illuminating work on it. However I have no idea why you think it is accessible to a larger audience than the language of mathematics. The mathematics in our article is taught to every second-year student of economics and all students of natural science. How many second-year students of anything are taught Hegelian dialectic?

As with your policy on algebraic content you have introduced on your own account a new and discriminatory policy at the precise point where only one side of the debate had been presented, informing no-one of this change until last week.

Thus, in summary, by introducing a unilateral and arbitrary change of *C&C's* editorial practice in the middle of a debate in progress, your decision discriminates in favour of a view which holds that Marx cannot possibly be right, and rules out the future possibility of publishing any possible response to this view, that is, it rules out further presentation of the case that Marx's theory is valid. I consider this utterly incompatible with the aim of developing 'a materialist critique of capitalism in the Marxist tradition'.

The discrimination is moreover extreme:

- (1) Both Carchedi and Mohun were published in the same issue of *C&C*
- (2) Arthur's response was published in the immediately following issue
- (3) Freeman and Kliman's response was lost and forgotten. We reminded you and re-supplied you with this response which was again forgotten. Finally, after further verbal pressure, you rejected our article.
- (4) The rejection was sent with thirteen months delay. This compares with three months to process Arthur through to publication.
- (5) You rejected our reply on grounds that, had they been applied to Arthur, could not but have led to its rejection.
- (6) You rejected our article on the basis of new editorial principles which were applied neither to the article to which we responded, nor to Arthur's submission.

One more point has to be made, no matter how harsh a judgement it may seem – and once again I have to stress that the issue for me is the consequence of a wrong policy that should be changed, and is in no way intended as a reflection on the personal characteristics of anyone on the EB.

Arthur asserts Marx is wrong. Freeman and Carchedi assert Marx is not wrong. If you sustain your position you will, though possibly unaware of it, go further than merely negating the right of reply and further than merely declining to publish works on value theory. You will make C&C actively responsible for promoting the prevailing hostility to Marx, and will commit it to the active suppression of scholarly works which establish the misrepresentation of his views and the falsehood of the attacks upon him. You are of course entitled to do this: what you are doing should, however, be transparently and openly stated, so that authors may decide whether or not to write for us and subscribers may decide whether or not to read what they have written.

Issues

Right of reply

I am grateful that you clearly state that in your opinion C&C does not offer a right of reply. The point is, it should. If it doesn't, it cannot fulfil the editorial mission expressed on its front inside cover.

The issue is not whether the right of reply is conditional or unconditional but whether authors have any such right at all. If there is such a policy, you have a duty to state under what conditions it is applied and to apply it consistently. The assertion that submissions are determined on 'merit' as (subjectively) determined by the EB is simply another way of saying that the policy is applied under no conditions and the EB will do whatever it wants unrestrained by either principle or consistency – as is shown by the difference in treatment accorded to Arthur and to ourselves.

Debate, dissent and difference are the lifeblood of a democratic left. The mere fact that your response places the journal on the right wing of a spectrum that includes the National Union of Journalists on its left should, I hope, give you pause to think. As the NUJ states:

The NUJ's Code of Conduct has set out the main principles of British and Irish journalism since 1936. It is part of the rules and all journalists joining the union must sign that they will strive to adhere to it.

1. A journalist has a duty to maintain the highest professional and ethical standards.
2. A journalist shall at all times defend the principle of the freedom of the press and other media in relation to the collection of information and the expression of comment and criticism. He/she shall strive to eliminate distortion, news suppression and censorship.
3. A journalist shall strive to ensure that the information he/she disseminates is fair and accurate, avoid the expression of comment and conjecture as established fact and falsification by distortion, selection or misrepresentation.
4. A journalist shall rectify promptly any harmful inaccuracies, ensure that correction and apologies receive due prominence and afford the right of reply to persons criticised when the issue is of sufficient importance.

Mohun's article, which you published, contains a harmful inaccuracy. It is harmful, to be precise, not just to countless individual scholars against whom it will be presented as truthful grounds for rejecting their work but against Marx himself and hence against the mission of the journal itself. Our response, using a great deal less jargon and mathematics than the original, corrects this.

Value theory

Turning now to pluralism, your principal ground for rejection is that the article is "impenetrable to anyone other than the small group of people working in this field." As established above, this is not a judgement on its merit but an editorial judgement that no article of this type should be carried henceforth. This is precisely what makes the issue one of principle, and not a personal dispute between the authors and the EB.

Perhaps what you really mean is not that the issues are inaccessible to the readership but that they are inaccessible to the editorial board. There is a simple remedy for this: you should recruit editors, of whom there are many, who do understand the issues at debate.

Your judgement is demonstrably wrong. Your assertion that interest in this topic is small reflects only the narrow composition of the EB. The central issue in the debate around Brenner's work in *New Left Review*, probably the biggest debate on economics on the left for a decade, is precisely that of value theory.

Brenner's endorsement of the prevailing interpretation of Marx and his consequential dismissal of Marx's explanation for the Falling Rate of Profit has been one of the central topics in London seminars of well over 200 people.

The debate on hermeneutics, sparked precisely by this and similar discussions in other schools of economic thought, attracted an audience of 60-80 people at last year's conference of the Association for Heterodox Economists.

Freeman and Carchedi's book *Marx and Non-equilibrium Economics* was purchased by 592 readers, somewhat more than half the readership of *C&C*. Kliman, Freeman and Wells' book *The New Value Controversy* has in one year sold 273 copies. Conferences of the International Working Group on Value Theory in the USA attracted at their height an audience of nearly 100, the number of papers being so large that we had to hold parallel sessions. It is hard to avoid noting that *Capital and Class* has been unable to organise a conference of any kind for the last three years, though it is to be hoped that the proposed 2006 conference will rectify this.

Your sister journal *Review of Radical Political Economy* systematically and regularly carries mathematical articles, as does a heterodox journal nominally to our right but with substantially larger sales than ours, the *Cambridge Journal of Economics*. *Historical Materialism* regularly carries mathematical material and is one of the most successful new journals on the British left.

But even if it were true that this issue was accessible to only a small minority your justification for excluding it is misguided and fails to understand what pluralism is, and why it is essential.

First and not least, *C&C's* declared aim of working with movements of liberation necessarily requires us to provide a space, and a voice, for multiple viewpoints which should be self-determined from within the movements themselves. There is not a single modern idea that was not at its inception a minority and poorly understood view. The difference between the orthodox and suppressive policy that is general throughout economics and the social sciences, and the radical policy for which *C&C* has stood until now, is that radicalism publishes what is new and challenging and orthodoxy publishes what is old and safe. *C&C's* just reputation arises precisely from the fact that it has sided with the minority in allowing it expression at a time when orthodoxy would have none of it. Thirty years ago, Green politics were virtually unheard of. The Labour Process debate was confined to the readership of Braverman's single book. Critical Realism was an eccentricity known only to a religious academic fringe. Feminist Economics did not exist and traditions now universal, such as avoiding gender-oppressive terms like 'he', were largely rejected as an unwarranted interference with editorial freedom. If your approach had been applied when *C&C* was young, it is unlikely to have considered a single one of the articles it is now famous for.

Secondly, I think you have not given adequate consideration to the scientific significance of your decision. Your decision is essentially subjective. You take no account of the intrinsic importance of the question. This creates the ludicrous spectacle of an avowedly Marxist journal that does not allow a discussion about Marx. What would anyone think of a Keynesian journal that did not discuss Keynes?

Marx's general political, sociological and philosophical theories are, if divorced from his theory of value, quite frankly marginal notes. It is the centre of his entire conception of what capitalism is about. It is the reason that *Das Capital* is the only work which he dedicated his life to bring to publication. The requirement to discuss Marx's value theory would exist no matter how few people were interested. The appropriate decision for the EB, if it really believes that this group of people is so small, would have been immediately to dedicate resources to changing this lamentable fact, seeking out and encouraging the debate, finding competent referees, organising special issues, and so on – just as it has so successfully done on other pioneering questions, and which is indeed its hallmark.

This is clear once we ask who is the 'small group' to which you refer? It is none other than the Conference of Socialist Economists whose journal you currently edit, which as anyone can see from the website has not only systematically carried debates on value theory from its inception, but has been the world's leader in this respect. *C&C* carried the debate about Marx's value theory from the very beginning, and this is indeed what it is best known for.

Mathematics

There is a valid discussion to be had about the extent to which the discussion of value theory requires mathematical language. This discussion is however the property of the readership and membership to which you as EB are accountable, and the movements with whom you seek to work to establish your critique of Capitalism.

I have already addressed the main issue which is that you have introduced a discriminatory principle against one particular specialist language. That said, there is of course a need to restrict and limit specialist language (of any kind, not just one kind) to the minimum technical necessary. In this respect the published guidelines of, for example the *Cambridge Journal of Economics* show that it is perfectly possible to limit mathematical formulations to a necessary minimum without denying the right to use such formulations where they are indispensable to the argument.

However the question 'how much mathematics is acceptable' cannot be divorced from the past of the debate. This is clear from the fact that in rejecting our reply to Mohun you have *changed* the previous policy. Mohun's article contains an explicit polemic against us in this very same impenetrable language, was published, and we did no more than reply to Mohun in the language which he himself used and you yourselves published.

What you have done is arbitrarily curtail the debate at a specific point at which it was incomplete, permitting one side to employ a discourse that you then deny to the other: this destroys all possibility of pluralism.

The real point is that it was absolutely right to publish Mohun's article, and you should have continued the editorial policy that led you to publish it, by publishing a reply. Moreover you should continue, as *C&C* has done since Issue Number 1, to publish material from all standpoints which contains algebra necessary to its content, just as it has permitted any and every theoretical current and discipline to use such specialist terms of discourse as it requires to make its specific point. That is what pluralism is about.

What makes the mathematics central to pluralism in this case is the question of interpretation. The attack on Marx has been conducted in mathematical terms. Indeed central to the invidiousness of this attack is the spurious claim that Marx was mathematically incompetent and that modern 'algebraic' methods have shown this.

But actually, the algebraic methods introduced by Marx's detractors have proven a vehicle for a completely different theory of value which has nothing to do with Marx's ideas and to which Marx was violently opposed. This theory is, however, promulgated as being Marx's own theory. To set the record right, it is therefore indispensable to establish that this is an act of misrepresentation. Therefore, if you deny the right to respond to Marx's calumniators in the language that they themselves introduced (and which *C&C* has carried for thirty years) you participate in perpetuating the censorship of Marx's own actual views which is systematically practiced throughout the economics profession.

We – those in the IWGVT and others who have challenged the ruling orthodox interpretation of Marx – would like nothing better than to set aside the oppressive mathematical discourse which Marx's detractors have used to substitute their own ideas for his theory. We would like nothing better than to deal with the real substance of Marx's theory shorn of unnecessary baggage (although any quantitative study requires an irreducible minimum of algebraic formulations, there is no need for these to be anything like as impenetrable as the past thirty years have been.

The problem is the following: the *attack* on Marx is framed in mathematical terms. For thirty years the editorial policy of *C&C* has been, in the interests of pluralistic discussion, to carry this attack. Kliman, Freeman and everyone in the IWGVT agrees with this, and its own conferences have always provided a completely open door for the supporters of all positions on value theory. But *in order to move on* to a non-mathematical discussion of the real substance of Marx's value theory, the mathematically-framed attacks have to be dispensed with and their falsehood has to be proven. If we were to simply assert without proof that Marx is right and his critics were wrong, then Ben Fine's insulting designation of such views as 'fundamentalist' would not be far from the mark.

Actually, we would prefer not to use the language of mathematics. As noted our article consciously restricted mathematics it to the minimum necessary to respond to a falsehood also stated in the language of mathematics. How else are we supposed to reply to an article which you yourselves published and which uses this self-same language to establish its case?

You have chosen to shut down the debate at the precise moment when a real and substantive response to a thirty-year mathematical onslaught on Marx has emerged, and when this onslaught itself is clearly running out of arguments and logic – on the grounds that its logic is inaccessible! Even were it possible to close out the excess of mathematics having completed this debate, this cannot be done *now* and most of all it cannot be done by fiat in the middle of the debate.

This does an incalculable disservice to Marx, to Marxism, to science, to knowledge, and not least to your readership. You will perpetuate for many unnecessary years, just at the moment when mass movements across the world are returning to Marx's ideas, the demonstrable falsehood that his theory is mathematically false, at the precise moment when the refutation of this falsehood is at last available to the general public.

We, and many others, wish to write in Marx's framework. This is what *C&C*'s own editorial policy, on its inside front cover, states. But this cannot be a matter of religious belief. There is a real debate and it has to be conducted, and finished. The mathematical refutations of Marx which *C&C* has carried for thirty years are false. *C&C* – and any journal that seeks to call itself 'Marxist' has to set the record straight by carrying, and fully facilitating, the response to the falsehoods that it has published for thirty years.

Moreover there is a special responsibility precisely because you choose to call yourself Marxist. Actually, currently there is nowhere else that such a debate can be conducted. Neoliberal, neoclassical, and even simple heterodox economic journals, will not carry this discussion precisely because, ironically, although they are perfectly happy to print mathematical material on topics other than value theory, they consider value theory itself a specialist topic for discussion in 'Marxist' journals. If the Marxist journals do not carry mathematical treatments of value theory, then in the present relation of forces, value theory will not appear anywhere. We will have participated in an almost universal academic suppression that now prevails.

Better to stop calling the journal Marxist, so that the Marxists can have a proper and clear fight to make the non-Marxist journals carry Marxist material. Otherwise it will

provide the excuse which allows the neoclassicals to censor Marxism; they will reject Marxist material on the grounds that it can be published in the Marxist journals – while the Marxist journals reject it on the grounds that they do not permit a discussion of Marx's theories. This is utterly intolerable.

Remedies

I find myself in an impossible position. I have no intention of using either my influence on the XC, or some kind of 'conditional resignation', to strong-arm the EB into doing something it doesn't want. It serves no purpose to push through formal resolutions which do not reflect the will of the EB. The only solution is if the EB itself changes its view. My impression, which I hope is wrong, is that the EB is extremely determined in its attitude and is unlikely to change, and for this reason with great regret, I cannot see the purpose of maintaining an association with *C&C*.

I think the future of *C&C* will be determined by whether or not it can re-found itself in a new relation to the emerging radical movements that took part in the European Social Forum – the biggest conference of any kind ever held in Britain – and in the new critical spirit in Economics that has manifested itself in the Association of Heterodox Economists. These movements will need to publish. The need to give expression to the views contained within them will not go away just because you have given up doing it. However, anyone that wants to fulfil this role will need to face up to past failures, analyse their reasons, and on that basis establish a different practice.

What is clear to me from this experience is that a totally new approach is required to the way in which the business of editing and publishing is conducted. Publishing is, to quote de Gaulle, an 'exorbitant privilege'. It conveys personal power and prestige on those who do it, and the left is not exempt from the distortions that this power introduces – indeed, if it does not establish the appropriate safeguards, it is perhaps more vulnerable even than the orthodoxy it seeks to replace, a sad state of affairs which in itself contributes in no small measure to its permanent self-imposed isolation.

There is no remedy available that relies simply on finding 'good people' or 'trusting judgement' or which resorts to vacuous and un-evidenced assertions about editorial policies that are never documented, never made available to authors or referees, and most of all never submitted to the judgement and decision of the readers. Even less is it a remedy simply to declare oneself to be a Marxist and hope that the name, like some kind of magical incantation, will confer exceptional editorial powers.

The outcome of this sad affair shows that what is required are principles: formal guidelines, that state exactly how an Editorial Board arrives at its decisions and which are subject to scrutiny and change

These transparent principles regulating editorial decisions will need to include formal instructions to referees, clearly and transparently stated guidelines for authors, a formal right of reply (which must of course be conditional – the issue is to state what the conditions in fact are) and a formal right of appeal against failure to adhere to such guidelines.

I am submitting proposals to the AGM which will I hope give people time to think, and will also I hope influence future EB members who may not share your present views. To minimise a conflict which I do not expect to be small, I am proposing simply that guidelines should be drawn up by yourselves, not imposed upon you.

Having said that, in the absence of such guidelines I think it will prove impossible for the journal to fulfil its declared mission. If there is no pluralism then there can be no critique, least of all in a Marxist framework; if there is no transparency, you will have lost the right to work with movements of liberation and they will rightly turn aside from you as having standards lower than their own; as having abandoned the very principles of justice, democracy, openness and equal rights for which they themselves are in constant battle with authority and orthodoxy.

Documents

Anna Melamed
C&C editorial Board
25 Horsell Road
London N5 1XL

Wednesday, 21 January 2004

Dear Anna

We enclose six copies of a submission to *C&C*, entitled "Replicating Marx: a reply to Mohun". It is jointly written by ourselves, Andrew Kliman and Alan Freeman.

This article replies directly to the article published in *C&C* Number 81 (Autumn 2003) by Simon Mohun. It is 3,000 words long.

We therefore think it fits in the category of 'Polemic' which the guidelines in *C&C* describe as "shorter, controversial pieces with a maximum length of 3,000 words" and would like the editorial board to consider it for this section, which we hope will encourage the already substantial debate on value theory within the pages of the journal.

For the same reason, if the article is accepted by the editorial board, we think that the debate would benefit from a relatively early publication date.

Also for this reason we have kept the mathematics to a bare minimum in order to ensure that the argument is accessible to the reader. However among other issues addressed by our response is a key qualitative assertion of Mohun which depends on a mathematical error that misinterprets our own view.

Mohun's article contains 26 equations. Our response contains three. We hope the board will agree that it is impossible to contest his presentation of our own view without referring to the equations which he uses to support his critique of it, and which have already appeared in *C&C*.

We hope that you will be able to bring these matters to the attention of the editorial board in the process of drawing their attention to our submission.

Please address correspondence to Alan Freeman at the address above; he will ensure that the same correspondence reaches Andrew Kliman

Yours faithfully

Andrew Kliman
Alan Freeman

From: Anna Melamed
Sent: Wednesday, February 11, 2004
To: Andrew Kliman
Subject: Reply to Mohun

Dear Andrew

Thank you for your polemical reply which we received in the post today. It is being considered by the editorial board.

From: "Drewk" <Andrew_Kliman@msn.com>
To: <cseoffice@gn.apc.org>
Subject: status of submission
Date: Mon, 23 Aug 2004 13:01:33 -0400

Dear Friends,

I am inquiring into the status of "Replicating Marx: A Reply to Mohun."

Alan Freeman and I co-authored this paper and submitted it for the "polemics" section of C&C nearly seven months ago. I received a message acknowledging receipt of the manuscript, but nothing since that time.

Thanks in advance for checking this out.

Andrew Kliman

From: Gerry Strange [mailto:gstrange@lincoln.ac.uk]
Sent: 02 September 2004 18:23
To: afreeman@iwgvt.org
Subject: FW: status of submission

Alan,

I'm afraid there's no evidence at this end the the submission referred to below has been received by the office or the EB. Can you shed light on this?

Date: 5 Sept
From: Alan Freeman
To: Gerry Strange

Gerry

Andrew received the following confirmation from Anna in February, which he forwarded to me at the time.

Hope this helps

Alan

From:Gerry Strange [gstrange@lincoln.ac.uk]Sent:Fri 18/03/2005 12:25
To:Alan Freeman
Cc:cseoffice@gn.apc.org
Subject:Re: Replicating Marx

Dear Alan

This is to confirm our brief discussion after the last exec meeting regarding your joint authored article (with Kliman) "Replicating Marx: a Reply to Mohun", submitted in February 2004. I must apologise for the apparent lack of communication although my understanding is that a letter was sent to you in September 2004. However, I have not been able to find a record of this letter.

As usual, the article was read by two internal referees. Both were of the opinion that the article should be rejected. The article also stimulated a more general discussion at the editorial board regarding the processing of articles which clearly do not conform to C&C guidelines on accessibility and readability. As a consequence of this discussion policy is now to immediately reject (i.e. without review) articles which substantially breach these guidelines.

The following is a summary of the main points raised by the referees:

The focus of the piece is far too narrowly specialised. The potential audience is extremely small.

The piece is impenetrable to anyone other than the small group of people working in this field.

The piece inevitably deploys a specialist jargon throughout which assumes a specialist understanding of the existing literature.

In the second half the paper is liberal in its use of algebraic formulations which is against the well-established editorial policy of C&C.

The piece is too personalised and polemical in its attack on Mohun.

C&C does not have an unconditional right to reply policy. Submissions are evaluated according to their merit in the view of the editorial board.

Once again I apologise for the extremely delayed correspondence for which I take full responsibility.

Yours with best wishes

Gerry Strange (Chair, editorial board)

Resolutions (passed by Annual general Meeting of April 2005)

- 1) The AGM endorses the editorial statement of aims printed on the inside front cover of *C&C*.
- 2) To accomplish the aim therein stated of 'developing a materialist critique of capitalism in the Marxist tradition within the labour movement and other movements of liberation' *C&C* must be pluralist and therefore:
 - a) It should give expression, in balanced and non-exclusive debates, to the full range of theoretical views, interpretations and paradigms relating to the analysis of capitalism which are to be found in these movements.
 - b) It should in particular continue *C&C*'s thirty-year tradition of publishing material relating to Marx's value theory, for which there are almost no other recognised academic outlets.
- 3) This requires formal editorial guidelines and principles which should be transparent and available to authors, referees and readers; for the implementation of which the EB and XC should be accountable to the readership.
 - a) This should not offer any threat to editorial standards such as clear expression, minimum necessary use of specialist jargon, and normal rules of evidence. Pluralism indeed entails tougher editorial standards than normally applied in social science publishing, in that it implies a duty on contributors to engage with views critical of their own standpoint.
 - b) Nevertheless, recognising that all discourses contain specialist language including mathematical language, the guidelines should aim to restrict this to the minimum necessary and, in particular, should recognise that the pluralistic principle of non-exclusiveness in debate must take priority if inherent content unavoidably demands specialist language.
- 4) AGM therefore requests the EB in consultation with the XC to draw up editorial guidelines to authors and referees based on a commitment to pluralism. These guidelines should explicitly commit the journal to:
 - a) provide space for views that are excluded from mainstream or orthodox publications
 - b) provide fair and open debate between conflicting paradigms, theories and interpretations
 - c) recognise the right of reply
 - d) recognise the right of appeal against a failure to adhere to the journal's published procedures.

Appendix 2: The RRPE and the Right of Appeal

In January 1999, Andrew Kliman, at that time a member of the editorial board of the *Review of Radical Political Economics (RRPE)*, appealed the rejection of a paper he had submitted for publication, in a letter appended to this paper. *RRPE* policy guarantees the right of appeal. This was refused. Kliman therefore made the following motion, at an editorial board meeting of April 10, 1999, to ask that the *RRPE* henceforth honor authors' rights of appeal:

Whereas: *The Review of Radical Political Economics* “encourages articles from all perspectives within a broad definition of radical political economics. If an article is within this general framework, the editors of the *RRPE* are committed to judging it on its merits and scholarship, not in terms of the political point of view of the author(s).”

And Whereas: To help safeguard its democratic, pluralistic perspective, the journal guarantees “authors' rights to challenge a vote.”

Be It Resolved That:

- (1) Authors' rights of appeal shall henceforth be honored.
- (2) The Editorial Board instructs the Managing Editor to process appeals, using past practices and precedents as guidelines.

The *RRPE* editorial board subsequently proposed a new policy to argue that it was honoring the right of appeal. Kliman argued in a letter of May 5, 1999:

The editorial board has decided that, if the panel of referees does not revise its opinion, the managing editor will decide whether or not I will be permitted to appeal the decision. That my opportunity to appeal is now subject to her discretion makes it a privilege, and no longer a right. the editorial board's action therefore abrogates the long-standing right of authors to appeal votes of referees.

Kliman's argument against the new *RRPE* policy was thus the following: it forces an author to plead her case before the same people – referees and managing editor – who were involved in the rejection of her paper, Thus, if a violation of norms of pluralism has occurred, those who have violated the norms are allowed to decide whether they have done so, without any independent verification. A genuine right of appeal provides for *independent* individuals to decide whether to overturn the original decision on grounds of error or theoretical, ideological, or political bias.²

The reader may or may not wish to follow the detail discussion on points of mathematics involved in Kliman's appeal. The material is available should she wish to do so. For us the decisive questions in terms of future policy are the following:

² Rumors were subsequently circulated by the *RRPE* or persons associated with it that Kliman sued its parent body and agents for having rejected his paper. This rumor is false.

(1) Kliman argued that the referees had incorrectly exercised their function, having failed to judge the paper objectively. He sought an appeal procedure which would review their decision, in exactly the same way that an appeal court reviews the decision of a lower court, to establish whether the procedures followed by the referees were valid.

(2) Kliman argues that appeal is a right, and not discretionary. The issue at stake is not 'whether the EB is willing to change its mind'. It is 'whether the editorial processes are subject to independent scrutiny'.

In discussing the right of appeal in our paper, we intend both these two senses of the concept of 'right of appeal': it should be a right offered to anyone submitting material for inclusion or exclusion in a medium of dissemination, which provides an effective safeguard against subjective or biased judgement.

Andrew Kliman's Rejected Request for an Appeal

Hazel Dayton Gunn
Department of City and Regional Planning
106. W. Sibley Hall
Cornell University
Ithaca, NY 14853

January 25, 1999

Dear Hazel,

As I indicated on the phone earlier today, I am dismayed at the rejection of my paper, "Physical Quantities, Value, and Dynamics" (Msn. 1666). Since the grounds for rejection violate *RRPE's* policy, I hereby initiate an appeal of the decision.

Let me call your attention to the final sentence of the third paragraph of "GENERAL INSTRUCTIONS FOR EDITORIAL BOARD MEMBERS," which I received from you at the end of the summer when I began my term on the editorial board. It asks us to

Please also take note of the statements which appear in the journal regarding the general editorial policy of the *RRPE* in terms of political/ideological perspectives and authors' rights to challenge a vote.

In appealing the decision, I am thus exercising an already-existing right, a right that remains in effect according to the "GENERAL INSTRUCTIONS" issued only five months ago. No special decision of the editorial board is needed before the appeal process can begin.

As I indicated on the phone, I am ready and willing to raise the matter at the editorial board meeting in April. I am confident, however, that it can be resolved, amicably and expeditiously, well in advance of that time. Because my manuscript was first sent to two referees who, as you wrote to me in November, did not feel

qualified to review it, I had to wait six months for the first decision. I'm sure you will agree that the appeal process should not be similarly delayed.

The basis of my appeal is that the decision to reject my paper violates *RRPE's* non-exclusionary policy, printed on the inside front page of the journal:

The *RRPE* encourages articles from all perspectives within a broad definition of radical political economics. If an article is within this general framework, the editors of the *RRPE* are committed to judging it on its merits and scholarship, not in terms of the political point of view of the author(s).

My paper was written from the perspective of a proponent of the temporal single-system (TSS) interpretation of Marx's value theory, an interpretation that carries certain well known "political/ideological" implications. Thus, the paper is clearly covered by the above policy. However, Ajit Sinha — a referee and eventually the editorial coordinator for the piece — did not judge it on its merits and scholarship. Rather, he allowed his theoretical biases, as well as subjective personal factors, to cloud his judgment.

In challenging one of my major conclusions, Sinha denies the validity of its temporalist conceptual framework, writing that "Value can only be defined for a point in time." According to the *RRPE* policy quoted above, he is not entitled to use his disagreement with the temporalist perspective, its conception of value, as a ground for rejection. Similarly, in challenging another point of mine, he states that "price is a relative concept [only]." This again uses his disagreement with the author's perspective as a basis to reject the paper. Finally, he challenges another line of argument on the same basis as he challenged the first one, complaining about my "inconsistent accounting of price at a given time point."

(The other major basis for his negative vote seems to be that I misinterpreted Sraffa. However, Sinha has misinterpreted me. He writes that the author "considers the example itself to constitute a devastating critique of Sraffa's system." In fact, I made no such claim; the example is not even intended as a critique of Sraffa's system.)

That Sinha has allowed his personal feelings, not only theoretical bias, to cloud his judgment can be seen from the insulting, abusive nature of his referee report. He (a) labels my method of attribution "very unprofessional," (b) accuses me of "incomprehensible talk," (c) implies that I have not "read through the whole of Appendix B" of Sraffa's book (which I take up in the paper), (d) pronounces the use of a non-basic as a numeraire to be an "absurdity," (e) accuses me of "smuggl[ing] in his/her own inconsistent method of measurement," (f) suggests that I am dishonest and "highly unprofessional" ("opponents' theories should be presented in an honest and clear manner"), and (g) charges me with having introduced the paper with "ridiculous and bombastic claims."

Yet the problem is not only that Sinha voted to reject my paper on these grounds. In addition, he was seriously derelict in carrying out his responsibilities as editorial coordinator for my piece. Instead of reviewing another referee's report carefully and thereby noticing the error it contains, Sinha used the mistake in order to confirm his

own preconceptions and to gain the other referees' asset to his judgment that the paper be rejected outright. (Both had voted to urge that I resubmit the paper). Let me explain.

Because you first turned to two referees who, as you indicated in November, did not feel qualified to review my paper, Frank Thompson agreed to pitch in. He seems clearly to have reviewed it in a rush. His referee report states that "I am a substitute for a substitute referee ... and the resulting delay demands my quick response. Thus I will deal explicitly with only the initial argument in the paper upon which the remainder of the paper crucially depends." The report goes on to claim, and to try to prove mathematically, that the argument in question is invalid.

However, as I will show below, my argument is indeed valid. There is a straightforward, elementary, and easily detectable error in Frank's math. Since he is highly competent in this area, the error is no doubt attributable to the haste with which he reviewed my paper.

Sinha seized on this error, using it as an opportunity to forge a consensus among the panel of referees. Other than reiterating his view that the paper "is flawed in a very fundamental way," the error in question is the sole substantive basis for rejection he provides in his letter as editorial coordinator. He writes:

Frank Thompson has shown that, even on your own premises, a critical proposition of yours is mathematically incorrect. And David Andrews is also not "convinced" of your central proposition that value rate of growth must be lower than physical rate of growth when labor productivity is rising; and he laments a lack of argument on this count.

The correctness or incorrectness of this proposition is thus a factor of central importance in the referees' decision. Its alleged incorrectness was *the sole* basis Frank gave for urging that I resubmit the paper, rather than accepting it. Sinha's referee report also alleges that it is incorrect, and David Andrews voted to urge resubmission largely because he was not convinced that my mathematical results in this section were correct. (His other two comments can be dealt with easily: a request to make the paper more accessible to non-specialists, and a request to explain why it is "paradoxical" that alternative physical value measures can lead to different economic outcomes.)

Thus, had Sinha noticed the error in Frank Thompson's report carefully, and thereby realized that his own intuition was incorrect, it seems that the ultimate decision may well have been more favorable.

Because so much is riding on whether it is my proof or Frank's disproof that is in error, it seems incumbent upon me to show that it is the latter. For simplicity, I limit myself to the continuous-time version of his argument, not the discrete-time version, though conclusions that are invalid in the one case are invalid in the other as well. In the third page of his report, Frank writes:

The continuous time argument is quite simple: While it is true that

$$K_v = I_v/K_v = (I_v/\pi_v)(\pi_v/K_v),$$

we need to unpack this formula to separate the change in the value of a unit of capital from the change in the number of (physical) units of capital, noting that

$$K_v = (\lambda K_p) = \lambda + K_p = \lambda + (I_p/\pi_p)(\pi_p/K_p) = \lambda + (I_v/\pi_v)(\pi_v/K_v) = \lambda + (I_v/\pi_v)r_v.$$

Thus, if all profits are invested, i.e.,

$$(I_p/\pi_p) = (I_v/\pi_v) = 1,$$

and unit labor value is decreasing, i.e.,

$$\lambda < 0, \text{ then } K_v < r_v.$$

This argument was made in order to refute the proposition that (given the appropriate assumptions) $K_v = r_v$. I will now show — *on the basis of Frank's own premises* — that this equality does indeed hold.

He states correctly that the first set of equalities are “true.” Thus

$$K_v = (I_v/\pi_v)(\pi_v/K_v). \tag{1}$$

The last pair of his second set of equalities is $\lambda + (I_v/\pi_v)(\pi_v/K_v) = \lambda + (I_v/\pi_v)r_v$, from which it immediately follows that

$$(\pi_v/K_v) = r_v. \tag{2}$$

Two lines later, he introduces the condition that

$$(I_v/\pi_v) = 1. \tag{3}$$

Substituting the right-hand sides of (2) and (3) into (1), we obtain

$$K_v = 1 \cdot r_v = r_v.$$

Q.E.D.

Let me also point out a related error. As Frank correctly notes, $K_v = (\lambda K_p) = \lambda + K_p =$

$\lambda + (I_p/\pi_p)(\pi_p/K_p)$. Thus if, as he assumes, $(I_p/\pi_p) = 1$, then $K_v = \lambda + (\pi_p/K_p)$. And since, from (4) we know, on the basis of the same assumption, that $K_v = r_v$, it follows that

$r_v = (\pi_p/K_p)$ if and only if $\lambda = 0$. This disproves the claim (implicit in the second set of equalities and also stated in page 2 of Frank's report) that $r_v = (\pi_p/K_p)$ also when $\lambda < 0$.

As you see, the algebra is trivial. Sinha would certainly have detected the error had his judgment not been clouded by bias.

The question now is what to do in order to rectify the situation. Let me reiterate the proposals that I made to you earlier today. *Either* of them is acceptable to me.

- (1) The paper and reviews are sent to a new referee, mutually acceptable to you and to me, who will act as arbitrator. That is, we will commit ourselves to abide by his/her recommendation (accept, accept with revision, urge resubmission, do not accept). If s/he recommends acceptance with revision or resubmission, s/he will also determine whether subsequent revisions have been made in accordance with her/his recommendation. If so, his/her recommendation will be acted upon.
- (2) Sinha's report is thrown out on grounds of bias. I revise the paper by making the revisions suggested by David Andrews and I show and explain why the objections leveled against my proof are invalid, thus addressing Frank Thompson's concerns as well. A new referee, mutually acceptable to you and to me, who will act as arbitrator, will decide whether I have done so adequately. In this case as well, we will both agree in advance to abide by this person's recommendation.

I am of course willing to consider other alternatives you may suggest. Given the treatment the paper has received to date, in violation of *RRPE's* policy of non-exclusion, I consider it essential that the paper be judged by a person or persons whom both sides can trust. I say this *not* only as an author, but as a member of URPE, a reader of the *RRPE*, and as a member of its editorial board. I think it is in URPE's interests, particularly when it is fighting discrimination on the part of the AEA, to show that its own standards and conduct are on a higher level, more scholarly and not tainted by political and ideological bias. It thus seems to me to be in the interests of *all* concerned parties that my paper be judged by someone qualified to do so, someone who was not part of the original review process, and someone who will act faithfully to carry out the letter and spirit of our journal's policy of non-exclusion.

I look forward to hearing from you soon. Again, I am confident that the matter can be resolved quickly and amicably, and without burdening the rest of the editorial board.

Sincerely yours,

Andrew J. Kliman

Assistant Professor, Economics

ⁱ The discussion that follows draws heavily on our "Introduction" (with Julian Wells) to Freeman, Kliman, and Wells (2004).

ⁱⁱ 'Single-system' interpretations that continue to adhere to simultaneous valuation (proposed by Wolff-Callari-Roberts, Ramos & Rodriguez, Chai-on Lee, and Fred Moseley) have met a similar fate. The 'New Interpretation' (or 'New Solution') of Duménil, Foley and others initially received a similar treatment.