

Why standards matter in heterodox economic publishing: the case of the Review of Radical Political Economics

Paper for the 2011 conference of the Association for Heterodox Economics

Nottingham Trent University, 7-10 July 2011

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For inclusion in the theme: "Withstanding regulatory capture: a pluralist code of conduct for heterodox economists" and in the panel (if accepted) "Round table on 'Issues in pluralist practice'"

Abstract

This paper aims to contribute to the development of a pluralist code of practice for economists through the study of a particular case in which, we argue, pluralism has been violated by a heterodox institution, the Review of Radical Political Economics. The violation led us to send an open letter to the editorial board, signed by fifteen leading heterodox economists, in which we requested the retraction of a book review which, we believed, seriously misrepresented Andrew Kliman's book *Reclaiming Marx's Capital*.

Had a misrepresentation of this order been published by an orthodox economics journal, we believe there would have been almost no question within the heterodox community that it was unacceptable, and indeed the matter could very well have become a *cause célèbre*. This implies that, in the minds of heterodox economists, there are two standards at work: one that they raise as a part of their criticism of orthodoxy, and the second which they apply in their dealings with each other.

We will argue that this profoundly wrong conception is responsible for the perpetuation of orthodoxy. It is precisely because heterodoxy has not, historically, developed standards of pluralist conduct applied to its own ranks, that it is so incapable, at moments of great crisis such as the present one, of mounting an effective counter to the regulatory capture of the profession which has led to its present state. We argue that a serious violation of pluralism has taken place, which is inimical to heterodoxy, and which can only be corrected by a retraction.

Keywords: value theory; code of ethics; economic methodology

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Introduction : the ethical responsibilities of heterodox publishers

This paper assesses the issues involved in a controversy, that began in 2009, after the Review of Radical Political Economics (RRPE) published two reviews of a book on Marx's value theory by Andrew Kliman (2007). In October 2010, fifteen signatories, including the authors of this paper, sent an open letter to the RRPE editorial board (Freeman and Wells 2010) which is reproduced in Appendix 1. This argued that one of these reviews, written by Ajit Sinha (2009), violated acceptable publication standards and should be retracted. It contained 'misrepresentations and falsehoods about the work under review,' and brought 'disrepute upon critical economics at the height of a crisis of confidence in mainstream economics'. The signatories requested a retraction. The RRPE board, in a letter reproduced as appendix 6, responded that it did not intend to retract.

Though we will subject the RRPE letter to examination, it is not the purpose of this paper to revisit the details of the dispute, on which readers can satisfy their own minds by reading the material reproduced in this paper and at a reference site we have established at www.iwgv.org/rrpe.¹ The purpose is to open a necessary and scholarly discussion on the *ethics of economics publishing*, in which the dispute itself figures as a case in point.

We will argue that any misrepresentation of opposing views is a serious breach of publishing ethics which calls for correction. As a methodological guideline we will pose the following question: let us suppose the review appeared not in a heterodox journal, but the *American Economic Review*, journal, and that the reviewed work was not Kliman's but Card and Krueger's (1995) seminal study disproving the orthodox case, until then uncontroverted, that the minimum wage resulted in job losses. Or perhaps a critical heterodox case for action against climate change. Let us suppose further the reviewers' case rested not on contrary evidence but on a simple and evident misrepresentation of the work reviewed. Would a correction not be called for? And if a retraction was sought by the authors, should the heterodox community not endorse their complaint?

It is almost self-evident that the RRPE board's response in such a case would differ significantly from its response to our open letter. This suggests there is a *de facto* dual standard in heterodox writing and publishing. Such a dual standard is inimical to the future of heterodox economics. We will argue that developing and adopting ethical standards among its *own* ranks is if anything, the *highest* priority facing heterodox economics .

First, heterodox economics should aim to reform economics: it should therefore show how to produce theories, teaching, and research, that are superior to what the profession has produced in the last thirty years. This cannot be done without standards. We cannot expect to call to account a profession dominated by orthodoxy, and which shows no interest in standards, unless we provide visible demonstrations that heterodoxy takes them seriously, and that they produce superior results.

Second, heterodoxy is the birthplace of orthodoxy. Every orthodoxy has at one time or another been a heterodoxy, starting with Walras and extending through marginalism, the Efficient Markets Hypothesis itself, econometrics, monetarism in its numerous variants and, in its time, the neo-classical synthesis itself. We can already see aspiring orthodoxies such as behaviouralism jockeying for a place in a new canon. If the heterodox movement itself does not school its aspirants in the

¹ The key material from the site, for convenience, is reproduced in the appendices to this paper. The remainder is accessible on www.iwgv.org/rrpe.

standards it expects of the mainstream, the best we can expect is that today's orthodoxy will give way to a new one, equally vulnerable to the systemic errors of the last, and equally suppressive of its less successful rivals.

Third, standards are guarantors of pluralism. Gratifyingly, pluralism has secured almost universal adoption among heterodox economists in the five years since Freeman and Kliman (2006), at the 2005 AHE conference, introduced the deeply contested view that pluralism was the *sine qua non* of heterodoxy. It is now the first article of the constitution of the newly founded World Economic Association. This constitutes progress. Yet almost no attention has been given to the paper's core argument: that effective pluralism will not appear unless scholarly and ethical standards are applied that define and guarantee it:

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

Moreover, the authors introduced the issue *not* in the first instance as a critique of orthodoxy but as a critique of *heterodoxy*. Heterodoxy, the 2006 paper pointed out, has an inherent tendency to function as a collection of cliques or doctrinal subgroups, each as suppressive of its opponents and dissidents as the orthodox institutions that now dominate the profession:

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 ... 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.

Much progress has been made since then, particularly within the AHE. The case in question, however, shows that this different way of doing things has yet to be adopted by many who claim to be heterodox, and all too many for whom pluralism is an attractive label, but have yet to think through what its defence really entails.

Truth, lies and the ethics of falsehood

The RRPE board's response concludes that no retraction is required, because 'We do not believe that the bounds of civil discourse were transgressed'. However the open letter did not raise civility as an issue. Its complaint is that the RRPE *published a falsehood*.

This, for us, is the primary issue. What is the purpose of scholarly debate? Why do we conduct research and publish it? To arrive at knowledge, which can benefit humanity. The open letter therefore raised, with the board, the complaint that it had published a falsehood, for the primary reason that this was a disservice to knowledge and thereby humanity. It is an issue of public ethics.

There are at least two aspects of knowledge: knowledge of truth, and knowledge of falsehood. Certain knowledge of economic truth is difficult to attain. Indeed this is why we need theory, research, and publication at all, since otherwise every citizen could understand the economy with no help from a special branch of the intellectual division of labour. This is why pluralism is required: the principal failing of economics is that it bases many of its most damaging policy recommendations on the false claim that they constitute certain truth and that no other answer is possible.

However though we may not know what is definitely true in economics, we can be sure that some things are definitely false. There are well-known, general usable methods of eliminating falsehood known throughout the sciences: the use of evidence, the application of logic, scholarly debate, systematic testing, and so on. There are also methods for detecting falsehood which apply the branches of evidence and reason in which economists specialise. We would reasonably reject an argument that could be shown to require the sum of value added in an economy to be greater than

the final consumed product, on grounds of economic reason; likewise we could reject, on grounds of reasonable evidence, a paper that required the monetary output of the USA to be currently smaller than that of China.

Thus, a workable starting definition of the scholarly duty of economists would be to eliminate falsehood, above all in testing and making judgements about economic policy and theory.

And indeed, on consideration we can see that eradicating economic falsehood is an ethical duty. The first words of the Hippocratic are 'First do no harm'. But economic falsehood does indeed do great harm, arguably more than any other falsehood. Economic judgements inform the actions of governments, in matters of great consequence for humans. The evidence is plentiful: structural adjustment and its effects, the Efficient Markets Hypothesis and the consequent institutional failure of economics to foresee or forestall the economic crash, the follies which led to the fates of Iceland, Greece, Ireland, and Portugal, and the austerity policies now being applied in Europe whose malign consequences are already becoming evident.

This is why the advice which Mishkin and Portes offered to the Icelandic Chamber of Commerce, so clearly exposed in Charles Ferguson's wonderful film *Inside Job*, was unethical. Not only did they provide the false advice that the Icelandic banking system was sound, but a large number of people were harmed by the course of action to which that this advice led. For an economics code of ethics, 'First do no harm', translates into or at least includes 'First tell no falsehood', or, to add precision, 'First tell no known falsehood'.

What methods are available to detect falsehood? Is there an absolute guarantee against it? Of course not, for the same reason that there is no absolute guarantee of truth. Our profession requires us, over and over again, to make judgements which are the 'best call' given the evidence and state of knowledge, knowing that experience may later lead us to conclude our judgements were wrong.²

However, and this is crucial, the lack of an infallible *and general* standard of falsehood does not mean there are no statements we can reliably identify as false. There are an entire range of statements which are known with considerable certainty to be untrue: for example, self-contradictory statements, statements like $2+2=5$, and statements which by general standards of scholarship conflict with available evidence, such as 'all things fall upwards'.

This is why the statements of Mishkin and Portes should be characterised as the deliberate promulgation of falsehood, and not merely mistaken, and this why their conduct is not merely misguided but unethical. They gave advice that presupposed that the finance system of a small country could sustain a foreign debt burden equal to nine times its GDP. This runs so contrary to all economic knowledge as to be equivalent to the statement that 'all things fall upwards'.

This bears directly on the duty of a publisher. The elimination of falsehood is the *function* of scholarly publishing. Of course, any economist is free to publish in the fantasy and fiction lists, and not a few serious mainstream works may well belong there. But *academic* publishing confers authority: lives are affected by decisions taken on the strength of what is given the *imprimatur* of peer-reviewed approval. Whilst falsehoods are an occupational hazard, the purpose of scholarship is to eliminate them. This is what procedures like peer review and editorial scrutiny are intended to achieve. A lone author may slip up, but an editorial board can catch such slips by means of an armoury of mechanisms. Its job is to deploy them. Its success in so doing is a hallmark of quality. It confers on them the ethical right to publish.

It follows that if a falsehood is published, editorial culpability is greater than for an individual author. It is at this point that the issue of retraction arises. Retraction, whilst not to be undertaken lightly

² It is for this reason that truth is not the simple opposite of falsehood. Statements concerning certainty, belief, or probability belong to what is known as modal logic. If a witness is caught in a lie, it does not mean that the truth must be the opposite of what the witness says. It simply means the testimony is not reliable.

and to be undertaken only with due safeguards such as right of appeal, is a legitimate and necessary ultimate recourse. Accidents happen; scientific journals have discovered that, despite their best efforts, they have lent authority to false statements they did not detect. The remedy – as recognised for example by the Medical profession, which has not a little in common with economics, is retraction. This is not an act of retribution but a means of reducing falsehood.

In conclusion, promulgating economic falsehood is a violation of public ethics. It does harm. The harm should be reduced, and retraction is a legitimate and necessary means to achieve this. An ethical policy on falsehood, which includes retraction of identified falsehoods, needs to be incorporated into a meaningful economics code of conduct. For all the reasons given in the last section, this is a duty on which heterodox economics should take the lead.

Falsehoods, damn falsehoods, and misrepresentation

This brings us to the question of interpretation and representation. Again, the editorial board's response fails to address the complaint of the writers of the open letter. It writes that "Questions of interpretation are different from questions of fact, and one job of a reviewer is to say what he thinks an author is arguing." Both parts of this statement are false.

If for example an author writes "Roses are red" and is reported by a reviewer as saying "Roses are blue" then this constitutes a false statement about the text. This is a question of fact. Interpretation is not free from the laws of evidence; we can distinguish valid interpretations from false interpretations, every bit as much as we can distinguish valid statements about the non-mental world from false ones. The distinction that the EB seeks to make is a false one.

Second, the job of a reviewer is not to say what he 'thinks' an author is arguing but to interpret the author without self-evident falsehoods. The reviewer is free to give her own opinion on the matters the author deals with, but is certainly not free to describe the author's views without regard to accuracy. A reviewer can think what she likes about the statement 'Roses are Red' and is free to say 'the author is wrong, for in fact Roses are Green', but if she writes 'This author claims that roses are blue' then this is a falsehood and the review is unacceptable. If it is published in full knowledge that it contains a falsehood, then it is a lie.

Moreover, the notion that interpretation is somehow free of the normal rules of logic and evidence is particularly perverse, because when we interpret a text – unlike when we research the external world – all the evidence is before us. The *only* evidence that an editorial board needs to consider, in judging a review of published text, is the evidence of the text itself. They do not need to worry that the world economy might throw yet another curveball, that the data is disputed, that economic evidence remains to be sifted, or concern themselves with any of the other great improbables and unknowns that haunt the conscientious economist. There is merely the text, and what is said about it. Precisely in the field of interpretation, it is uniquely possible – though not necessarily easy – to distinguish falsehoods and eradicate them.

Indeed, the board contradicts itself, having stated that interpretation is not a matter of fact, since it immediately enters a debate on the whether Sinha's interpretation is factually accurate:

Kliman did in fact [sic] write—and presumably wished to persuade his readers—that "The very existence of the TSSI [has a consequence that] the allegations of inconsistency are unproved...." The inference that Sinha drew from this remark may or may not be what Kliman meant, but it is not obviously a "significant inaccuracy" or a "willful falsehood"; no doubt Sinha would contend that it is a plausible reading. Readers ought to be allowed to decide for themselves

The board thus offers evidence of its own that Sinha's interpretation of these words did not constitute a falsehood. Its defence is moreover weak in the extreme and indeed, compounds and comes close to repeating Sinha's own falsehood. At issue, in the dispute with Sinha, is precisely the

interpretation of the words 'the existence of the TSSI'. As our open letter makes totally clear, the only reasonable meaning to attach to these words is the existence of a body of theory, just as is the theory of gravity or, for that matter, Sraffian economics. Sinha, to the contrary, perversely, against all the evidence of the text, and having been clearly advised of his error more than a year prior to publication, deliberately falsifies this to mean 'a group of scholars', that is, a set of people who have merely asserted a theory without testing it. This is not an 'inference' any more than '2+2=5' is an 'inference' from 'two plus two equals four' on the grounds that the reviewer 'thinks' that when the author wrote 'four', he clearly meant '5'. Sinha's presentation of Kliman's arguments is simply false, which the reader can easily verify to her own satisfaction if she reads the evidence.

But the very fact that the EB has chosen to debate what Kliman says, contradicts its claim that questions of interpretation are different from matters of fact. If this is so, then why enter into a discussion, however ill-thought out, of the factual veracity of the interpretation?

Having established that questions of interpretation indeed are questions of fact, one is of course entitled to ask how important they are. The board's response is that interpretation does not matter: that the standards one applies in judging statements about the physical world become somehow unnecessary when we are dealing with the mental world and its products.

Let us apply our methodological test to this idea. Suppose an *orthodox* or mainstream journal were to carry a self-evident misrepresentation of a heterodox work, what would our response be? What should we require of orthodox or mainstream publications, to correct the degeneration of economic theory over which they have presided? In short, does interpretation *matter*?

To answer this, let us begin with the best-known episode of misrepresentation concerning economic theories in US history. This was called McCarthyism. Let us recall how 'communism' – and with it, virtually all policy alternatives to those in favour with the business class – got to be discredited. There were two mechanisms involved. The first was the usual deployment of the repressive apparatus – denial of jobs, imprisonment, in the extreme electrocution. But the principal, greatly more pervasive, and highly effective mechanism was the one invented by Goebbels – the repeated lie. The lies involved were, moreover, not lies about the 'facts' but *about what people said*. Misrepresentation was the primary instrument of McCarthyism.

Redbaiting ensconced, in the public domain, the idea that every statement of the mildest progressive views was a declaration of disloyalty and treachery. How could such an idea become current or even receive an audience in a 'civilised' country? Because Senator McCarthy, his predecessors and his acolytes, presented the views of the advocates of child labor laws, of peace, of women's rights, or indeed of communists themselves, as something other than what they were. They took their words and turned them into something these words never said. In short, they misrepresented them.

McCarthyism – both in society in general, and in academia in particular – got to where it was by misinterpreting, consciously, deliberately, and systematically, and without regard for evidence, not about the way the world worked, but about what people said about it. Moreover, sad to say, McCarthyism made heavy and effective use of the left's own scant regard for the duty of truth: what turned witch-hunting from an isolated rant into a repressive and barbaric social movement, as Miller's *Witches of Salem* so graphically portrayed, was not the power of the witch-hunters, but the eagerness of the hunted to convert into hunters.

Nor is this predilection for casual denunciation a mere passing or past phase in US history. Study, today, the wild attributions of the Tea Party leaders. Consider how health care is presented as socialism, socialism as dictatorship, and Obama as a Kenyan Muslim Bomber. In the USA *of all countries*, the notion that interpretation should be subject to no scrutiny, that there is no academic duty to avoid misrepresentation, is not merely unethical but verges on scandalous. The history of the United States itself – in which RRPE is based – should surely demonstrate that due diligence, when it

comes to an accurate representation of what a citizen actually says, holds, or believes, is not something to be abandoned or forgotten lightly.

Interpretation, the production of correct theory, and academic freedom

We next consider what might be the particular and distinct duty of the scholarly publication, which is after all a special domain of public life, in which moreover, issues such as academic freedom and the pursuit of pluralistic debate are of special importance. What is the duty of the intellectuals faced with the universality of the public lie? Are academics exempt, in their work, from the very standards of probity that they themselves demand of public life? To the contrary, the intellectual has a *special* duty to function as the upholder of truth. We can make no sense of the idea that academic standards of truth should be lower than those of public life: to the contrary, the ethical duty of intellectuals is to serve as guardians and protectors of truth in public life.

The editorial board of RRPE offers three justifications for publishing a false representation of Andrew Kliman's views. The first is that they gave these views a lot of attention. The second was that the false review was published beside a different review. The third is that to suppress the false representation would constitute a restriction of academic freedom. We deal with the first two in this section, and third in the final section, of this paper. The RRPE board states that:

Sinha's critical assessment was published alongside Rick Wolff's glowing appraisal of Kliman's book. Reclaiming Marx's Capital received twice as much attention as we typically devote to any book, and about half of that attention was unambiguously positive—circumstances which might be expected to stimulate reader interest in the book. We therefore do not see that there are any substantive grounds for complaint about the treatment the book received in the RRPE...

Both these arguments are both false and irrelevant. First, the issue was not the 'amount of attention' given to Kliman's book. The EB have clearly confused the function of a scholarly journal with that of a boxing promoter. At the hands of Senator McCarthy, Marxism and Communism received more attention than at any time past or present in US history. This attention deprived the great majority of Communists of their livelihood and some of them of their lives. In the 1980s when Paul Samuelson was busily engaged in purging Marx's ideas from academic discourse, these ideas received a great deal of 'attention', which successfully buried the victim of the attention. It was the falsehood that was seized on by institutions only too happy to seize on 'official' evidence to support the suppression they wished to conduct. 'Attention' protected nobody and nothing.

Second, the publication of a falsehood is not corrected by publishing a truth next to it. A falsehood does not become true if it is published next to a truth. Truth does not consist of a 'balance' of identifiable lies and potential truth. It arises. The advancement of truth requires that falsehoods should not be published at all. Let us suppose, for example, that the RRPE board received an article claiming that Obama's economic stimulus could not possibly work because he came from Kenya – a currently popular view in the US. It would (one would hope) reject the article because the argument is self-evidently false. What it would *not* do is publish it beside another article, no matter how convincing and no matter how much space was allocated to it, and arguing the contrary.

In fact, as the writers of the open letter noted, it is precisely *because* the Wolff review correctly represented Kliman's argument – and not, incidentally, because it was a positive review, since even a hostile review which correctly presented Kliman's argument permits scholars to reach a properly informed judgement – that the publication of the Sinha review is so questionable. The EB has published, side by side, two reviews that cannot possibly both be true. Even without any closer examination of either, it should therefore have *known* that one of the reviews *must* be false. A simpler way of judging falsehood could not have been available had it walked into the RRPE offices in a powdered wig and handed out business cards.

'Balance' is the commonest and, it must be said, basest argument offered by the press when it wishes to justify giving credence or air time to a view, or an argument, which it has no ethical right to give space to. Even in the case of the press, any mildly critical commentator recognizes that the primary duty is not to publish falsehood. Indeed, the UK Press Council itself provides a clear public right to require the correction (that is, retraction) of false information. This is an absolute right, not conditional on any other, and moreover may not be remedied by the granting of some other, different right. Thus, the Press Council (PCC 2011) also provides recognises the right of reply. It does not, however, state that this constitutes a remedy for the publication of false information: it requires the retraction of the false information. If even the press recognizes this duty, what special virtue entitles economics publishers to ignore it?

Academic freedom and the conduct of debate

The issue of balance arises in a different context, which it is worth examining in more detail, since it lies at the core of the misconceptions that underpin both RRPE's original decision, and its response to the open letter. From where do duties such as balance, right of reply, the duty to debate, and indeed the duty of pluralism in general arise? To consider these properly, let us consider the last of RRPE's arguments:

Pressing the Editorial Board collectively to disavow the considered opinion of one of our book reviewers strikes us as an attempt to stifle free debate

This is false because misrepresentation itself stifles free debate, as the history of McCarthyism makes only too clear. As with any liberal right, the right of free expression extends up to, but does not include, the point at which the exercise of a freedom by one individual removes the same right from another. In the concluding part of this paper, we will argue that the right of free expression extends up to, but does not include, the right to publish misrepresentations, precisely because misrepresentation is itself a primary instrument for suppressing academic debate. To understand this, let us explore further the issue with which we began, namely the pursuit of knowledge and truth.

As noted absolute, incontrovertible truth is highly elusive, and nowhere more so than in economics. Indeed, as we have noted in many places (see for example Freeman 2009, 2010a, 201b) the primary source of the dogmatic and religious character of economics is its insistence that it is able to deliver a single, certain truth, and this is also, paradoxically, the primary reason it actually gets so many things wrong. This fact in turn is the primary motivation for pluralism.

Even setting this aside, the progress of science shows that the advance of knowledge involves continually overthrowing past certainties – Galilean astronomy overturning Copernican, Newtonian gravitation overturning Aristotelian, Relativity overturning Newtonian theory, and so on. True science takes it for granted that no knowledge is final or definite, that at any stage of history what we call 'knowledge' is always the best we can do at the time. All 'truth' is therefore, in science (as opposed to religion) subjected to continuous doubt and re-examination. This is not, in case some of our more anti-pluralist critics choose to get heated about it, a post-modernist view. We make no claim that all claims to truth are equally valid, or that truth is relative. At any given time, no matter how many claimants to truth there may be, science possesses methods of deciding which claim is superior. We note *only* that absolute truth is unattainable, the proof being that in history, no view of truth has proven stable.

This is why a special branch of labour is needed to get at the truth: it is why researchers, journals, and publishers are needed. As already noted, if truth were obvious, there would be no need for all this work. But in consequence the 'raw material' of research and *also* of publication consists of material of which the truth is uncertain. There always are, and always will be, at any given time, a number of different ideas about what might be true – including even what is most widely held to be true, such as Einsteinian relativity. Contest between these ideas is essential to improve upon truth.

The *purpose* of academic freedom, of publication, of debate and discussion, is to *test* these different ideas, these different candidates for truth-hood.

It is therefore a great error to suppose that the testing of truth is conducted outside of the sphere of academic discussion, and that the function of publication is mere dissemination; that economic truth is produced in some kind of mythical laboratory, where, once unearthed, it is carefully wrapped in paper and served up in journals. *The debate that goes on in journals is an essential part of the research process.* The manner in which economists put their ideas to the test is to subject them to debate and criticism. This is done in conferences, seminars – and journals. It is an ethical duty of the organizers to ensure that the discussion takes place in them, and that all relevant currents to a discussion are heard at them.

Therefore, we would not only agree but insist that academic freedom, balance, and open debate are an ethical duty of a publisher. The biggest problem facing pluralist conduct arises because, in practice, limits always need to be placed on what *enters into* the debate – what is treated as relevant and necessary for the discussion to be productive. The organisers of academic debate have what, in the IWGVT guidelines, was called a *gatekeeping* function. They determine what we will call *admissible* views and contributions (see also Kliman and Freeman 2011 for a discussion of this term).

Admissible contributions are defined in a number of ways; for example, whether they deal with the subject matter of the debate, whether the language is clear, and so on – in short, the function of editorial control, along with peer review for journals, is to specify what is admissible. To define a contribution as admissible is not to recognise it as *valid*, that is, true or even true to the best of our knowledge. It is to define it as a legitimate component of academic debate. The purpose of debate is then to establish, among the many admissible views in a discussion, which are valid: which have a reasonable claim to contain truth.

What is the primary conclusion that Sinha draws from his review? Not that Kliman's theory, or TSSI theory in general, are *invalid* – which would be a perfectly legitimate, subjective conclusion. It is that TSSI, and Kliman's book in particular, is *inadmissible*. It is that, in Sinha's words, "the book utterly fails to achieve its aim due to numerous logical errors and misunderstandings of the interpretations it purports to criticize." It does so moreover, according to Sinha, because of absurd and elementary failures of logic. As evidence of this, he attributes to Kliman an error which, had Kliman made it, would indeed be absurd and elementary (Sinha 2009):

Andrew Kliman claims that his aim is "to reclaim Marx's Capital from the century-old myth of internal inconsistency." Then the reader is told that there exists a group of scholars who claim that no such internal inconsistency exists. And therefore, according to Kliman, "The very existence of the TSSI [such an interpretation, generally called the Temporal Single System Interpretation] carries with it two important consequences. First, the allegations of inconsistency are unproved. Second, they are implausible." Following such reasoning, one could then also argue that the existence of a group of scholars who argue that the theory of evolution is false and that creationism is consistent with empirical evidence, must lead us to reject the claims of evolutionism as unproved and implausible. The same must follow from the existence of a group of scientists who question greenhouse effects and global warming. This foreshadows the major weakness of this book: a lack of rigor in reasoning.

The function of this argument is to disqualify Kliman's work from due consideration. A theory based on such elementary failures of logic would indeed be inadmissible. Who should waste precious time on a researcher who commits such a mistake, or on a theory or group of scholars which employs such logic? But the failure of logic does not appear in Kliman's book. The 'therefore' in the third line of this quote does not exist in the text. The inference, as was pointed out from Sinha, follows from the existence of a *theory* – a tested, scrutinized, coherent body of work – not a 'group of scholars'.

The reader should note that we have no objection, and cannot possibly object, to a *legitimate* attempt to prove that Kliman's argument is illogical and not worthy of consideration. This is not the point: Sinha's entire argument rests on a demonstrably false claim: on the misrepresentation of what Kliman says. His allegation against Kliman is that Kliman has advanced an argument on the basis of which one may prove anything one wants. *If this were true* it would be a legitimate basis to treat Kliman's argument as inadmissible in all but a handful of marginal debates, if any at all. The problem is that it Sinha's allegation is not true: Kliman did not make the argument that Sinha claims, and his primary argument for Kliman's inadmissibility is without foundation.

The actual threat to academic freedom is, thus, the precise opposite of what the RRPE board claims. The threat to academic freedom comes from the publication of a demonstrable falsehood on the strength of which the claim is advanced that Kliman's work is not worthy of further consideration – that is, inadmissible. In short, the RRPE decision to publish this review without correction of this point itself constitutes an attack on the academic freedom of all TSSI scholars to be included as a legitimate view within scholarly discussions about Marx.

Further still, it is an attack on the academic freedom of everyone who wishes to examine Marx's own ideas and theories. As long as the false claim is allowed to stand that Marx cannot possibly be consistent, anti-Marx publishers and institutions the world over have at their disposal the precise excuse they need to suppress Marx's own ideas – and any work based on them. Sinha's review is therefore deeply and profoundly suppressive of academic freedom, a suppression that extends moreover to the whole body of TSSI scholarship. Thereby, since if TSS is a valid interpretation of Marx, then Marx's own theory is admissible, the publication of the falsehoods contained in Sinha's review contribute directly, substantially, and materially, to the economics profession's totally unwarranted exclusion of Marx's ideas from its canon. The harm thereby done is no minor matter.

Yet the ethical principle at stake extends even more widely. If reviewers and authors are left editorially free to misrepresent others in any way they choose, then it is precisely those to whom this license has been extended, who will be able to prove anything they want. No obstruction will exist to the assassination of perfectly legitimate theories by straightforwardly lying about what the theories consist of. This has nothing to do with freedom, nothing to do with science, and nothing to do with the advancement of knowledge.

Attempts to declare theories inadmissible on false evidence are the stock-in-trade of dogmatic method and the enemies of both science and freedom. The Inquisition declared Galileo's theory "absurd, philosophically false, and formally heretical" (see Halsall 1998). Böhm-Bawerk's (1949) classic defence of the marginalist theory of value rests, critically, on the claim that Marx's theory should be discarded because it is logically contradictory (see Freeman 2010c). Samuelson (1971) updated this exact same claim in 1971, silencing Marxism in orthodox journals for forty years to date. The most serious development of recent times is, unfortunately, and with unpleasant echoes of the history of McCarthyism within the USA, that the lead in such suppressive attempts has now been taken, and is now firmly in the hands, of the Marxists themselves. Freeman (2010d), Kliman (2010) and Freeman and Kliman (2011) document a relentless degeneration in Marxist scholarship. The principal objective of debate has virtually ceased to be a dispassionate and objective examination *of the theory* that an opponent advances, and has come to be – on the side of TSSI critics – almost entirely that of *discrediting the authors*. Rhetorical devices, pugilistic and pugnacious debating tricks, the elevation of minor technical issues into the central focus of discussion, have become in 'Marxism' an almost complete substitute for straightforward scientific discussion of the actual issues in dispute.

This brings us back to our initial methodological premise. Had we seen, in an orthodox journal, the methods of scholarly discussion, and the ethics of publication, that are defended by the RRPE letter, in our view heterodox and critical economist would rightly be scandalized, and would seek to put a

stop to it. There is no excuse for failing to apply the same ethical standards within our own ranks – indeed, the duty is if anything higher.

In Pogo's immortal words 'we have seen the enemy and he is us'. It is a profound and ultimately suicidal mistake for heterodox economists to permit themselves to be used as the persecutors of heterodoxy. With the publication of this paper, we call for the widest possible discussion to correct this error, and for action to prevent it happening again.

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Appendix 1: Open Letter to the Editorial Board of the *Review of Radical Political Economics*

15 October 2010

Dear colleagues

We are dismayed you have published a book review containing misrepresentations and falsehoods about the work under review. This brings disrepute upon critical economics at the height of a crisis of confidence in mainstream economics. In line with general scholarly standards, for example the Committee on Publication Ethics' *Code of Conduct* (publicationethics.org/files/u2/New_Code.pdf), we request you retract the review.

The reviewed work is Andrew Kliman's book, *Reclaiming Marx's "Capital": A refutation of the Myth of Inconsistency*, and your review, by Ajit Sinha, appears in the Summer 2009 issue of your journal. For brevity, we limit ourselves to the most obvious of the several falsehoods it contains but are happy to supply further detail on request. The review begins as follows:

In the preface to this book, Andrew Kliman claims that his aim is "to reclaim Marx's *Capital* from the century-old myth of internal inconsistency." Then the reader is told that there exists a group of scholars *who claim that no such internal inconsistency exists. And therefore, according to Kliman, "The very existence of the TSSI [such an interpretation, generally called the Temporal Single System Interpretation] carries with it two important consequences. First, the allegations of inconsistency are unproved. Second, they are implausible."* [emphasis added]

Kliman did not write what your reviewer claims. He did not state that the allegations of inconsistency are unproved and implausible because *a group of scholars claim that no such inconsistency exists*. Immediately prior to the sentences that Sinha quoted, he wrote that the allegations are unproved and implausible because "[a]n alternative interpretation developed during the last quarter-century—the temporal single-system interpretation (TSSI)—*eliminates all of the apparent inconsistencies*" (*Reclaiming Marx's "Capital,"* p. xiii, emphasis added).

As editors, you should have been aware of the misrepresentation in your reviewer's argument. Firstly, it is evident from any proper reading. Secondly, you first published, in the same issue of your journal, Richard D. Wolff's review of *Reclaiming Marx's "Capital"* which correctly characterizes the author's thesis in its opening paragraphs, flatly contradicting Sinha's review:

What the critics actually "proved" was that *their* particular interpretations of Marx's arguments contained logical inconsistencies. What Kliman explains is that alternative interpretations— at least as warranted by Marx's writings as the critics' interpretations — yield no such inconsistencies. ...

By showing that alternative interpretations are possible and reflect the interpreters' different theoretical commitments, Kliman pulls the rug out from under the critics.

Sinha's review further falsely, and tendentiously, charges the reviewed author with "lack of rigor in reasoning":

Following such reasoning, one could then also argue that the existence of a group of scholars who argue that the theory of evolution is false and that creationism is consistent with empirical evidence, must lead us to reject the claims of evolutionism as unproved and implausible. ... This foreshadows the major weakness of this book: a lack of rigor in reasoning.

This is based on the same elementary failure to distinguish between an unsubstantiated claim that something is false and an interpretation of the evidence according to which it is false. It is one thing to write that X is unproved because some people *claim* that X is false, and an entirely different thing to write that X is unproved because there exists an interpretation of the *evidence* according to which X is false. The first statement is ludicrous; the second is quite reasonable.

These falsehoods were willful. In November 2007, Sinha was informed that the paragraph's claims were false (see <http://ricardo.ecn.wfu.edu/~cottrell/ope/archive/0711/0219.html>). He responded to this information shortly thereafter, a year and a half before you published your review (see <http://ricardo.ecn.wfu.edu/~cottrell/ope/archive/0711/0230.html>)

Ajit Sinha was a long-term member of your editorial board, for eleven years, from 1993-1995 and from 1997-2006. A conflict of interest is involved. You should have taken especial care, given Sinha's close association with your journal, to ensure that his status did not lead to your publishing a review that intentionally misrepresented the reviewed work. You could easily have established the willful nature of Sinha's allegations but did not do so.

You should now make clear that Sinha's review breaches normal standards of scientific integrity, and retract it.

Sincerely yours,

Alan Freeman
Alejandro Ramos
Anders Ekeland
Bill Lucarelli
Brendan Cooney
Eduardo Maldonado Filho
Ioana Negru
John Ernst
Julian Wells
Michel Husson
Nick Potts
Paresh Chattopadhyay
Radhika Desai
Rick Kuhn
Andy Denis

Appendix 2 : Ajit Sinha's published review³

Reclaiming Marx's "*Capital*" A Refutation of the Myth of Inconsistency

Andrew Kliman; Lanham: Lexington Books; 2007; pp. 230; Paperback.

DOI: 10.1177/0486613409335813

Accepted June 30, 2008

In the preface to this book, Andrew Kliman claims that his aim is "to reclaim Marx's *Capital* from the century-old myth of internal inconsistency." Then the reader is told that there exists a group of scholars who claim that no such internal inconsistency exists. And therefore, according to Kliman, "The very existence of the TSSI [such an interpretation, generally called the Temporal Single System Interpretation] carries with it two important consequences. First, the allegations of inconsistency are unproved. Second, they are implausible." Following such reasoning, one could then also argue that the existence of a group of scholars who argue that the theory of evolution is false and that creationism is consistent with empirical evidence, must lead us to reject the claims of evolutionism as unproved and implausible. The same must follow from the existence of a group of scientists who question greenhouse effects and global warming. This foreshadows the major weakness of this book: a lack of rigor in reasoning.

Though the aim of the book is to show that Marx's reasoning is "consistent" and that the received criticisms of Marx's transformation procedure and his thesis of the falling rate of profits are incorrect, the book utterly fails to achieve its aim due to numerous logical errors and misunderstandings of the interpretations it purports to criticize. For example, Kliman offers a critique of V. K. Dmitriev's valid argument that a fully automated production system which produces more outputs than it uses as inputs would have positive prices and profits even though the system utilizes no labor. Kliman's refutation of Dmitriev's mathematical proof of this proposition is as follows. Let us say we are in a one-good world with the input-output system given as:

$$5p = 4p + r(4p).$$

Since it is a one-good system the price is always equal to one and the solution of the above equation gives us the rate of profit r equal to 25 percent. Kliman disagrees. Why? Because he thinks that in this system p should be zero and not one: Dmitriev "took for granted that the machine has a positive price. In other words, he assumed precisely what he needed to prove. If the machine is free, then $p = 0$, and it is impermissible to divide through by p as he did. Instead of finding that the rate of profit is positive, we find that the original equation becomes $0 = 0$. The rate of profit is therefore undefined" (43). Dmitriev, however, did not "assume" positive prices; he understood that in a one-good model, if price has any meaning, it is always equal to one. But what about the clause "if the machine is free"? But why should the machine in the above equation be free? According to Kliman, "There is good reason to believe that the machine will indeed be free. Dmitriev's fully automated economy is able to generate an ever-increasing output of the machine, unconstrained by any natural resource limitations, and at no additional cost. It is thus quite plausible ... that the machine's price would quickly fall to zero" (43). If the above reasoning was true, then it would imply that positive prices could prevail only if there are diminishing returns in the system; if constant returns prevail then prices would quickly fall to zero. But the question is: why should the price of the machine in the above equation fall to zero? It costs four machines to produce 5 machines, so why should machines become free? Kliman does not entertain such questions.

³ This review was published in RRPE as Sinha (2009). An earlier, prepublication version was the subject of a detailed critique and discussion on the OPE-L discussion list. The basis for the complaint, in the open letter, that Sinha's falsification was willful, is that his attention had already been drawn to it. Both the earlier review, and the discussion, omitted for brevity from this paper, are available on www.iwgv.org/rrpe

Let us move on. The purpose of this book is not to “reclaim” *Capital* in all its aspects but rather to argue that the dominant interpretation of Marx’s theory of value, which Kliman calls “simultaneist,” is a misinterpretation and the so-called TSSI is the correct one. The problem with the simultaneist interpretation, according to Kliman, is that the “simultaneist” approach maintains that input prices are equal to output prices, which is supposed to be a mistake. He again takes the example of a one-good corn model where 10 bushels of corn-capital after a year yields a harvest of 12 bushels of corn (79ff.). Thus the rate of profit, according to the “simultaneist” approach, would be 20 percent. Kliman claims that this is incorrect. He argues: “Assume that the price of corn is \$156/bushels at the start of the year, so that \$1560 is invested. Also assume that the price of the corn output is \$156 if eleven bushels are harvested, falling to \$143 if output is twelve bushels or \$132 if output is thirteen bushels. In all three cases, sales revenue is \$1716, profit is $\$1716 - \$1560 = \$156$, and the rate of profit is constant at 10%. The rate of profit no longer depends solely upon physical quantities. It also depends upon the decline in the price (=value) of corn that results from the increase in productivity” (80).

What Kliman fails to understand is that the “simultaneists” do not claim that input prices are equal to output prices in the sense he understands it. Let us assume that the corn-capital market is perfect, so that the rate of interest is equal to the rate of profit. Now, if I lent you a ton of corn at the beginning of the production period then I must receive 1.2 tons of corn at the end of the production period. Thus my 1 ton of corn at the beginning of period 0 exchanges against 1.2 tons of corn at the beginning of period 1. Therefore, in this sense, the input prices are not equal to the output prices. The rate of profit is the discount rate for the output prices over periods of time. Now if we move from a one-good to an n-good world and maintain the stipulation that the rate of profits is uniform across the sectors—a stipulation that Marx maintains for his prices of production and Kliman accepts—then the exchange ratios of the commodities in period 0 and period 1 must be the same. However, if the rates of profit were not uniform across the sectors, then, of course, the discount rates for different goods would be different and therefore the exchange ratios of commodities in period 1 would not be the same as the exchange ratios of the commodities in period 0. The point that needs to be understood here is that when an economist says “prices” she usually means the exchange ratio of commodities in terms of one particular money-commodity, say gold or silver. So we have one set of prices or exchange-ratios of commodities against gold for period 0 and another set of prices or exchange-ratios of commodities against gold for period 1. If these two sets of ratios are one-to-one equal then by definition of prices the prices in period 0 are equal to the prices in period 1. This does not mean that corn in period 1 is the same commodity as corn in period 0; they are two different commodities. Now, it should be obvious that if the discount rate applied to all the prices in period 0 was uniform, then the ratios between all commodities would remain the same irrespective of whether the system is in equilibrium or not. Thus whether the relative input prices are equal to the relative output prices depends upon whether the rate of profits is uniform or not. Kliman is therefore incorrect when he writes that simultaneists “not only assume, along with Marx, that rates of profit are equalized after innovation is adopted. They also take the liberty of equalizing input and output prices” (117). His critique of “simultaneism” is rooted in his misunderstanding of what economists mean by “price.”

At this stage it may be useful to clarify another misunderstanding of Kliman which runs through one end of his book to the other. He consistently argues that if the economy or a particular sector is experiencing a rise in labor productivity then output prices would be lower than input prices, and that would mean the economy or the sector may experience losses even though physically it is producing a surplus. Since Kliman’s examples in terms of a one-good corn model are theoretically weak, I will present his case in a stronger manner. Let us suppose that we are in a n-good world and there is a sector x which produces a non-basic good and does not use itself as an input. Let us also suppose that there is continuous increase in labor productivity in this sector; whereas in all other sectors (including the gold sector) labor productivity remains constant. In this case the price of commodity x would continuously fall. Let us suppose that commodity-capital worth \$100 in terms of

gold was used as inputs (including wages) at time 0, to produce 100 units of x in the beginning of period 1. Let us suppose that in period 0 the price of x was \$1.2 per unit of x . However, since the capitalists have introduced a more productive technology in period 0, the price of x falls to \$1 in the beginning of period 1. Kliman argues that this means that capitalists in sector x have made zero profits. But this is simply not true. Since in period 1 the capitalists would need only 83.3 percent of the inputs that they used in period 0 to produce the same 100 units of x , they can continue their business as usual at the same level and pocket \$16.7 as profit. In his examples of continuous technical change, Kliman forgets that even if input prices remain the same and the price of output is falling, the profit rate need not fall; for the quantity of inputs needed to produce the same amount of output are also continuously shrinking due to the rise in labor productivity. Alternatively, let us assume the capitalist had borrowed her initial \$100 at the rate of 5 percent per year interest (note that in this system the rate of interest cannot be higher than 16.7 percent), then she should be able to pay off her principal in less than 10 years and continue with her business. If Kliman's argument was correct, then such a possibility must be ruled out. If x , however, was a basic sector, then a continuous rise in labor productivity in this sector would have a complicated effect on all prices. Okishio (1961) showed that in this case prices must change in such a way that the uniform rate of profits in the system rises. Kliman, on the other hand, has no theory of prices. He simply takes arbitrary prices at two different periods and concludes: *Voilà!* I have proved Okishio wrong!

Now let us see how Kliman "rescues" Marx from the criticism of internal inconsistency. The internal inconsistency argument is rooted in a long-standing problem in capital theory since the time of Ricardo. Since Adam Smith, political economists have maintained that in a competitive capitalist economy the rate of profits should be uniform across sectors because of the tendency of capital to seek the highest rate of return. The problem is that capital goods are produced by the system and are heterogeneous. Thus to measure the rate of profits on capital investments one needs to homogenize the heterogeneous capital-goods. This calls for a theory of price or "value." If we know how prices are determined, we could measure all capital goods in terms of their price-unit. Ricardo, and following him Marx in volume I of *Capital*, proposed that prices are determined by the ratios of direct and indirect labor-time needed to produce the various commodities, including the money-commodity. If this proposition is correct, then we can reduce all capital goods to their values in terms of money, since prices are determined solely by the techniques of production in use. But Ricardo had noticed a problem with this theory of price. Let us suppose that a bag of wheat and a bottle of wine take an equal amount of direct and indirect labor-time to produce, but the capital locked in producing a bag of wheat gets released after a year while the capital locked in a bottle of wine takes five years to be released. Thus if one bottle of wine exchanges against one bag of wheat, then the same amount of capital invested in wheat production would make much larger profits in five years than the capital invested in wine production; this occurs because interest on capital accrues at a compound rate. Thus the condition of equal rate of profits on equal capital requires that a bottle of wine must exchange against more than one bag of wheat. The simple labor theory of value is therefore not a correct theory of price, and a measure of capital in terms of labor-values would be an incorrect measure of capital. The problem is that one needs the rate of profits to determine the prices and prices are needed to determine the rate of profits. Marx was well aware of the problem through Ricardo (1821). He argued that his theory of "surplusvalue" allowed him to determine the rate of profits independently of prices, so that he could use the rate of profits to determine long-period prices of production.

The problem with Marx's solution is that it is predicated on the assumption that prices are determined by the labor theory of value. If this hypothesis is incorrect, as Marx agrees that it generally is, then his rate of profits would in general be an incorrect rate of profits and his solution for prices of production would also be incorrect. Kliman, on the other hand, argues that Marx's procedure of determining the prices of production is logically sound. The argument rests on the claim that Marx did not define commodity value as total direct and indirect labor-time needed to produce a commodity. According to Kliman, Marx's labor-values should be calculated by adding

direct labor-time to market-prices of capital goods at the time they were purchased. Here market-prices of capital goods are taken to be available historical data. But how does one add labor-time to an ounce of gold (say a \$ stands for an ounce of gold)? We are told: "If each hour of socially necessary labor adds \$60 of new value ..., the MELT [monetary equivalent of labor-time] is \$60/hr" (25). But we can know the monetary value of an hour of socially necessary labor only if we not only know the prices of the inputs as historical data, but also the price of the output. Suppose that constant capital investment was \$100 and the direct labor-time was 10 hours which produced an output that sold for \$150; from this data we can compute that 10 hours of labor adds \$50 of "new value" and therefore \$1 must represent 0.2 hours of labor. From here we go back to the \$100 capital investment, counted as 20 hours of labor and add 10 direct hours to it to arrive at 30 hours of labor: Kliman's measure of the value of the commodity produced. Whatever this calculation of 30 hours may represent, it is clear that the exercise is meaningless as far as the determination of prices and the rate of profit is concerned, since both the input prices and the output prices have to be taken as given to arrive at the value figure, and the rate of profit can be directly calculated as $\$50/\$100 = 50$ percent without any help from the value measure. (To add to the confusion, Kliman's assumed conversion rate, the MELT, remains constant even when all the techniques, prices, rates of profits, rates of surplus value, etc. are continuously changing. It is a curious ghost of the theory, which remains unexplained throughout the book.)

Let us leave the problem with MELT behind and go along with Kliman in assuming any arbitrary conversion factor he chooses and see how he solves the problem of transforming values to prices of production. On pages 163ff Kliman takes an example of two sectors (throughout the book Kliman's tables are made up of simple numbers without any units attached to them; I work out his example by interpreting his numbers with proper units). The total capital investment in commodity 1 is \$200 (including wages) and total direct labor-time is 8 hours of labor. Kliman assumes $\$1 = 1/3$ hours of labor. Thus \$200 capital investment is converted to 66.66 hours of labor and Kliman's value of the commodity is calculated to be 74.66 hours of labor. He also assumes that the wage bill is equal to \$8, hence in labor terms 2.66 hours of labor. Thus the surplus value in labor terms would be 5.33 hours of labor. For commodity 2, the corresponding figures are \$40 for capital investment and 16 hours of direct labor. Using the conversion factor $\$1 = 1/3$ hours of labor, we get Kliman's value of commodity 2 equal to 29.33 hours of labor and surplus value equal to 10.66 hours of labor. If the two commodities exchanged one for one then the "value rate of profit" in sector 1 would be $5.33/66.66$ and in sector 2 it will be $10.66/13.33$. Since the two "value rates of profit" are different, Kliman argues that the two goods would not exchange one for one. The transformation of those values so obtained into "prices of production" requires that we equate the rate of profits in the two sectors. This is done by adding up the surplus values of the two sectors to 16 hours of labor and dividing it by total capital investment of 80 hours of labor. This gives us a rate of profit for the aggregate system as a whole to 20 percent, which should be applied to both the sectors. When we apply a 20 percent rate of profits to both sectors, then their respective prices of production turns out to be $(74.66 \times 1.2) = 89.592$ hours of labor and $(13.33 \times 1.2) = 15.996$ hours of labor. Using the conversion factor of labor into money units, Kliman claims that commodity 1 would sell for \$269.776 and commodity 2 should sell for \$47.988. Since Kliman does not specify the techniques in use for producing the two goods, the reader will have to work out an input-output system of her own to confirm that in this procedure, in all likelihood, input prices would be different from output prices if we start with any arbitrary input prices. Kliman thinks that this is the strength of his transformation procedure.

Now let us suppose that the system keeps repeating itself exactly at the same level with same techniques. Since Kliman's output prices are different from the input prices and the capital in every next cycle of production must be reckoned at the new output prices, the reader can immediately work out that both prices of the two commodities as well as the equal rate of profits itself would be changing from one period to another. This fact itself should give an immediate alarm that something is logically wrong with this exercise. If all the parameters of price determination are kept constant,

then why should prices change? If the reader keeps iterating the same exercise by equating the rate of profits in every cycle and using the output prices as input prices for the next cycle, she would find that eventually output prices would converge to input prices and the rate of profits would equalize and stabilize. Interestingly those prices and rate of profits would be exactly the same if the two production equations were simultaneously solved for relative prices and the rate of profits. What this describes is a well-known method of solving a simultaneous equation problem. You can start with any arbitrary prices, impose the condition of equal rate of profits, and keep iterating the system of prices till you get to a position such that no further iteration brings about any change in the variables. Morishima (1974) and Shaikh (1977) have suggested that Marx was perhaps using this method of solving the simultaneous equation problem but had left off after the first iteration. The problem with Kliman's procedure, however, is that he interprets what must go on inside a calculating machine as what goes on in real historical time in the real economy.

This is the long and the short of the theoretical arguments by which Kliman purports to "reclaim" Marx from the century-old "myth" of inconsistency. The book utterly fails to achieve that aim.

Ajit Sinha

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Appendix 3: response from the editorial board of RRPE

Hazel Dayton Gunn
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October 31, 2010

Dear Alan et al.

I am writing on behalf of the Editorial Board of the *RRPE* in response to your letter concerning Ajit Sinha's review of *Reclaiming Marx's Capital*. The Board is in full agreement that nothing in the review warrants a retraction.

The point at issue is Sinha's interpretation of Andrew Kliman's rhetoric. Your letter refers to the COPE *Code of Conduct*. The only clause of the code which has a bearing on this matter reads as follows: "Whenever it is recognized that a significant inaccuracy, misleading statement or distorted

report has been published, it must be corrected promptly and with due prominence.” Sinha’s review expresses strong and generally unflattering opinions on what he understands Kliman’s argument to be. But questions of interpretation are different from questions of fact, and one job of a reviewer is to say what he thinks an author is arguing. Kliman did in fact write—and presumably wished to persuade his readers—that “*The very existence of the TSSI* [has a consequence that] the allegations of inconsistency are unproved....” The inference that Sinha drew from this remark may or may not be what Kliman meant, but it is not obviously a “significant inaccuracy” or a “willful falsehood”; no doubt Sinha would contend that it is a plausible reading. Readers ought to be allowed to decide for themselves.

Anyone who puts before the public an argument on a topic as contentious as the labor theory of value or the transformation problem must expect that some readers will disagree with the argument, and that a few of them might criticize it in extremely harsh terms. This is what has happened in the case at hand. We do not believe that the bounds of civil discourse were transgressed, even if there is room to debate how near the review comes to pushing up against them. Our policy is to give our reviewers considerable latitude to say what they think. Readers always have the option to consult Kliman’s book to determine whether the criticisms leveled against it by Sinha have merit.

Moreover, as your letter notes, Sinha’s critical assessment was published alongside Rick Wolff’s glowing appraisal of Kliman’s book. *Reclaiming Marx’s Capital* received twice as much attention as we typically devote to any book, and about half of that attention was unambiguously positive—circumstances which might be expected to stimulate reader interest in the book. We therefore do not see that there are any substantive grounds for complaint about the treatment the book received in the *RRPE*. If Andrew Kliman believes that the review is distortive, the appropriate response would be for him to clarify his views when next he writes about the topics covered in the book. Pressing the Editorial Board collectively to disavow the considered opinion of one of our book reviewers strikes us as an attempt to stifle free debate.

Finally, you raise the issue of a conflict of interest. You do not say in what the conflict consists, though you mention that Sinha was once a member of the Editorial Board. Precisely how that constitutes a conflict of interest is unclear. The implication appears to be that his review got special treatment and was accepted for publication without being held to the usual standards. This is a serious accusation which disparages the integrity of the entire Board—two dozen people; yet you offer not a shred of evidence, other than your opinion that the review is inaccurate and unfair. Sinha’s review passed through the same review process as do all book reviews, and indeed underwent two rounds of significant revision from the first to the final draft.

Yours, Hazel Dayton Gunn, Managing Editor, *RRPE*, for the Editorial Board

Appendix 4: IWGVT guidelines

These guidelines were developed by the International Working Group on Value Theory (IWGVT) in 1996 and can be found on the IWGVT website at <http://www.iwgtv.org/about%20us.htm>

Preamble

We are convinced that the de facto function of mainstream selection procedures is to exclude. Mainstream selection criteria are subjective and therefore discriminate against theories and arguments which the reviewers and editors hold in disfavor. Conversely, the following guidelines put forth some objective criteria to which, as we have learned and as we teach, good scholarship should conform.

It is common in academic discourse for proponents of one perspective to exclude, ignore, and deny legitimacy to opposing perspectives. Against this, the aim of the guidelines is to achieve a style of debate in which different perspectives engage with one another. We seek to foster a dialogue which

is pluralist, because no interpretation of a theory, and no presentation of the facts, will be ruled out a priori, but also critical, because proponents of various perspectives will need to confront the alternatives.

Inform Readers of the Alternatives

An argument is not well-grounded unless the extant alternatives have been addressed. This means that all points of view are legitimate until proved otherwise. Engage and cite the views of others involved in debating the issues you are addressing, and treat them as equals acting in good faith. If you want other people to attend to what you are saying, then attend to what they are saying.

Don't Deny Legitimacy to Alternative Views

The aim of debate is clarity, not demolition. Avoid turns of phrase such as 'absurd', 'ridiculous', or 'impossible' to deny the legitimacy of opposing views, or phrases like 'as is widely known' or 'of course' to prove your own views are undeniable.

Identify the conceptual basis of "facts"

Economic data are not undisputed facts of nature but the result of a theoretical interpretation which should be explicit. 'The real output of the UK economy in 1994 was £570,722m' is a false claim. 'Output as measured by the UK NIPAs, deflated using the HMSO GDP deflator, was £570,722m' specifies the conceptual framework that produced the claim, and lets the reader trace the assertion back to its source.

Distinguish Original Texts from Subsequent Interpretations

You must distinguish clearly between an original text and subsequent interpretation. John Maynard Keynes did not say that equilibrium in the goods and money markets is given by the intersection of the IS and LM curves. This is Hicks' interpretation of Keynes. Karl Marx did not say that value is a vertically-integrated labour coefficient: this is the interpretation of Marx proposed by Linear Production Theory.

Argue from Evidence

Both statements about the world and interpretations of texts must be supported by empirical evidence, from the world or from the text, respectively. Appeals either to authority or to popular wisdom do not constitute evidence. Avoid Ad Hominem reasoning: don't try to substantiate or refute an argument by reference to any characteristic of the person presenting it.

Distinguish Between Internal Inconsistency, Interpretive Difficulties, and Disagreement

If you justify your approach by asserting that opposing views are inconsistent, you are declaring they cannot possibly be right and you hence exclude them from discussion. If you have only demonstrated the inconsistency of your own reading of these views, then your proof is false because you have not exhausted the alternatives; but you have closed down the dialogue. If you want to say a view is inconsistent, provide evidence that it cannot be interpreted otherwise. Unless you can do this, instead say that you have difficulty making sense of the argument, or that you disagree with it, as the case may be.

Characterize Schools of Thought in the Preferred Manner

Do not use a characterisation for the purpose of dismissal. In debate, refer to other schools of thought by the name they prefer (for example, 'surplus approach' in preference to 'neoricardian')

unless you are including them in a wider grouping with no recognised name. In the latter case, try to provide an accurate, descriptive term.