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Freeman, Alan

The University of Greenwich

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Alan Freeman

The University of Greenwich

afreeman@iwgvt.org

Abstract

This paper undertakes a critical examination of the concept of 'centre of gravity' as adapted by economics from classical mechanics, relating it to the idea of 'long-run' profits, prices and quantities, as presented in the work of the post-Sraffians.(1) It will also address the origin of this concept of 'long-run' in Marshall's distinction between long-run and short-run determinations of economic magnitudes.

It shows that economists have generally conceived of centre of gravity as a theoretical magnitude which is not observed, but around which observed magnitudes oscillate either randomly or in some deterministic manner; this much is generally agreed. This idea has, however, been interpreted in two distinct ways in the history of economic thought:

(1) as an attractor dynamically determined at each point in time by path-dependent historical processes which have led the economy to be in its present state.

(2) as a hypothetical static equilibrium state of the economy determined independent of history by its current exogenous parameters (utility, technical capacity, etc)

It demonstrates that these two ideas are necessarily distinct and that both must be taken into account in any pluralistic research programme. Mathematically the attractor of a variable is not in general equal to its hypothetical static equilibrium, except in highly restricted circumstances such as the absence of technical change. Moreover, again outside of exceptional circumstances, the divergence between the predictions of observed magnitudes given by the two approaches increases over time, so that it cannot even be accepted that one converges on the other. Error will therefore result if it is assumed *a priori* that (1) is identical to (2).

The fact that the two conceptions lead to different predictions does not decide that either one is correct. This should be determined empirically and therefore, an agreed empirical test should be established by the community of social scientists or, better still, society.

The paper will argue that, empirically, the 'test variable' against which both conceptions should be checked is the time average of the variables in question. This is not a distinct concept of 'centre of gravity' but an empirical observable. In a pluralistic programme, the predictions of both conceptions should be evaluated against this proposed test variable.

The second part of the paper examines the common basis for the critical stance taken by both Keynes and Marx to the second conception, which is rooted in a common attitude to the relation between substance and accident, and a correspondingly similar conception of uncertainty. It will relate this to the work of Quetelet and the development of the statistical method in sociology which, it will argue, is rooted in an ontologically distinct conception of social magnitudes to that found in economics, closer to the concept which Keynes and Marx shared.

It argues that the post-Sraffian conception of long-run is based on a fallacious identification of these two distinct concepts, rendering the post-Sraffian approach equally incompatible with Keynes's and Marx's theories.

It argues that the post-Sraffian conception of centre of gravity is 'intrinsically antipluralistic' in that it depends absolutely on the conflation of two concepts which are in fact necessarily distinct, leading to the suppression of the non-equilibrium concept as an alternative to the scientific procedure of testing the predictions of both concepts against an observable.

AN INVASIVE METAPHOR: THE CONCEPT OF 'CENTRE OF GRAVITY' IN ECONOMICS

Alan Freeman

For AHE 2006

> Männer umschwirren mich wie Motten das Licht,
> und wenn sie verbrennen, ja dafür kann ich nichts
> – Marlene Dietrich

Introduction[1]

This paper critically examines the concept of 'centre of gravity' in physicalist[2] thought. Kurz and Salvadori (1995:p1), in a book dedicated to the idea, explain it thus:

> This book is concerned primarily with the investigation of economic systems that are characterised by a uniform rate of profit and uniform rates of remuneration for each particular kind of 'primary' input in the production process …

> The classical as well as the early neoclassical economists did not consider these prices as purely ideal or theoretical; they saw them rather as 'centers of gravitation', or 'attractors' of actual or market prices.

It is integral to this idea that such a 'centre' is, and can only be, the hypothetical equilibrium state of the economy. This is a *leitmotif* of such writings, as expressed explicitly by Duménil and Lévy (1993: p147):

> [T]he economy begins out of equilibrium, and the study demonstrates the ability to move to the equilibrium position, in the absence of further perturbation. This model studies the centripetal forces which pull the economy back to equilibrium, the central aspect of the classical paradigm of competition.

> However, recurrent perturbations (centrifugal forces) force the economy into a constant gravitation around the equilibrium position. A state of gravitation is a 'stationary' (but agitated) regime in which centrifugal forces are matched by centripetal convergence forces…

> There is a direct relationship between the speed of convergence towards equilibrium in the absence of shocks, and the amplitude of the gravitation of the variable around its equilibrium value, when exogenous stationary shocks are added. For the same shock amplitude, a faster convergence would lead to a gravitation of lesser variance, and conversely for a slower convergence.

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[1] I am indebted to Julian Wells for his invaluable input to this article and for the excellent scholarship in Wells (2005) on which I have drawn heavily, and to Claudia de Lozanne Jefferies (2006) for her introduction to the extensive literature on metaphor in economics. Errors are my own responsibility.

[2] By 'physicalist' I mean those writers who derive prices and profits, to within one degree of freedom, by solving a set of simultaneous equations whose coefficients are exogenously-given quantities of physical goods consumed, labour required, and commodities produced. In this paradigm the existence of a solution depends on the equilibrium condition that prices are constant. See Sraffa (1960), Steedman (1977,1981), Pasinetti (1977), and Freeman and Carchedi (1995)

The essence of this argument is that although market prices are not equal to equilibrium prices, nevertheless, they oscillate around these prices in some sense analogous to the way that planets oscillate around the sun.

This idea is a metaphor. No-one claims that a price literally has weight or mass which can be measured on a pair of scales; or that one profit rate exerts a force on another profit which can be measured with a spring balance or a gravitometer. The argument is that nevertheless, they behave as if these identifications could be made.

It might be thought that, this idea being only a metaphor, any problems with it do not matter. Metaphorical statements – it is generally held – are a pedagogical device, an aid to understanding, and should not be taken literally because the rigorous proof is to be found elsewhere.

The problem is that 'elsewhere' does not exist. The rigorous proof is not to be found; in those rare cases where an attempt is made at it, the logic turns out to be flawed. The metaphor has thus become a substitute proof. It has become a false link of irrational belief in a chain of otherwise rational deduction. Its function is to protect that link from the unwonted incursion of logic.

The metaphor exploits a fatal ambivalence: the word 'centre' as applied to an oscillation has two meanings which are collapsed on to each other: that of the equilibrium[3] or fixed point[4] at which no movement takes place (which the physicalists also term the 'long-run' position), and that of centre of motion or trend around which movement fluctuates.[5] The physicalist literature treats these two ideas as one and the same:

> Kliman and McGlone further confuse matters by adopting the traditional uniform profit rate condition, without stopping to realize that the average profit rate is a conceptual center of gravitation that — flukes aside — can prevail *only* when market prices coincide with long-period prices of production. (Mongiovi 2002:16, my emphasis)

The denial of this ambivalence is antipluralistic; Mongiovi, in the above text, literally rules out the possibility that another conception of 'centre' could exist than the fixed point of the system. The principal purpose of this article is to show that the physicalists have assigned to the signifier 'centre' *two distinct signified objects in the real world which do not and cannot coincide*.

The centre of a periodic motion is its fixed point under a specific and limited circumstance which does not hold in general in economics: this is when the periodic motion converges to that fixed point, that is, when the fixed point is 'asymptotically

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[3] Throughout this article, as elsewhere, I use the term 'equilibrium' to refer to the hypothetical state of an economy in which prices and profit rates are assigned magnitudes at which the economy simply reproduces itself without change. In this I follow the forthright directive of Duménil and Lévy(1993:37) "When prices of production prevail, the rates of profit are equalized and, thus, capital is not moving from one activity to another…Although there is always some reluctance within classical economics to use the term *equilibrium* or *general equilibrium,* this situation should be denoted as an equilibrium.'

[4] This, mathematically, is the fixed point of the system defined as follows: if x is a vector of endogenous variables such as prices and profits and a is a vector of exogenous parameters such as technical coefficients or consumer tastes, and if the state of the economy at time $t+1$ is given by the state function $x_{t+1} = f(a_t; x_t)$, then the fixed point for any given a_t is the solution of $x^* = f(a_t; x^*)$. See Freeman (2007) for a detailed discussion.

[5] Even 'possessing a centre', as we shall see, is by no means an automatic or obvious feature of fluctuating dynamic behaviour. However in this article the key illusion I wish to dispel is the *idée fixe* that not only does a centre have to exist, it must also be a fixed point or 'equilibrium'.

stable.'[6] Moreover when the fixed point itself moves secularly over time, as is generally the case in economics because of technical change, *even if* it is a stable fixed point, it ceases to be the centre of the motion. This is the reason, for example, that the average profit rate falls in price or value terms, when the equilibrium profit rate is rising.

This latter deviation from the fixed point becomes qualitative: the two go in opposite directions and come back together *only* when the secular change ceases.

In consequence the claim that 'because of gravitation', the equilibrium, fixed or 'long-run' position of a system must serve as the centre of prices and profits is not only false but as already noted in Mongiovi's text, antipluralistic: it converts something which should be tested empirically (whether the fixed point is or is not in fact the centre of the motion) into a declaration of faith: from the physicalist standpoint *no other conception of centre is possible*. The purpose of this article is to refute this false idea, which has never been established logically in the first place and which, precisely for this reason, has never been properly questioned.

Limits of the gravitational metaphor

The objections above might be overruled if an economic system *literally* behaved like a gravitational system, so that prices or perhaps profits changed over time in a manner so closely resembling those of astronomical objects that, for all intents and purposes, we could argue that they must surely be governed by a similar set of equations even though we haven't yet discovered them.

This is not altogether fanciful. Simple types of equation that govern oscillatory behaviour can be found in many different contexts – a pendulum, a simple electronic oscillator, a circular orbit, and so on.[7] It is common to say that an electronic circuit behaves 'like' a pendulum and engineers even 'borrow' ideas from one kind of oscillation to another, for example 'hysteresis' which is used both of solid objects and electrical circuits. So why not argue that, at a more complicated level, a market is just another kind of pendulum? After all it is strongly cyclical and the cycles have an almost chronological regularity. Surely, it must be governed by a similar set of equations derived from the laws of economics, rather than the laws of electricity or gravity, and the metaphor is therefore an innocent one. The only problem in that case would be – it could be argued – that these equations are more complicated, and so it will be a while before a sufficiently clever and, presumably, sufficiently well-funded researcher writes them down.

The problems come when we join up the dots and ask how the metaphor could really work: connect signifiers to signified objects, equating let us say a profit rate to a planet, and profit rate differentials to planetary distances. We then find that mechanisms which are supposed to show us how an economic system works not only don't work but cannot work. The idea doesn't in fact make sense. It is impossible for economic objects to behave like gravitational ones.

To take one simple objection among many: if prices really behaved like planets, and if economic equilibrium really did behave like a gravitational centre, then the closer a price was to equilibrium, the faster it would move – since the closer two objects are to each other, the stronger the force between them. But economics posits the exact opposite

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[6] A fixed point is asymptotically stable if, in some sufficiently small region of the point, any trajectory of the system tends towards it, that is, becomes indefinitely close to it after the lapse of sufficient time.

[7] Essentially, second order linear differential equations with constant coefficients and, possibly, a forcing or damping term.

relation – the 'force' upon a price that departs the alleged equilibrium is said to be the stronger, the farther away it is from this equilibrium.

To take a more complex difficulty: a gravitational system possesses no repelling force. Gravity does not make objects move apart. It pulls them together. The statement that "A state of gravitation is a 'stationary' (but agitated) regime in which centrifugal forces are matched by centripetal convergence forces" is either a schoolboy error or a redefinition of gravity: in a real gravitational system there is no such thing as a centrifugal force.[8] It is motion, not force, that moves planets apart.

Economic forces are to the contrary repulsive. Capital migrates away from, not towards the average, in search of the highest possible rate. Sellers seek to raise their price, and buyers seek to cut it – in each case away from any possible fixed point. The countertendency, towards convergence, arises from economic movement – that is, competition. The paradox is therefore not that profit rates diverge – this is easy. The difficult thing is to understand why they should nevertheless come together again.

An economic system is thus the exact opposite of a gravitational system; planets naturally come together, and it is only physical motion that moves them apart. Profits and prices naturally separate, and it is only economic motion that brings them together.

Finally, and most decisively, oscillatory motions do not in general centre on their equilibrium, even when that equilibrium really exists (which, in economics, it does not) and even when these motions possess a centre, which frequently they do not. Most important of all, when they do possess a recognizable centre, in the sense of a moving time average, *once there is a systematic and secular change in the exogenous parameters, this moving time average does not follow the same path as the hypothetical static equilibrium but to the contrary, under the conditions generally prevailing in economic systems, moves in an entirely different, and generally contrary direction to the hypothetical static equilibrium.*

The differences between a moving average centre and a moving static equilibrium are not minor. They are qualitative: where a gravitational system goes up, and economic system goes down. 'Gravitational' predictions simply do not work. They are used, nevertheless, to impose upon the reader and the discipline the wholly illegitimate and antipluralistic conclusion that nothing else is possible.

All these points will be demonstrated in the final section of this paper. For now, it suffices to note that the metaphor is a very poor substitute for logic. It would in fact be astonishing if prices and profits were to behave in the same way as objects in a gravitational field. The function of the metaphor is therefore to trap the reader into accepting a false premise without logical examination; to excuse the physicalist schools from a normal test of logic, namely, exhibiting a chain of argument which leads from stated premises to proven conclusions in such a way that each step of the reasoning can be independently verified

Origins

This article has a secondary purpose to which, for reasons of space, I will give less attention: I question the genealogy which writers of this school give to the idea. Kurz and Salvadori in particular, above, devote a large part of their book to establishing that the idea of 'centre of gravity' was held in one or other form by most previous economic

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[8] School textbooks are full of warnings to students against speaking of a 'centrifugal force' when talking about planets, because such a force arises *only* if the object is constrained, for example by a piece of string. Even then the 'centrifugal' force is virtual: it comes from the string, not from the gravity.

writers. And more or less all physicalist writers nowadays relate the idea immediately and directly to what they term the 'classical' tradition of Smith, Ricardo, and Marx.

This constitutes a rewriting of what these authors actually said: it constitutes what has elsewhere been termed a 'Whig History' of economic thought. It retrospectively imposes on past thinkers a modern idea which they demonstrably did not hold, and could not have held.

Wells (2005) provides voluminous textual evidence that the idea of gravitation played a considerably different role in classical writing and indeed, that it is questionable it was of central importance at all. Although Smith (1980a,b) wrote two treatises on astronomical thought, he introduced the idea of gravity only once in his writings.[9] As Cremaschi (2002) points out:

> Smith also believes, with the whole tradition of rhetoric stemming from Aristotle, that metaphor is one figure of speech that carries many dangers. He shares the suspicion widespread among modern philosophers (starting with Francis Bacon and the royal society, that only 'when the sentiment of the speaker is expressed in a neat, clear, plain and clever manner' (Smith 1983: i.v. 56: 29) is language used as a proper vehicle for ideas and that figures of speech 'have no instrinsic worth of their own.'

It is stretching a point to infer an entire structure of logic from a single use of the word 'gravitating' which Smith himself carefully brackets with the phrases 'as it were'.

Ricardo does not even use the word 'gravity' anywhere at all; the only proof offered of his commitment to the idea is his continuity with Smith. Glick (1985:11) cites the following passage

> In speaking then of the exchangeable value of commodities, or the power of purchasing possessed by any one commodity, I mean always that power which it would possess, if not disturbed by any temporary or accidental cause, and which is its natural price. (p.92)

from which he concludes (p11) that Ricardo believed that 'the constant shocks and disturbances which affect the economy will lead to a gravitation around equal long run centres of gravity'. This attribution bears, unfortunately, little or no resemblance to the cited passage. Glick simply does not seem to see that to state a certain price will hold 'if not disturbed' tells us nothing at all about what will happen if it is disturbed, either in the real world or in the head of Ricardo. He has simply superimposed on Ricardo his own belief that the equilibrium point cannot be other than the center of disturbances. The mere fact that Ricardo speaks of equilibrium has been converted into evidence of a fully-fledged theory organised around a set of words which Ricardo never even used.

Marx is, of course, the prime target of such rational reconstruction and we will return to this at more length since one of the key theses of this paper is that Marx fully understood and elaborated a very different set of both descriptive and analytical categories for discussing the fluctuations of prices.

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[9] 'The natural price, therefore, is, as it were, the central price, to which the prices of all commodities are continually gravitating. Different accidents may sometimes keep them suspended a good deal above it, and sometimes force them down even somewhat below it. But whatever may be the obstacles which hinder them from settling in this centre of repose and continuance, they are constantly tending towards it.' (Smith 1980:58)

Kurz and Salvadori go to considerable lengths to establish the antecedents of the idea in previous economic thought. But as with Glick, their *oeuvre* is not a work of historical scholarship in the sense of ascertaining what past authors actually said or believed in their own terms. It is a work of re-interpretation; a 'rational reconstruction' of what the authors believe these earlier writers might or even should have thought, had they been operating with the same concept as the rational reconstructor. Thus in Schumpeter's (xxxx) *History of Economic Thought* there is no index reference either to gravity, gravitation or centre of gravity; nor for example in Howard and King's (xxxx) monumental *History of Marxian Economics*. Nor does it even figure in Sraffa (1960), in Pasinetti (1977) or in Steedman's (1977,1981) key works on Marx's economics. It is, it would appear, an entirely modern invention.

Where did it come from?

The first explicit discussion of the idea appears in a discussion of competition among US and German Marxists in the early 1980s. Their key goal was to distance Marxist thinking from then-dominant ideas of what we might loosely term the *Monthly Review* school, which held that persistent profit rate differentials were a permanent feature of modern capitalism in its 'monopoly' stage. They also sought to differentiate themselves from Kaleckian and Keynesian theories of markup pricing, which also drew on the assumption of at least a limited degree of monopoly power on the part of capitalist firms. Finally, they sought to argue that what they termed the 'classical' school – an post-hoc amalgam of Smith, Ricardo, and Marx – possessed a model of competition which differed from the neoclassical model, even though – they argued – this so-called classical tradition shared the conception of general equilibrium with the Walrasian and more generally with the neoclassical tradition

The basic idea was to argue that prices and profits would actually depart from equilibrium in real time in a systematic way, nevertheless oscillating around their equilibrium values.

Writers such as Semmler(1984:21-22) argued that, first of all, profit rate differentials were a normal feature of actual capitalism and part of the mechanism of competition; that, second, there was no evidence that these empirically-observable differentials were the result of monopolistic barriers and, that, finally, over time these profit rate differentials were eliminated by competition.

This enterprise diverged from the Walrasian or neoclassical model of adjustment, which as has long been recognised suffers from the limitation of being a purely virtual process outside of time. The 'auctioneer' calls prices until equilibrium or fixed point prices have been arrived at, and neither trading nor production takes place until these have become the actual market prices. The competition model described by Semmler envisages a 'real' rather than a 'virtual' adjustment process; profit rates really do depart from the hypothetical uniform rate and in consequence prices really do depart from equilibrium production prices. However those profit rates that are higher than average are brought down, because capital migrates to these sectors, increasing production and decreasing prces; and those lower than average are raised up as capital migrates out.

It then had to be explained how, in spite of the tendency of competition to eliminate profit rate differentials, such differentials in fact persisted. The reason, it was argued, is that as a result of this adjustment process, each individual profit rate would oscillate, sometimes above and sometimes below the average. At any one time, therefore, the observer would see a distribution of different profit rates. However no particular sector or firm would remain for ever, or even for a long time, significantly above the average.

The 'image' that was sought to describe this process required to show why profit rates did not simply, having been disturbed, converge to the fixed point and stop there. An obvious

analogy was a planetary system in which, indeed, objects do not move to any fixed point but move in circular or elliptical orbits.

At this point, the mathematically obvious step is to abandon any *prior* assumption about a regulating or fixed point profit rate and write down a set of differential equations which show *how fast* prices adjust to a divergence of supply from demand on the one hand, and how fast capital moves into high profit-rate sectors on the other. There is no need of any concept of equilibrium to write down such a set of equations. For example one may assume that capital movement is determined by the distribution of all profit rates, that is the schedule of available investment opportunities. This step was indeed taken by Duménil and Lévy, who stand out from most other authors in recognising that it is necessary and in attempting to use it to construct a proof of the gravitation concept. We return to this later, for it is the core of any proper assessment of the metaphor.[10]

For now, we note that the great majority of physicalist authors – notably Kurz and Salvadori in what can be considered the definitive modern statement of this idea – do not take this step and indeed, do not understand its necessity despite the many contradictions of simply assuming that the fixed point is the centre, which were once noticed, and then subsequently dismissed. Thus Semmler, drawing on earlier work of Brody (xxxx), recognised a rather important point later re-enforced by Albarracin (1984) that if profit rates are not equal then came close to this idea, the average profit rate is in general not the same as the uniform profit rate.[11] Semmler also recognised that once profit rates begin diverging from the fixed-point uniform profit rate, it becomes completely moot as to whether this fixed-point rate would still be the center of the motion:

> Considering the value vector as the market value – representing here only single process industries – we see that the centers of gravity, around which the market prices are supposed to fluctuate, can themselves change strongly in certain cases (worked out in appendix I). Such cases, of course, would raise the problem of an adjustment process, i.e. the working out of the market process, which would allow the new market values to become the new centers of gravity for actual prices, the market prices. Such an adjustment process, however, may be hard to imagine, since not only does it presuppose a perfect mobility of capital and labor and very high flexibility of prices and quantities in industries, to allow the market process to establish the new centers of gravity; it also presupposes that during the adjustment process no new technical change is initiated by the prevailing market prices. We can conclude from this that long-run relative prices derived from linear production models, in certain cases, might not be very relevant as centers of gravity for market prices.

> The analysis of changes in of prices of production as more concrete centers of gravity for market prices in capitalist production is more complicated than that of values...this brings up again the problem of an economic adjustment process that could provide that the new production prices serve as centers of gravity for market prices...therefore we can conclude again that in certain cases the equilibrium solution of the new production price system might not provide us with a very relevant determination of the long-run prices of production. (Semmler 1984:xx)

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[10] See Freeman (1992) which exhibits a computer simulation of exactly such a model.

[11] Incidentally, directly contradicting the assertion of Mongiovi which we cited in the introduction

This admirable caution did not translate into a recognition that, to put it in a nutshell, the method simply did not work: the fixed point of a system that is not fixed does not in general provide enough information about its dynamic behaviour. Despite caveats, Semmler made a fatal identification which characterises the limits up against which this school perpetually comes: he supposed that *even when profit rates are not uniform, nevertheless prices would be given by the fixed point of the price system*. Instead of simply recognising that market price is the sum of costs and (differential) profit rate, he calculated (Semmler 1984: 147-149) 'long-run' prices which would allow the economy to reproduce indefinitely at these differential profit rates. Then, he argued

> The modified production prices … can be considered the centres of gravity for actual prices, at least for the period of time for which differential profit rates prevail. Because competition and mobility barriers to capital produce deviations from the average rate of profit – and uniform profit rates never exist – it would be more realistic to assume those modified production prices as centers of gravity for market prices for a certain period of time

This is, first of all, a contradiction in terms. The whole point of the competition model which Semmler sought to introduce is that prices and profits are in motion because profit rate differentials cannot be sustained. Capital moves into a sector with high profits, raising the quantity produced and this lowering the price. Therefore, the last thing such an economy can be expected to do is reproduce itself.

Secondly, it actually abandons the idea that the regulating centre of market price is given by the uniform profit rate prices of production and in short, contradicts every claim subsequently made to the effect that Smith and Ricardo's 'natural price' or Marx's 'price of production' have to serve as the regulating centre.

In spite of this and Semmler's well-placed caveats, particularly concerning the consequence for the model of a change in technology during the process of 'adjustment', reservations ceased to figure in writings in which the claim was increasingly advanced that a valid mathematical model can be constructed in which observed prices and profits oscillate around the static equilibrium prices and profits, regardless of the impact of technical change

This idea rapidly attained the status of urban myth, as authors increasingly simply cited each other as evidence of proven conclusions.

A new dimension was added by a second idea: that the 'classical' model of competition is *empirically confirmed* by the behaviour of long-run time averages of profit rates and prices. In Mark Glick's (1985) seminal PhD thesis such long-run averages were actually calculated and shown to converge become more and more stable, the longer the period over which the average is taken. This was increasingly (and by Glick himself) taken as proof not just of the general idea that competition consists of an oscillation of prices and profit rates around some trend, but *also* as confirmation of the theoretical validity of a model which had never in fact been exhibited – in which this trend was identical to the fixed point of the system. Notwithstanding, Glick's work was hailed as a methodological breakthrough for dissident economists:

> Up until a few years ago Stigler's and Brozen's work on [competition and concentration] represented the orthodox opinion, but it was based on limited datasets and primitive econometrics. This opened the door to a more systematic attack on the problem by Mark Glick, Hans Ehrbar and others starting from the Classical point of view of equalization of profit rates using more sophisticated econometrics and uniform, comprehensive data sets.

Subsequently, as Wells notes, it became highly influential among Marxists and a series of works has appeared applying his methodology in a variety of contexts (Christodoulopoulos 1996; Glick and Erbar 1988; Glick and Erbar 1990; Maldonado-Filho 1998; Tsaliki 1998; Wolff and Dollar 1992).

Mongiovi (2002) establishes the perceived conceptual connection between these two ideas: in his view Marx's value analysis is only possible within the Sraffian framework, and that framework *is* a 'centre of gravitation' conception.

> Marx's *value analysis* does indeed contain errors that render it untenable. In view of the complexity of the problems the theory was meant to solve, and the relatively unsophisticated character of the tools Marx had at his disposal, these errors would have been difficult to avoid, even for an intellect of his caliber. But his basic theoretical framework, in support of which he developed the value analysis, is untouched by Bortkiewicz's critique.
>
> That framework conceives of prices as long-period centers of gravitation regulated, together with the rate of return on capital, by the technical conditions of production and the real wage. Profit emerges as a residual, or surplus, whose magnitude depends upon the degree to which workers can be made to produce more output than they and the production process consume. This surplus approach can be developed without reference to Marx's problematic value categories, as in Sraffa's *Production of Commodities by Means of Commodities* (1960). Such formulations not only capture Marx's principal claims about how the profit rate, wages and prices are connected to one another, but also demonstrate that these claims are fundamentally correct.

There is one vital exception to this: the work of Duménil and Lévy, who fully understand the necessity to construct a dynamic model prior to any assertion about equilibrium and who do offer a proof that such a model produces a behaviour they term gravitating. The final part of this paper is dedicated to a deconstruction and disproof of their argument.

However first we have to deal with the pernicious form the idea usually takes, which is the purely metaphorical construction that it works 'because it is like gravity'. In order to deal with this, it is time to connect the signifiers to the signified and ask how gravity, in fact, itself works.

Does the metaphor work?

Figure A below shows the orbit of a typical comet, or perhaps a rather eccentric planet,
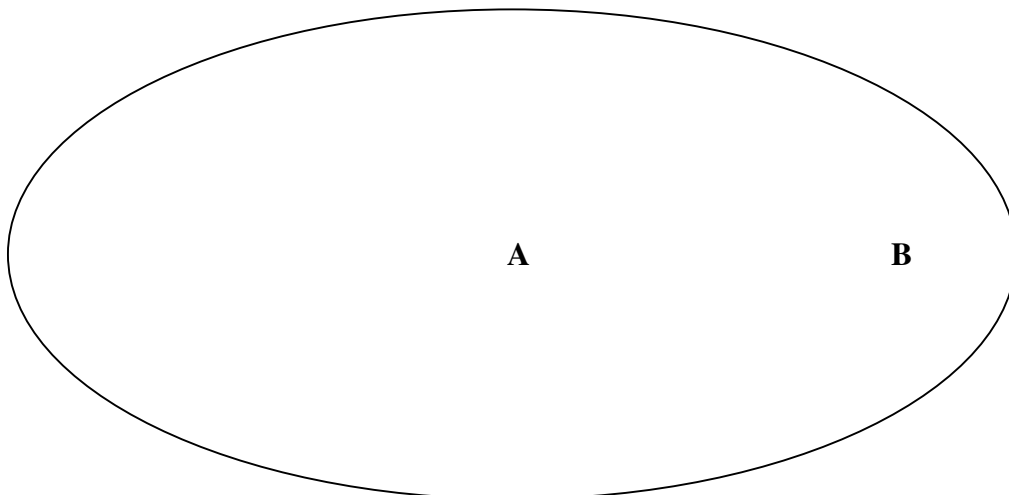
about a typical sun.

Let us consider a few 'questions to the reader' which we can then use as a measure of the applicability of the gravitational metaphor. First, and not least, where actually is the 'centre' of the orbit? There are a number of different places that might occur to us. An unsophisticated idea would be point A, in the middle – not a bad place to start when trying to define 'centre'. It has a number of useful geometric properties which qualify it : it is at the intersection of the minor and major axes of the ellipse; put a straight line through it, and this will intersect the ellipse at two points equidistant from A, and so on.

Is it the centre of gravity of the system? [12] No. This is to be found at B, which is one focus of the ellipse. [13] Is it a fixed point of the system? No; unless one is prepared to move the sun as well as the planet in which case anywhere in the universe is a fixed point since, if
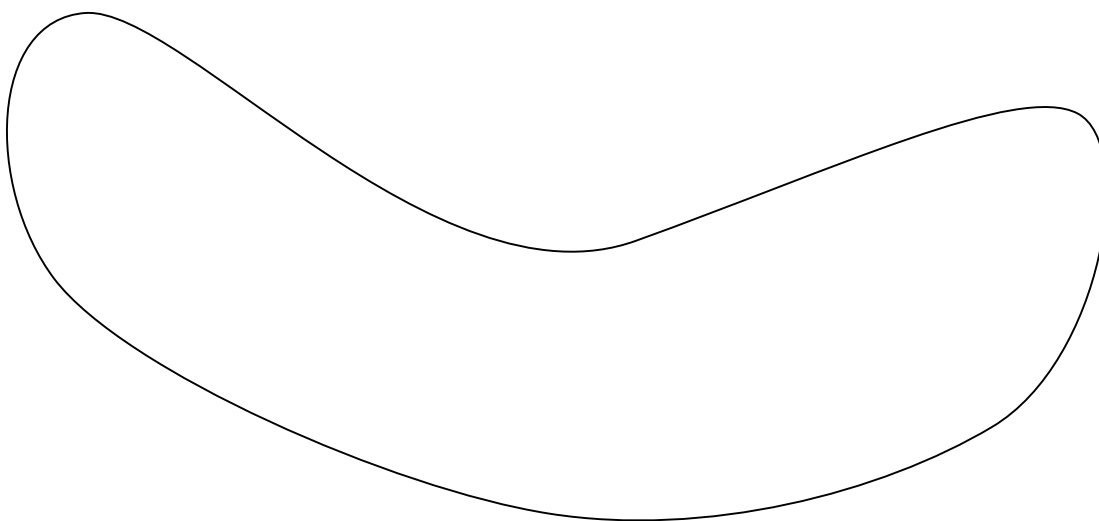
you put the two together they will not move apart.

Figure 2: non-symmetric orbits

Actually, there are a number of disadvantages of point 'A' as centre. It relies on the symmetry of the ellipse but if had a non-symmetric motion, as in figure 2 above, then there is no obvious 'centre' in the sense of point A. What about the time-average of the position? This raises a further obvious objection to 'A', namely it takes no account of the time that the planet spends in various locations. So what about the time-average of the position of the planet?

As figure 3 shows, this is even worse for the 'centre of gravity' conception. In this diagram we have solved Kepler's equations of motion to show the actual time that the planet spends in various places. The left and top quadrant shows its motion along the y-axis and the bottom right quadrant shows its motion along the x-axis. *Most* of the time the planet is actually quite distant from the sun as the y-axis diagram shows. Its movement consists of a series of 'swoops' towards the sun which are the peaks of the curve, interspersed by long periods far away from the sun – where, since gravity is weaker, it moves more slowly.

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[12] As Feynmann rightly points out, the centre of mass is not the same as the centre of gravity and in an astronomical system where there are no rigid constraints, it is not in the same place. But this is only a metaphor, so we will leave this point on one side.

[13] We have assumed for simplicity that the sun is so massive that the centre of gravity is effectively the centre of the sun itself. Actually, the centre of gravity is somewhere between the sun and the planet, being a weighted average of their positions.

The time average of this motion puts the comet at the *opposite* end of the system to the centre of gravity.

Figure 3: non-equivalence of centre of gravity and time average for planetary orbits
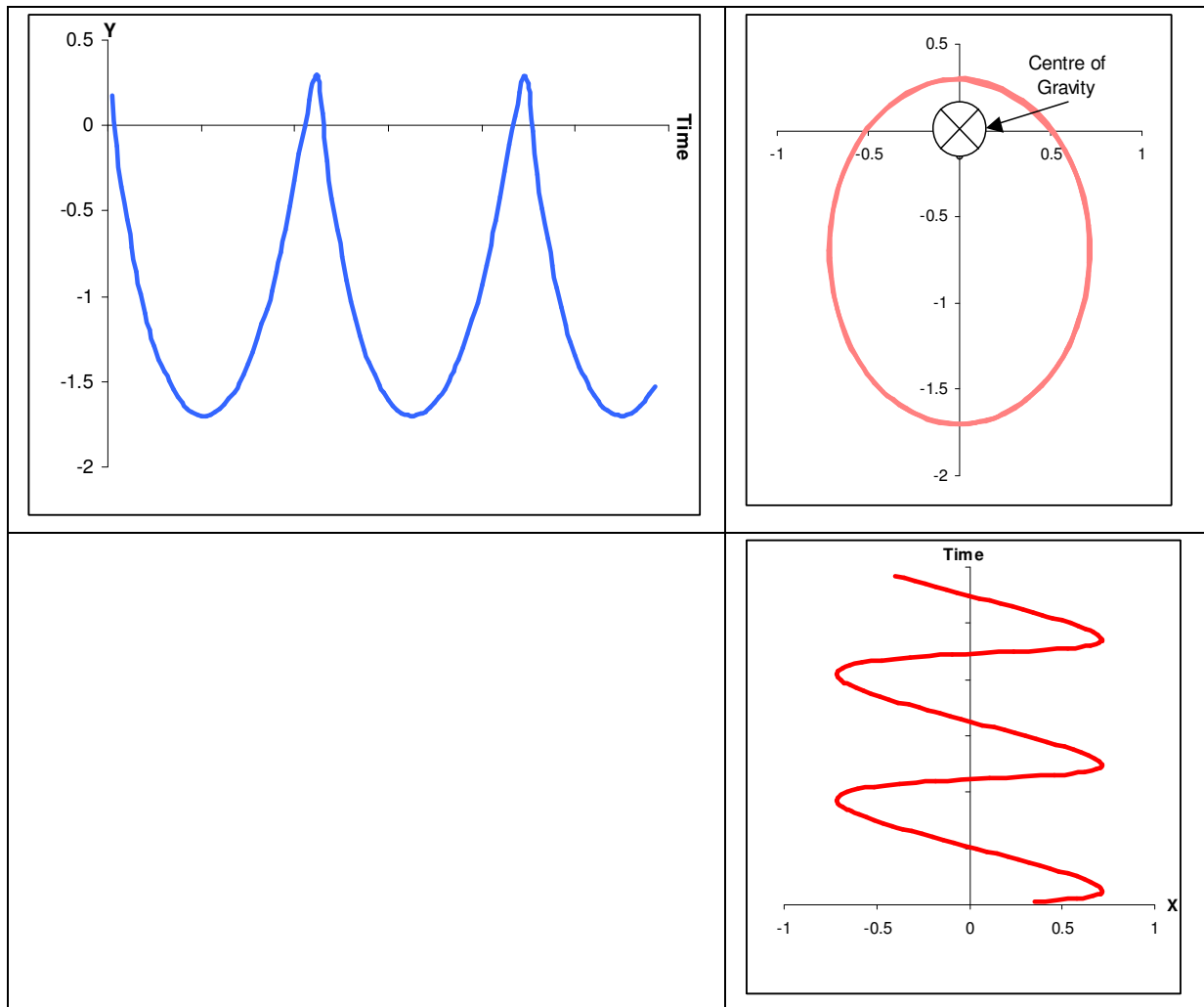
What is the relation between all these ideas and the central concept of the classical astronomical theory of orbits, which is what is technically termed the 'focus' of the conic of motion. There are in fact two of these, one at each 'end' of the ellipse.[14] A metaphor assigns, to each of its terms, a corresponding term in the real world structure it is intended to explain or imitate. Each 'signifier' has a 'signified'; thus in the centre of gravity metaphor, a solid particle corresponds to an enterprise or branch of industry, its price to distance, the 'force' upon it to some relation between distance and speed of change, and so on. The problem is that in the centre of gravity metaphor there is a 'missing signifier' – the centre of gravity itself. There is no economic equivalent of this idea. It does not exist. But in a mechanical system such a centre does exist as a consequence of the specific form of the laws relating position to acceleration. The mind therefore connects this 'missing signifier' to the static equilibrium of the system which is then endowed with almost mystical powers of attraction. It is assumed that this equilibrium must be the absent

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[14] Also possible is a hyperbolic orbit, in which an object has sufficient energy to approach once, and leave for ever. Thankfully, no author has yet sought to identify this signifier with anything in economics.

centre, the point around which things oscillate. Why? Because the system 'needs' a centre. Why does it 'need' a centre? Because a real gravitational system has one.

A little thought shows that, first of all, there is no logical necessity, in speaking of the 'centre' of any fluctuating motion, to have any model of that motion at all, let alone the prior assertion that it is attracted to that centre or that this attractor is a stable or equilibrium point of the system. Indeed, *Glick's studies already achieved this*. He provides a simple arithmetical calculation, which is the average over some definite time period of whatever variable is being studied. The time trend of any motion, as any economist recognises, can be calculated completely without recourse to any model at all.

Second, if this motion involves a large number of independent profit rates, then a very simple *alternative* notion of 'centre' presents itself, which is the arithmetic average at any one time of these profit rates, weighted of course by the sizes of the capitals involved.

There is absolutely nothing about the idea of an average profit rate which says that it also has to be a fixed point. The economist may *superimpose the hypothesis* that, were all individual profit rates actually to become equal to the average, the economy would stop moving.[15] But this hypothesis is not required to calculate this average. An average is an average is an average. It is obtained by adding and dividing.

To summarise, a perfectly valid and usable idea of 'centre of the motion' of a single profit rate or price is to hand which makes no reference to fixed points, namely the time average or trend of its magnitude; and a perfectly valid alternative *explanatory* variable is to hand for this trend, namely the instantaneous simple arithmetic average profit rate for profits and, for prices, cost marked up by this average.

A 'persistent' profit rate differential would then show up as a time average that different systematically from the moving instantaneous average. Movement 'about a centre' would show up if the time average, over a long enough interval, converged to the moving instantaneous average.

This is an *empirical* matter. It cannot be settled by some prior diktat which states that only the fixed point can serve as the centre of the motion.

Why economic systems oscillate: Convergence, non-convergence, and limiting behaviour

We now turn to the correct approach adopted by Duménil and Lévy, of first constructing a dynamic system and then attempting to establish its limiting properties. The problem in this case lies not in the fraudulent insertion of the missing signifier but in the *assumptions concerning competition* which are introduced by their model. The model in effect contains a *petitio principi*: it has, built into it, the assumptions that bring about the result which is to be proved.

Why do economic systems oscillate? Why don't they simply follow a straight track to wherever they are going? One aspect of this is pure chance; random disturbances.

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[15] The economist would be foolish to do so because, first of all, this never happens and, second, it wouldn't stop moving. As soon as a new investment opportunity arose as a result of a change in tastes or a new invention, capital would flow into this new opportunity attracting a differential profit either because of exceptionally low costs (higher productivity) or exceptional demand (change in tastes).

Two fundamental lines of thought in economics: that market systems are *inherently disturbed* but nevertheless exhibit regular behaviour, and that they are *inherently regular* but nevertheless get perturbed.

Our basic point at the start; repulsion is an inherent feature both of market behaviour and of capital movement.

Equilibria are *not stable*; divergence does not come about through shocks but through the natural behaviour of the system.

Repulsion and non-linearity. The fundamental problem this appears to introduce is, since the system is inherently repulsive, why does it not just fly apart? Goodwin: non-linearity. There is a limit on divergence caused by an tendency to convergence which *increases* with distance.

Natural dynamic behaviour of an economic system is hence a limit cycle, not a single attractor.

A little thought about the dynamical system that this implies shows that convergence is a logical impossibility: the closer together are the objects in the system, the less the tendency for them to approach each other. In the limit, there is no reason for them to coincide at all. In short, the limiting process of the tendency towards equilibrium, as theorised by virtually all writers on equilibrium, is that this tendency is indefinitely long and, logically, never completes itself.[16]

This leads us to the second problem which is that there is no unambiguous concept of the 'centre' of a system that does not actually converge. A non-convergent system does not possess a limit point. As noted above, the fact that a system possesses an equilibrium, even a stable one, does not at all mean that this equilibrium must be a limit point. Many, and indeed most, systems possess equilibrium points to which the objects in the system never converge – not least, the solar system, *corpus mysticum* of the post-Sraffian universe.

In consequence the property to which Duménil and Lévy refer, of 'faster' or 'slower convergence' does not occur. There is no convergence at all. The very fact that their model predicts it, shows that this model cannot be true. This is empirically confirmed: no known economic variable converges to anything. The profits on, and the prices and quantities of, all goods consumed and produced, not least labour, never stop changing. in no meaningful sense do they 'get closer' to anything. 'Convergence' is a purely ideal construction which corresponds neither to anything logically compatible with the concept of equilibrium, nor to anything empirically observed.

This leads to our third problem. As long as there is a limit point, there is indeed a highly seductive, single, and 'obvious' definition of what we might mean by the idea of the 'centre' of a dynamic motion, and it is indeed identical to the equilibrium. But if there is no limit point, there is no longer an obvious definition of the idea of 'centre'. What might this word mean, if it does not mean 'that to which motion empirically tends'?

Once we begin to think without recourse to the concept of convergence, an entire different series of possibilities emerges. In effect, once we seriously attempt to conceive of the idea of the 'centre' of a periodic motion without reference to an equilibrium limit

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[16] This loose idea has a precise meaning which dates back to Zeno's paradox. An oscillating process can terminate, in theory, in a finite time. One well-known example is a bouncing ball in which each bounce is half the height of the previous bounce. Although logically, the ball 'never stops bouncing', nevertheless, there is a definite finite time interval after which it cannot be bouncing. The point is that for a system in which the speed of movement towards equilibrium diminishes, the smaller the distance from equilibrium, such a definite finite time interval does not exist. Convergence *literally* takes for ever.

point, it is only at this point that we realise what a complex idea this actually is. It is by no means an obvious concept: it only appears so, if we presuppose convergence. Nor may we, without careful textual study, suppose that by 'centre of gravity', the classical writers entertained any idea of convergence. Indeed, if they were truly influenced by the current conceptions of gravitational centre, it is very unlikely that convergence was what they had in mind, since a gravitational system *par excellence* does not converge. Neither the remotest comet, nor the closest planet, ever ends up *in* the sun. Without exception, the heavenly bodies revolve *around* it. If, therefore, the classics entertained the notion of convergence, or indeed the notion of equilibrium, they must have had in mind something altogether different from the idea of 'centre of gravity'.

Well, is a different conception of 'centre' available? Do we need to presuppose convergence in order to discuss locus? At least two intuitively reasonable alternative concepts of 'centre' were common coin in the early nineteenth century and remain to hand today. One such is the *trend* or time average of a single price or profit, and the other is the *instantaneous* average of a large number of prices or profits – as it happens, the closest analogy to the conception of 'centre of gravity' in mechanics, which is not defined with respect to time. Neither of these ideas presuppose any stable equilibrium or fixed point. They are simple statistical parameters, which any first-year economic student learns how to calculate.

Post-Sraffian authors suppose without question that these trend or instantaneous averages are simply another variant of the so-called 'long-run' state of the economy, because their analytical framework admits of only a single rate of profit and only a single set of prices that correspond to it, namely, the fixed point of the system. It does not appear to enter their minds, and certainly does not enter their texts, to note that this is an empirical matter: there is no necessary relation between fixed point and average, and in an actual economy the two may coincide, or they may not.

This brings us to the fourth and final problem, and the main issue which this article addresses: what happens when the exogenous conditions which fix the position of the static equilibrium are themselves changing over time, so that the alleged 'attractor' is itself in motion and it becomes *impossible* for the prices or profits to stop moving? Since, as we have noted, the time of convergence of any economic system is logically infinite, it is surely not unreasonable to ask whether, during the infinite time which the economy requires to reach its equilibrium state, the exogenous conditions determining the position of this equilibrium may conceivably have changed.

But, we will show, whenever the 'long-run' or moving equilibrium position changes over time, the centre of hypothetical convergence parts company from the dynamic average. Drag a pendulum across a paper and the average of its shadow is *not* the same as the shadow of its average. In the simplest conceivable dynamic models of an economy in which technological change is taking place, I will show, *the trend of the dynamic system departs from the trend of its long-run equilibrium*. This departure moreover is not of some minor or ignorable nature. One prediction goes up, and the other goes down. Nor is it transient. As long as technological change is taking place, the two predictions diverge. The greater the pace of change, the greater the divergence. They diverge, moreover, for ever. There is no respite, no wriggle room, no space to argue that one trajectory in any meaningful sense 'tracks' the other. Convergence begins *only* if technical change ceases. In short, we confront *two alternative predictions of the real movement of the economy*: the post-Sraffian prediction, which is in general that of economics as a whole, that its real course is a fluctuation about a moving static equilibrium in which the 'adjustment' process can have no impact on the final outcome, and the dynamic system prediction.

Logic, metaphor and proof

This idea is the core of the post-Sraffian argument, but in fact it is empirically false and logically ungrounded. There is absolutely nothing about the idea of gravitation which obliges us to suppose that the centre of a system must in any meaningful sense be the equilibrium state of this same system, and in general the two do not coincide.

Therefore, the idea that the real movement of prices and profits is governed by their hypothetical equilibrium or long-run position – an argument sustained neither by logic nor evidence but which depends critically on the 'centre of gravity' metaphor – sets limits on our understanding which are not only fatally injurious to the advance of knowledge but are indeed hold it back.

I will show that classic references to 'gravitation' employ a different metaphor, free of the limitations of the post-Sraffian construction, in that in their thought no necessary connection is to be found, explicit or otherwise between the centre of gravity of the system and the hypothetical equilibrium state of the system.
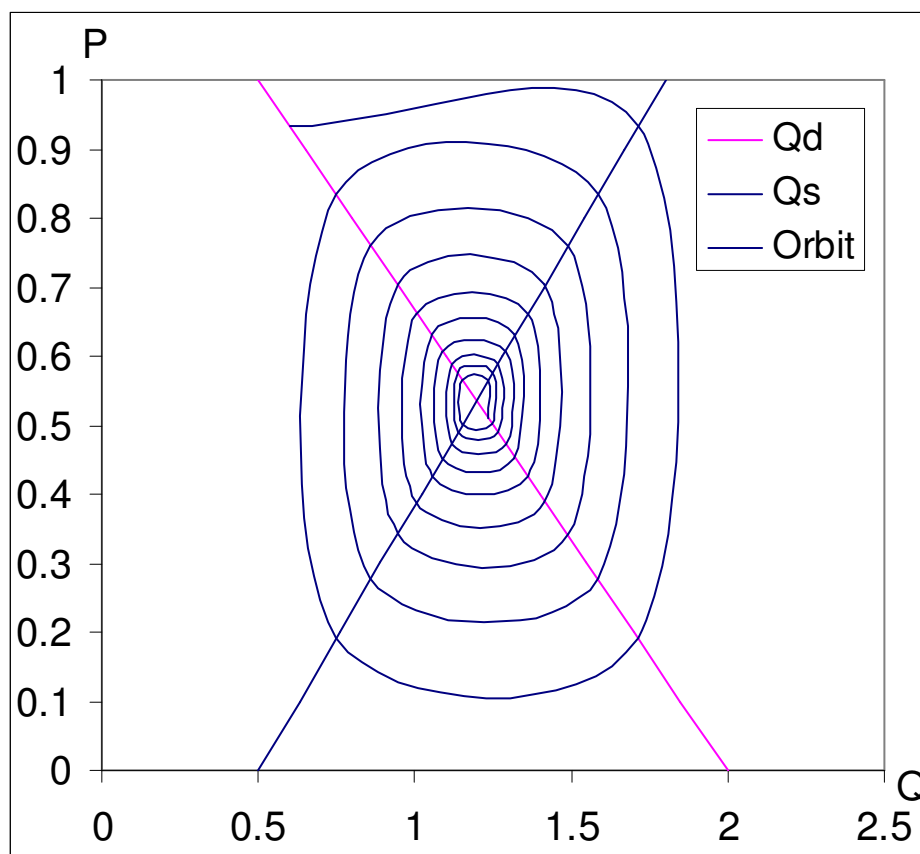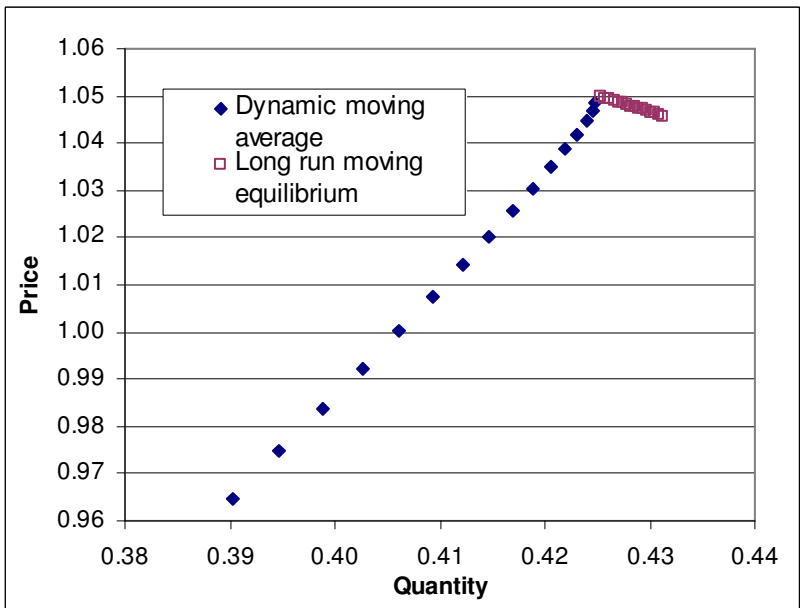

'Attractor' – a point to which motion converges
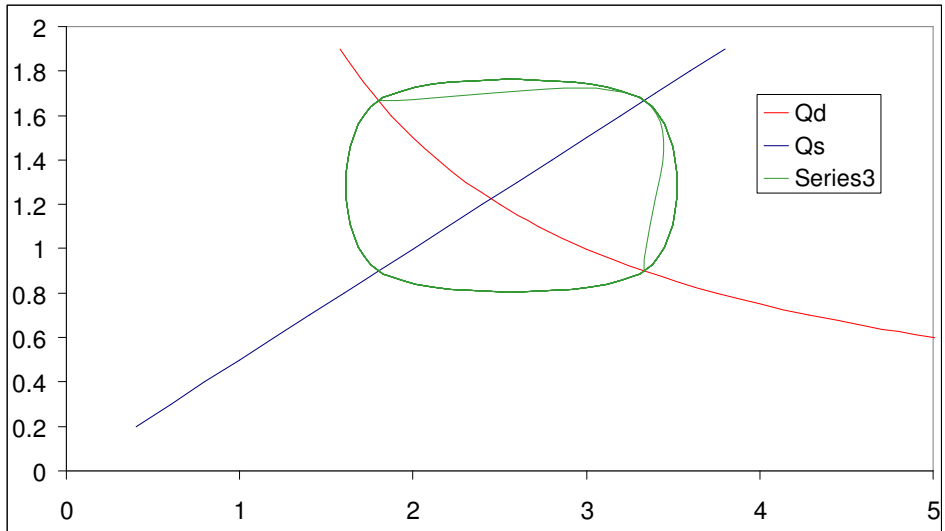
But in a limit cycle there is no convergence. Where is the average of this motion? Why should it be considered identical with the point which would be the attractor, were the oscillation convergence? Convergence is a hidden signifier.

Dynamic cobweb

Suppose that, while the cobweb is in motion, technological progress takes place. Then the supply curve itself moves. In particular, its slope declines over time, that is, the price per unit output, as a function of output, falls. Each time around the cobweb, we find that the supply curve has moved, be it ever so little, downwards and to the right. In consequence, the quantity which they will supply, at any given price, is ever larger and so the long-run moving equilibrium moves to the right. But when the cobweb hits the supply curve, it finds itself in a different place to the equilibrium and so the dynamically-determined quantity moves to the left.

If the supply curve moves while the motion is going on the two conceptions part company. The dynamic moving average moves in a different direction from the long-run moving equilibrium.

What centre of gravity is not

It is not the place that everything would end up, if movement ceased.

It is not the place that the planets tend to.

It is not the time average of the movement of the planets

It is not the sun

The pressure is not greater, the further from the body concerned.

It is not imaginary

The claim of historical authenticity

In earlier SM writings the concept was employed more cautiously. Shaikh (1998:225) writes:

> The purpose of this chapter is to explore the theoretical and empirical properties of what Ricardo and Smith called natural prices, and what Marx called prices of production. Classical and Marxian theories of competition argue two things about such prices. First, that the mobility of capital between sectors will ensure that they will act as centres of gravity of actual market prices (Marx, 1972, pp 174-5; Shaikh, 1984, pp.48-9) Second, that these regulating prices are themselves dominated by the actual structure of production.

The cited passage from Marx makes no reference to centre of gravity and expresses an idea rather distant from that of Duménil and Lévy, as we shall see:

> The determination of price by the cost of production is not to be understood in the sense of the bourgeois economists. The economists say that the average price of commodities equals the cost of production: that is the law. The anarchic movement, in which the rise is compensated for by a fall and the fall by a rise, they regard as an accident. We might just as well consider the fluctuations as the law, and the determination of the price by cost of production as an accident – as is, in fact, done by certain other economists. But it is precisely these fluctuations which, viewed more closely, carry the most frightful devastation in their train, and, like an earthquake, cause bourgeois society to shake to its very foundations – it is precisely these fluctuations that force the price to conform to the cost of production. In the totality of this disorderly movement is to be found its order. In the total course of this industrial anarchy, in this circular movement, competition balances, as it were, the one extravagance by the other.

> We thus see that the price of a commodity is indeed determined by its cost of production, but in such a manner that the periods in which the price of these

> commodities rises above the costs of production are balanced by the periods in which it sinks below the cost of production, and vice versa.[17]

There is in this passage no notion at all of equilibrium, unless one simply asserts without proof (as many do) that by the term 'cost of production' Marx simply must have meant equilibrium price. Moreover Marx here ('we might just as well consider the fluctuations as the law') identifies the central issue in any study of the relation between stasis and movement, namely is the centre a dynamic outcome of the movement, or is the movement merely an accidental deviation from the centre?

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