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Intriguing pendula: founding metaphors in the analysis of economic fluctuations

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The paper is an inquiry into the definition of the early econometric programme, namely into the discussions which Frisch and Schumpeter held in the early 1930s about the most suitable model for representing innovations, change and equilibrium in economics.

The argument and its framework are briefly presented in the first section. The 1931 correspondence between the two founders of the Econometric Society is discussed in the second section. It provides a magnificent example of the importance of rhetorics in economics, of the heuristic role of constitutive metaphors in a research programme and of the difficulties in defining the most suitable mathematical formalism for dealing with cycles and structural change. The third section presents the conclusion of the story: the bifurcation between the resulting contributions made by Frisch (Propagation problems and impulse problems in dynamic economics, pp. 171–205 in Koch, K. (ed.), *Economic Essays in Honour of Gustav Cassel*, London, Frank Cass, 1933) and Schumpeter (*Business Cycles*, New York, McGraw, 1939; and the posthumous volume, *History of Economic Analysis*, London, Routledge, 1954). Finally, the fourth section presents an alternative epilogue, highlighting some of the hidden implications of these verbal accounts of pendula as the founding metaphor for business cycles.

The paper is based upon as yet unpublished papers that were found in Frisch's Collections (Oslo University Library and Frisch's Rommet at the Institute of Economics) and Schumpeter's Collection (Harvard University).

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JEL classifications: B23, B31, C32, C51, C62

Joseph Schumpeter's main contribution to economics was a passionate defence of the historical approach to cycles as representing the crucial feature of the dynamics of capitalism. Although a staunch supporter of the use of mathematics, as well as a founder and thereafter one of the leading figures and President of the Econometric Society, Schumpeter distinguished himself as an intensely dedicated researcher in the field of concrete historical

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* ISEG/UTL, Lisbon. I wish to thank Mrs Ragna Frisch Hasnaoui for permission to publish parts of her father's archives. I am also grateful for comments and suggestions by David Acheson, Marcel Boumans, Chris Freeman, Hooshang Hemami, Arjo Klamer, David Lane, Marji Lines, Alfredo Medio, Mary Morgan, D. P. O'Brien, António Sousa Ramos, Geert Reuten, António St. Aubyn, Rui Vilela-Mendes, Roy Weintraub and two anonymous referees. Nevertheless, they are innocent of all the mistakes in this paper. Preliminary versions of this paper were presented at seminars and conferences in Antwerp, Campinas, Lisbon, Amsterdam and Athens.

processes and not as a mathematical economist. He eventually became the most frequently quoted economist in the first decades of the century, until the glamorous triumph of Keynes' *The General Theory*. Schumpeter's main publications are historical in the sense that they represented applied historical and conceptual work (*Business Cycles*, 1939; hereafter BC), as well as being controversial in their interpretation of historical and contemporary trends (*Capitalism, Socialism and Democracy*, 1942; hereafter CSD) and providing the historical account of the science itself (*History of Economic Analysis*, published posthumously in 1954; hereafter HEA). His single most important contribution, and indeed the major reason for contemporary attention to his work, was his analysis of innovation, creative destruction and disequilibrium processes in modern capitalism.

This paper presents the important but largely ignored debate that challenged Schumpeter's concept of innovation. His own definition was discussed from the viewpoint of the requirements for an econometric approach to cycles and structural change, as presented by his close friend Ragnar Frisch. This discussion highlights the crucial importance of one metaphor, that of the pendulum, for the purposes of persuasion and representation, as well as for the creation of new conjectures in economics, and in particular for the development of the econometric programme for the study of fluctuations and time series. Among other metaphors (the rocking horse, the rock hanging over a river, the violin, etc.), the pendulum may be singled out as an exceptionally powerful representation of oscillations under capitalism. This metaphor also indicates a paradox: in spite of being devised as an argument in favour of equilibrium and the traditional inference from time series, the pendulum concept allows for a variety of alternatives, some of which imply a regime of simple gravitation towards equilibrium while others imply chaotic attractors.

1. Inner or outer?

Schumpeter's concept of innovation has been widely known ever since the publication of his first influential book, *The Theory of Economic Development* (1911). Innovation was systematically presented as the encapsulation of a driving force for change that emerged from economic development, a process akin to that of biological mutation, pioneered by entrepreneurs, who were able to incorporate into the economic world new methods of organisation, new products or processes, or to create new markets. This concept was influenced by both the tragic tradition, so present in the German cultural environment of the time, and the early impact of J. B. Clark's 1899 book, *The Distribution of Wealth* (Louçã, 1997, pp. 237f.). Yet Schumpeter developed it from an original viewpoint that accounts for his fame.

Schumpeter's lifelong project was to create a general theory superseding and including that of Walras, an economist he admired more than all the others, but whose theory was considered to be wrong if taken in isolation, since it just accounted for static processes. 'I felt very strongly that this [the presentation of economics as the explanation of exclusively static processes] was wrong and that there was a source of energy within the economic system which would of itself disrupt any equilibrium that might be attained', wrote Schumpeter about his conversation with the ageing Walras in 1909 (Schumpeter, 1937, p. 160). Therefore, a truly general theory ought to include equilibrium and statics as well as disequilibrium and dynamics, i.e., economic processes describing the reality of capitalism. This was repeatedly emphasised by Schumpeter in his most important works and represented his crucial contribution to the study of innovation:

industrial mutation—if I may use the biological term—that incessantly revolutionises the economic structure from within, incessantly destroying the old one, incessantly creating a new one. This process of Creative Destruction is the essential fact about capitalism. (CSD, 1942, p. 83)

Or, as he categorically stated in his last major text:

Social phenomena constitute a unique process in historic time, and incessant and irreversible change is their most obvious characteristic. If by Evolutionism we mean no more than recognition of this fact, then all reasoning about social phenomena must be either evolutionary in itself or else bear upon evolution. (HEA, 1954, p. 435)

This evolutionary approach included several important features that are not discussed in this paper, such as the consideration of distinct modes of change and time dimensions (Kondratiev cycles of infrastructural change and Juglar cycles of industrial change), supposed to determine the analysis of fluctuations. But the decisive point, the one that distinguished Schumpeter from his colleagues, was the claim that innovation and destructive change are to be seen as central characteristics of the self-organisation process under capitalism. The evolutionary process incorporated internal change in the structure of the economy. An undated manuscript found at Harvard,[1] *Statistical Evidence as to the Causes of Business Fluctuations*, presents the argument in a nutshell:

Summing up, it may be stated that statistical evidence suggests and in a sense even proves that business fluctuations are produced:

(a) By the impact of factors external to the business organisation;
(b) By an evolutionary process within the business organism which is what is popularly meant by economic progress;
(c) By the reactive response of the business organism to both.

This is a reasonably faithful representation of Schumpeter's lifelong adherence to the distinction between external and secondary factors in the development process, and the internal changes that represented the strength and the essence of entrepreneurial capitalism, in the same sense as he emphasised this distinction later on (BC, 1939, p. 68). His close friend Ragnar Frisch, the main driving force behind the foundation of econometrics, shared the same concerns and considered the understanding of business cycles to be the primary task of economists. But he addressed the question from a rather different viewpoint, since he suggested a mathematical approach for the sake of obtaining the level of formal rigour best suited to the normative action that was desired. Their correspondence discussed at great length the possible options for the representation of the economic system and its cycles: while Schumpeter described a very complex causal system, Frisch represented this same system as a rather simple mechanism. As a consequence, there was a rather obvious contradiction between Schumpeter's approach and the quite different representation of his own theory by Frisch, who proposed the deterministic and passive system and the exogenous but small perturbations as the sole factors responsible for fluctuations and the dynamics of the economic system. If this is true, we have exogenous causality determining the movement, plus an endogenous filtering mechanism, determining the shape of this movement. This latter mechanism is identified through its mechanical properties, i.e., accepting a clear distinction between (exogenous) causality and intelligibility (understanding the mechanism itself).

[1] Manuscripts from the Harvard Archive are indicated as HU, those from Oslo University or Oslo University Library as OU. Frisch's letters were clearly typewritten, whereas Schumpeter's were handwritten and are in a very poor condition (some of the words are quite difficult to decipher, and whenever this was not possible they are marked as [.]). Any emphasis is given by the authors, unless otherwise stated.

Much later, Frisch's paper on this subject, the one published in 1933 in the volume printed in honour of Cassel, was to win him the first Nobel Prize to be awarded in Economics (1969, shared jointly with Tinbergen) and represented a crucial departure for the econometric approach of time series and cycles. Schumpeter referred to this paper repeatedly and always approvingly in his later books,[1] in spite of the obvious differences between his own explanation and this model, which reduced the cyclical mechanism to exogenous impulses impinging on the propagation and equilibrating system. This followed Wicksell and Åckerman's metaphor of the rocking horse, which soon became the long-standing paradigm for the analysis of cycles. Erratic shocks were considered to be the source of strictly exogenous impulses, and therefore the theory could not account for internally generated mechanisms of historical change. The extension of the model, provided by Frisch to account for Schumpeter's objections, was not fully satisfactory for the latter, as we shall see.

This contradiction has remained unnoticed by most authors working on the subject, since the intellectual relationship between Schumpeter and Frisch has yet to be studied, and since the relevant documents, some private letters, were never published. Evidence shows that both authors discussed these topics at length, that their concepts did not match, and that consequently much misunderstanding remained. Furthermore, Schumpeter never fully accepted the powerful explanation and method his colleague was using and, consequently, could never follow the econometricians in their own particular terrain in the study of cycles.

2. Magellan's dreams

Schumpeter and Frisch first met in February 1928 at Harvard.[2] At that time, Frisch was giving a series of seminars on time series at Yale at the invitation of Irving Fisher, and Schumpeter was also travelling in the United States. From the first moment, they engaged in fruitful discussion, in spite of the differences between them, which were quite striking. Frisch was 12 years younger, a mathematically inclined economist with left-wing ideas, whereas Schumpeter was a respected and widely quoted theorist, who had already published a number of influential books, having occupied the position of Austrian Minister of Finance and later even directed a bank, and he was—politically at least—a conservative. Yet they became close friends and shared great enthusiasm for a number of projects, such as the creation of the econometric movement, the publication of *Econometrica* and their research into long and short cycles.

They corresponded intensely for many years, until Schumpeter's death (1950), and, whenever possible, meetings were arranged. In the period under consideration here, they met again in September 1931 at Lausanne and Bonn, where Schumpeter was teaching before his departure for Harvard. The material considered in this section dates from just before that meeting.[3]

[1] References to Frisch's 1933 paper can be found in *Business Cycles* (171n., 181n., 189) and in *History of Economic Analysis* (1162n.). Schumpeter never made any direct criticism of the paper in public, although he discussed its major features in private letters, as we shall see.

[2] The meeting took place on 29 February 1928, at the Colonial Club at Harvard. Haberler was also present, and the purpose of their discussion was to establish the list of the mathematical economists to be contacted for the creation of the Econometric Society. The Society was in fact founded in December 1930, and the leading figures in the first years were Frisch, Schumpeter, Roos and Fisher.

[3] Some of the letters from this period are apparently lost, since they could not be found either at Harvard or in the Oslo Collection. Furthermore, the fact that they met in September after an intense correspondence in the spring and summer of 1931 implies that some of the discussion was never actually put into writing.

It should be added that by the end of the 1920s and in the early 1930s, Schumpeter and Frisch did not just share a passion for the creation of the *Econometric Society*: they were simultaneously engaged in time series analysis, although they used different methods and concepts. Frisch had just circulated his paper on time series (1927) and was preparing what came to be known as 'Propagation Problems and Impulse Problems in Economic Dynamics' (hereafter PPIP, 1933), his mature work on cycles, whereas Schumpeter was already engaged in the preparations of his *magnum opus*, the seminal work *Business Cycles* (1939). There was an obvious common ground that they were glad to recognise: they both intended to explain how change occurred, they accepted the existence of different modes of oscillations and sought to construct a formal model of the cyclical process within an analytical and rigorous framework.

It was therefore quite natural that their first meetings were largely devoted to discussing this topic. Evidence nevertheless shows that their respective points of view were quite different and that it was not easy for them to create a common conceptual language that could be used to understand and compare their respective approaches and models. Furthermore, there is evidence that Frisch took the initiative in this argument, since he was better equipped from the point of view of formal and mathematical reasoning. In 1927, 1928 and the early 1930s, Frisch began the construction of the model, establishing the distinction between the 'impulse' and the 'propagation' problem. This also proves that, by the time of their first discussion, the idea of a mechanical representation of a damping system was already clearly formulated and that its implications were well understood by Frisch, who tried to reduce Schumpeter's theory to his own conceptual model.

The pendulum was already at that time an important reference for the analysis of cycles. Indeed, it had dominated the rhetoric of cycle analysis prior to the use of the rocking horse analogy: Marx, Fisher, Yule and Hotelling, among others, had used the pendulum metaphor in previous years. Frisch used it in 1931, when he made his first efforts to model cycles as fluctuations submitted to friction, and so did Tinbergen in 1935. Strictly speaking, there was no substantial analytical difference between the dissipative pendulum and the rocking horse, since both were conceived of as mechanisms filtering and damping free oscillations, although the analogy of the horse suggested a somewhat more interventionist impulse system.

Given his need to impose a new basis for looking at the formal models of business cycles, which he represented as a mixed system of differential and difference equations, Frisch also used the analogy of the rocking horse, in order to extend the deterministic system and to introduce exogenous random shocks into the reasoning. The metaphor was originally suggested in a footnote by Wicksell, and then referred to by Åckerman in his doctoral thesis. And both references would probably have been condemned to obscurity, if Frisch had not considered Wicksell the greatest economist and if he had not been a member of Åckerman's jury in 1928: he quickly incorporated the metaphor in his own research and formulated a seminal model of cycles inspired by this insight.[1] Curiously enough, this metaphor of the rocking horse, in spite of its relevance for the diffusion of the

[1] The Cassel paper wrongly refers to the origin of this reference. Wicksell's metaphor appeared in his 1918 review of a paper by Petander, 'Karl Petander: Goda och darliga tider', in *Ekonomisk Tidskrift*, vol. 19, 66–73, in a footnote to p. 71: 'if you hit a rocking horse with a stick, the movement of the horse will be very different from that of the stick. The hits are the cause of the movement, but the system's own equilibrium laws condition the form of the movement' (quoted in Thalberg, 1990: 115n.; also Velupillai, 1992, p. 70n.). Frisch gave 1907 as the date for the original formulation of this metaphor. It constituted the single most important starting point for the econometric analysis of the cycle, and the metaphor explicitly or implicitly dominated the research programme for a very long time (Louçã, 1997, pp. 117f.).

piece in the Cassel *Festschrift*, did not play any important role in his correspondence with Schumpeter, since both immediately felt that it could not account for the specificity of innovations.

Frisch's intellectual strategy was precisely based on the antinomy between the role of Slutsky's random shocks, which generated change, and the stabilising properties of the body of the system, which reduced such impacts to the precise form of the cycle. As a consequence, the movement of a damping propagation mechanism was represented by the wooden horse, which was supposed to be under the impact of frequent kicks, making it rock. But when Frisch corresponded with Schumpeter, both easily understood that this rocking horse, moved by the unexplainable kicks, could not represent major systemic changes, in particular the bursts of innovation Schumpeter had in mind. Instead, Frisch used a peculiar version of the previously accepted metaphor, which was to dominate his construction of the argument—that of a pendulum hit by exogenous shocks. This metaphor became a powerful heuristic device that contributed to the orientation of future research: it was in fact more suitable, since the rocking horse suggested the dominance of a damping mechanism, while the pendulum suggested instead the greater influence of 'innovations'.

The following discussion highlights some of the reasons for the simultaneous use of both metaphors and the distinction between them, and underlines Schumpeter and Frisch's attempts to reach an agreement that ultimately collapsed, although neither of them explicitly recognised its failure or the great chasm between their conceptions. The first piece of the evidence is the letter Frisch wrote to Schumpeter on 28 May 1931. It indicates that Frisch was already approaching the definition of his analytical solution:

> I think I understand now your point about dynamics. Those things you mention: the more or less unpredictable innovations are those things that in my terminology would form the substance of the *impulse problem*, as distinguished from the *propagation problem*. Some other time I want to write you more fully about this. (OU)

Schumpeter answered on 10 June 1931.[1] From the outset, the letter openly stated his reservations about the pendulum analogy:

> This [the discussion of the nature of statics, 'a problem à la pendulum'] would be all, if data did not vary except by influences which we could call influences 'from without' or by 'growth'. But there is an agent, within the economic world (=system of quantities) which alters data and with these the economic process: entrepreneurial activity, which I have elsewhere given the reasons for considering as something sui generis (and the sociology of it). (. . .) It not only destroys existing equilibrium, but also that circuit-like process of economic life, it makes economic things <u>change</u> instead of making them <u>recur</u>. And its effects are not recurring—Ford can never be repeated—but 'historic' and definitely located in historical time. They are also irreversible. This distinction acquires importance owing to the importance of the phenomena incident to the mechanism by which 'innovations' come into existence. I do not like the analogy with 'growth', else I could express that distinction by comparing it to the distinction between the circulation of blood in a child and the growth of that child. Biological mutations would be a better analogy.

And Schumpeter added an illuminating postscript to the same letter:

> On rereading this letter I do not know I have succeeded in clearing things up. But always think of the pendulum which, given mass force and so on, and no resistance of medium, would eventually swing in the same way, perfectly [.], and displaying no relevant historical dates. Now let its mass swell from within or a new force act upon it with a sudden push, shifting and deforming it <u>for good</u>, and you have a case of 'Dyn. S.' or 'Evolution'. (OU; underlining by the author, is in the following quotations)

[1] This is the first letter dispensing with formal treatment between Schumpeter and Frisch. It was sent from San Francisco.

This letter defines the terms of the discussion, as far as Schumpeter was concerned. Firstly, it argues that the relevant movements were the irreversible changes occurring in economies ('Ford can never be repeated'), historical changes and mutation instead of simple and mechanical recurrence. Secondly, it points out the nature of the changes emerging from internal forces (entrepreneurial activity) which determine economic evolution. Thirdly, internally generated change is not a process of simple physical growth, and the analogy with biological mutation is thus more appropriate.

Consequently, Schumpeter added the postscript: if the model is to be represented by the pendulum, then the mechanism should eventually be subject to deformations and changeable by the impacts of innovations, so that it could 'display relevant historical dates'. In that sense, and just two weeks later, on 24 June, Schumpeter insisted on his critique of the pendulum analogy:

I am not *quite* satisfied by your classification of the 'innovations' as part of the impulse problem (. . .), because this seems to coordinate them with events, which come from outside the economic system such as chance gold-discoveries. The problem with these is simply to discover the reaction of the economic system on them. (. . .) Now as I look at it, any innovations are something different to impulses in this sense. They come from inside, they [.] economic phenomena sui generis. (. . .) If you class innovation simply among impulses you (. . .) miss what seems to me the heart of the matter: you only catch the 'vibrations' [.] to the impact of the 'impulse' and not the phenomena attaching to the impulse itself. (OU)

The critique was very clear: innovations should not be considered as part of the small and random impulses, since this would imply ignoring both their causes and their real qualitative impacts. For Schumpeter, innovations were part of the economic system itself, 'coming from inside', and that was indeed his unique contribution. Otherwise, the 'heart of the matter' would be missed, since the effect of the phenomenon would be studied without any attempt to inquire into the causes of the phenomenon itself—as implied by the mechanics of the pendulum.

The long and detailed reply by Frisch is a magnificent example of an attempt at persuasion, and a quite effective one, as we shall see: it is a rhetorical monument. The letter was dated 5 July 1931,[1] and recognises the continuing differences between both authors. Furthermore, it argues that a mechanical analogy is indispensable for developing the argument and defining the problem:

You say that you are not satisfied with my classifications of the innovations as disturbances (part of the impulse problem), and I think I understand now why you are not satisfied, but I believe you will be so when you have read this letter. Before I received your last letter (of June 24) I had started again pondering over your point of view, and I began to see clearer why you would not capitulate entirely to my pendulum.

Let me tell you right away that I am glad you did not smooth out our differences in a more or less formalistic adoption of my pendulum analogy, but took the trouble to attempt to convince me that there is something fundamental which is not represented in the picture of the pendula as I gave it originally. We all have our peculiar way of working, and I for one, never understand a complicated economic relationship until I have succeeded in translating it either into a graphical representation or into some mechanical analogy. (. . .) I think I am able to do so now. Your San Francisco letter [10 June 1931] must have been working in my subconscious even after I sent you my all too simple answer classifying your innovations under the impulse heading.

[1] To the best of my knowledge, this is the only letter quoted here that had already been partly reproduced elsewhere (Stolper, 1994, pp. 70f.).

Frisch then proceeded to demonstrate his new mechanical analogy: he considered a pendulum with friction, and water flowing at a constant rate into a container above the pendulum. A pipe connected that container to the lowest point in the pendulum, with a valve in the left side of the bob. The peculiar feature of this system was that the opening of the valve should vary with the velocity of the device, increasing when moving to the right, decreasing when moving to the left. As a consequence, this was a system that provided a self-maintained oscillation. Finally, Frisch applied this analogy to explain the two different sources of impulses, Schumpeterian innovations and random shocks:

Of course you understand already the whole analogy: The water represents the new ideas, inventions, etc. They are not utilised when they come, but are stored until the next period of prosperity (or even longer, some of the molecules in the container may rest there indefinitely). And when they are finally utilised they form the additional surplus of energy which is necessary to maintain the swings, to prevent them from dying out. (. . .) This picture may now be completed by taking into account random disturbances of the type which I had originally in mind: Imagine a series of random impulses, working either to the right or to the left and being distributed in time and size according to some sort of chance law. (. . .)

Which one of the two that is actually the most important in the sense of representing the largest source of 'energy' for the maintenance of the economic swings I think nobody can say today. This can only be found out by painstaking studies that are *econometric* in the best sense of the word. I should be very much mistaken if such studies would not lead us to new Magellanic Oceans in cycle theory. At any rate I think I see now the two-sidedness of the problem. One side I have seen long ago, and the other I have finally realised through your patient explications. (HU)

The drawing in Figure 1 was included in Frisch's 1933–4 lectures at the Institute of Economics in Oslo, which were later compiled as *Makrodynamikk*, and closely follows the description included in the correspondence between the two economists (Frisch, 1933–4, p. 8505-2). Schumpeter reacted to the letter less than two weeks afterwards. On 17 July 1931, after dealing with the preparations for the Lausanne meeting of the *Econometric Society*[1] (22–4 September 1931), he insisted on the need to consider irregularities, deformation and shifts in the body of economic relations throughout the cycle:

I want to hurry on to our discussion on 'impulses'. I have been fascinated by your analogy, which I think is much superior to the one I had formed myself: I tried to think of the process I have in mind (and which claim precedence as against irregularities, which are the consequence of influences acting from without the economic sphere, but being part and parcel of that sphere itself and sure to display themselves, even if we abstract from outside or chance disturbances as we must in a theoretical approach) as of a force acting intermittently on a body (or system), which is partly elastic and partly subject to deformation.

This force pushes the body or system up, deforming it in the process, after which we observe a sagging back of [.] with further deformations, and besides vibrations, elastic reactions, etc. A new system (or form and position of the body) establishes itself, after which the force starts acting again. Now your analogy grips one element of the whole thing so elegantly that it will be possible to proceed with it while very little progress seems (in spite of Volterra *et al.*) to me to be possible with that clumsy and complicated model of mine.

Again and again, Schumpeter returned to his main objection: in order to represent reality, the system had to allow for change and deformation as a condition of its own movement.

[1] At that meeting, Tinbergen presented several models of endogenous and regular cycles. Frisch was of course, much closer to the subsequent solution that would come to be accepted as the pattern of cycle models, which Tinbergen also later adopted. But both had long shared the same fascination for Aftalion's explanation of the cycle as the product of lags in the production of capital goods.

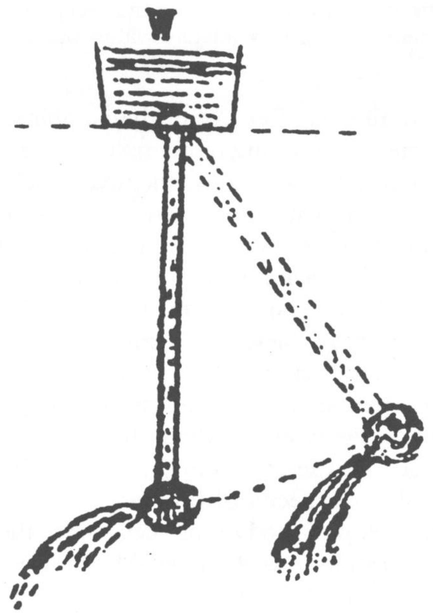

Fig. 1. *Frisch's representation of the 'Schumpeterian Pendulum'*

Otherwise, the model would be able to grasp just one element of the whole process and nothing else. Therefore, the pendula could not be accepted, since the metaphor ignored the importance of structural change and its consequences on the cycle itself, the intertwining of cycle and trend, of fluctuation and evolution:

They are, however, of less service for another type, which in constructing its models (the word being now taken in a wider sense, of which the model in the sense of a mechanical contrivance is a special case) primarily thinks of the inner life and structure of the economic process (. . .). Judging my *Entwicklung* [TED] you must not forget that it was problems such as this I was aiming at. In this connection I beg leave to touch upon two things. First, being truly glad to see that my *manière de voir* may possibly, in your hands, [.] of being gripped by the tools of the other type, I am anxious to point out where I am not yet quite satisfied with your brilliant construction. On the other hand, something within me rebels at our pendulum keeping its suspension point. I do think it a great improvement, provided it be feasible, to shift the suspension point in the process. *Il y a plus*: we both surely agree that it would, for many reasons, be highly unsatisfactory to set aside the shifting (I need not explain what economic facts I mean by this) by means of some of the vulgar methods of [.].

Other considerations quite apart, this cannot be done because the shift is no phenomenon sui generis, around which the cycle moves, as another phenomenon sui generis, but the net result of the cyclical movement, which is the essential point of evolution, *de la sort* that our model, to express the theory, would have to be constructed such that the water must arrive at the same time [creating] the pendulum movement, [which] displaces the suspension point and does so only by and through the pendulum movement (. . .) and of disturbances (. . .).

Finally, Schumpeter listed some of the inconveniences of Frisch's model:

I do not quite like the mass of the pendulum remaining constant. I should like our water mechanism to increase it in the process. Finally, if the pendulum is to represent not only the social product in the sense of the consumer goods, but the whole of the economic system with all the higher values of goods and commercial activity, an inner vibratory system would be extremely useful if it could be [.].

(. . .) However, even so your model would be most useful. And my comments are independent of any unreasonable hopes about what is immediately within our reach. Magalhães' [Magellan's] dreams? (OU)

Dreams indeed, because this requirement of the combined explanation of internally generated changes of regimes, of cycle and trend, pointed out towards non-linear models, and this was still unexplored territory. Consequently, Frisch interpreted Schumpeter's thoughts as mere literary *rococo*, with no analytical correspondence to the formal and rigorous treatment econometrics was already able to deliver by the time. The unreachable and mathematically untreatable Schumpeterian model was consequently completely ignored. Thus, Frisch replied with a polite letter on 24 July, dealing with the preparations for the Lausanne meeting and briefly acknowledging Schumpeter's comments but adding no further argument on the pendulum question. It is quite obvious that he considered the matter to be closed and his pendulum metaphor to be enough, in spite of Schumpeter's remarks and their continuing disagreement about the major issues.

Their differences concerned at least two major points. The first was the formal and analogical representation of those specific disturbances: 'inventions' as Frisch called them or 'innovations' as Schumpeter preferred to call them, since they emerged from inside the system itself.[1] As Frisch's reasoning was dominated by the need for a mechanistic formulation, susceptible to mathematical treatment, he could envisage only two possibilities: either the variable was endogenously determined by the system of equations describing the process and therefore was explained by it, or it was exogenous to that system and therefore served to explain the changes in the process. The representation of two classes of exogenous variables—*à la* Slutsky and *à la* Schumpeter—is a recognition of this difficulty, since there are obvious epistemic differences between them, one representing an aggregation of unknown irrelevant exogenous impacts, and the other a description of crucial changes in the economies. This undefined character of the random variables—indistinctly dubbed 'shocks', 'perturbations' or 'stimuli' in spite of the difference of meaning involved in each denomination—was one of the many consequences, and by no means the least of these, arising from the cursory discussion about the nature of randomness in the early econometric movement. In particular, the discussion between Schumpeter and Frisch on the nature of the variable of innovation in their models is one of the most important instances of the research into randomness, and illuminates the pervasive difficulties of simple mathematical formalism applied to economics.

Furthermore, Frisch's model insisted that the variables accounting for innovation do not alter the structure of the process, and merely generate a recursive cycle—a *perpetuum mobile*, although irregular. Schumpeter's reasoning was dominated by a completely different requirement, a non-mathematical representation of the innovative process, based on a literary approach and influenced by an undefined biological metaphor—mutation—which was designed to explain the driving force behind change in capitalism. Consequently, the mechanical contrivance of the formal model Frisch had in mind had no place in Schumpeter's system. Indeed, one of the relevant contradictions in this controversy is related to the epistemic distinction between the concepts of 'exogenous' and 'endogenous' variables. Frisch used the traditional distinction in reference to the formal models of systems of equations, whereas Schumpeter used the non-equivalent concepts of 'external' and

[1] This difference of conceptualisation already suggests their alternative approaches: invention can eventually be considered as exogenous and part of the scientific system, whereas innovation was precisely described as the result of the market selection process of invention, i.e., of the specific economic system. Innovation could never be described as a purely exogenous variable in the Schumpeterian model.

'internal' forces defined in relation to the scope of the theory itself and depending on the limits of what could be explained in this way. In short, the difference was that, for Schumpeter, causality was not equivalent to mechanical implication, which was the only form of determination Frisch could accept within the framework of his model.

Indeed, Frisch worked within the confines of formal mathematical models, whereas Schumpeter worked according to narrative and appreciative theories, and consequently their discussion was largely a case of mistaken identities and untranslatable languages. For the former, the heuristic richness of the metaphor was precisely related to its capacity to impose discipline in the mathematical formalism of the cycles. For Schumpeter, this was the tragedy of the model: it could not account for change, the only relevant subject matter. Nevertheless, and paradoxically, the rhetoric used by the authors provided the possible space for communicating—indeed, this case highlights the importance of their use of metaphors in order to create a shared conceptual platform and to understand each other's arguments. In spite of their misunderstandings and difficulties, this is a fascinating example of a rich conversation between economists using different analytical tools.

Yet a solution that would satisfy both parties was particularly difficult to achieve, since there was a second decisive difference in their positions: Schumpeter was in fact rather naïvely searching for a very complex system to represent the process of innovation. If the pendulum were adapted so as to encompass changes in its suspension point as well—as a result of the cycle itself, it should be noted, so that the trend would indeed be indistinguishable from the oscillations—and if its mass were also to increase or its shape were to be deformed as part of the effects of the 'inner vibratory system', this obviously implied a non-linear representation. Of course, Schumpeter argued for this solution while remaining unsure of how to proceed, since he did not and could not formally represent that model. It required and still requires an adventurous journey into the unknown, like the one Magellan made into *mare incognita*. And Frisch knew this better than anyone else at that time.

3. Epilogue to the discussion

Although the topic was never again discussed properly, Frisch and Schumpeter maintained their respective positions and, writing in reference to the previous exchange, elaborated on them two years later. On 25 October 1933, Frisch wrote to Schumpeter, announcing the conclusion of his paper in honour of Cassel, 'Propagation Problems and Impulse Problems in Dynamic Economics' (PPIP). The text again mentioned the two types of impulses, random shocks and Schumpeterian innovations, and added that such a distinction had 'satisfied you to a considerable extent':

> You will probably remember our long correspondence back and forth about the pendulum analogy in business cycles. You will perhaps also remember that I developed a mechanical model, that satisfied you to a considerable extent, expressing that feature of the business cycle which you have particularly insisted upon and which you found was not present in the example with the ordinary pendulum hit by erratic shocks. In a rather big paper to be published in the volume in honour of Cassel I have insisted upon these two ways of looking upon the maintenance problem: on the one hand the idea of erratic shocks (starting with Wicksell, being developed by Slutsky and perhaps having been carried to a sort of relative completion by my theory of linear operators and erratic shocks soon to be published in *Econometrica*) and on the other hand your idea of the stream of energy coming in through the 'innovations'. I hope you will be satisfied with my mention of your ideas in this field. In the paper in the Cassel volume I was not able to devote more than a brief section to your theory (. . .), but I hope that I have succeeded in exhibiting *the gist of your view-point as contrasted with the viewpoint of erratic shocks*. (OU; my italics for the last phrase)

It is obvious that Frisch minimised or ignored the objections previously raised by Schumpeter in his letter of 17 July 1931. In his 1933 paper, Frisch was trying to clarify and develop his model of cycles, and for this purpose he used both the metaphor of the rocking horse (with Slutskian shocks as the source of energy) and that of the pendulum (with Schumpeterian innovations as responsible for the generation of the movement). The pendulum was evoked in order to describe the second 'source of energy' maintaining the oscillations and acting in a 'more continuous fashion' than the random shocks. Frisch went so far as to mention that '[a]fter long conversations and correspondence with Professor Schumpeter I believe the analogy may be taken as a fair representation of his point of view' (Frisch, 1933, p. 203).

At the end of the 1933 paper, Frisch considered some of Schumpeter's points very briefly. Recognising that the analogy provided a picture of an oscillatory system, but 'not of the ['irreversible'] secular or perhaps supersecular tendency of evolutions', Frisch suggested that a simple solution would be to make the suspension point a consequence of the movement itself, so that the trend would be generated by the cycle. Within such a framework, 'there will be an intimate connection between the oscillations and the irreversible evolution' (Frisch, 1933, p. 205). Nevertheless, although insinuating that it would be a simple task, Frisch decided neither to formulate this mathematical model nor to elaborate on it, restricting his own work to the discussion of the simpler case.

Throughout his life, Frisch argued for this general approach to cycles, and indeed considered it to be one of his major contributions to economics. His model established the linear stochastic differential or difference equations as the most suitable representation of the cycles, and buried the contemporary alternative non-linear models. This was a major part of his writings on cycles and economic evolution, as well as part of his teaching.

Schumpeter took a long time to reply to the October letter, since he was travelling at that time. On 28 December 1933, after a digression on the Baroque and mediaeval cathedrals of France, he added only a few lines politely alluding to his reservation in relation to the solution suggested by Frisch:

> I am greatly [.] looking forward to both your papers, the one on the erratic shocks (if these are only small, many, independent!) and the other in the Cassel volume (. . .), from which I hope to derive the usual help in my perplexities (. . .). (OU)

Later on, in the preparation and writing of *Business Cycles*, Schumpeter repeatedly returned to the same perplexity, implicitly indicating a completely alternative solution to the mechanical device of Frisch. The *leitmotiv* was obvious: 'It [innovation] is an internal factor because the turning of the existing factors of production to new users is a purely economic process and, in capitalist society, purely a matter of business behaviour' (BC, p. 86). As a consequence, the innovative process of change and destruction should be modelled as an internal feature of capitalism, and this would be the proper explanation in economics (*ibid.*, p. 7). Furthermore, he argued for a definite rejection of the mechanistic metaphor, since the relevant external events could not be appropriately represented as random shocks on a pendulum: 'But the influence of external factors is never absent. And never are they of such a nature that we could dispose of them according to a scheme of, say, a pendulum continually exposed to numerous small and independent shocks' (*ibid.*, p. 12). This is the clearest indication of his rejection of one of the decisive features of this mechanistic metaphor.

But Schumpeter took pains to explain that Frisch's model of impulse and propagation was really quite distinct from the available alternatives, namely from the allegedly *perpetuum* *mobile* systems such as the one proposed by Kalecki. He went so far as to attempt to distinguish Frisch's model from those of Wicksell and Slutsky, which had served as the early inspirations for the rocking horse (BC, p. 181n., 189, 560n.). The reason for such complacency is difficult to explain, although one may hypothesise that Schumpeter essentially wanted to preserve the feeling of intellectual closeness to Frisch, the only major econometrician to welcome his *Business Cycles*.

Finally, in his *History of Economic Analysis*, Schumpeter suggested a metaphoric shift, insinuating that the crux of the question was the limited value of the available mathematical representations. His distance from the mechanical analogies was expressed in an inspiring manifesto against reductionism, which suggested a new and alternative metaphor, that of the violin being played by a gifted musician:

> It has been said above that macrodynamics helps us to understand mechanisms of propagation. It will perhaps assist the reader if he will look upon the economic system as a sort of resonator, which reacts to the impact of disturbing or 'irritating' events in a manner that is partly determined by its physical structure. Think for instance of a violin which 'reacts' in a determined manner when 'irritated' as the player applies the bow. Understanding the laws of this reaction contributes to a complete 'explanation' of the phenomenon that we call a violin concert. But evidently this contribution, even if reinforced by the contribution of the neurophysiologist, does not explain the whole of it: aesthetic evaluation and the like apart, there is a range of purely scientific ground that acoustics and physiology are constitutionally unable to cover.

And here Schumpeter introduced a powerful critique of the claim of the unlimited explanatory power of formal models:

> Similarly macrodynamics, while quite essential to an explanation of cyclical phenomena, suffers from definite limitations:[1] its cyclical models are what acoustic models of resonators are for the violin concert. But its votaries will not see this. They construct macrodynamic models that are to explain all there is to explain, for economists, in the cyclical phenomena. The very attempt to do so involves several definite errors of fact.[2] And flimsy structures based upon arbitrary assumptions are immediately 'applied' and presented as guides to policy, a practice that of course completes the list of reasons for irritation in the opposite camp. One sometimes has the impression that there are only two groups of economists: those who do not understand a difference equation; and those who understand nothing else. It is therefore a hope, rather than a prognosis to be presently fulfilled,

[1] A note by Schumpeter emphasised the evolutionary character of economic data and therefore strengthened his critique:

> The simile limps, of course, like all similes. Cycles run their course in the *historical* evolution of the capitalist economy. Even neglecting all the economic sociology that must therefore inevitably enter into their explanation, we cannot help recognising that their theory or, to avoid this word, their analysis must be largely bound up with the theory or analysis of evolution rather than with dynamics, which is the theory or analysis of sequences that do not carry any *historical* dates. No doubt there are certain mechanisms that played as great a part in 1857 as in 1929. And these must be taken account of in any observed cycle by more or less generally applicable macrodynamic schemata, just as must, on a lower level of technique, the ordinary theory of supply and demand. But they are only tools and do not in themselves suffice, even if supplied with all conceivable time series, to reconstruct the phenomenon as a whole and, of course, still less its long-run outcomes. (HEA, p. 1167n.)

[2] Again, Schumpeter's second footnote to the same text is very revealing:

> Three of these may serve as illustration. They will at the same time show why the respective objections do not tell against the models themselves but only against the claim alluded to. (1) Macrodynamic models, presented with that claim, involve the proposition that the 'causes' of the business cycles must be found in the interaction between the social aggregates themselves, whereas it can be proved that business cycles arise from sectional disturbances. (2) With the same proviso, macrodynamic models carry the implication that the structural changes that transform economics historically have nothing to do with business cycles, whereas *it can be proved that cycles are the form that structural changes take*. (3) Constructors of macrodynamic models, almost always, aim at explaining all the phases of the cycle (and the turning points) by a single 'final' equation. This is indeed not impossible. But it spells error to assume that it must be possible and to bend analysis to that requirement' (HEA, p. 1168n.; my italics).

which I am expressing if I venture to say that this entirely unnecessary barrier—but one which is no novelty in our science—to fertilising interaction will vanish by virtue of the logic of things. (HEA, pp. 1167–8)

This was the methodological stance of Schumpeter towards the end of his life: he strongly but nostalgically argued for a *Sozialoekonomie*, combining concrete historical inquiry with theoretical practice, statistical research and inference. In defence of this programme for economics, he fought his last battle and made his last public intervention in the 1949 Conference organised by the NBER and some Universities, combating the mainstream econometricians and speaking in place of Wesley Mitchell, who had recently passed away (Louçã, 1997, pp. 284f.). Structural change, irreversibility and history, all this should be part of the general vision of the economies—precisely the conditions he had tried to impose on the pendulum metaphor earlier.

Now, the reader must accept that this is a convenient although rather dubious epilogue for the story of an intense, fruitful and almost completely ignored discussion on the foundations of the econometric programme for the analysis of cycles. Schumpeter was apparently under the impression that the mathematical capacities of his friend and colleague restricted his thought to a narrow domain and prevented any consideration of the decisive qualitative features of innovation under capitalism. In spite of this, he was conditioned by the public claim, made in Frisch's influential 1933 paper, that the pendulum accurately represented his own point of view. He chose not to challenge this claim. Yet he repeatedly stressed that a mechanistic representation could not incorporate change, evolution and irreversibility in economics as well as the aesthetic pleasure of a violin concert—and that the explanation was still somewhere submerged in the immense Oceans of Magellan's fantasies or dreams.

As a consequence, the episode highlights the crucial role of metaphors as a way of directing the construction of the argument, its formal representation and the definition of possible alternatives. Although these metaphors were unable to solve the puzzle Schumpeter and Frisch were discussing, they provided the framework for the dialogue.

4. The hidden implications of the pendula

Tables 1 and 2 summarise the narrative so far. In the first columns, we have three plus six metaphors, which were used to describe the relevant features of cycles in economics. They cover the period of 1890–1950, and two very distinct approaches can be noted. The first is marked by Marshall's assessment of the complexity of economic processes: in his view, the understanding of purposeful action, particularly if superimposed on the real-life complexity of natural processes, escaped the scope of formal reasoning that the common economic models were able to develop. Here is how he presented his argument:

But in real life such oscillations are seldom as rhythmical as those of a stone hanging freely from a string; the comparison would be more exact if the string were supposed to hang in the troubled waters of a mill-race, whose stream was at one time allowed to flow freely, and at another partially cut off. Nor are these complexities sufficient to illustrate all the disturbances with which the economist and the merchant alike are forced to concern themselves. If the person holding the string swings his hand with movements partly rhythmical and partly arbitrary, the illustration will not outrun the difficulties of some very real and practical problems of value. For indeed the demand and supply schedules do not in practice remain unchanged for a long time together, but are constantly being changed; and every change in them alters the equilibrium amount and the equilibrium price, and thus gives new positions to the centres about which the amount and the price tend to oscillate. (Marshall, 1890, pp. 288–9)

Table 1. *Pendulum: non-mechanistic versions*

Metaphors	Literary and heuristic treatment of the metaphor	Formal treatment of the primary subject	Comment
1. Pendulum driven by purposeful human action	Marshall (1890)	–	*Complicated or chaotic movement*
2. Stream of fluid flowing in an uneven riverbed	Åckerman (1928)	–	*Turbulence*
3. Violin	Schumpeter (posthumously 1954)	–	No formal model

Table 2. *Mechanistic metaphors in the early analysis of business cycles*

Metaphors	Literary and heuristic treatment of the metaphor	Formal treatment of the primary subject	Comment
1. Simple pendulum for the representation of cycles	Fisher on Pareto (1911)	–	Oscillation
2. Simple pendulum with friction, hit by shocks	Yule (1927), Hotelling (1927), Frisch (1933), Tinbergen (1935)	Frisch (1933)	Maintained oscillation
3. Rocking horse	Wicksell (1918), Åckerman (1928), Frisch (1933)	Frisch (1933)	Maintained oscillation
4. Double pendulum	Frisch, manuscript notes (1932)	–	*Chaos*
5. Forced pendulum	Frisch, interpreting Schumpeter (1933)	–	*Chaos*
6. Triple pendulum	Frisch (1932–3 and 1950), interpreting Marshall	graphical treatment in Frisch (1950)	*Chaos*

The outcome of this very complex process of human and natural turbulent flows—sometimes controlled and sometimes free—and the intentional action of the person holding the string is indeterminate: it can either tend towards equilibrium or aggravate disequilibrium. Indeed, Marshall introduced this argument precisely in order to emphasise the difficulties for the inclusion of the time dimension in economic reasoning. But, instead, the pendulum metaphor was interpreted in economics as the *leitmotiv* for the irreducible nature of real processes. One of the most remarkable triumphs of this dominant version of the intriguing pendulum is how it came to be transformed into the simplistic framework of mechanical modelling, imposing discipline and organising the following research on cycles.

Table 1 presents some of the metaphors that were suggested along the same lines as those described by Marshall. But, as argued before, these metaphors did not impress the scientists who were engaged in quantitative and statistical analysis, or in theorising the new econometric and probabilistic approach, who preferred a clearly defined framework for the analysis of evolutionary processes. And here the main contender was Frisch's approach to mechanistic processes. Table 2 presents the most relevant examples of this new generation of metaphors, and emphasises Frisch's role in their elaboration and modelling.

This second line of argument was based on a shift of emphasis, from a narrative approach to complexity towards analytical simplicity: first the intuitive functioning of the pendulum and then the well-researched mechanical properties of the simple damping pendulum were invoked as a representation of the movement towards equilibrium. Consequently, the metaphor was developed as a powerful heuristics for the equilibrating mechanism, under the equivalent forms of the simple dissipative pendulum or that of the rocking horse, both subjected to friction as well as to shocks maintaining the oscillation. The first interpretations in the same sense occurred very early on: Fisher described Pareto's 1899 model of business cycles as an analogue for the pendulum (Fisher, 1911, p. 70, fn. 1), and Pietri-Tonelli used the metaphor of the pendulum for the representation of cycles in 1911 (Pietri-Tonelli, 1911, p. 220). This is how Yule described his model some years later:

> unfortunately boys get into the room and start pelting the pendulum with peas, sometimes from one side and sometimes from the other. The motion is now affected, not by *superposed fluctuation* but by *time disturbances*, and the effect on the graph will be of an entirely different kind. The graph will remain surprisingly smooth, but amplitude and phase will vary continually. (Yule, 1927, p. 268, his emphasis)

The irregularity of the graphs describing real processes was consequently explained by the superimposition of these small shocks. Yet Hotelling understood that this metaphor introduced an element of uncertainty, related to the skill and determination of the boys. Therefore, the implication could be much the same as the one that Marshall had deduced:

> Like a weight suspended from a spring, an index of the business cycle moves up and down, but as when the spring is in the hands of a small boy, one can never be quite sure what is going to happen next. (Hotelling, 1927, p. 290)

So, the metaphor was also used to explain uncertainty, the unpredictable variation of events and their effects on the economy. Ragnar Frisch put an end to these divagations and, towards the end of the 1920s and in the early 1930s, imposed a new concept of dissipation—describing the process of convergence to a stable equilibrium—in which he defined random shocks as the means of maintaining the oscillations. Therefore, Yule's hypothesis became computable and Marshall and Hotelling's uncertainty was suppressed. Along the way, a new and decisive revolution was introduced into economics with the acceptance of the adequacy of the probabilistic approach to time series.

It was Frisch who made the decisive step forward. By the 1930s, he was the driving force behind the formalisation of the metaphor and the establishment of linear differential, difference or mixed systems of equations as the legitimate mode of argument in the analysis of economic fluctuations. Indeed, Frisch is the only name appearing in the third column of Table 2, which indicates the formal treatment of the primary subject.[1]

The exceptions, such as Åckerman's river bed or Schumpeter's violin, were literary excursions suggesting, as Marshall did, the inadequacies of the mechanistic metaphor. As they suggested quite another language, these metaphors or critiques were easily ignored or disregarded because they were so far removed from the rigorous approach that econo-

[1] Frisch preferred a mixed system, such as that used in 1933, or, as Boumans rightly proved, a system of differential equations in order to represent cycles. The use of difference equations by Tinbergen in his later work for the League of Nations became the subject of an important debate between the two friends and colleagues (Boumans, 1992; the texts of the polemics were recently reproduced in Hendry and Morgan, 1995, pp. 407–23).

metrics was already requiring and beginning to establish. But these alternative metaphors were also ignored because they were at odds with the then prevalent econometric approach. And the predominance of this simplistic approach was such that non-linear alternatives were hastily dismissed and only reconsidered some decades later. Finally, Table 2 also includes some of the afterthoughts, such as Frisch's representation of cycles in the distinctive Marshallian time dimensions as a triple pendulum. Although this was not discussed in any great detail, Frisch obviously believed the example to be in line with his previous work on the matter. He was, however, wrong on that score. The hidden implications of these pendula are the topic to which we now turn.

4.1 The simple and the damping pendulum

In 1928, Frisch wrote a paper, 'Changing Harmonics', that represented a crucial departure for his work on the pendulum as a representation of the movement of cycles. He studied the mathematical frictionless pendulum over a gravitational field and the general solution provided by mechanics for the case of small oscillations around equilibrium. Although this was not stated, he exclusively concentrated on the specific case of a linear approximation to the non-linear equation. From this equation, Frisch considered three distinct cases of changing harmonics. The first was that of a non-constant period or amplitude of movements, for instance due to variations in the length of the pendulum. The second was a very interesting case of coupling between two or more components, through the joint effect of their 'beating', each of them having a constant frequency and obtaining greater amplitude of the combined movement if the frequencies were sufficiently close. Finally, the third case, the only one that the author studied in detail, was that of the change in the initial conditions, or the superimposition of erratic shocks upon the damping system. For his subsequent research, only the last one was considered.

The choice was not innocent. Although an economic interpretation could be offered for them, the first two cases did not lead to the desired clear distinction between the 'propagation' problem and the 'maintenance' problem, since they implied the predominance of exogenous shifts imposed on the system or, worse, a non-linear process. These authors, and Frisch in particular, could not accept that the irregular features and the continuation of the oscillation of the equilibrating system should be explained either by the non-determined system of unknown and unknowable variables or by the rather obscure process of coupling. As an alternative, free oscillations (the mathematical pendulum), with friction and a new source of energy, could account for the desired properties of the model: consequently, the construction of the system of equations followed this option.

This specific choice anticipated the Cassel paper and defined its constitutive metaphor as well as the choice of the class of changing harmonics with which the economic theory should be concerned.[1] Indeed, the structure of the paper is in itself very revealing. Firstly, in sections two and three, Frisch presented an economic theory for the rocking horse—a three-dimensional deterministic system representing the accumulation of capital, '*encaisse desirée*', and the structure of lags in the production of capital equipment—and simulated its cycles under defined parameters. Then, in section five, he introduced the pendulum metaphor in order to bring the erratic shocks into the picture.

[1] 'By a changing harmonic I understand a curve that is moving more or less regularly in cycles, the length of the period and also the amplitude being to some extent variable, these variations taking place, however, within such limits that it is reasonable to speak of an *average* period and an *average* amplitude' (Frisch, 1933, p. 202).

The equation of the pendulum can be derived from Newton's Second Law or from the First Law of Thermodynamics. Adding the damping factor one obtains:

$$\ddot{\theta} + \beta\dot{\theta} + \alpha \sin \theta = 0 \tag{1}$$

As the solution is obtainable only with a Jacobean elliptic function, Frisch chose to indicate the linear approximation to the damping pendulum, ignoring all but the first term of the expansion of sin θ. This alternative is, of course, only valid for small oscillations, and Frisch used the following form:

$$\ddot{\theta} + 2\beta\dot{\theta} + (\alpha^2 + \beta^2)\theta = 0 \tag{2}$$

θ being the angular deviation from the vertical. The general solution for this case is

$$\theta\ (t) = He^{-\beta t} \sin(\phi + \alpha t) \tag{3}$$

where β is the parameter for friction, α is the frequency, ϕ the phase, and H the amplitude. One naturally obtains complex conjugate roots and therefore an oscillatory regime in the damping system. According to Frisch, the solution to the determinate dynamic system should be interpreted as the weighting system for the accumulation of the erratic shocks.

Again, this required several major simplifications. First, a linear form was chosen to approximate the equation of the pendulum. Secondly, and this was the subject of great discussion, the result required a rather implausible set of parametric values, otherwise the horse would not rock. Zambelli argued this point: only for an improbably large oscillation would the story hold as Frisch told it—the propagation mechanism is not 'intrinsically cyclical' and the convergence to equilibrium proceeds in a non-cyclical manner (Zambelli, 1992, p. 52). Thalberg, on the other hand, reconsidered the model with additive random shocks, normally distributed and serially correlated or uncorrelated, with zero expectation and finite variance: the conclusion was that the shocks maintain the cycle with a high degree of damping, but the cycle itself is very irregular and unpredictable (Thalberg, 1990, p. 108). When other repercussions are considered in a reformulated model, e.g., the effect of investment on consumption, instability grows (*ibid.*, p. 110). Even under linear specifications, the conclusions are obviously dependent on the specific modelling strategy and on the values chosen for the parameters, and therefore may lead to rather different implications.

Thirdly, coupling was completely disregarded, even under the simpler form of the possible resonance of the repeated disturbances with the natural frequency, affecting the amplitude of the movement. Indeed, Frisch espoused the point of view of linearity and simplicity and the consequences could not be more damaging: 'The concrete interpretation of the shock e_k does not interest us for the moment' (Frisch, 1933, p. 200–1)—and this was the case for the whole piece. Furthermore, the author considered the deterministic oscillation and the perturbations to be completely independent contributions to the composite movement, and the shocks to be independent of each other, so that the final computed deviation would be simply the summation of all the small deviations to the normal trajectory caused by each shock. This additive property was even represented by a number of isolated pendula equivalent to the number of shocks, and this implied the definitive exclusion of one important form of changing harmonics, the coupling effect. The final result was the claim that unexplained independent shocks accounted for the irregularity of the fluctuation: the history of the dynamic process depended on the unproved properties of these external sources of energy.

This elimination of coupling sheds some light on the cursory treatment of the random shocks. Indeed, while preparing the final proofs of PPIP, Frisch engaged in a correspondence with Arthur Cowles, then in charge of the laboratory of the Cowles Commission in Colorado Springs. One of the experiments developed at that laboratory concerned the measurement of the effect of a series of erratic shocks on damped oscillations, which was mentioned to Frisch in a letter on 6 September 1933; the latter asked for clarifications two weeks later. On 9 October, Cowles suggested that the matter was equivalent to the task of computing 'a composite of the deviations from equilibrium of a room full of rocking chairs, which are being set in motion at different intervals of time by blows of different intensities'. On 18 October, he insisted that this 'really almost represents the case you had in mind when referring to a pendulum subjected to a stream of erratic shocks. Possibly the idea of a roomful of rocking chairs (or pendulums [*sic*]) presents a useful concept of what is more likely to be the situation in a complex modern economic system.' Just one week later, on 25 October, Frisch wrote back in order to check his previous results: 'Is it correct to say that the ordinate of the curve at the point of time *t* is the sum of a great number of damped sine curves, each of these being started at some time in the past with an initial ordinate equal to zero and an initial *velocity* equal to some accidentally determined quantity, the point of time where these curves were thus started being also distributed accidentally?' Finally, on 1 November, Frisch acknowledged that this metaphor of the room full of rocking chairs (horses) wonderfully accounted for his model and actually for the concrete mode of computation of the effect of the disturbances, and promised to refer to this conclusion and to acknowledge Cowles's work in his forthcoming paper.[1] Cowles concurred on 13 November 1933: 'The ordinate of the curve at the point of time *t* would be the sum of a greater number of damped sine curves started at erratic intervals with erratically varying velocities.'

Yet Cowles abandoned the measurement project, since it was very difficult to obtain accurate values. Instead, a galvanometer was used at the laboratory: it was adjusted to one cycle with a damping effect, and operated by means of a switch connected to a rheostat in order to represent the variable intensity of the shocks, following a suggestion by Davis. Apparently, Frisch ignored this development, since he was quite happy with the previous result, which he considered to be a sufficient confirmation of his conjectures, although not by empirical means.

The room full of rocking chairs, or of pendula, strongly suggests the relevance of the coupling effect that Frisch had been able to discern in his previous work on the general conditions for changing harmonics. Indeed, in mechanics the best-known phenomenon of coupling was that of two pendula, and the same results applied to the complex setting of the room full of pendula. In both cases, there was apparently no possible way of escaping the problem of resonance: the investigation of the frequency of the disturbances and of the dynamic mode locking of the oscillations was the major challenge. And that was why the assumptions regarding the nature of the random shocks were so decisive: transformed into a black box, the insignificant random shocks should necessarily be considered as meaningless in order to perform their important and meaningful theoretical function, to explain the maintenance of the movement. Within such a framework, no query was

[1] The final proofs of PPIP had already been sent to the editor the previous August, but the paper was then presented to the Leyden conference of the Econometric Society. Frisch possibly had in mind future corrections or additions. In spite of this, he made no such reference to the Cowles's experiment. This omission may, of course, be interpreted in several distinct ways; one must consider namely the intrinsic difficulty Frisch felt in the treatment of resonance, as proved by his subsequent work.

relevant regarding their nature: they were by definition unquestionable, and that is why they were considered to be explanatory.

4.2 The forced pendulum

Since Frisch understood the need to explain the new and extraordinary source of energy represented by innovations, the Schumpeterian pendulum was invoked. But the debate with Schumpeter introduced further entropy into this formulation, since the latter could not accept the idea of a purely exogenous source of energy accounting for innovation and the dynamics of capitalism. As previously mentioned, Frisch tried to convince his colleague of the accuracy of his mechanical metaphor, but then stopped insisting, persuaded as he was that Schumpeter's ideas were satisfactorily represented by his model and that nothing more could be done to accommodate the latter's lasting reservations.

But the Schumpeterian pendulum requires more than a trivial extension of the model of the simple damped pendulum. It can be represented as a device that generates relaxation oscillation, in the Van der Pol–Liénard tradition, and specifically as a forced oscillator (Corbeiller, 1933, p. 330; Velupillai, 1992, p. 68). This general approach was available, since the econometric group was aware of the work done by Le Corbeiller, who was present at the first Econometric Society meeting in Lausanne and published his paper in the first issue of *Econometrica*, edited by Frisch, and the equation of the forced pendulum is easily obtained from that of the damped pendulum. It was the simplest way to model the Schumpeterian innovations following Frisch's interpretation, although this was not in keeping with the concept of free oscillation under exogenous shocks, which was necessarily lost. It was consequently a pity that Frisch did not compute the equations for this model, merely pointing to the forced pendulum as an illustration of his argument: as a result, he missed the implications of his own model.

The following equation represents the external parametric forcing in the non-linear framework:

$$\ddot{\theta} + \beta\dot{\theta} + \sin\theta = \varrho\cos\varpi_{\mathrm{D}}t \qquad (4)$$

where ϱ is the intensity of the driving frequency and ω_D is the angular forcing frequency. As before, this is a dissipative system, but it now has three dimensions, allowing for periodic oscillations and limit cycles, as well as for chaos. For some values, namely if the driving frequency exceeds the natural frequency, the pendulum locks onto the driving frequency and periodic motion is obtained; but if the driving frequency is slightly inferior to the natural one, then resonance may lead to chaos. The largest Lyapunov exponent is positive (λ_1=0·16, λ_2=0, λ_3=–0·42), indicating the presence of chaos, and the sum of the exponents is negative and approximately equivalent to $-\beta=\Sigma\lambda_i$, indicating dissipation (Moon, 1987, p. 157; Baker and Gollub, 1996, p. 122). Kapitaniak, following the Melnikov method, established the necessary conditions for the chaoticity of this system (Kapitaniak, 1991, pp. 123f.). The following bifurcation map shows the behaviour of the solutions of the system for a range of parametric values that pass through critical points:

For a range of values of a given parameter, the bifurcation map in Figure 2 highlights the effect of the addition of a number of new stationary solutions, since the Jacobian of the function representing the flow acquires eigenvalues with zero real parts at a stationary point. The loss of stability may indicate a route to chaos, as is proved by the study of the behaviour of the latent roots of the Jacobian of the system as the parameter varies (Gandolfo, 1997, p. 479). Applying the Kaplan–Yorke conjecture, for the values of the parameters here considered, the Lyapunov dimension is approximately 2·4 (d_{L}=2+

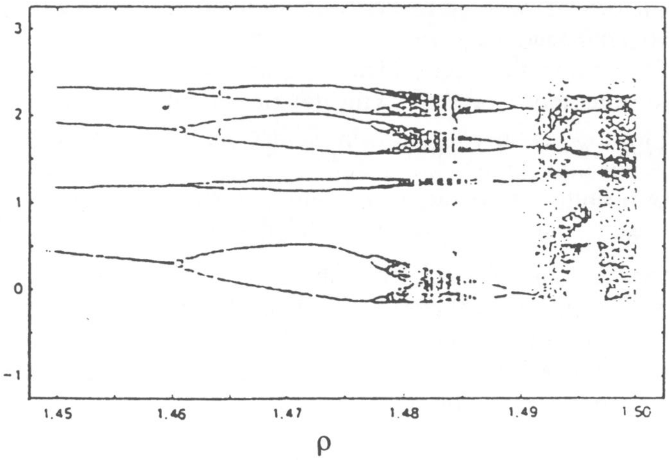

Fig. 2. *Bifurcation map for the forced pendulum (r = 1·4954 and b = 0·25)*

(0·16/0·42)=~2·4). As Figure 2 shows, we have a period doubling scenario, the Feigenbaum route to chaos (Eckman, 1981, p. 650). Under such conditions, the conclusion by Frisch becomes suspect:

> One could even imagine that the movement [after the forcing] would be more than maintained, i.e. that the oscillations would become wilder and wilder, until the instrument breaks down. In order to avoid such a catastrophe one may of course, if necessary, add a dampening mechanism which would tend to stabilise the movement so that the amplitude did not go beyond a certain limit. (Frisch, 1933, p. 204)

The requirement of dampening is no longer sufficient to ascertain the stability of the model, given the problem of coupling between the two frequencies—natural and forcing—which can be aggravated by the disturbances. Moreover, and crucially, the non-linearity may imply sudden changes to the regime of oscillation and the presence of a chaotic attractor. In fact, in the framework of non-linear differential systems of equations with three or more dimensions, the traditional concept of equilibrium is lost (Granger and Terasvirta, 1993, p. 14): catastrophe and chaotic outcomes emerge from the model, and we obtain a second explanation for endogenously driven erraticity (Goodwin, 1991, p. 425). Frisch ignored this, since he did not look at the formal representation of the forced pendulum.

Furthermore, the treatment of the non-linear specification was far removed from the knowledge available in the early 1930s, and he did not have at the time the analytical tools—the iterative simulation by computer—needed to investigate the trajectories of this non-linear process. So he contented himself with a literary reference to the 'Schumpeterian pendulum', believing that the same general properties would be respected. Yet, they were not: from the simple damping pendulum to the forced one there is a dramatic change, which is the intrusion of chaos.

4.3 Double and triple pendulum

Other contemporary work by Frisch provides further outstanding evidence of his perplexed concern with these strange pendula. One such example is the double pendulum hanging from a spring, represented by Frisch in his notebooks, on 24 August 1932, as 'a gravitational theory of economic phenomena' (Figure 3).

If the effect of the spring is ignored and no damping is considered for the sake of simplicity, the system of equations representing the double pendulum as follows:

$$-l_2m_1\,(g\sin\theta_2 + l_1\sin\theta_1 - \theta_2\dot{\theta}_1^2 + l_1\cos\theta_1 + l_2\ddot{\theta}_2 - \theta_2\ddot{\theta}_1) = 0$$

$$-l_1[(gm_1\sin\theta_1 + gm_2\sin\theta_1 + m_2l_2\sin\theta_1 - \theta_2\dot{\theta}_2^2 + l_1m_1\theta_1 + m_2l_1\ddot{\theta}_1 + l_2m_2\cos\theta_1 - \theta_2\ddot{\theta}_2)] = 0 \qquad (5)$$

The assumptions are massless rods with different lengths (l_i) and masses for each bob (m_i). Under such circumstances, Figure 4 represents the plotting over time of both angular deviations from the vertical, indicating that the second pendulum initially transmits energy to the first and then gets energy from it, from an initial condition of a small deviation of the second pendulum (3°) from the vertical:

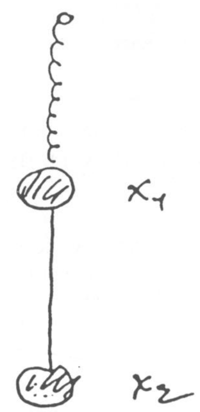

Fig. 3. *Frisch's notebook representation of a 'gravitational theory of economic phenomena'*

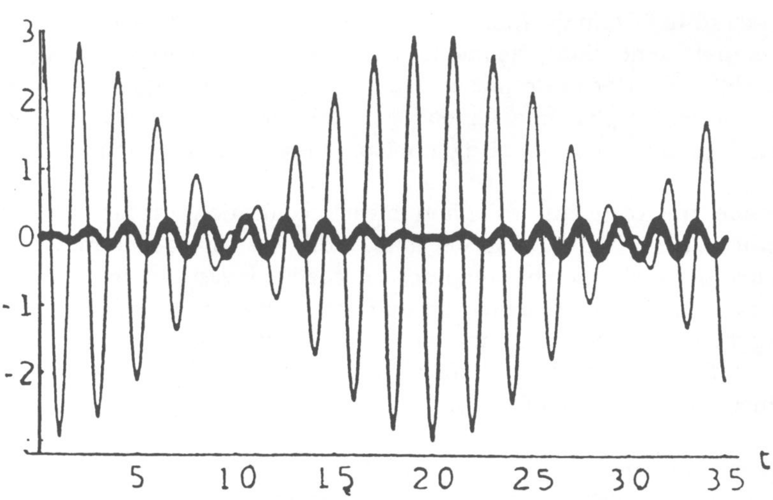

Fig. 4. *Trajectory of the two pendula over time*

This simple representation requires the system to be conservative, and in that case each initial condition generates a single orbit (Moon, 1987, p. 19). It is of course a Hamiltonian system, which preserves the total sum of energy, and for which the attractor is the basin of attraction itself. At low amplitudes, the three-dimensional trajectory lies on a torus, and the theory of KAM tori applies: the system is well behaved for a large range of initial conditions, and for instance it generates periodic motion near 63·3°. But then a transition to chaos occurs near 80° as the phase portrait shows (Figure 5).

Otherwise, if we consider a dissipative version of the double pendulum, a meaningful concept of chaos requires providing the means for sustaining the movement. In that framework, the previous conclusions on the forced pendulum may be generalised to the new case.

At roughly the same time, Frisch also represented in his notebooks a similar 'interaction between the components' as a triple pendulum. But he did not provide any mathematical treatment of this case just as he did not for the double pendulum. Yet he returned to this problem some years later, which proves that this was not a minor issue for him. In 1950, at the insistence of Chamberlin, Frisch published an interpretation of Marshall's theory of value in the *Quarterly Journal of Economics*, based on the 1933–8 lectures he had given on the subject—just after formulating his first hunch on the double and triple pendulum and the conclusion of PPIP.

The paper is based on the distinction between short-term temporary equilibrium, normal equilibrium over short periods and normal equilibrium over long periods. On the assumption that different economic factors determine the price formation for each time

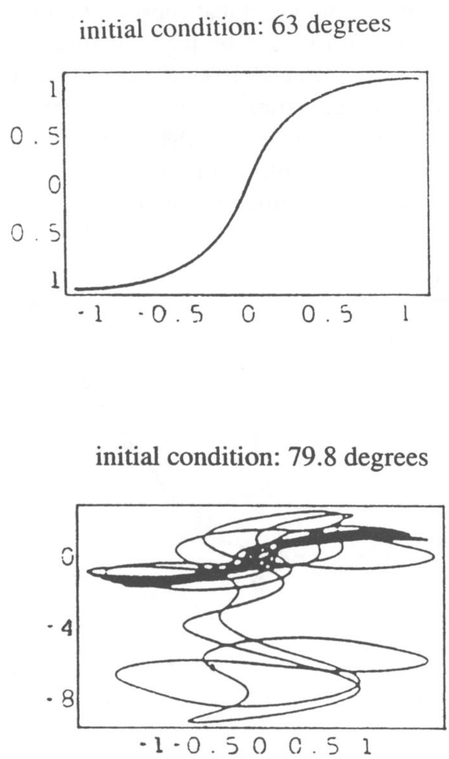

Fig. 5. *Periodic motion and chaos in the double pendulum*

dimension, a mechanical illustration was provided in order to interpret the process of value: three pendula hanging from each other, each pendulum being studied as a separate component of the final movement. The assumptions were rather stringent: the larger pendulum did not move in the relevant period for the smaller one, and the latter did not exert any influence at all on the movement of the former (Frisch, 1950, p. 496). The linear superimposition and strict independence of the three movements was assumed: 'When each pendulum is studied in this way, the composite movement can be built from the separate movements' (*ibid.*, p. 497).

According to Frisch, this implied giving up 'full dynamic analysis' (*ibid.*, p. 498), and accepting the *ceteris paribus* rule: 'The motion of each pendulum illustrates the price component *which would be the result* if a certain set of conditions remained constant long enough for the realisation of the effects pertaining to these conditions' (*ibid.*), just as Marshall had considered (Figure 6) (Marshall, 1890, p. 304).

The system of equations of the triple pendulum is as follows:

$$\ddot{\theta}_1(m_2l_1 + m_3l_1l_2)\cos(\theta_1 - \theta_2) + \ddot{\theta}_2(m_2 + m_3l_2^2) + \ddot{\theta}_3m_3l_2\cos(\theta_2 - \theta_3)$$
$$= -g(m_2 + m_3l_2)\sin\theta_2 + \dot{\theta}_1^2\,(m_2 + m_3l_1l_2)\sin(\theta_1 - \theta_2) - \dot{\theta}_3^2\,m_3l_2\sin(\theta_2 - \theta_3)$$
$$\ddot{\theta}_1l_1\cos(\theta_3 - \theta_1) + \ddot{\theta}_2l_3\cos(\theta_2 - \theta_3) + \ddot{\theta}_3 = -g\sin\theta_3 - \dot{\theta}_1^2\sin(\theta_3 - \theta_1)l_1 + \dot{\theta}_2^2l_2\sin(\theta_2 - \theta_3)$$
$$\ddot{\theta}_1(m_1 + l_1^2m_2 + m_3l_1^2) + \ddot{\theta}_2(m_2l_1 + m_3l_1l_2)\cos(\theta_1 - \theta_2) + m_3l_1\ddot{\theta}_3\cos(\theta_3 - \theta_1)$$
$$= -g(m_1 + m_2l_1 + m_3l_1)\sin\theta_1 - \dot{\theta}_2^2\,(m_2l_1 + m_3l_1l_2)\sin(\theta_1 - \theta_2) - l_1m_3\dot{\theta}_3^2\sin(\theta_1 - \theta_3) \qquad (6)$$

Since Frisch limited himself to the graphical representation of the model, no further conclusions were to be drawn from it. Yet he felt that under different assumptions his conclusion would not hold: in particular, if dependence between the pendula movements were hypothesised, the linear composition of the movement and the previous results would not hold. Indeed, according to Frisch, the simplistic approximation required a significant difference between the lengths and weights of the three pendula, in order to minimise and even to ignore their interaction. But, if otherwise the pendula were

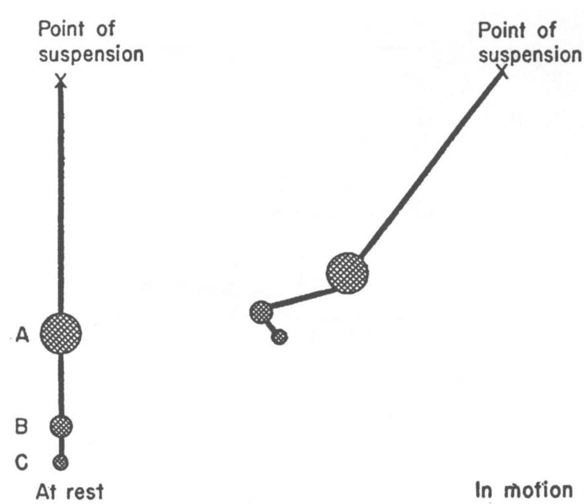

Fig. 6. *Frisch's triple pendulum, representing Marshall's views on the time dimensions*

supposed to be quite comparable or if the movement of one impacted on that of the others, a new analysis and theory would be required:

In this case the system must be seen as a whole and we must study specifically for instance how one pendulum, when swinging, acts as a moving force on the others. Translated into economic language this means that we have to deal *with a truly dynamic analysis of evolution, a theory of progress*. (Frisch 1950, p. 497, my emphasis)

Of course, this is the strange feature that we encountered earlier: it is the process of coupling, of changing harmonics! This 'truly dynamic analysis of evolution, a theory of progress' is therefore the only one that is suitable for studying the global behaviour of the system whenever we discard the radical simplifying assumptions, as previously presented. It is also the only one that is suitable, even if we keep these assumptions, for explaining large differences among the component pendula, since the same strange patterns emerge.

The simplistic case of the linear superimposition is just a figment of graphical imagination. Indeed, if 'one pendulum, when swinging, acts as a moving force on the others', we obtain complex resonance between the pendula, which is a generalisation of the process of coupling. That was the intuition of Henri Poincaré, and what the discussed under the heading of the three-body problem. Yet nothing indicates that Frisch or other economic mathematicians knew this contribution.[1] In this case, as in those of the forced and the double pendulum, a chaotic regime may emerge, with a positive Lyapunov exponent indicating sensitivity to initial conditions (Moon, 1987, p. 93; Tsonis, 1992, p. 138). It is a possible outcome—and actually a necessary one—for certain ranges of parameters and initial conditions.

5. Conclusions

The metaphor of the pendulum is spread throughout the history of science and haunts many of its more creative insights. In 1581 or 1583, Galileo Galilei deduced the constancy of the period of the pendulum for small amplitude oscillations while he was attending Mass at Pisa's Cathedral and considered the use of such a discovery for designing the mechanism of the clock. In 1632, he discussed again the properties of the intriguing pendulum in the *Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican*. Huygens published *Horologium Oscillatorium* (1673), based on a previous work sketched some years before (1658), popularising the pendulum clock. In 1687, in his *Principia Mathematica*, Isaac Newton discussed the collision of two pendula as an expression of the relationship between two bodies. Half a century later, both Leonhard Euler and Daniel Bernoulli studied the movement of several pendula hanging from each other; in a 1738 paper, Bernoulli included and discussed graphs of the double and the triple pendulum. This approximation to the problem of the three bodies was an important step in their research: they identified the natural modes of oscillation and some simple forms of coupling, for instance the long period movements obtained when the pendula were 'beating' in phase and the short period movements obtained when they were out of phase.

The remarkable impact in economics of the developments of mechanics, the science of movement, was studied thoroughly as far as the marginalist revolution is concerned

[1] Frisch had studied in France and knew some of the major works by Poincaré. These books were part of Frisch's library, but the handwritten notes at the margin tend to show that he was mostly interested in Poincaré's concepts of science and worldviews. In 1933, Frisch gave some lectures at the Poincaré Institute in Paris, but he did not mention these mathematical intuitions about non-linear resonance, of which apparently he was not aware.

(Mirowski, 1988, 1989). In the same sense, here is another instance of this type of metaphorical incorporation: the argument of this paper is that these successive versions of the metaphor of the pendulum provided a bridge between the traditional approaches to cyclical movements in economics, and between these and the modes of formal analysis available in physics, which were widely seen as the hallmark of scientificity.

In particular, the metaphor was used to build the econometric revolution and to apply the new mechanistic insights in order to discuss the major puzzle in economics: fluctuations of the state of affairs. Previously to this episode (or in the tradition of previous theoretical trajectories), economics treated the role of agents and the outcome of their social interaction from two competing viewpoints. First, the Walrasian general equilibrium established a clear deduction of macroeconomic behaviour from micro foundations: the rational choices of agents, maximising their utility (or, in a later interpretation, defining a maximising strategy), would necessarily lead to aggregate states satisfying a criterion of market clearing (or Nash equilibrium). In this way, simple one-dimensional attractors were obtained. It is well known that for a long time this approach was predominantly narrative, and that it was not formally developed until the seminal contributions of Arrow and Debreu (and Nash). The second approach, on the other hand, suggested that economies could be conceived of as trajectories of dynamic systems, to be described by difference or differential equations representing changes over time in the states of the model. In this case, one- or two-dimensional attractors were typically obtained in the available models.

But these approaches were unable to provide explanations for changes in the system over time, as far as the first one was concerned, or for the process of aggregation in the behaviour of agents, as far as the second one went. And they were both dramatically unable to explain the emergence of new patterns of behaviour throughout history.

The virtue of Frisch's programme—the adoption of the pendulum as the natural explanation of cycles in economics—was that it provided a partial answer, both conceptual and technical, to some of these difficulties. It established a clear demarcation between the domain of the explainable, the mechanism, and the domain of the unexplainable impulses impinging on the system. It attributed to the first domain the property of stability, and therefore considered that the trajectory of the endogenous variables represented the path towards equilibrium. It assumed reversible time, since all events were reduced to irrelevant random shocks upon a repetitive mechanism. It imposed a definite epistemic distinction between the explanatory endogenous variables and the causal exogenous variables. Based on this distinction, it provided the means for formal treatment of the statistical series: the double decomposition between growth and cycle, and between propagation and impulses in cycles (Louçã, 1997, p. 139f.).

Yet, this programme could not satisfactorily address structural change either in the statistical series or in models of social interactions. Consequently, Schumpeter challenged Frisch's ability to represent his concept of innovations and of the process of creative destruction. As a response, Frisch defined a parametrically forced damped pendulum in order to give a precise content to the shocks, represented as Schumpeterian innovations, which led to a crucial deviation in relation to the original properties of the model. Later on, as seen in this paper, he toyed with the idea of the double and the triple pendulum, in order to provide a rather simplistic framework for describing some distinctive natural frequencies as modes of temporal oscillation.

On the basis of the Schumpeterian pendulum, Cars Hommes suggested that our current chaotic models of cycles are also inherited from Frisch: 'The nonlinear pendulum described by Frisch presumably can exhibit complicated erratic dynamics. Therefore, one may view the recent contributions on 'chaos in economics' within the same line of thought already suggested by Frisch' (Hommes, 1991, p. 276). This is highly questionable: the distinctive appeal of these models is their endogenous generation of very complex patterns, impossible to find in the linear framework, and consequently quite far apart from the traditional explanation by Frisch. For the shocks and propagation approach, instability is necessarily exogenous to the system, whereas in the non-linear framework one discovers a possible endogenous source of permanent instability. In fact, it was precisely this challenging reunification of the context of explanation and that of causation that recently encouraged a number of scholars to investigate chaotic models and to depart from the Frischian framework. Examples abound: chaotic models have been developed for so diverse topics as multiplier-accelerator dynamics (Gabisch, 1984), Cournot oligopoly (Puu, 1993), neoclassical growth (Boldrin and Montrucchio, 1986), R&D expenditure generating chaos (Baumol and Wolff, 1992), IS–LM economies (Day and Shaffer, 1985; Gandolfo, 1997), cobweb models and inventory dynamics under rational expectations (Hommes, 1991), consumer behaviour (Benhabib and Day, 1981), Walrasian general equilibrium (Gandolfo, 1997), overlapping generations models (Grandmont, 1985), equity bond pricing under rational expectations (Van der Ploeg, 1986), Lotka–Volterra populations (Gandolfo, 1997), spatial pattern formation and the Hotelling model for population dynamics (Puu, 1993) and so many others. Endogenous explanations have tended to supersede the previously accepted explanations of exogenous changes within an endogenously driven stable equilibrium system.

Consequently, Hommes' argument is largely *ad hoc*. The Schumpeterian pendulum is admittedly far removed from the traditional econometric approach, based on a linear approximation to reversible processes, and Frisch experienced more than the understandable mathematical difficulties in dealing with it since it implied a major displacement of his conceptual framework as well. Indeed, chaos is persistent instability, which is just the exact opposite of what the simple versions of the pendulum were striving to demonstrate.

For the general case, this genetic difference is recognised at first glance by Benhabib: 'at first blush (. . .) cyclical and chaotic dynamics do not sit well with the idea of strict economic equilibrium' (Benhabib, 1992, p. 3). But he also argues that both the traditional and the complexity approach are compatible and complementary:

> It is more helpful to consider endogenously oscillatory dynamics as complementary to the role of stochastic elements in accounting for economic fluctuations. After all, it does not really make a big difference if endogenous mechanisms by themselves generate regular or irregular persistent oscillations or whether they give rise to damped oscillations that are sustained by stochastic shocks. (*ibid.*)

Of course, as the option depends on the intellectual strategy used for designing the model and not on any meaningful feature of reality, the dichotomy is relative to the space of the representation:

> Whether fluctuations are endogenously or exogenously generated, stochastic or deterministic, is a property of a model, not of the real world. Only if there were a true model, in much more precise correspondence with the real world than are macroeconomic models, might be a useful shorthand to speak of the actual business cycle as being 'stochastic'or'deterministic'. (Sims, 1994, p. 1886)

But the representation here discussed still provides some important insights to be explored for the analysis of real-life processes, as the non-linear framework is obviously more suitable than the linear framework for investigating interaction among agents, as well as the emergence of new properties and structural changes over time. For this purpose, chaotic models, such as those encountered in this narrative, are just the simplest subset of a larger class of complex dynamic systems.

In spite of the intrinsic simplicity, imposed by the deterministic character of the equations generating the orbits, these models already imply major paradigmatic changes in economics, in particular in five domains. First, unlike traditional dynamic models, intertemporal arbitrage leads to non-equilibrium in this case, and, furthermore, even agents with rational expectations cannot avoid sensitive dependence on initial conditions (Gandolfo, 1997, p. 530). Secondly, it is recognised that inhabitants of the fat tails of the distribution typically drive the processes of change (Arthur *et al.*, 1997, pp. 5f.). Thirdly, the rich process of social interaction that is modelled requires a new vision of the very process followed in the formation of expectations: there is an ecological evolution in the population of interpretative devices available to the agents, which are part of their non-linear adaptive networks or complex adaptive systems. Fourthly, new econometric models and non-linear inferential techniques are required in view of the drastic reduction in the confidence interval of the forecasts: 'Instead, what is needed are new classes of combinatorial mathematics and populations-level stochastic processes, in conjunction with computer modelling' (*ibid.*, p. 4). Fifthly, statistical inference itself is subject to severe restrictions, given the irreversible nature of the processes under scrutiny, and the changes occurring in real history. This was the motivation for Joan Robinson's powerful arguments against the pendulum metaphor a quarter of century ago:

> Once we admit that an economy exists in time, that history goes one way from the irreversible past into the unknown future, the conception of equilibrium based on the mechanical analogy of a pendulum swinging to and fro in space becomes untenable. The whole of traditional economics needs to be thought afresh. (Robinson, 1973, p. 5)

In spite of all these limitations and within the framework of this new agenda, the implications of the chaotic nature of the models implicit in Frisch's literary excursions away from the simple damped pendulum are outstanding. First, in the cases of the forced pendulum and of the double and triple pendulum, the system is not necessarily moved by random shocks and endogenously determines its trajectories; furthermore, it is not necessarily driven back to an asymptotically stable equilibrium. Depending on the assumptions about the parameters, it can move further away from equilibrium and generate new patterns of organisation that can only be understood within the framework of the model's complex resonance. Secondly, free oscillation provided a useful and self-evident distinction between endogenous and exogenous variables, the former being responsible for the understanding of the intrinsic oscillatory properties and the latter for the maintenance of the movement. By way of contrast, forced oscillation in the chaotic regime blurs this distinction: it is not only random shocks that may eventually be considered, but also a second type of shock is introduced, as for the 'Schumpeterian pendulum', characterised by a certain structure, its specific frequency and resonance with the natural frequency. The non-linearity, representing the mode of interaction between the variables, is itself responsible for moving it into unpredictable trajectories. The metaphor of the pendulum, imposed by a strategy for the reduction of economics to simplicity, could paradoxically favour the task of thinking economics afresh, as asked by Robinson.

As previously noted, Frisch did not attempt to deal with these complex cases: non-linearity was still a long way ahead and the problem had just been recognised. It was indeed Schumpeter who acknowledged the intrinsic limitation of the mechanical analogy, more so than Frisch, but no further implications were discussed regarding the nature of the most suitable theory. Once again, Frisch stopped at the edge of chaos.

The paradox is that he was in good company—including some of those whose lack of formalism he tried so hard to address and supersede. Marshall, for one, clearly understood the limits of the analogy with the free oscillations of the simple pendulum. He therefore argued that in realistic descriptions one should take into account the will of the person hanging the pendulum as well as the turbulent flows of the environment: rhythmic as well as arbitrary movements would thus obtain (Marshall, 1890, pp. 288–9). And since this was imposed by the very nature of the flow of time, dynamics was considered to be the proper method for conducting the investigation on evolution: that was the Mecca for economics. It has not been sufficiently emphasised that this required a new type of non-deterministic dynamics, what would in fact come to be called the 'indeterminism of the second generation' (Mandelbrot, 1964), but Schumpeter intuitively understood the difficulty. That is why he resisted Frisch's intense efforts to incorporate his theory of innovations in the pendulum metaphor. In his view, a dynamic study of innovations should concern the inner vibratory system, the deformations, and the long-term trends—the emergence of new properties of self-organisation, in modern parlance.

In other words, for both Marshall and Schumpeter, realism required dynamic non-linear models, somewhere between the stone hanging over a turbulent river and the rocking horse metaphors. There was a trade-off between the richness of this insight into the organic structure of the real economies and its computation requiring simplicity; and, since both were desired, static approximations and dynamic narrative descriptions coexisted for a time. But true complex dynamic models, in the sense of the Marshallian version of the pendulum metaphor, required another intellectual framework, one which favoured historical inquiry into irreversibility and change, as well as the study of local attractors. It also implied moving away from the general conclusions on the global properties of equilibrium. Heterogeneity instead of homogeneity, and strategies instead of universal patterns, should be considered and generate a new heuristic programme replacing the self-satisfying assumption of perfect rationality once and for all.

Frisch, on the other hand, tried to construct a new theory based on well-researched mechanical metaphors and their rigorous mathematical representation. But he quickly reached the limits of the metaphor: the forced, the double and the triple pendulum, used to represent Schumpeter and Marshall's theories, led to a rather difficult dynamic non-linearity. He was therefore pushed back to the beginning, to an appreciative narrative of the complexity of economic cycles. Victorious in his drive for the mathematisation of the discipline, Frisch found himself confronted with the intrinsic limits of his endeavour: the immense and still unexplored Magellanic oceans of economic theory.

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