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Rupture and continuity in the original divide between micro-dynamics and macro-dynamics

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Abstract

In 1933, Ragnar Frisch introduced a distinction between micro-dynamics and macro-dynamics in his paper on “Propagation problems and impulse problems in dynamic economics.” His claim that he proposed the first macro-dynamic analysis and that micro-dynamic schemes were limited to the dynamics of specific markets or behaviors had a lasting impact on the field. But the introduction of this separation created a narrative hiding what had been done before and introduced a tension between the two approaches. By going back to the content of micro-dynamic analysis, we are led to two lines of research that were pursued during the 1920s and early 1930s: cobweb models and intertemporal optimization. A pivotal economist for going beyond micro-dynamics was Jan Tinbergen, who had contributed to both these approaches, and went beyond with new analytical tools. However, the idea of intertemporal optimization met with some opposition when it was scaled up to the whole economic system. This prompted Frisch to propose his new approach, which met with immediate success as more schemes were proposed. Tinbergen was himself one of the first converts to macro-dynamics, but the links between the two approaches and the new tensions created by their separation remained. This tension between the newly created categories can be viewed as a result of opposing views on causality, which were rooted in differing mathematical approaches, a point explicitly made by the next generation.

JEL codes: B21,B22,B23,C50

Key words: micro-dynamic, macro-dynamic, Ragnar Frisch, Jan Tinbergen, Charles Roos, Paul Samuelson, cobweb, intertemporal optimization, causality

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I. Introduction

Writing in 1927 in the pages of the *Journal of Political Economy*, Henry Schultz argued that "The mathematical theory of economics has recently been changed from a static to a dynamic theory, and has been stated in a form admitting of quantitative application." (1927: 705). For him, the reformulation of the "epoch-making" static equilibrium into a dynamic mathematical theory was a major advance, and he urged his fellow economists to board the new train: "No disciple of any school of economics can afford to close his eyes to a new discovery, obtained from another point of view, which will not fit in with his own ideas, nor must he treat it as unimportant, if not incorrect." (*ibid*.: 706).

The idea that dynamic economic relations had to be formulated in a quantitative, formal way through the use of mathematical relations may be less revolutionary today, although the precise form that this formulation should take is still not consensual.[2] And it was not then either, for the advances that Schultz was greeting were soon questioned and their early proponents clearly lost the battle of ideas. The subject of this early battle was none other than how to model the economy as a whole, and its consequences were far-reaching for the early decades of macroeconomics.

In 1927, Schultz was referring to a paper by Charles F. Roos which was published in the same issue of the *Journal of Political Economy*. Roos' approach is somewhat unfamiliar today, because it was eclipsed in the early 1930s by the work of Ragnar Frisch, Jan Tinbergen and Michal Kalecki, who established the study of business cycles as the territory of "macro-dynamics," and relegated Roos' work to "micro-dynamic" studies. These two terms were introduced by Ragnar Frisch in 1933 to differentiate the methodology of his paper on "Propagation problems and impulse problems in dynamic economics" (Frisch, 1933) from what had already been done in dynamic economics. By introducing this distinction, Frisch took control of the existing literature and established a rift that blurred what had preceded him, and the ways in which it had influenced macro-dynamics.

During the 1920s, many writers felt that the general equilibrium or even partial equilibrium theories worked out at the end of the 19th century were too static and needed to account for time to become relevant. The accumulation of statistical time series also posed new problems of interpretation, as they were measured in different intervals of time, but aside from the statistical issues, the question became rather clear for a number of authors: How was it possible to provide a dynamic analysis of the economy? This question contained two different issues, the dynamic problem and the matter of talking about the economy as a whole, and the objective of early dynamic models was to find an answer satisfying both of these questions.

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[2] This is illustrated by the diverging opinions on the issue of progress in macroeconomics, and whether a consensus has really been reached. Woodford (2009) and Fair (2012) argued both sides of the debate, the first in favor of the idea of a convergence especially around the idea of dynamic general equilibrium model, while the second argued in favor of the superiority of the "old" macroeconometric approach which had been rejected by DSGE models.

The dynamic problem was tackled first. Economists started from what they already knew, and tried to find out dynamic laws of demand and supply. In this regard, there were two main approaches developed during the 1920s: the first was rooted in the idea that profits, utility or the yield of a machine were maximized over a period of time. By taking into account a time derivative in a maximization program, it became possible to derive the dynamics of prices or other variables of interest. This approach of intertemporal optimisation was developed in particular by Griffith C. Evans, Charles Roos and several Italian writers, and it influenced many other economists in Europe and the United States, who structured this literature around a common theme. The second approach was built partly independently, and partly as an answer to the lack of empirical grounding for intertemporal optimization. It stemmed from the empirical investigation of quantities and prices and the introduction of a time lag in the reaction of producers to changes in prices, and was loosely contained in the work of Henry Moore (1925,1929). The latter's work led Umberto Ricci (1930), Henry Schultz (1930) and Jan Tinbergen (1930) to propose independently the well-known cobweb diagrams by explicitly introducing the time-lag of the supply curve, and the idea was subsequently taken up by many other researchers, especially in the study of markets for a single raw good.

Both groups have already been partially studied,[3] but neither the distinctions nor the links between them have been put together as part of a larger narrative of the construction of dynamic economics, let alone of macro-dynamic analysis. In this regard, a pivotal economist of this period was Jan Tinbergen.[4] He contributed to both approaches in his early years, and went beyond both of them when, in 1931, he proposed a model of shipbuilding cycles made up of a time lag and a time derivative. This allowed him to obtain a wider range of oscillations which showed a period that could be longer than the production lag, something that was crucial in getting out of the simple cobweb oscillations and more in line with empirical evidence. It also drew him away from the idea of intertemporal optimization, a trend which was reinforced as

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[3] Ezekiel (1938) gave a discussion of earlier writings on the cobweb that became the primary source of many subsequent studies, and Lendjel (1998) based his account of early cobweb developments on a comprehensive view of the original texts. Moore's difficulty in building a dynamic approach and his poor reception by his contemporaries, except his most devoted student, Henry Schultz, was described in Epstein (1987, chapter 1), who also analyzed Tinbergen's 1930 paper. Morgan (1990, chapter 5) discussed the development of "dynamic theories of demand" which linked the work of Roos and Evans with that of Moore although it was based on different formalisms (1990: 152-155). Dimand (1988) had also shown this connection, and reexamined the importance of Roos on the early development of econometrics (see also Dimand and Veloce [2007] on this influence). The work of Roos and Evans has also been studied particularly from the point of view of intertemporal optimization in the interwar, in particular by Duarte (2016) and Pomini (2018). Boianovsky and Tarascio (1998) and Pomini and Tusset (2009) centered on the history of the "Paretian school," and its attempts to dynamize general equilibrium theory, the latter especially during the interwar. Another examination of Evans' ideas and the reasons behind his "marginalization" can be found in Weintraub (2002, chapter 2).

[4] Several studies have already established Tinbergen as one of the main economists of the interwar period, in particular Morgan's previously cited study (1990, chapter 4), which centered on his macroeconometric program of the second part of the 1930s. Boumans (1993) examined his doctoral work and some of his early models in (Boumans, 2013). A comprehensive biography of his intellectual trajectory from the point of view of cultural history is forthcoming (Dekker, 2021).

micro-dynamics lost ground to macro-dynamics, at a time when the central problem increasingly became that of describing the movements of the whole economic system.

The idea of saying something about the economy as a whole was nevertheless important in the schemes of Moore (1929), Evans (1931) or Roos (1927,1930), but they had tried to work this out in the context of a general equilibrium and their progress were impeded by the difficulty of describing the whole economy from this point of view. Another solution was that of a "representative agent," used by Franck Ramsey (1928) in works that were based on similar tools as those of Roos and Evans, but it was also rejected on different grounds.[5]

Thus while the dynamic problem was well-grounded and researched at the beginning of the 1930s, the problem of saying something useful about the economy as a whole remained largely open. The solution came when Ragnar Frisch and Michal Kalecki, using the tools of mixed difference and differential equations worked out by Tinbergen, were able to shed the remnants of general equilibrium to concentrate instead on the relationships between aggregates.[6] Frisch explicitly opposed his own approach based on index numbers to the "micro-dynamic" analyses of his predecessors, from whom he could however borrow some of the dynamic tools that they had worked out in the context of micro-dynamics. Tinbergen became one of the most important proponents of the new approach, and contributed to settling the new boundaries between micro- and macro-dynamics.

The birth of macro-dynamics as well as its huge success in the context of the depression have obscured the steps by which it came to be created, and the role in this respect of what became the micro-dynamic approach. However, the latter did not disappear; studies on the cobweb continued throughout the 1930s and beyond, with a particular emphasis on the question of expectations, and eventually they became a ground for John Muth's rational expectations hypothesis in Muth (1961), who referred to the early work of Schultz, Tinbergen and Ricci. During the 1930s, intertemporal optimization was kept alive by the "Paretian school" in Italy and by disseminated researchers in Europe and the US, and its importance exploded in the postwar period, especially after the "discovery" of Ramsey's work by Samuelson and Solow (1956). After it became once again a prominent part of economic analysis, it was applied to explain macro-dynamic movements, with more success this second time, which contributed to dislodge macro-dynamics and macroeconometrics from the positions obtained after the work of Frisch and Tinbergen.

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[5] Ramsey's approach was studied comparatively in Duarte (2016), and his influence on subsequent growth theories in Duarte (2009). Gaspard and Missemer (2019) have questioned the proximity between Ramsey and Hotelling.

[6] Frisch's major role in the construction of macro-dynamics has been the subject of numerous studies; Dupont-Kieffer (2003) has shown especially the interrelation of his modeling work with his approach of economic policy. Dupont-Kieffer and Bjerkholt examined among other things the origin of his micro-macro divide (2010: 46 ff.). Bjerkholt (2007) gave a thorough account of the construction of his 1933 model, while our recent work (Carret, 2021) explored some overlooked issues in his analytic solution. Kalecki's role in anticipating the "Keynesian revolution" of the late 1930s has been the subject of heated debates since Patinkin (1984). Assous et al. (2017) and Assous and Pottier (2018) have studied his macro-dynamic approach from the point of view of aggregate instability.

II. Dynamic schemes of market and individual behaviors

The idea that there were dynamic processes at play in the economy was always acknowledged by economists, without necessarily leading them to recognize that they could have adverse effects on the equilibrium notions of static economics. If dynamic processes were only a convergence toward the well-understood static equilibrium, why should the economist bother with them? The discovery of business cycles and the emergence of a field centered around their explanation was central to question this belief. It also created the necessity of redefining what equilibrium meant or of finding a way to explain those fluctuations as an equilibrium process. Vilfredo Pareto was very concerned with this problem, but he became much more pessimistic on the possibility of creating a truly dynamic approach in his later years, around the beginning of the first world war (Pomini and Tusset, 2009: 316).

It was the work of another italian, the mathematician Vito Volterra, who proved essential to go beyond the mathematical difficulties that confronted Pareto. His importance for Evans, who visited him as a student, has been underlined (for instance by Weintraub [2002: 42 ff.]), and his work sparked a renewed interest in the dynamization of general equilibrium during the 1920s, especially by his student Roos who developed the use of the calculus of variations to derive dynamic equations from maximum principles. These developments quickly returned to the European continent, where they were particularly taken up in Italy by those followers of Pareto that still wanted to realize his vision of dynamics. In 1925, two years before Schultz talked of the transformation of economic theory into a dynamic science, Harold Hotelling thus celebrated the "dawning economic theory based on considerations of maximum and minimum which bears to the older theories the relations which the Hamiltonian dynamics and the thermodynamics of entropy bear to their predecessors." (1925: 352). In the same paper, he pointed out the usefulness and generality of the new theory, as well as its implications for "economic and even ethical theory" (*ibid.*: 353), and he went on to apply those ideas during the following years to different problems, such as the optimal exploitation of natural resources.

To understand the questions and difficulties raised by this approach, and why it was eventually left aside, we concentrate on Evans' and Roos' work. Their starting point was the producer, and the idea that he would maximize his profits over a certain period of time ahead of him, which would give the trajectories of prices and rates of production as a solution to his intertemporal maximization program. They recognized that this problem had already been solved in physics with the creation 150 years earlier of the calculus of variations by Euler and Lagrange, and sought to import this tool in economics. Lagrangian mechanics showed that the trajectory of a particle over a certain interval of time would minimize[7] a quantity named "action," taken to be the integral between two time instants of a function of a vector of positions, its derivative with respect to time and time itself:

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[7] Make stationary would be more correct but is irrelevant for our purpose.

$$S = \int_{t_1}^{t_2} L(x, \dot{x}, t)dt \tag{1}$$

and the solution to this problem in the form of the Euler-Lagrange equation is:

$$\frac{\partial L}{\partial x} - \frac{d}{dt}\frac{\partial L}{\partial \dot{x}} = 0 \tag{2}$$

Evans was convinced that this approach was natural to economics, where maximum principles had been a guiding concept of static equilibrium.[8] From a practical point of view, at least two problems must be solved to transfer this idea to economics: first some bounds were necessary for the integral, and several approaches were developed to solve this issue. Second, it is apparent that the lagrangian must depend on time explicitly, or implicitly through a time derivative. Evans and Roos looked for a way to import those ideas by solving these two problems.

The difficulty was thus to find a way to make decisions depend on time or a time derivative, for without the time derivative, we note that the familiar maximum condition $\frac{\partial L}{\partial x} = 0$ is obtained immediately, something that was underlined by Roos to show that he could derive older results but was more general in considering dynamic cases. Evans was wary of the concept of utility and was mainly interested in profit maximization over time, so that he proposed what became known as the "Evans demand functions," which entered the maximization program of the producer to determine his profit. In his functions, demand depends not only on current prices, but also on their rate of change; the fact that this introduction of time derivatives was entirely dictated by the mathematics of the calculus of variations is illustrated by the conspicuous absence of any statistical estimation of those curves by Evans or Roos in their early papers.[9] Most of the subsequent work of Evans and Roos consisted of different variations on this same theme and generalizations from monopoly to competition.

The calculus of variations became used by others, for instance Hotelling (1931), Ramsey (1928) or Amoroso (1929). These economists developed a wider array of approaches for the first problem: Evans and Roos had considered a time period bounded by t_1 and t_2, representing a sort of horizon for the producer over which parameters could be taken as constant (Roos, 1927: 638). Roos moved on to introduce time-discounting in his models, something which Ramsey discarded as "ethically indefensible" as it would amount to giving more weight for today's pleasures.[10]

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[8] "In conclusion it may be remarked that the relation of economics to the calculus of variations is not accidental, nor the result of a generalization from previously found differential equations, since it is in the nature of an economic system that there should be a striving for a maximum of some sort" (Evans, 1925: 94-95). A similar idea can be found in Evans (1930: 167).

[9] Dimand (1988: 157) argued similarly that "[t]he strongest reason for adopting this modelling strategy appears to have been mathematical convenience."

[10] See Duarte (2016) on the different approaches to time-discounting in interwar economic models and how this assumption became necessary in postwar models.

Ramsey thus introduced the notion of “bliss,” which avoided discounting future pleasure, although this did not convince other economists. Hotelling, in his studies of the optimal exploitation of resources, justified using time-discounting by arguing that capital was productive and future pleasures uncertain, in particular because the distribution of the benefits of a mine did not enter in his equations (Hotelling, 1931: 145).

Another economist who contributed to this approach was Tinbergen, who wrote his thesis about “Minimum problems in physics and in the economy,” (Tinbergen, 1929) although the economic part was reduced to an appendix. In it, he sought to expose systematically how most physical laws could be derived from extremal principles, in many branches of physical sciences (mechanics, thermodynamics, optics…). Tinbergen developed several schemes involving the rate of change of a variable: he considered problems of stocks, problems of “friction” by which he meant day-to-day changes in production, and problems of delays between variables. These latter problems were simplified by transforming the delays into rates of change, while he took the exact opposite approach in his later work by transforming rates of change into discrete-time differences, an approach more suited to statistical estimation. Although he warned that Evans’ and Roos’ theories were “extremely theoretical” (Tinbergen, 1929: 52), he did not propose at the time something more empirical. He pursued this approach in subsequent years and detailed the framework of intertemporal optimization by introducing the “economic horizon” and expectations: the first determined the endpoint of integration, and the second introduced a time derivative into the integrand (Tinbergen, 1933b). The idea was to justify Evans’ demand functions, for instance by arguing that part of the demand was speculative and depended on price increases, something he did for instance in Lausanne in 1931 with reference to Evans, Roos and Amoroso (Tinbergen, 1933a: 40).

But this did not solve the problem of empirical estimation, which was becoming one of the most important preoccupations of economists in the 1920s. Concerns about how to handle statistical time series that were measured at different intervals of time, particularly in the case of single commodity markets, were one of the main drivers of the construction of dynamic demand curves, but contrary to Evans’ approach they were by necessity modeled as a discrete time process. This gave rise to a second way of dynamizing economic relationships, which was developed at least in part as a reaction to the problematic lack of empirical basis in most of the literature based on the calculus of variations. Schultz, who had hailed the coming of dynamic analysis in 1927 with respect to Roos’ work, was led by his own explorations of demand curves to disbelieve the suitability of the form retained by Evans, and during a discussion of the latter’s theory of crisis he underlined that his statistical exploration of demand functions did not lead him to give more importance to those that depended on the rate of change of prices.[11]

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[11] “Evans' formula can, therefore, hardly be an improvement over (3) or (4) as long as we confine ourselves to the representative consumers' goods and to such 'normal' periods as that of 1896-1914.” (Schultz, 1931: 71).

The solution came from Moore, who estimated his demand and supply curves from the same series of prices and quantities by assuming that supply depended on the prices of the preceding period while demand depended on the prices of the current period (Moore, 1925;1929). While he did not propose a model of this relationship, the publication of his 1929 *Synthetic Economics* had enough publicity that three economists managed independently to propose the famous interpretation of the cobweb the following year. Schultz, who was Moore's student at Columbia, took up his ideas and formulated one of the earliest illustrations of the cobweb in Schultz (1930), where he estimated the demand and supply schedules on different primary good markets, leading him away from the idea of basing dynamics on intertemporal optimization. The two others were Umberto Ricci and Tinbergen, who produced two papers published in the same issue of the *Zeitschrift für Nationalökonomie* (Tinbergen, 1930; Ricci, 1930).

Ricci, while admirative of Moore's ambitions, criticized his approach by showing that the demand and supply functions he had estimated led to oscillations of increasing amplitude around the equilibrium: "So the equilibrium, once broken, is lost forever. The American economy is, at least as far as the potatoes are concerned, at the mercy of the tragic fate of a growing disequilibrium!" (Ricci, 1930: 656). While Ricci insisted on the problem of stability, Tinbergen was more interested in the problem of self-sustained fluctuations, and remarked that they relied on some very specific parameters. Tinbergen often referred to this model as the "hog cycle," because Hanau, a german economist of the Berlin Business Cycle Institute, had worked out a similar empirical relationship for the pork market in 1928 (Hanau, 1928). It became the "cobweb" after the different diagrams proposed by Ricci, Tinbergen and Schultz were picked up by Kaldor (1934) and Ezekiel (1938), who gave the first english accounts of this model. One problem of the cobweb was the triviality of its assumptions, and the resulting limited dynamic properties of the model. The only dynamic element was the supply lag, and it was based on a rudimentary treatment of expectations, something that was criticized very early on, for instance by Lachmann (1936) who argued that a better treatment of expectations, especially in relation to stock formation, would generally stabilize the model.

Thus by the early 1930s there were already at least two competing approaches of economic dynamics, but both described mechanisms rooted in one market or the behavior of producers or consumers. One was more theoretically oriented and based on intertemporal optimization, while the second was more empirical and based on the influence of supply lags on market dynamics. The fact that these were established approaches to dynamic economics can be seen for instance in another article by Tinbergen about dynamics, where he underlined the contributions of these models to "the expansion of the static theory of social economy necessary for an understanding of real economic movements" (Tinbergen, 1931a: 169).[12] After contributing to both lines of research, Tinbergen was ideally positioned to know their limits, and eventually he was the first to go beyond them. His 1931 paper, "Ein Shiffbauzyklus," while in direct continuity with these different works, allowed him to go beyond the rigid supply lag of the cobweb theory by

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[12] He added Moore's (1929) approach as a third one, which he called "comparative statics."

introducing a time derivative outside of a framework of optimization, which led him to find a period of oscillation longer than the production lag, as well as many other possible movements.

His model was similar in some sense to the cobweb scheme, as he was still interested in the movement of one market, but it also differed in that he studied a durable good instead of the usual perishables (cotton, sugar, potatoes…). Tinbergen sought to find endogenous fluctuations, and studied the behavior of an equation of the type $f' =- af(t - \theta)$, stating that the rate of increase of the total ship tonnage is inversely proportional to the total tonnage from a previous period. The detailed solution that he gave to this equation proved subsequently very useful for other economists looking to solve dynamic models based on mixed equations.

Importantly, Tinbergen linked the movements he observed for his shipbuilding model to more general "trade cycles," and underlined that his approach "points to a method for judging the stability of the economic system in general." (Tinbergen, 1931b: 162). The interest in the economy as a whole and not simply on isolated markets was expressed several times by Tinbergen, and he was not the only one to try and go this way. Roos and Evans in particular had always had in mind the idea of talking about a more general trade cycle.

In most of the work of the early proponents of intertemporal optimization, there was in fact a clear objective of saying something about the economy as a whole, underlining that this early approach to dynamics was not limited to micro-phenomena. This was clearly visible in Ramsey's model, whose objective was to find the optimal social rate of saving by generalizing the framework of utility maximizing agents to the whole society, with the device of a representative agent.[13] Evans and Roos, although they started small with monopolists or duopolists, also aimed at building much larger dynamic theories that would account for business cycles and crises. This was not simply done through the extension of theories to the case of *n* producers, such as in Roos (1927), but also with the use of index numbers and the hypothesis that their evolution could also be interpreted as the consequence of a maximum principle (Evans, 1931). After the 1929 collapse, they both developed a theory of crises that tried to explain from the same extremal principles the turning points of the business cycle.

In 1930, Roos tried to build a model connecting economic theory and business cycle research.[14] A crucial point of his model was that the time interval was supposed to be very small, so that coefficients could be considered as constant in this interval and that the integration over time would be possible. The driving force of cyclical fluctuations was that the optimizing producer, faced with an abrupt change in one of the system's coefficients, would be led to revise his plans of production and start optimizing again with the new parameters (Roos, 1930: 513). The

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[13] That this was not accepted in the interwar has been underlined by Duarte: "Ramsey's model combines mathematical formalism, utility-maximizing agents, and aggregate-level control with clear normative content. … the latter two aspects were considered to be mutually exclusive prior to the 1950s, and such incompatibility is also behind the delayed recognition of Ramsey's 1928 analysis among economists." (Duarte, 2009: 164).

[14] "In spite of these researches [on cyclical fluctuations], no successful attempt to relate the theory of business cycles with economic theory has been made." (Roos, 1930: 501)

economic mechanism was feeble to say the least; identifying several possible types of solutions (monotonous or cyclical, explosive or damped), Roos proceeded to explain the alternation of cyclical phases by the transition from one type of movement to the other after a change in parameters simply by going down the list of possible movements. He also seemed to abandon the idea that the optimal path was really taken: "It would be folly to maintain that a producer obtains a maximum profit. The curve which gives a maximum profit is the ideal sought by a cut-and-try method and only approximated in a rough sort of way." (Roos, 1930: 515); he argued that "good" producers, that is, those with good foresight, would be closer to their actual curves. In periods of panic and crisis, he even hypothesized that there were no more optimal paths, and the dynamic trajectories were left unexplained.[15]

The following year, Evans proposed "A Simple Theory of Economic Crises," at the annual meeting of the American Statistical Association. He remarked that in his model, "the stable situation is likely to be the exception in the economics of moving prices, rather than the crisis" (Evans, 1931: 64), especially when the time interval of maximization became too long. To explain aggregate fluctuations, he turned to the theory of price indices and their "average motion."[16] The evolution of his system and the explanation of the "crises" was then similar to that of Roos; at some point during the evolution of prices and trade, they would "pass beyond the point where the hypotheses are tenable, however high that may be" (*ibid*.: 67). After that, a "retrograde movement" set in, with a "similar character of permanence," until a point was passed again below which the economic structure changed and forced a revision of dynamic paths.

Perhaps the biggest difficulty of this approach was thus that it did not really managed to explain economic fluctuations; even if the resulting trajectory was a cycle, there was no easy way to interpret this as the causal consequence of something endogenous to the economic system: it was just the best adaptation of the agent to the economic conditions, themselves often considered as fluctuating exogenously.

In his thesis, Tinbergen also explored cases where there would only be one "ophelimity" function optimized, and underlined that this could be used at the aggregate level: "we remind the reader that it is not only the problem in which only one person occurs, who is faced with given circumstances, but also every problem in which an organized group occurs, and every monopoly problem." (Tinbergen, 1929: 52). Tinbergen was interested in optimal policies that could be scaled from the producer to the government, as was argued by Boumans (1993: 144), and he took the seasonal fluctuations as given in his thesis, to find the optimal policy with respect to these movements (Boumans, 2004: 33). There was thus an important difference between Tinbergen's work (or even Ramsey's), where intertemporal optimization was openly used as a normative tool to find a best policy, and Roos' and Evans' theories of crisis, where intertemporal optimization

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[15] "Prices tumble, production is curtailed, and a crisis results. Producers become panicky and forget all about profit over an interval of time." (Roos, 1930: 520)

[16] "These formulae [price index] enable us to coördinate prices throughout a system of economics and discuss their average motion." (Evans, 1931: 65)

was the basis for a description of economic fluctuations. That the latter failed in their objective was one of the main impetus for the development of a new approach.

III. Macro-dynamics between rupture and continuity

We have shown that in the early 1930s, there were already several mathematical approaches to economic dynamics, including one that was compatible with static economic theory. But the schemes relying on intertemporal optimization proved quite dissatisfactory to many economists who saw the difficulty of conciliating their premises with the economic events of the Great Depression. Instead of rejecting altogether what progress had been made by intertemporal optimisation or the cobweb analysis, Frisch took the approach of circumscribing these preexisting dynamic works to what he called "micro-dynamic analysis,"

> by which we try to explain in some detail the behaviour of a certain section of the huge economic mechanism … The essence of this type of analysis is to show the details of the evolution of a given specific market, the behaviour of a given type of consumers, and so on. (Frisch, 1933: 2)

Contrasting it with micro-dynamics, Frisch argued that "[t]he macro-dynamic analysis, on the other hand, tries to give an account of the fluctuations of the whole economic system taken in its entirety." (Frisch, 1933: 2). While before him, there were no real limits to what the micro-dynamic approach would study, because it was not really thought of as "micro-dynamics" but simply as dynamics, his distinction clearly relegated it to the study of isolated markets and individual behaviours. This created a void which he was ready to fill with his own approach: "At present certain examples of micro-dynamic analyses have been worked out, but as far as I know no determinate macro-dynamic analysis is yet to be found in the literature." (ibid.: 3). That the rift introduced was so successful and had such an important influence on economic modeling should not hide that it was a rhetorical distinction and not something self-evident as he argued;[17] on the contrary, it introduced a lasting tension which has exploded from time to time since its creation. But it also gave him a control over the literature that was sprouting up fast around economic dynamics, reinforced by his place as a central character of the econometric movement and the editor of the just created *Econometrica*.

His influence cannot be overstated; in a very short time after creating these new categories, Frisch managed to redirect the approach of business cycles toward the framework of macro-dynamic analysis. He had in fact several occasions to explain and clarify his position on his newly established distinction during the year 1933, in the Spring when he gave the Poincaré lectures in Paris, at the annual European meeting of the Econometric Society in Leiden in the Fall, and finally through the publication of his essay in the *Cassel Festschrift*.

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[17] "When we approach the study of business cycle with the intention of carrying through an analysis that is truly dynamic and determinate in the above sense, we are naturally led to distinguish between two types of analyses: the micro-dynamic and the macro-dynamic types." (Frisch, 1933: 2)

Frisch himself had started from a "micro-dynamic" perspective when he had looked for economic analogies to physical concepts, especially during the late 1920s.[18] In Frisch (1926), he developed a "force field" version of marginal utility and mentioned the dynamics that could be drawn from such a theory, while he built a more complete example of a micro-dynamic scheme in Frisch (1929). This paper was partly translated in Frisch (1992), but the micro-dynamic model was not translated, and only the first four sections which developed a general view of dynamics were present; this attests the relative ignorance in which micro-dynamic models, which were dominant in 1929, had fell after Frisch's macro-dynamic approach was developed and spread. Frisch (1995 [1933]) was a paper on "polypoly" which was presented at the 1932 Paris meeting of the Econometric Society and published in april 1933. In the conclusion of this paper he argued that

> one essential dynamic element is still lacking, namely the analysis of the speed of movement and the connection between the concept of speed and that of force. That is a subject with which I propose to deal elsewhere. This concept, which is essentially dynamic, will lead to the notion of cyclical oscillation. We shall there again meet the concept of friction, and we will have to discuss this fundamental dynamic problem: what is the source of energy which maintains these oscillations and which keeps economic life in a state of perpetual flux where static equilibria are never realised? (Frisch, 1995 [1933]: 359)

Three related things can be gathered from this quote: the first is that Frisch wanted explicitly to deal with friction, or in other words, the dissipation of mechanical energy which is not easily handled by the calculus of variations, because this approach is based on the assumption that energy is conserved. The second is that he looked for the connection between speed and force, or in other words, an economic analogue to Newton's second law, which he eventually proposed in his famous rocking horse model. The third is that he recognized that a nonconservative system needed some kind of external energy to be revived, which led him to place a central importance on the idea of external impulses acting directly on the system and not on the parameters as in Roos' and Evans' theories of crises.

That the rocking horse model was viewed as the dynamic theory needed to explain economic movement (or propagation) is attested by the Poincaré lectures, where we find the same quote during the second lecture (Frisch, 2009: 35), which was followed in the next conference by the exposition of the rocking horse model. During the Poincaré lectures, he also proposed an example of a micro-dynamic system, which was supposed to appear in Frisch (1933), but was edited out to shorten the paper (Bjerkholt, 2007: 473).

The same theme was at the center of his presentation on "Some Problems in Economic Macrodynamics," which led to a discussion about the separation that he had introduced. After his

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[18] On Frisch's use of mechanical analogies and physical concepts, see Dupont-Kieffer (2003), in particular the last part of the thesis (Chapter 5, p.283 ff.).

talk, Schultz asked whether the principle of profit maximization ruled the economic system: "In his answer Frisch denied that the profit maximization principle could be taken as a general principle governing the economic system," and Frisch added that "[t]here seems to be present a phenomenon of inertia - or perhaps friction - that is in opposition to a general principle of profit maximization." (Marschak, 1934: 192).[19] This was a clear rebuke of the approach taken by Roos and Evans in their theories of crises. The following year, during a lecture at the London School of Economics around the same themes, he distinguished again the microdynamic analyses associated with Evans, Roos and Tinbergen from the macrodynamic schemes of himself, Kalecki and others (Bjerkholt, 2007: 477).

The distinction was quickly accepted and endorsed, in particular by Kalecki and Tinbergen, who quickly came to form the core of the macro-dynamic group with Frisch. Kalecki, who had made a presentation entitled "Essai d'une théorie des mouvements cycliques construite à l'aide de la mathématique supérieure," renamed his paper "A Macrodynamic Theory of Business Cycles" when it was published in *Econometrica* in 1935. Tinbergen also endorsed the dichotomy when, in his 1935 survey of business cycle theories, he argued that the use of index numbers was the main distinction between micro-dynamics and macro-dynamics, although we have seen that Roos and Evans had tried to use them in their "micro-dynamic" theories.[20] By the 1935 Namur meeting of the Econometric Society, it was clear that the intertemporal optimization approach of economic systems or business cycles had fallen out of favor in Europe; after presenting Tinbergen's contribution in the report of the meeting, Georges Lutfalla noted that "the major concern with the deduction [of dynamic laws] from first principles (free competition, monopolies with maximal profit or maximal utility) has completely disappeared. … It is not an exaggeration to say that with Mr. Tinbergen, some econometricians have deliberately abandoned the spirit of the Lausanne School." (Lutfalla, 1936: 542).

The importance of micro-dynamic schemes for the construction of the first macro-dynamic models should not however be downplayed. Tinbergen proved to be a crucial connection; his 1931 study of the shipbuilding cycle was the basis of Kalecki's solution for his model for instance (Kalecki, 1935: 333). In addition, both Frisch's and Kalecki's models depended on a production lag similar to the production lag of the cobweb mechanism, which appeared a lot less controversial in aggregate systems than profit or utility optimization. Their innovation was to link it with the aggregate relationships between consumption or profits, and to introduce, as Tinbergen did before them, a rate of change of some quantity (consumption in Frisch's model,

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[19] In the report, Marschak added that "[t]his point had been emphasized by Professor Fréchet in a recent discussion at the Institut Henri Poincaré." (Marschak, 1934: 192), a clear reference to the Poincaré lectures, where Fréchet's presence is attested by his correspondence with Frisch (see in particular Fréchet to Frisch, February 16th, 1933, and Frisch to Fréchet, March 13th, 1933, RFA, where they discussed the organization of the conferences).

[20] "The grouping of the elements, which has its statistical counterpart in the calculation of index numbers of all sorts, is characteristic of what Frisch calls *macrodynamic* economics in contrast with *microdynamic* economics" (Tinbergen, 1935: 243, original emphasis).

investment in Kalecki's). The fact that they examined aggregate relationships allowed them to use index numbers to estimate the parameters of their systems, albeit in a rather primitive way.

As an early convert, Tinbergen produced pioneering new macro-dynamic models, and the proximity with earlier micro-dynamic schemes was even more pronounced in his case. In 1934, he explained the aggregate fluctuations of prices, production and purchasing power by a simple equation where supply was equal to purchasing power divided by prices, something very similar to the equation that he had used in Lausanne in 1931 to describe the dynamics of one market. But his perspective had changed, and the fluctuations in purchasing power were explained by changes in activity and employment, although the production lag remained and Tinbergen took care to note that the cobweb (or rather, Hanau's pork cycle) could be found as a particular case of his equations (Tinbergen, 1934: 302).

Nevertheless, the program underlined by Frisch in the Poincaré lectures, and forcefully exposed in his rocking horse model, effectively reoriented dynamic analysis toward a non-optimizing approach of macroeconomic phenomena.[21] Frisch launched the "macro-dynamic" research program by decoupling it from intertemporal optimization of forward looking agents, and grounding it on the causal systems stemming from such forces as the multiplier or the accelerator, which were at the heart of many macro-dynamic models of the 1930s and beyond. The macroeconometric research program, elaborated at the end of the 1930s by Jan Tinbergen, with the extension of macro-dynamic models toward statistical estimations of simultaneous equations, derived its equations from macroeconomic relations without optimizing foundations, and the subsequent marriage between Keynesian theory and macroeconometric modelling, particularly in Lawrence Klein's work, may very well explain the absence of intertemporal optimization in those models dominating economic policy in the postwar period, and the marginalized role of expectations in postwar mainstream keynesianism.[22]

But macroeconometric modelling was not the only symptom of the new program. Tinbergen's conversion to macro-dynamics prompted him to explore in depth the possibility offered by the new approach, and he showed a breadth and originality of ideas that have been somewhat lost after the success of the macroeconometric industry. Developed in papers written in Dutch, German and French, his models cast a particularly interesting light on the renewed perspective on causality in macro-dynamic systems, especially from the point of view of time asymmetries.

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[21] Wade Hands (2012) argued that Walrasian models and certain Keynesian models (principally IS-LM) were characterised by an absence of path-dependencies and irreversibilities, while "such path-dependencies and irreversibilities were common features of many of the demand theories the Walrasian program was competing against during the 1930s and 1940s." (2012: 109). We believe that it is precisely this program of path-dependencies that characterised the macro-dynamic research program, as developed below. A more detailed examination of the protagonists of this program, and of the importance of the stability and instability issues that shaped it, can be found in Assous and Carret (2021a).

[22] The fact that Klein was interested in causality is also noted by Hoover (2004: 162). More generally, Hoover makes the case that "causal language flourishes when empirical economists start with data, but withers when they start with mathematical theory." (2004: 161), and that the rise of Walrasian general-equilibrium models were the dominant factor in the disparition of causality in the postwar period, which is consistent with the story developed here.

An early example of the importance of rooting causality in time asymmetry can be found in an article written in German by Tinbergen in 1936, which was aimed at explaining to economists the usefulness of quantitative relationships (Tinbergen, 1937).[23] Tinbergen built a simple model of four difference equations in this paper, and explained the relationships between the variables with a diagram of arrows running from cause to effect (figure 1).

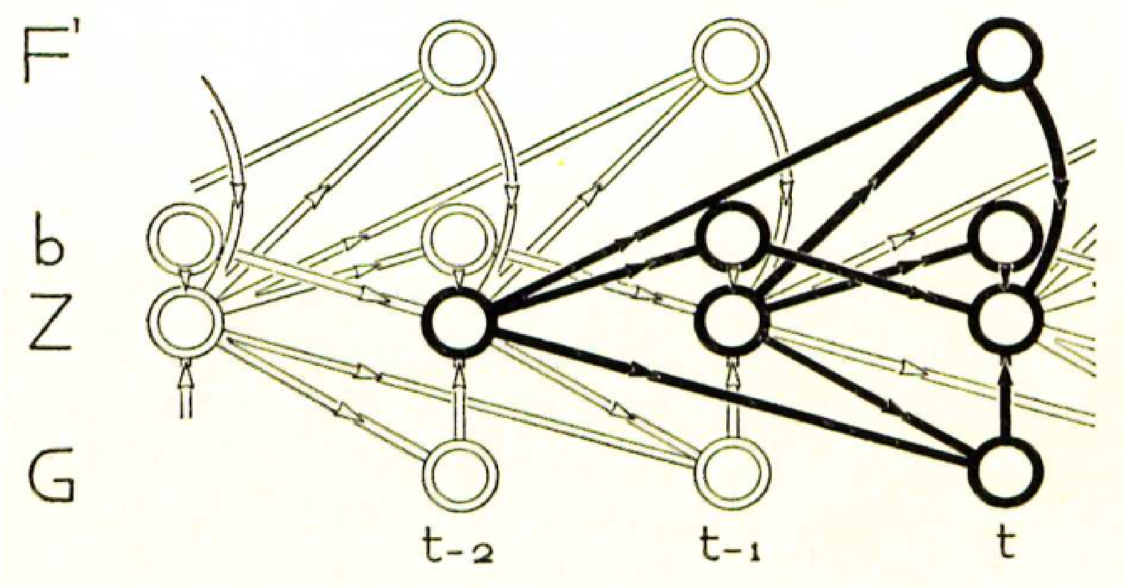

Figure 1: diagrammatic representation of causality flows between variables in Tinbergen (1937)

Tinbergen also constructed several nonlinear dynamic models with multiple equilibria which gave a much more dangerous meaning to shocks; in a paper published in German (Tinbergen, 1936), he built a simple dynamic model where the ratio of (aggregate) demand and supply determined the evolution of prices. Supply was itself dependent on prices two periods ago, while demand depended on the prices of the previous period. This model had two equilibria, and only the highest of these two equilibria was stable: if a shock pushed the economy too far away from this equilibrium, it could gather enough momentum to go over the other, unstable equilibria, which would repel it downward and provoke a complete collapse of prices, production and purchasing power.[24] This gave an overriding historical significance to shocks and initial conditions, and prevented any attempt to root dynamics in an atemporal, path-independent modelisation based on maximum principles and conservative fields. That this latter approach was incompatible with an effective causality came directly from its physical origins. Although economists had argued that they were only using formal analogies, unwittingly or not they were also importing centuries of debates around the problems of causality and teleology in dynamics.

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[23] This was done particularly as a consequence of Tinbergen's meetings with the most prominent (literary) theorists of the time in Geneva (to discuss Haberler's upcoming book and his own) and Vienna (at the initiative of Morgenstern). For a context on these meetings, see Boianovsky and Trautwein, (2006) and Hagemann (2020: 369).

[24] See Assous and Carret (2020b,2021b) on these models and others developed by Tinbergen, which were elaborated in particular to discuss the influence of economic policies and to interpret the long depression of the early 1930s.

IV. Causality and teleology in the micro- macro-dynamic divide

The problems associated with causality and teleology that were raised by a dynamics based on intertemporal optimisation can be found in a more diffuse way in the economic literature throughout the following decades. Because a lot of the mathematical economists' work of this time was based on mechanical analogies, the historical origins of the calculus of variations give some very important elements to understand the limits and difficulties of its transfer in economics, and the "ethical" questions it raised.[25]

At the end of the 17th century, Newton built mechanics from the relations between external forces acting on the motions of objects.[26] This was embodied by the three laws of motion, in particular the second, $F = m\dot{v}$, where F is a vector of forces, m the constant mass of an object and $\dot{v}$ the time derivative of its speed (its acceleration). This formed the basis of classical (Newtonian) mechanics, but other approaches were subsequently developed under the umbrella term of analytical mechanics, which arrived at the same results by different means. Arising from different mechanical and optical problems, one of the first developments was the idea that the trajectory of a particle followed a path minimizing some quantity. This became the root of Lagrangian mechanics, which led to the discovery of energy principles developed throughout the 19th century and the construction of Hamiltonian mechanics. But in the 18th century when it was first formulated, which quantity was supposed to be minimized or maximized was not at all clear, and the discovery that trajectories followed certain paths of least resistance or least action bewildered scientists. This was particularly the case with Maupertuis, who developed around 1750 the principle of least action and saw in his principle the proof that God existed and had built the world according to a harmonious principle. Before him, this had already been a central tenet of Leibnitzian philosophy, based on Leibnitz's understanding of the physical world which anticipated the development of extremal principles, and these optimistic approaches were severely mocked by Voltaire's *Candide* as justifications for tyrannies.[27]

After Maupertuis, Euler and Lagrange began to stir away from the teleological aspect of the principle, and developed much of the mathematical apparatus to solve problems with the calculus of variations, so that we still call the solutions of the form (2) Euler-Lagrange equations today. In equation (1), the function $L(.)$ was a mechanical lagrangian equal to the difference between

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[25] This overlaps the story told by Mirowski (1989), although he was mainly concerned with the development of static general equilibrium from energetics, which led him to left Newton aside (Mirowski, 1989: 404), while we argue that it is important to understand the difference in perspective between Newton's approach and analytical mechanics. Several studies have since tried to expand his story to dynamics, for instance Wulwick (1995) whose account begins after the second world war.

[26] The study of motion is more precisely the subject of the dynamic branch of mechanics, while the study of equilibrium is the territory of statics and kinematics is only interested in a description of the movement without reference to its causes. Etymologically, "dynamikos" is greek for "strong, powerful" and was introduced by Leibnitz who wrote an "Essay de Dynamique" in 1692, where he described it as the science of forces moving bodies.

[27] On Maupertuis, Leibnitz, Voltaire and the controversies around the first priority claims for the principle of least action see Radelet-de Grave (1998).

kinetic energy and potential energy in the case of a conservative force. By applying the Euler-Lagrange equation to (1), we can find Newton's second law of motion, but the important caveat is that this only works in the case of conservative systems and it cannot generally account for friction, which is a non-conservative force. This means that, under certain conditions, approaching the problem from the energetic point of view gives equivalent results as the approach that starts from forces and acceleration. However, the two processes are entirely different: in the former, the trajectory of an object is viewed globally, as that motion which will make a certain quantity stationary or minimum over an interval of time; in a sense, this is an equilibrium trajectory, and the only one possible for the system. On the other hand, Newton's second law considered the trajectory from the local point of view, as influenced at each point in time by different forces, and this allows to model much more efficiently the frictional forces characteristic of the real world. This distinction overlaps with two very different conceptions of causality, with important consequences for economics: in the first case, the trajectory selected is that which makes a certain (optimal) situation happen, a conception of "final causes," while in the second case the trajectory results from the contingent events happening at each instants, or the idea of "effective causes."[28] One can add that in the first case, dynamic laws are deduced from a general principle (maximisation or minimisation over time), while in the other the precise form of the forces are governed by many different laws that were deduced from experiments and measurements (such is the case for the gravitational force or Hooke's law of springs, both developed during the 17th century).

Economists took different approaches to deal with the metaphysical and methodological issues raised by the transfer of the calculus of variations. Tinbergen argued that he was merely interested in the "formal analogy" between a number of problems that could be represented under this form of a "minimum problem," an idea taken up by Boumans (1993). But he was also conscious of those issues, and he used this argument of purely formal analogy to explicitly avoid addressing the question of the teleological aspect of the minimum concept, arguing that he was inclined to think that the "striving for a minimum" was something that lied in the nature of an economic system (Tinbergen, 1929: 2), just as Evans had done before him.

Others chose to remain strictly within the territory of micro-dynamics as defined by Frisch, for instance in subsequent studies using the calculus of variations such as in Tintner (1937,1938) or in Samuelson (1937).[29] When these authors talked about aggregate systems, they did not rely on maximum principles (such as in Samuelson [1939,1941,1947] or in Tintner [1942]), and Tintner

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[28] This distinction, drawn in 1748 by Euler (1750), corresponds to the last two causes of Aristotle's four causes: material, formal, efficient and final causes. See Falcon (2019) for a presentation of Aristotelian causality, and Hoover (2008) for a discussion of the different concepts of causality in economics and econometrics, particularly after the Cowles Commission work of the 1940s-50s.

[29] The fact that the community formed around the program of intertemporal optimization was still alive during the 1930s is attested by the fact that Frisch, upon receiving Tintner's manuscripts for *Econometrica*, wrote to Hotelling to obtain his opinion on their value (Frisch to Hotelling, September 18th, 1935). I thank Marion Gaspard and Antoine Missemer for giving me access to these letters.

eventually questioned the profit maximizing assumption from the point of view of uncertainty even at the micro-dynamic level (Alchian, 1950: 212).

Frisch's remarks during the Leiden meetings can just as well be seen as a rejection of maximization and their underlying conservation principles in business cycles studies. The whole model of propagation and impulses rendered the whole dynamic analysis very difficult from the perspective of a maximum principle, because the energy of the system was not constant, so that there was no way to find an intertemporal utility function to maximize, in particular because shocks were not independent of the system: they were one of its main driver, and their random character prevented any projection in the future.[30] The change in perspective that he advocated was in fact much more compatible with a local, newtonian approach, which sought to explain "how the past determines the future" or "how a subsequent situation grows out of a previous situation" (Frisch, 2009: 48) as he explained during the Poincaré lectures, in stark contrast with the approach saying that the evolution of aggregate production was guided by the optimization of a global profit function over an "economic horizon"!

The problem was that while intertemporal optimization was an acceptable hypothesis at the individual level, when it was transposed at the aggregate level it immediately became a normative statement instead of an explanatory device.[31] This is apparent in the fact that Frisch came back toward optimization later on to develop models of planification which were openly normative in their aim. But the blurred distinction between the political and scientific aspects of the question was the source of a wider rejection, which could be seen for instance in the historical account of Harold Davis, an early member of the Cowles Commission who summarized the different approaches developed to "account for cycles" in the introduction of his 1941 monograph. After reviewing the theories of Roos and Evans, he underlined the ethical charge of the maximum principle:

> There are many who decry the principle of maximum profits. It seems a sordid and egocentric maxim for mankind to follow. The collectivist theory would replace it by the

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[30] Of course the problem of stochastic optimization became a topic of intense research after the war but from the point of view of the 1930s economists' methodology based on the classical calculus of variations this was an intractable problem.

[31] This is the argument made in an overlooked article by Tönu Puu, who puts this critique in very clear terms (in a Swedish tradition dating back to Myrdal's 1930 critique of the "political element in economics"). Final causes are always at risk of becoming teleological explanations, which explain a phenomena in terms of its function for the subsistence of a biological or social organism: "value judgements become involved in teleological explanations. The stationary continuance of the functioning of certain social institutions is easily regarded as desirable when those institutions are 'explained' in terms of the - possibly vital - functions they are performing in the existing society." (Puu, 1969: 114). Puu defends the position that individual intertemporal optimization is not teleological, because it is based on expectations that may not realize, while an explanation of certain institutions (such as markets) in terms of final causes or the functions they perform in order that a certain goal is obtained "involves obscure analogies between societies and 'organisms' and may serve to conceal conflicts of interests between different groups and individuals under 'social purposes'. … This is why it is much more satisfactory to deal with firms that act in such a way as to obtain maximum profits, than with competitive markets, where prices are merely fixed in such a way that all agents may obtain their optima simultaneously." (1969: 125).

> principle of maximum production and maximum distribution of the things produced. … perhaps, the perversity of human nature has established the profit motive as the dominating principle of all enduring economic systems. (Davis, 1941: 51).

Samuelson in particular seems to have been heavily influenced by the dichotomy inaugurated by Frisch. It structured his thesis, the *Foundations of Economic Analysis*, a book that was as much the product of the 1930s as it shaped the postwar period. In a similar pattern as that followed by Frisch fifteen years before him, Samuelson began his inquiries into mathematical economics with his work on utility and its measurement, before producing one of the high points of macro-dynamic analysis, his famous multiplier-accelerator model (Samuelson, 1939). In the beginning of the 1940s, he became interested in the problem of the stability of equilibrium, and devised the "correspondence principle," that sought to explain how comparative statics results could be derived from stability considerations. The important point is that Samuelson derived this principle precisely to avoid using intertemporal maximization at the aggregate level: he divided his book in two parts, the first dealing with micro-level interactions and based on the maximum principle characteristic of microeconomics, while the second dealt with aggregate systems, where he argued that there could be no optimization, and thus one had to rely on the "correspondence principle" and the stability properties of an equilibrium point to derive useful, operational results:[32]

> when we leave single economic units, the determination of unknowns is found to be unrelated to an extremum position. In even the simplest business cycle theories there is lacking symmetry in the conditions of equilibrium so that there is no possibility of directly reducing the problem to that of a maximum or minimum (Samuelson, 1947: 5)

Samuelson was most certainly influenced in this approach by the work of Frisch and other macrodynamists, and the triumph of their approach to macro-phenomena over the maximizing approach developed by Ramsey, Hotelling, Roos and Evans. His skepticism of the arguments related to intertemporal optimization appeared in a 1943 discussion of Ramsey (1928) where he underlined "the unreal character of the assumption that men maximize utility in terms of an infinite horizon," going as far as to dismiss the usefulness of intertemporal optimization: "It is questionable whether the whole process of saving is illuminated by the attempts to explain it in terms of adjusting consumption streams over time." (Samuelson, 1943: 68). The most interesting part about Samuelson's approach to dynamics is that he was acutely aware of the philosophical issues at hand, perhaps more so than any other economist, and that he developed his views about this problem on at least two occasions.

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[32] On this peculiar approach of Samuelson to macroeconomic dynamics, see Boianovsky (2020) and Assous and Carret (2020a). The former underlines that Samuelson remained faithful to this idea even through his work on optimal growth and capital accumulation, while the latter show how his insistence on stability properties decoupled from optimizing principles shaped the economic modelling approach of Oscar Lange.

The first occasion was a paper written for the book *Cause and Effect*. Samuelson's paper was titled "Some Notions on Causality and Teleology in Economics" and he offered a discussion of how economists modeled causality, describing difference and differential equations, presenting simple models of Cobweb fluctuations, malthusian population growth and predator-prey evolution (Lotka-Volterra models). In the latter case, Samuelson illustrated graphically two possible cases: one where the oscillations were damped due to "diminishing returns and dampening," the other a limit cycle that he titled "conservative oscillations of Volterra-Lotka." This was the occasion for him to discuss the "issue of time reversibility" (Samuelson, 1965: 109) and to show the "important differences for teleology between the conservative and dissipative cases" (*ibid.*: 110). Indeed, as Samuelson explained, the introduction of frictions made the system depart "from the conservation of energy condition of classical mechanics, as the forces of friction lead to the creation of heat" (*ibid.*: 117), which meant that it was necessary to introduce thermodynamic considerations and the inevitable irreversibility of processes that comes with it. This brought him to the question of the "Conservation of energy in economics" (*ibid.*: 121). According to him, there were no conservation principles from which to derive dynamic laws of undamped fluctuations because "the system is not an isolated and stationary one" (Samuelson, 1965: 122). In fact, it was precisely the introduction of stochastic shocks that could reconcile the asymmetry in time of economic processes with the determinism of difference and differential equations: precisely what had been done by Frisch in 1933.[33]

This led him to his discussion of teleology and causality, and to underline that these two approaches corresponded to the two physical approaches represented by Newton for the causal point of view, and Maupertuis, Euler, Lagrange and Hamilton for the teleological point of view.[34] Of course, this distinction also corresponded to the one between conservative and dissipative systems "Hamilton's Principle is applicable to all conservative mechanical systems and not merely to falling bodies. According to it, Nature is a great Economist, or economizer. Nature acts *as if* she has purpose and aims." (*ibid.*: 126). Proving his point was then just a matter of showing how the Lotka-Volterra limit cycle could be derived from a maximum principle; but Samuelson firmly contested the "mystical notion of teleology introduced by Maupertuis and other eighteenth-century philosophers of nature and deism" (*ibid.*: 129), and noted that the simplest frictional terms destroyed the extremal principles, and created a causal system to which "there is no simple teleological counterpart" (*ibid.*).

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[33] "Just as Ehrenfest and other physicists had to add probability to the causal systems of physics to get around the time-reversibility feature of classical mechanics that was so inconsistent with the time asymmetry of the Second Law of Thermodynamics, so we must, in the interests of realism, add stochastic or probability disturbances to our economic and biological causal systems." (Samuelson, 1965: 122)

[34] "Let us watch a ball roll down an inclined plane. Newton describes this causally: at each point and velocity, its rates of change of position and velocity are uniquely determined, as by equation (7). We saw that Galileo could describe this behavior by difference equations (6) alone. But Hamilton (and before him, Fermat, Maupertuis, Euler, and Lagrange) can describe the ball's motion thus: It moves with that special velocity which minimizes a certain integral of action, in comparison with any other behavior-pattern which starts and ends at the same points in the same time!" (*ibid.*: 125)

The paper finished on several examples of causal or teleological models in economics, where Samuelson showed that the line could be very thin between the two approaches, but now he was not so adamant to condemn models such as Ramsey's approach of saving, although they "reek[ed] of teleology" (*ibid.*: 135). He went further along this road, showing that he did not have the same reluctance as earlier when it came to the possibility of finding extremal principles in macroeconomics: "Economists deal with maximum systems, and these behave in definite ways. After you have dealt with such systems for a long time, you may fall into the natural anthropomorphic habit of imputing 'will' and 'volition' to them. This is perhaps only a figure of speech; but often it is a useful one, for the maximum systems do react to certain disturbances as if they were reasoning beings" (*ibid.*: 140). He went on to argue that the aggregation of farmers in a market would behave in the same purposeful way as individual profit maximizers (*ibid.*: 141). The discussion was then linked to welfare economics and the invisible hand, that Samuelson linked to Adam Smith and the idea that it was guided to "a certain kind of optimum - even though each individual is merely pursuing his own selfish well-being." (*ibid.*).[35]

The same theme was at the center of Samuelson's Nobel Prize lecture. The link was drawn between welfare economics, as he developed it in the late 1930s with Abram Bergson, and the maximum principle in a global system: "Along with my close friend, Abram Bergson of Harvard, I have tried to understand what it is that Adam Smith's 'invisible hand' is supposed to be maximizing." (Samuelson, 1972: 260). This came as the "Finale" of a discussion along the same line of the 1965 article on the usefulness of maximum principles for physicists and scientists;[36] although he rejected again the "deistic view of Maupertuis that the laws of nature are the working out of a simple teleological purpose" (*ibid.*: 251), he recognized that there was no escaping the teleological aspect of deriving dynamics from a maximum principle; but while it was an easy task to deride teleological and deistic ideas in physical sciences, this was not the case for economic system where "[i]t must be stressed that in economics, true minimization is what is important because the actors are postulated to have purpose from the beginning." (Samuelson, 1972: 258).

The problem was that the purpose of an individual consumer of entrepreneur is acceptable, while the purpose of an economic system led to the same deistic views as in physical systems: this was clearly something considered by Frisch and other econometricians in the early 1930s, which prompted them to construct macro-dynamics as a way to explain dynamic macroeconomic phenomena as separate from any optimizing principles or consideration of rationality, and ultimately to Samuelson's separation of the *Foundations* in two parts. The fact that he changed

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[35] The idea that the "invisible hand" was a concept invented by Adam Smith to show how divergent interests were coordinated to obtain an optimal equilibrium at the aggregate level was a trope largely created by Samuelson himself in the successive editions of his textbook, as Kennedy (2010) has convincingly shown.

[36] In the same idea as the quotation in note 38 above, Samuelson clearly distinguished the two possible approach to movement: "Newton's falling apple can be described in either of two ways: its acceleration toward the earth is a constant; or its position as a function of time follows that arc which minimizes the integral, taken from its moment of release to the terminal time at which it is observed, of an integrand which can be written as the square of its instantaneous velocity minus a linear function of its position." (Samuelson, 1972: 251).

his mind or entertained a confusion about the problems at hand should not hide the historical development of macro-dynamics as a rejection of aggregate rationality.

In 1965, Samuelson had tried to justify the presence of a maximizer in the economy by evolutionary metaphors, resorting to the idea that it was merely a figure of speech, or referring to current works showing the acceptability of the hypothesis (in particular, Alchian [1950]). This strategy became part of a wider effort to push the acceptance of a representative agent as a device adapted to study macroeconomic intertemporal phenomena, as Hands (2017) has shown. This strategy finally triumphed with the rise of new classical economics and the new neoclassical synthesis, avenging in some way the earlier defeat of Evans and Roos.

But the ambiguous attitude of Samuelson toward the possibility of maximizing something in an aggregate system remained; in 1965, after asserting that the invisible hand would lead the economic system to its optimum against (or with) the individual research of self-interest, Samuelson added that "modern economists know that there is a germ of truth (along with a virus of falsehood) in this theorem" (Samuelson, 1965: 141). In the middle of his Nobel lecture, he stopped to ponder about "nonmaximum problems," arguing that "I must not be too imperialistic in making claims for the applicability of maximum principles in theoretical economics. There are plenty of areas in which they simply do not apply. Take for example my early paper dealing with the interactions of the accelerator and the multiplier." (Samuelson, 1972: 258). Here, again, we find almost forty years later the distinction between micro-dynamics and macro-dynamics introduced by Frisch, when Samuelson distinguished between the dynamics coming from maximizing principles, illustrated by a Ramsey model, from the dynamics of accelerator-multiplier models.[37] In light of recent macroeconomic debates, the issues at hand are still unsettled.

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[37] "Like the light rays of physics that I mentioned earlier, the optimal growth paths of the theories that have grown out of Frank Ramsey's pioneering work of more than forty years ago, themselves provide a rich dynamics. Such a dynamics is quite different from that of say a positivistic accelerator-multiplier analysis." (Samuelson, 1972: 259).

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Ragnar Frisch's Archives (RFA)
Harold Hotelling's Archives

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