LETTER TO THE EDITORS: THE EMPEROR HAS NO CLOTHES: A REPLY TO GINOUX AND JOVANOVIC

BY

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In this letter to the editors of the Journal of the History of Economic Thought, *I present my results on Ragnar Frisch's rocking horse model, published in an earlier issue of the same journal, and detail why the comments by Jean-Marc Ginoux and Franck Jovanovic on my paper have no grounding. I explain the role of initial conditions on the amplitude of cycles and trend in Frisch's solution, and emphasize that my contribution was to show that Frisch built a model where cycles and growth came from the same economic mechanism.*

I. INTRODUCTION

In a letter to the editors of the *Journal of the History of Economic Thought*, Jean-Marc Ginoux and Franck Jovanovic claim that my work on Ragnar Frisch has "no merit in either mathematics or economics" (unless otherwise noted, page citations are from Ginoux and Jovanovic, Letter to the Editors, present issue; p. 505). I will gladly take this opportunity to explain my research on Ragnar Frisch and why it remains unscathed from their hand-waving. It remains unscathed because their narrow criticism of my mathematical solution of Frisch's model has no grounding. But it is also unscathed because they do not address the historiographical points I was making in my paper.

My interest in Frisch came about as I was reading the macrodynamic literature of the 1930s and 1940s, and my initial objective was to see if I could solve the model and follow Frisch's mathematical solution. As this project unfolded, I came to recognize that Frisch left some questions unanswered. Saying this is not an indictment of Frisch's qualities but a recognition that the way in which we solve equations has evolved in the past ninety years. But in solving his model, I also came to realize the extent to which Frisch built a system where cycles and growth (which he called a "secular trend", Frisch

1933, p. 188) were intrinsically linked, as they were the product of the same economic mechanism. Given the subsequent separation of growth and cycles in the literature, I came to see this as one of Frisch's most original contributions, and the main point of my paper, hence the title I chose: "Fluctuations and Growth in Ragnar Frisch's Rocking Horse Model."

This was the point elaborated in section III of my paper (Carret 2022a), where I presented the results I had worked out and coded in the programming language R. The mathematical reasoning on which it is based can be found in my working paper on Frisch's model (Carret 2020, pp. 19–25), and the code is available online (the link was given in my original working paper, Carret 2020, pp. 14, 27).[1] The solution is also published in my book with Michaël Assous (Assous and Carret 2022, pp. 77–83), contrary to what Ginoux and Jovanovic affirm after quoting me (p. 508): "Unfortunately, Michaël Assous and Carret (2022) did not provide all the calculations of the inverse Laplace transform they used. Carret's work is therefore unverifiable." My calculations can also be found in my dissertation (Carret 2022b, pp. 72–80), which I am happy to send to anyone interested.

In the rest of this reply, I explain my interpretation of Frisch's propagation model. I then detail why the "solution" of Frisch's model given by Ginoux and Jovanovic is wrong, and in the last section, I address their claim that I ignored the economic consequences of my choices.

II. FRISCH'S PROPAGATION MODEL

The main difficulty of Frisch's model lies in the fact that it mixes both continuous and discrete time. Among the three equations of his model, some of them rely on delays, the dependence of current values of a variable on its previous values, while others rely on the rate of change of a current variable. This makes Frisch's system one of mixed difference-differential equations with a number of properties that are not found in a simpler model.

One of these properties is that the model's solution can take the form of an infinite sum of exponential functions with real and complex roots. A homogeneous dynamic system usually has a number of independent solutions; this number is related to the number of rates of change in the case of a differential equation and to the number of lags in a difference equation, and is infinite in the case of a mixed difference-differential equation. Each of these independent solutions corresponds to a root, which is one of the solutions of a characteristic equation, a function of the model's parameters. A general solution is a linear combination of all these independent solutions. This solution is general because all the independent solutions can be obtained from it by an appropriate choice of constants by which the solutions are linearly combined (I note that this is what Tenenbaum and Pollard [1985, p. 208–201] explained in the passage quoted by Ginoux and Jovanovic (p. 6); Tenenbaum and Pollard do not talk about the superposition principle).

This solution is also general in the sense that the constants by which independent solutions are combined are arbitrary only to a certain extent: they are in fact determined by initial conditions. If a numerical solver on a computer is used to simulate a dynamical

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[1] The code can be found here: https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5 (accessed April 5, 2023).

system, it will ask for initial conditions, and the simulated solution will be the same as the linear combination of the general system, with the coefficient of the combination being determined by the initial conditions chosen. These two points will be important in the following.

Frisch started his solution by producing a characteristic equation, in fact a system of two equations (Frisch 1933, p. 184), and proceeded to find the first four roots of this system with the help of his assistants (Frisch 1933, pp. 186–187). In computing those roots, Frisch realized that one of them was real and three were complex. What this means is that there would be one purely exponential solution, the "trend" (Frisch 1933, p. 188), and at least three oscillating solutions. Frisch presented the general form of those solutions in equations (16) and (18) of his paper (Frisch 1933, pp. 188–190). But he then proceeded to give different initial conditions to the trend and the cycles, by considering them as if they were differential equations isolated from each other (Frisch 1933, pp. 188, 190, 192). In fact, as these components are the product of the same mathematical system of equations, they depend on the same initial conditions.

Because initial conditions determine the amplitude of the cycles and the trend in the general solution, this led Frisch to give a higher amplitude to his first cycle than for the trend. This prevented him from seeing that the superposition of the cycles and the trend would not produce an apparent oscillation for the parameters he chose originally. This explains why Stefano Zambelli (1992, 2007), when he simulated the model with Frisch's parameters, found a monotone return to equilibrium. This result is a consequence of the fact that, when started from the same initial conditions, the cycles have a very small amplitude and are dominated by the real exponential solution.

To find a solution to Frisch's model, I started reading the subsequent literature on the models built by Frisch, Jan Tinbergen, and Michał Kalecki. I realized that Richard Bellman, a mathematician who is known to economists primarily for the development of dynamic programming, contributed during the 1950s to the theory of difference-differential equations, eventually publishing a textbook with Kenneth Cooke (Bellman and Cooke 1963; see also Bellman and Danskin 1954). What was particularly interesting is that these researchers referred to the papers by Frisch and Harold Holme (1935) and by Kalecki (1935), and one of the contributions of my article published in the *Journal of the History of Economic Thought* is to trace out the way in which this literature developed (Carret 2022a, sec. IV).

Bellman and Cooke made extensive use of the Laplace transform and of contour integration to study difference-differential equations. Ginoux and Jovanovic argue that:

> according to Carret, the Laplace transform and its inverse are "modern mathematical tools that [Frisch] did not know" (2022a, p. 624). This is an astonishing claim to make, given that the Laplace transform was introduced in 1737, that the first use of its modern formulation dates back to 1910, and that in "the 1920s and 1930s it was seen as a topic of front-line research" (Deakin 1992, p. 265). (p. 508)

Reading Deakin's paper, one would learn in fact that:

> The modern Laplace transform is relatively recent. It was first used by Bateman in 1910, explored and codified by Doetsch in the 1920s and was first the subject of a textbook as late as 1937. In the 1920s and 1930s it was seen as a topic of front-line research; the

> applications that call upon it today were then treated by an older technique—the Heaviside operational calculus. (Deakin 1992, p. 265)

I take "front-line" here as referring to the handful of mathematicians who were working on the modern Laplace transform; Deakin's point, made again later, is that engineers used different tools at that time but that they converted very quickly between the end of the 1930s and the 1950s (Deakin 1992, p. 272). Perhaps Frisch had heard about the transform and about contour integration, but he did not use it in the 1930s; and the first textbook expositions were published after he wrote, both for the Laplace transform and for the theory of differential-difference equations.

The main advantage of the Laplace transform was that it allowed me to obtain the same sum of exponential functions that Frisch had worked with but with an analytic solution showing the dependence of the coefficients combining the independent solutions on the same initial conditions (a continuous function over an entire interval in the case of mixed difference-differential equations). The Laplace transform is useful because it transforms linear equations involving derivatives and differences into linear equations that do not involve them (Bellman and Cooke 1963, p. 1). We can solve these transformed equations, and then use the inverse transform to obtain the solution of our initial problem. The inversion algorithm I used followed the contour integration described by Bellman and Cooke and applied by them to other differential-difference equations (1963, pp. 9ff. and ch. 3). In addition, to find the location of the roots on the complex plane, I used the Lambert W function (Corless et al. 1996). This function has the added bonus that it gives an intuition of why there can be one real solution and an infinity of cycles for certain parameters, as is described in Assous and Carret (2022, app. to chs. 2, 3, and 5).[2]

Another advantage of the Laplace transform is that the initial conditions are "built in," as Roy Allen pointed out (Allen 1959, p. 159), although Ginoux and Jovanovic misrepresent what Allen said. They write (p. 508):

> As Roy Allen (1959, pp. 155–156) explained, the Laplace transform is a "trick" of mathematicians. One of the main problems with this trick is that when we use the Laplace transform and its inverse, we automatically introduce new constants (i.e., new initial conditions). Thus, "when the solution is obtained, it has the initial conditions 'built in,'" and "n arbitrary constants, to be 'fitted' or evaluated with great labor from the initial conditions" (Allen 1959, p. 159).

Allen's quote is: "when the solution is obtained [with the Laplace transform], it has the initial conditions 'built in'; there is none of the bother about a solution with n arbitrary constants, to be 'fitted' or evaluated with great labour from the initial conditions" (Allen 1959, p. 159). What Allen argued is that the Laplace transform allows us to obtain a solution where the relationship between initial conditions and amplitude or

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[2] I benefitted greatly from the introductory chapters of Ahlfors (1979) and from the blog http://residuetheorem. com/ (accessed April 7, 2023) in my understanding of Cauchy's theorem and complex analysis. My code implementing the Lambert W function in R is based on the c++ implementation by István Mező available here: https://github.com/IstvanMezo/LambertW-function (accessed April 7, 2023). My implementation is at the beginning of my code, available here: https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5 (accessed April 7, 2023).

phase are directly obtained from the transform and its inverse. On the other hand, when we solve a simple differential equation without using the Laplace transform, a general solution of the form $x(t) = c_1 e^{\lambda_1 t} + c_2 e^{\lambda_2 t}$ has two arbitrary constants c_1 and c_2 that have to be evaluated with initial conditions, for instance at $x(0)$ and $\dot{x}(0)$. Allen pointed out that the Laplace transform would give a solution in terms of x and its first derivatives evaluated at $t = 0$, "a particular, though very common, case" (Allen 1959, p. 159). This was actually the case considered by Frisch, who gave initial conditions on each cycle so that "it shall be zero at origin and with velocity $= \frac{1}{2}$" (Frisch 1933, p. 192). In the case of a differential-difference equation, it also makes it clear that the initial condition we need is actually a continuous function over an initial interval.

Yet another useful consequence of the Laplace transform is that it directly gives a particular solution for the model when the system is not homogeneous, as was the case in Frisch's model. Frisch used throughout his paper a constant c equal to 0.165 (Frisch 1933, p. 188), the same I used throughout my work. I did not state explicitly that I used the same numerical value for this constant, or the initial conditions that I used for the published figures, although they were written in my code; the initial development was to maintain the model at 20% of its equilibrium level and then releasing it.

III. FRISCH'S "PROVISO"

Ginoux and Jovanovic argue that Frisch (1933, p. 191) gave a "proviso" that "the sum of the coefficients k_j, which are the weight of each cycle, must be equal to unity" (p. 2). My interpretation of Frisch's paragraph, which I detailed in my contribution to a symposium in *History of Economic Ideas* (Carret 2022c), is that Frisch was merely pointing out that a particular solution of an non-homogeneous system should be counted only once in the general solution. A homogeneous system is one where the dynamic equation is equal to zero. A non-homogeneous system is one where the dynamic system is equal to a constant or a forcing function. The homogeneous system is solved by each of the individual solutions in (18) given by Frisch, and also by any linear combination of them. But Frisch said, rightly, that in the case of the non-homogeneous system,

> if $c \neq 0$ the constant terms a_*, b_* and c_* must be added to (18) in order to get a correct solution. If these constant terms are added, we get functions that satisfy the dynamic system, and that have the property that any linear combination of them (with constant coefficients) satisfy the dynamic system provided only that the sum of the coefficients by which they are linearly combined is equal to unity. This proviso is necessary because any sets of functions that shall satisfy the dynamic system must have the uniquely determined constants a_*, b_* and c_*. (Frisch 1933, p. 191)

The terms a_*, b_* and c_* are the particular solution of his system. Frisch's point, particularly apparent in the last sentence, is that when we add two functions that have both one solution of the homogeneous system and one particular solution, if we want to count the particular solution only once, the coefficients by which we add the two functions must sum to 1. I have not "ignored a well-known theorem" (p. 5) but tried

to make sense of what Frisch was saying with a simple example (Carret 2022c). This is in any event a hypothetical case that does not give license to add cycles together with ad hoc "weights"; the "proviso" is just an illustration of a well-known fact, and any solution having only one particular solution will satisfy Frisch's proviso.

However, Ginoux and Jovanovic take this paragraph as a license to add cycles with arbitrary numbers in front of them. With their latest set of "weights" (Ginoux and Jovanovic 2022), they multiply the amplitude of the first cycle by 1, the amplitude of the second by 30, and the amplitude of the third by -30. But an elementary fact of dynamic systems is that the amplitude (and the phase in the case of cycles) is given by initial conditions of the system, as Frisch himself knew (Frisch 1933, p. 183). When they multiply Frisch's cyclical components by a factor of 30, Ginoux and Jovanovic are multiplying the amplitudes of these cycles without relating them to any initial conditions. There are many sets of three numbers that sum to 1; the key is not to produce those numbers out of thin air but to relate them to initial conditions, which govern the amplitude of the cycles. Frisch's "proviso" was not a license to multiply the amplitude of individual cycles but the mere recognition that one should not write the particular solution more than once in a general solution of the dynamic system.

I also note that there is no "closure relation," and Ginoux and Jovanovic transform what Lionello Punzo said when they refer to his comments published in *History of Economic Ideas* (Punzo 2022). Ginoux and Jovanovic cite his comment as supporting their demonstration (pp. 2, 8), but Punzo is talking about the parameters of the system and not "weights" when he speaks of a "closure set":

> Under *generic conditions on the parameter space*, it is hard to expect that two harmonics be bound up in such a way as to yield a monotonic behavior. Generic conditions mean that, *for open sets in the parameter space*, a given proposition is true. Hence, if Zambelli is right, it is because Frisch was working on the closure of such a set, where anything could happen. Thus, in order to prove that the rocking horse *does not rock, with n = 3*, Zambelli should have shown that an open set in R^n be *empty*. He did not do it, it seems to me. He only showed that Frisch was picking up a special harmonics (*i.e.* in a closure set). (Punzo 2022, pp. 173–174; emphasis by Punzo)

What Punzo argued is that Frisch chose parameters on a special area in the parameter space, a choice that made his model not fluctuate. Reading Punzo, I find in fact that he agrees with Zambelli that Frisch's model does not fluctuate for his original parameters but argues against Zambelli that it was wrong to say that this was a general case. What Zambelli has not proven is that the model could never fluctuate, and in fact, with my solution using the Laplace transform, I was able to find other parameters for which Frisch's propagation model oscillates.

The "solution" given by Ginoux and Jovanovic rests on one paragraph of Frisch that led them to introduce new constants whose origin is unknown, and unrelated to any initial conditions. This interpretation, I argued here, is not correct. I pointed out this problem to the authors in my comments published in *History of Economic Ideas* (Carret 2022c), including the fact that the sum of these constants was not even equal to 1 as they claimed (Ginoux and Jovanovic 2023, p. 19). The authors recently published a correction of one of their papers with new constants without acknowledging our debate and my comments (Ginoux and Jovanovic 2022).

IV. AN ECONOMIC ARGUMENT?

Ginoux and Jovanovic try to elaborate an economic justification for the existence of "weights" in front of the cycles at the beginning of their comments. They start by arguing that the "proviso" is necessary for economic reasons: "The closure relation implies that Frisch considered that these different cycles (such as Kitchin and Juglar cycles), which do not have the same origin according to the economic theories, do not impact the economic activity with the same amplitude" (p. 504). But in fact all of Frisch's cycles, as well as the trend, come from the same economic mechanism. Ginoux and Jovanovic go on to say that "Carret, however, states that these different cycles impact the economic activity with the same amplitude" (p. 504). I did not claim that, but in fact the opposite: "it [the primary cycle] has a relatively high magnitude (compared with the trend and other cyclical components)" (Carret 2022a, p. 632). Figure 2 of my paper shows clearly that the first cycle has a bigger amplitude than the second. And, again, the amplitude of each cycle is determined by the initial conditions and not by "weights" that would be arbitrarily chosen. When Ginoux and Jovanovic chose "weights" of 1, 30, and -30, they did not change the amplitude of the 8.5-year cycle but multiplied the amplitude of the 3.5-year cycle by 30 and of the smaller, 2.2-year cycle by -30. They provide no economic explanation for this.

The second "fundamental problem" is: "What business cycle did Carret try to replicate with his model?" (p. 3). I was not trying to reproduce a business cycle but to show how we could obtain apparent fluctuations along a trend line in Frisch's model, and I changed Frisch's parameters because his model does not fluctuate along the trend for his original parameters. Ginoux and Jovanovic also misrepresent what I argued (p. 3). I indeed obtained a primary cycle that was shorter than Frisch's, but when Ginoux and Jovanovic say, "In Carret's view, such differences do not represent an issue: 'do[es] not think that it necessarily is [a problem]' (2022a, p. 633)" (p. 3), they are mixing different paragraphs of my paper. In the complete paragraph I explain the "caveat" that the model's damping is much slower with my parameters:

> There is, however, one caveat, compared with Frisch's original article: in order to obtain apparent cycles at the aggregate level, we had to decrease the damping of the system. In fact, the return to equilibrium is much longer than in Frisch's original article. Is this a problem? We do not think that it necessarily is so: the propagation mechanism was only one part of the whole model, the second part being the impulse mechanism, which was used to explain the persistence of otherwise damped cycles. The fact that the propagation mechanism itself can explain a larger part of the persistence of cycles appears to be in line with Frisch's original objective of explaining the phenomena of sustained fluctuations in the business cycle. It is also true that we merely presented some examples of fluctuations, and that others could be found with different combinations of parameters, maybe quicker to return to equilibrium. (Carret 2022a, p. 633)

Let me point out again that I was interested in showing an example of the superposition of trend and cycles, and not in reproducing a business cycle. I also invite the reader to try other parameters by visiting the interactive application I built.[3]

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[3] The application is available here: https://cbheem.shinyapps.io/Frisch/ (accessed April 7, 2023). I welcome any feedback or questions on this application and can share the R code, which is essentially the same as the one I used to draw the figures of my paper, with the addition of the interactive parts.

Was my work without merit? Beyond my historical contributions, I was able to point to several properties of Frisch's model through my solution using the Laplace transform. I checked my results by solving the model using two more additional approaches, by discretizing the model, and by using a differential equation solver in R. The discretized solution can be found in my working paper (Carret 2020, pp. 25–26), in the book with Assous (Assous and Carret 2022, pp. 83–84), and in the code I wrote in R.[4]

COMPETING INTERESTS

The author declares no competing interests exist.

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4 The R code is available here: https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5 (accessed April 7, 2023). The solver I used to check my result is available in the package "deSolve," which is described in a tutorial available here: http://desolve.r-forge.r-project.org/slides/tutorial.pdf (accessed April 7, 2023), and in the standard documentation.

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