

Carret, Vincent

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THE EMPEROR HAS NO CLOTHES:
A REPLY TO GINOUX AND JOVANOVIĆ

VINCENT CARRET

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The Emperor Has No Clothes: A Reply to Ginoux and Jovanovic

Vincent Carret¹

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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 is “useless” and has “no merit” (p.8). 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 very narrow criticism of my mathematical solution of Frisch’s model has no grounding. But it is also unscathed because they do not even 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-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

¹ Contact: vincent.carret@duke.edu. Duke University, Center for the History of Political Economy. I thank Michaël Assous, Pedro Duarte and Kevin Hoover for their comments on an earlier draft. In the following, the page numbers I use to cite Ginoux and Jovanovic as “(p.#)” refer to the note they uploaded on SSRN (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4275166).

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: 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: 19-25), and the code is available online (the link was given in my original working paper [Carret, 2020: 14 and 27]).² The solution is also published in my book with Michaël Assous (Assous and Carret, 2022: 77-83), contrary to what Ginoux and Jovanovic affirm after quoting me (p.10): “Unfortunately, Assous and Carret (2022) did not provide 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: 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.

² The R code I used can be found here:
<https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5>.

I. 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 one does not find 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, 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 Tenebaum and Pollard [1985: 208-210] explained in the passage quoted by Ginoux and Jovanovic (p.6-7); they do not talk about the superposition principle).

We can illustrate this with a simple example which I will use in the following note: consider the differential equation $\ddot{x} - x = 0$ (where $\dot{x} = \frac{dx}{dt}$). Pose a solution of the form $x(t) = e^{\lambda t}$. To find the roots λ , we derive the characteristic equation of the system by

replacing x in the differential equation; we obtain $\lambda^2 = 1$, which has for solutions $\lambda_1 = 1$, $\lambda_2 = -1$. A general solution will have the form $x(t) = c_1 e^t + c_2 e^{-t}$. This solution is also general in the sense that the arbitrary constants c_1 and c_2 , by which independent solutions are combined, are only arbitrary 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 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: 184), and proceeded to find the first four roots of this system with the help of his assistants (Frisch, 1933: 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: 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: 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: 188, 190, 192). But 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 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, Tinbergen and 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 models of Frisch and Holme (1935) and 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: Section 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

Moreover, according to Carret, the Laplace transform and its inverse are 'modern mathematical tools that [Frisch] did not know' (2022b, 3). 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.10).

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: 265)

I take “front-line” here as referring to the handful of mathematicians that 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: 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: 1). When we apply the transform, dynamic equations with real variables, living in the time domain, are transformed into complex variable equations living in what may be called the frequency domain. The inverse operation (from the frequency to the time domain) can be done by several means but I mostly followed the use of contour integration described by Bellman and Cooke and applied by them to other differential-difference equations (1963: 9ff., Chapter 3). My computations are detailed in my working papers (Carret, 2020: 19-25),

in my book with Michaël Assous (Assous and Carret, 2022: 77-83) and in my dissertation (Carret, 2022b: 72-80). In addition, to find the location of the roots on the complex plane, I made use of 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: appendices to Chapters 2, 3 and 5).³

The operation of shifting back and forth between time and frequency domains can be illustrated like so: imagine a circle, and a bead moving on that circle. You can plot against time one of the coordinates of that bead from the center of the circle, and obtain a sinusoidal curve. Or you can describe this bead according to the time it takes to go a full circle (the period); the inverse of the period is the frequency, which can be multiplied by 2π and plotted on a vertical axis. Now imagine that instead of moving in a perfect circle, the bead goes toward the center as it is turning. The temporal coordinate will show a certain amount of damping as the bead moves toward an equilibrium at the center, where it won't move. This damping can be represented by a number, which can be plotted on an horizontal axis in the same plane as the frequency. In that way, we obtain a complex plane, where the x (horizontal) and y (vertical) coordinates of a point on this plane form a complex number, indicating the damping and frequency describing the movement of a bead around a circle or a spiral. These numbers are the roots of the

³ I benefited greatly from the introductory chapters of Ahlfors (1979) and from the blog <http://residuetheorem.com/> 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>. My implementation is at the beginning of my code, available here: <https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5>.

characteristic equations of dynamic systems mentioned earlier, and an important part of solving the system is to find the location of these roots.⁴

Another advantage of the Laplace transform is that the initial conditions are “built in”, as Allen pointed out (Allen, 1959: 159), although Ginoux and Jovanovic misrepresent what Allen said; they write (p.10-11):

As Allen (1959, 155-6) 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, 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: 159). What Allen argued is that the Laplace transform allows us to obtain a solution where the relationship between initial conditions and amplitude or 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 which have to be evaluated with initial conditions, for instance at $x(0)$ and $\dot{x}(0)$. Going back to our earlier example, we had obtained the general solution $x(t) = c_1 e^t + c_2 e^{-t}$, which derivative is

⁴ There are a number of connections between this analysis and the fields of acoustic and sound; a connection that was made by James and Belz (1938: 331) in their work about mixed difference-differential equations published in *Econometrica* following Frisch’s research.

$\dot{x}(t) = c_1 e^t - c_2 e^{-t}$. If we pose $x(0) = x_0$ and $\dot{x}(0) = \dot{x}_0$, we obtain $c_1 + c_2 = x_0$ and $c_1 - c_2 = \dot{x}_0$, or $c_1 = \frac{x_0 + \dot{x}_0}{2}$ and $c_2 = \frac{x_0 - \dot{x}_0}{2}$.

On the other hand, applying the Laplace transform to the same differential equation, $\ddot{x} - x = 0$, we obtain the transformed function $X(s) = \frac{\dot{x}(0) + sx(0)}{s^2 - 1}$, where the initial conditions come from the process of integrating the equation. Using the contour integration described by Bellman and Cooke and which I applied to Frisch's model reduced to one equation, we obtain $x(t) = \frac{1}{2\pi i} \int_{(b)} \frac{\dot{x}(0) + sx(0)}{s^2 - 1} e^{st} ds$ where (b) is a contour that we shift to the left. The contour will be equal to the sum of residues multiplied by $2\pi i$ (this follows from the residue theorem) and the residues can be evaluated by replacing s with the zeros of $s^2 - 1$ (the characteristic equation) in the expression $\frac{\dot{x}(0) + sx(0)}{2s} e^{st}$. The solution we obtain is the sum of those residues, or $x(t) = \frac{x_0 + \dot{x}_0}{2} e^t + \frac{x_0 - \dot{x}_0}{2} e^{-t}$, the same solution we obtained above by "fitting" the arbitrary constants c_1 and c_2 with initial conditions. The caveat underlined by Allen is that the solution of the differential equation obtained with the Laplace transform is subject to initial conditions on x and its first derivative evaluated at $t = 0$; in the other case, we could have given two initial conditions on $x(0)$ and $x(1)$ for instance; but Allen (1959: 159) argues that this is a "very common" case, and, in fact, it is the case implied 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: 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: 188), the same I used throughout my work. I did not state explicitly that I used the same numerical value for this constant, nor the initial conditions that I used for the published figures; in this case the initial development was to maintain the model at 20% of its equilibrium level and then releasing it.

II. Frisch's "Proviso"

Ginoux and Jovanovic argue that Frisch (1933: 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.4). 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 a nonhomogeneous system should only be counted once in the general solution. A homogeneous system is one where the dynamic equation is equal to zero. A nonhomogeneous system, 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 nonhomogeneous 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: 191)

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 one. I have not "ignored a well-known theorem" (p.6) 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, 2022b), 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) are given by initial conditions of the system, as Frisch himself was aware (Frisch, 1933: 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. The sets of three numbers 1, 30 and -30; 29, 29 and -57; and 5, 5000

and -5004 all sum to one, but 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.

Ginoux and Jovanovic also produce a "demonstration" of the necessity to have a sum of "weights" equal to one (p.5). This demonstration is simply wrong, as its first step is to take the initial condition given by Frisch for one cycle (Frisch, 1933: 190), and to transform it into an initial condition for the sum of all the cycles (eq. (2) p.5). The first step in their reasoning is wrong, and the last step is also wrong, when they say that "according to Frisch (1933: 190), we have: $A_j = \frac{1}{2\alpha_j}$ " (p.5). This is not a condition given by Frisch but the result of his analysis for one cycle, and not for all j . Indeed, Frisch (1933: 190) starts from the two initial conditions $x_1(0) = 0$ and $\dot{x}_1(0) = \frac{1}{2}$ (for the first cycle, and not the sum of all the cycles). Now according to (18) in Frisch (1933: 190):

$$x_1(t) = A_1 e^{-\beta_1 t} \sin(\phi_1 + \alpha_1 t) \quad \text{so that}$$

$$\dot{x}_1(t) = A_1(-\beta_1) e^{-\beta_1 t} \sin(\phi_1 + \alpha_1 t) + A_1 \alpha_1 e^{-\beta_1 t} \cos(\phi_1 + \alpha_1 t).$$

Replacing t by 0, we obtain $x_1(0) = A_1 \sin(\phi_1) = 0$ so that $\phi_1 = 0$, as Frisch wrote. We also obtain $\dot{x}_1(0) = A_1(-\beta_1) \sin(0) + A_1 \alpha_1 \cos(0) = \frac{1}{2}$ so that $A_1 \alpha_1 = \frac{1}{2}$. This does not imply anything about a sum of "weights" for the cycles, which is a concept invented by Ginoux and Jovanovic. The idea that "Frisch normalized the weight of each cycle" (p.4)

is also not grounded in anything related to differential equations, beyond the recognition that there is only one particular solution in a nonhomogeneous system.

I also note that there is no “closure relation” and Ginoux and Jovanovic transform what Punzo said when they refer to his comments published in *History of Economic Ideas* (Punzo, 2022: 173-174). Ginoux and Jovanovic cite his comment as supporting their demonstration (p.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: 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 they interpreted in a way 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 one as they claimed (Ginoux and Jovanovic, 2022a: 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, 2022b).

III. 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:

In ignoring Frisch’s closure relation, Carret and Zambelli used $k_1 = k_2 = k_3 = \dots = 1$. They did not mention this clearly anywhere, but we were able to establish it from their figures. In so doing, they fail to weigh the impact of different economic cycles. Does Carret’s hypothesis make sense from an economic viewpoint? Does it make sense to state that different cycles (such as Kitchin and Juglar cycles) impact the activity with the same amplitude? (p.4).

There are no “weights” k_j to consider, and no “closure relation”; in fact all of Frisch’s cycles, as well as the trend, come from the same economic mechanism. I also did not claim that cycles influence the economy with the same amplitude, but in fact the opposite: “it [the primary cycle] has a relatively high magnitude (compared with the trend and other cyclical components)” (Carret, 2022a: 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 in fact 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 economic point made by Ginoux and Jovanovic is that I did not reproduce exactly the same cycles found in Frisch's paper, because I changed the parameters of his model (p.8). My aim was not 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.9). 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, 12)." (p.9), 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: 633)

From their biased reading, Ginoux and Jovanovic conclude that "Carret's work is useless for understanding Frisch's because he worked on a solution of Frisch's model

that is different from the original one and which does not make it possible to reproduce the observed cycles that Frisch sought to reproduce” (p.5). 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.⁵

* * *

Was my work useless? It certainly wasn't for me, and I enjoyed it very much. Was it without merit? Beyond the historical contributions of my work, 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: 25-26), in the book with Assous (Assous and Carret, 2022: 83-84) and in the code I wrote in R.⁶ Anyone willing to discuss this with me is welcome to reach out and ask for precisions on my demonstrations and code.

My knowledge of rhetoric is limited, but I've recently learned that making a point against an argument that was never maintained is the fallacy of *ignoratio elenchi*. I am ignorant of latin, but I know that “the pot calling the kettle black” is the rhetorical device consisting in projecting one's deficiencies onto someone else. In the future, I hope that

⁵ The application is available here: <https://cbheem.shinyapps.io/Frisch/>. I welcome any feedback or questions on this application and can share the R code which is essentially the same as the one which I used to draw the figures of my paper, with the addition of the interactive parts.

⁶ The R code is available here: <https://gist.github.com/vcarret/2c0832e815dcf1f7918fbfd140d57ba5>. 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> and in the standard documentation.

Ginoux and Jovanovic can adopt a more civilized tone when commenting on the work of others, instead of bullying the readers into thinking that the arguments they thread are made of something other than hot air.

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