Comment

Christian Groth
UNIVERSITY OF COPENHAGEN

Until recently, and perhaps still, investigators of dynamic economic problems have been inclined to reject models not containing asymptotically stable steady states or limit cycles. However, the mathematical theory of non-linear dynamics and 'chaotic attractors' has opened our eyes to the possibility of irregular, fluctuating and *yet bounded* motion arising from a deterministic dynamic system. This gives new ways of interpreting the seemingly random character of economic time-series. It also throws new light (or 'darkness' if you want) on the theory of expectations and forecasting because small errors or uncertainties in the initial conditions can give rise to evolutions which are completely different after some time. If a dynamic system is chaotic – i.e. contains bounded trajectories that are neither asymptotically periodic nor asymptotically stationary – then knowledge of the state of the system for an arbitrarily long time does not in practice enable one to predict the subsequent evolution past a limited time range.

The recent literature has shown several examples of chaotic motion generated by discrete-time dynamics based on economic first principles. In continuous-time systems, which in order to generate chaos must be of dimension three at least, such examples seem, by now, rare (if we disregard coupled oscillators and forced oscillators).

The paper by Lorenz has drawn our attention to an interesting application by the physicists Arneodo, Coullet and Tresser,[1] of a theorem by Shil'nikov on existence of a 'strange attractor' (an attractor which is neither a sink nor a limit cycle) in a third-order differential equation. Lorenz provides a model which is, as far as I know, the first attempt to utilise the Arneodo *et al.* conditions in economics. Although I have my reservations as to the particular model proposed by Lorenz, I find this attempt very useful as an inspiration for further research.

Now to the specific model offered by Lorenz as an economic example of the Shil'nikov–Arneodo *et al.* scenario. The model is claimed to constitute a minor modification of Metzler's (1941) inventory cycle model with extrapolative expectations. There is no doubt that it is modification, though it is possible to question whether it is minor. The first step in the modification consists in a transformation to continuous time of the original discrete-time version by Metzler, a transformation which corresponds to a model in Gandolfo (1983). The second step in the modification is to change the linear savings and constant fixed investment hypotheses proposed by Gandolfo.

The model with which Lorenz ends up can be written:

$$\dot{Y} = \alpha(B^d - B) \qquad \alpha > 0 \tag{1}$$

$$B^d = kY^e \qquad k > 0 \tag{2}$$

$$Y^e = Y + \theta\dot{Y} + \frac{\theta^2}{2}\ddot{Y} \qquad \theta > 0 \tag{3}$$

$$\dot{B} = Y - (C + I) \tag{4}$$

$$C = C(Y) \tag{5}$$

$$I = I(Y) \tag{6}$$

with Y, B^d, B, Y^e, C and I, i.e. output, desired inventories θ time units ahead, actual inventories, expected output θ time units ahead, consumption, and investment in fixed capital, as the endogenous variables. The expectations in (3) are extrapolative in the sense of following a second-order Taylor approximation.

In order to make this example fulfil the Arneodo *et al.* conditions for a strange attractor Lorenz assumes:

(i) $\theta \approx \sqrt{2}$;
(ii) $\alpha > \frac{1}{k\theta}$; and
(iii) the function $f_k(Y) \equiv \frac{2}{k\theta^2}\,[Y - C(Y) - I(Y)]$ has a hump, cf. Figure 8.2.

Presupposing the marginal propensity to consume to be non-increasing, it is a weakness of this example that it depends crucially on investment in fixed capital being a strictly convex function of the output level. Making investment a function of the expected rate of change of output or of the gap between actual and desired fixed

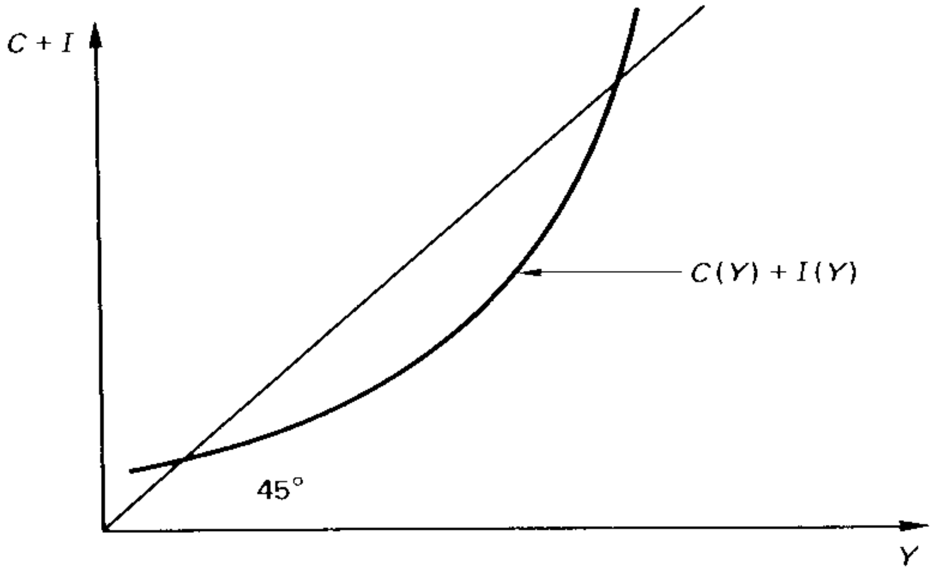

Figure 8.3

capital, as it should rather be, destroys the example.

Furthermore, the model seems susceptible to the following criticism

(a) output should adjust not only to a discrepancy between actu and desired inventories, but also to a gap between curre production and sales (equation 1));
(b) desired inventories should be related to expected sales rathe than expected activity level (equation 2).

One might therefore ask whether remedying these two poin would help making the Shil'nikov – Arneodo conditions more plau ible. At least the following model which, albeit still *ad hoc*, is more i line both with the old Metzlerian ideas and with empirical researc on inventories, leads to the tentative answer: No!

$$J^d = \lambda(B^d - B) \qquad \lambda > 0 \qquad ($$

$$\dot{Y}_c = \gamma(J^d + C - Y_c) \qquad \gamma > 0 \qquad ($$

$$B^d = kC^e \qquad k > 0 \qquad ($$

$$C^e = C + \theta\dot{C} + \frac{\theta^2}{2}\ddot{C} \qquad \theta > 0 \qquad (1$$

$$\dot{B} = Y_c - C \tag{11}$$

$$C = C(Y) \tag{12}$$

$$Y = Y_c + I_0 \qquad\qquad I_0 > 0 \tag{13}$$

where J^d is desired inventory investment, Y_c is production of consumption goods, and C^e is expected sales of consumption goods θ time units ahead. We assume investment in fixed capital, I_0, constant, because entering a proper investment function would result in a *fourth* order dynamical system.[2]

Now, this model gives a third-order differential equation in Y containing the multiplicative terms $\dot{Y}\ddot{Y}$ and $\dot{Y}^2$ (if $C'''(Y) \neq 0$, also $\dot{Y}^3$), i.e. the proposition of Arneodo et al. cannot be used. Replacing extrapolative expectations by adaptive expectations still leaves us with the term $\dot{Y}^2$. To get rid of these multiplicative terms we need $C''(Y) \equiv 0$, but then we just have a linear (and instable) system.

What all this amounts to is first that it appears hard to base the needed hill-shaped function $f_\mu(x)$ on excess supply as a function of the activity level. But other candidates for a hill-shaped function, usable in other theoretical contexts, are possible.[3] Second, it seems difficult to maintain that 'traditional models in business-cycle theory can exhibit chaotic motion when economically irrelevant modifications to the structural relations are undertaken' as claimed in Lorenz's introduction. Indeed, one might call into question this project, because of the *ad hoc* character of traditional business-cycle models. They seem more in need of economically *relevant* modifications.

This leads to my final remark. I see no reason to favour the nonlinear business-cycle approach as compared with the linear-with-exogenous-stochastic-shocks-approach on the grounds that 'from a theoretical point of view it is rather unsatisfactory when non-economic forces are finally responsible for the persistence of economic cycles', as Lorenz says towards the end of his paper. Random disturbances to the economy from the environment (weather, nature, biology, science, politics etc.) seem inevitable. And – to speak in the language of Frisch – even if the *impulses* are external shocks, the *propagation* mechanism is of an economic character. Which of the approaches is the better, remains, I suppose, a question of their ability to explain the empirical regularities we observe.

Notes

1. For references, see Lorenz's paper.
2. This is so, even if the second derivative in the Taylor expansion (10) i ignored, or if we use adaptive expectations instead of extrapolativ expectations.
3. The evolution and diffusion of new technology is an example, cf. Good win's paper (Chapter 16).