Notes and
Communications

End of the Debate?

I must apologize for the delay in my answer to Professor Dopfer's article in this journal's March issue [Dopfer 1991]. I think we may state that this answer completes the debate, with Dopfer as the victor.

This answer deals with four issues, under the headings linearity, forecasts, policy, and chaos theory. The following remarks complete, I think, the exchange of ideas.

What I had written about the use of linear relations was indicated as useful for the estimation of relationships over small intervals only [Tinbergen 1991, p. 34]. I fully agree with Professor Dopfer that in a number of problems nonlinearity is essential. I show this in my discussion of the shape of utility functions and production functions. I discussed two nonlinear utility functions, the logarithmic and the parabolic function [Tinbergen 1990, pp. 4–6]. In both cases the assumed non-linearity is essential indeed: in both cases it introduces the diminishing marginal utility and in the second case the phenomenon of satiation.

In forecasts I am as skeptical as Dopfer and can agree with his mentioning of three to six months as a period over which forecasts are reliable. I add, however, that as forecasts over long periods are needed and no other information is available, I prefer to use concrete (but unreliable) information over doing nothing. Also I think that forecasting is not the most important function of economic science. The most important function rather is to search for the most desirable policy, including the choice of institutions.

The only information we have on long-term movement sometimes is past movement; we then have to use this, even though we know that "history does not repeat itself." One way of obtaining better information may be the assumption of rational expectations. This assumes knowledge of the mechanism that determines the phenomenon studied. An example is knowledge of demand or supply, or both functions.

With regard to the subject of economic policy my impression is that Professor Dopfer and I completely agree. This applies in particular to the portion of my 1972 publication on the optimum regime, which he quotes on p. 66 of his article on the complexity of economic phenomena in this journal [Dopfer 1991].

Finally, some words on chaos theory. Here I simply have to admit that I never studied or applied it and that I increasingly have become aware of its potentialities. It is a relatively new area, although Ragnar Frisch came very close to it some decades ago. Dopfer is well informed on it and I am not. It is a subject of the future.

The research processes we discussed in our last few articles must and will go on. My own contribution was my last in this area; lately I have concentrated on other topics: the measurement of some concepts that had not been measured so far. Examples are the velocity of integration [Tinbergen 1991] and the length of the development process [Tinbergen, 1990a].

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References

Dopfer, K. 1991. The Complexity of Economic Phenomena: Reply to Tinbergen and Beyond, *Journal of Economic Issues* 25 (March): 39–76.

Tinbergen, J. 1990. *World Security and Equity*. Brookfield, Vt.: Edward Elgar.

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What Is Saving, and
Who Gets the Credit (Blame)?

In a recent, provocative contribution to this journal, Walter C. Neale examined the ideological characteristics of the *attribution* of savings [Neale 1991]. His discussion included two different definitions of