This explosion of the literature in the methodology of economics would alone have warranted a second edition, in order to take account of new developments in the field. Moreover, my central message has sometimes been misunderstood, no doubt because it was badly expressed, tempting me to restate my argument. In addition, some of the case studies in the second half of the book were too flimsy and others needed updating. Finally, new developments in macroeconomics, general equilibrium theory, and international trade theory encouraged me to prepare a new edition.

At first, I had ambitions to double the length of the original book by new chapters on post-Keynesian economics, experimental economics, game theory, and the crisis in econometrics, resolving the clash between Bayesian and classical theories of inference. But in the final analysis, intellectual laziness and a disinclination to rush in where even angels fear to tread have produced a second edition which is only marginally longer than and different from the first. I have amplified my discussion of general equilibrium theory, the Heckscher–Ohlin theory of international trade, monetarism, and the new classical macroeconomics, and have added a new section on the rationality postulate as the "hard core" of mainstream economics. In the main, however, the new edition is substantially the same book as the old. The ambitious additions I had hoped to insert I leave to another book.

Let me now try to restate the central message of the book by way of a comparison between my own account of the methodology of economics and that of Bruce Caldwell's *Beyond Positivism.*[2] Our two books are in striking agreement on many of the substantive issues in economic methodology: *methodology* is not just a fancy name for "methods of investigation" but a study of the relationship between theoretical concepts and warranted conclusions about the real world; in particular, methodology is that branch of economics where we examine the ways in which economists justify their theories and the reasons they offer for preferring one theory over another; methodology is both a descriptive discipline – "this *is* what most economists do" – and a prescriptive one – "this is what economists *should* do to advance economics"; finally, methodology does not provide a mechanical algorithm either for constructing or for validating theories and as such is more like an art than a science. We also agree that economic theories must sooner or later be confronted with empirical evidence as the final arbiter of truth but that empirical testing is so difficult and ambiguous that one cannot hope to find many examples of economic theories being decisively knocked down by repeated refutations (but there are nevertheless striking examples of precisely that phenomenon, as we shall see). It is vain to seek an empirical counterpart for

[2] The pages that follow borrow heavily from my "Comment" in Wiles and Routh (1984, pp. 30–6).