environmentalism was to influence the national parks movement whose South African expression is recounted in an excellent paper by Jane Carruthers. In detailing the history of three national parks, she reveals the changing and competing pressures of preservation and use, whether by game hunting, tourism or local community use. At the end of her paper, she reflects on the function of national parks in the post-apartheid era and raises the question of whether international initiatives to create biosphere reserves and world heritage areas—in which I believe more than a whiff of Brownian environmental evangelism still wafts—might create a new kind of imperialism. This, to me, directly engages the topic of the conference. There are another eight papers which I have no space to mention here, except for MacKenzie's paper in the fifth part which categorises the historiography of writing to date on the imperial environment.

How well then does *Ecology and Empire* meet reasonable expectations of such a collection and what is its place in environmental history? Three points should be made. First, there are enough good papers and new material to justify the publication: I have already mentioned the overviews by Flannery and Payne and the papers by Carruthers, Grove, Powell and Robin. Two scholarly papers on South African pastoral production by William Beinart and Shaun Milton should also be noted here. Second, the collection, prepared mostly by historians, reflects what is probably the majority trend to write what I call 'environmental history without the environment'. That is to say it focuses on what happened in the human communities without giving equal weight to what actually happened in detail to the non-human communities. Third, the theme of empire and the place of settler societies is only engaged directly by Powell and Carruthers which indicates to me that it is likely to be a fruitful field for further work. Overall, I found *Ecology and Empire* to be a useful addition in an emerging field.

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On the Reliability of Economic Models

Daniel Little (Ed.)

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The essays in this wide-ranging volume are written by philosophers, with commentary by economists. The purpose, Little tells us, 'is very specific: to stimulate a discussion of the epistemology and methodology of economics that works at the level of detail of existing

"best practice" in economics today' (p. 2). It is a laudable aim, for the majority of economists probably do not know their epistemology from their episternum. Fledgling economists are routinely warned to suppress any interest in methodology as a dangerous distraction when learning to fly, yet epistemological curiosity, which this book will feed, may well emerge once doctoral wings have been won. Of course, in the lofty eyries where the eagles of the profession reside, methodological controversy proves to be irrepressible.

The book will appeal most strongly to generalists who keep up with the action outside their own arena. and who believe that economists have something to learn from external critics. The selection of topics accurately reflects the hot spots in economics over the last 20 years: econometric searches for causation among macroeconomic variables; extended applications of game-theoretic solutions to optimal choice problems; laboratory experiments testing the basic axioms of choice theory; the intrusion of cliometrics (the quantitative study of history) into economic history; functional explanations for the existence and organisation of firms; and computable general equilibrium models as a policy guide for economic development. The philosophers' contributions are of a generally high standard; if their questioning sometimes seems naive to an economist, it may be in part the naiveté of the child viewing an emperor whose sartorial eccentricities the economist no longer sees.

Two philosophers take on the issue of causation in econometrics: James Woodward is a newcomer to the discussion, and is followed by the veteran, Nancy Cartwright. The advantage for non-economist readers is that Woodward begins at the beginning. The disadvantage is that he does not progress much beyond what was pilloried by Christopher Gilbert in 1986 as 'the AER view' ('Average Economic Regression', he says, but a play no doubt on the *American Economic Review*). A typical macroeconomic issue is whether changes in the money supply 'cause' inflation, or does inflation 'cause' changes in the money supply? Economists of either persuasion can tell a plausible story and construct the corresponding model. The models' reduced forms (endogenous variables regressed on exogenous ones) are however, likely to be 'observationally equivalent'. The (caricatured) AER view argues the case one way or the other on 'goodness of fit'. Woodward suggests instead a criterion of 'autonomy', meaning that parameter estimates in the equation(s) expressing causation should be robust to changes (or manipulation) of their independent variables and to changes in background conditions (the 'causal field'). Unfortunately, as Kevin Hoover comments, one causation story may be robust to one set of changes while another story is robust to another set. A very different approach, which Gilbert termed 'Professor Hendry's methodology', has for 20 years or so been achieving consensus as best practice. It begins with a search for exogeneity in a set of variables, trying out first and higher order differences and rearranging regressions until the resulting time series residual has been reduced to 'white noise' (a time series which is completely random). Only then are causation assumptions introduced to see whether they are 'congruent' with the data, meaning that the residual term continues to resemble white noise. Although Cartwright is obviously aware of the newer methodology, her contribution suffers from what appears to be a hasty and drastic condensation of other work. As a result, her chapter is insufficient either to support or to explain her summing up: 'You shouldn't think that the probabilistic approach avoids ontology. It just chooses one ontology over another. To my mind, it makes the wrong choice' (p. 73).

Cristina Bicchieri examines the epistemic foundations of Nash equilibrium in game theory, defined as an outcome in which each player's choice is an individual optimum, *given* the choices of all other players. The question is whether mutual knowledge of the game's structure and mutual assumption of rationality sufficed to lead players to a unique Nash equilibrium, and hence to a predictable outcome. Not surprisingly, the answer in

most cases is no. In order to guarantee a Nash equilibrium, players require mutually consistent beliefs and Bicchieri finds nothing endogenous in the generic *one-off* game to generate them. She concludes that repeated games offer better prospects of convergence to a determinate solution, if standard game theory is supplemented with a theory of learning. Lipman disagrees not with Bicchieri's analysis, but with the purity of her implied purpose for game theory. He argues that much of its value comes not from prediction, but from clarifying the roles and interactions of incentives in particular settings, and that in any given application, processes by which expectations might be coordinated are legitimately part of the play. Apart from the predictability question, Bicchieri's overall account of contemporary game theory will be most useful to readers with a general background knowledge, say to the level of an undergraduate textbook. Experts are likely to find her discussion self-evident, whereas neophytes will probably be overwhelmed.

Schmidtz reports on laboratory experiments relevant to the 'free rider' problem. In its American context, an example would be a fund drive by a local public radio station: a single $10 donation will improve programme quality by less than $10 worth to the donor, but if 10 000 listeners each donate $10, then programme quality can be improved by an amount that is worth more than $10 to each of them. The free rider problem is that the optimal choice for a personal wealth maximiser is to donate nothing and let 9999 other listeners chip in $10; the consequence of 10 000 such decisions is a lost opportunity for mutual gain. Experiments are especially useful here because, as Schmidtz, points out, the monetary worth of something like radio quality to any individual is unobservable, but in the laboratory it is possible to distribute actual money paybacks according to a specific formula and to run sufficient trials so that subjects learn from experience of what the paybacks are for differing strategies. Although non-contribution is the dominant individual strategy according to game theory, experiments find that it is not followed in practice; at the same time, the total amount contributed is less than that which would maximise the net gain to the entire group. Schmidtz and his colleagues are particularly interested in the effects on contribution levels if (i) a minimum provision point is introduced such that payback is zero unless total contributions reach a specified level; and (ii) the provision point is accompanied by a guarantee that donors will get their money back if the minimum is not attained. The results are intriguing.

The critique of cliometrics by Schabas is the weakest chapter of the book. One need not be a partisan for orthodox economic theory to dispute Schabas's assertion that neoclassical analyses 'are predicated on the assumption that ourselves, our knowledge about the world, and the world itself—both physical and social—are fixed' (p. 184). Nor is it easy to accept her claim that the assumption of individual rationality in earlier ages entails a belief that our forebears lacked the 'human frailties of sympathy or regret or weakness of the will' (p. 192). This reviewer knows little about cliometrics and read only the reviews in 1974 of her primary example of it, *Time on the Cross*. The chapter confers legitimacy on remaining ignorant, for Schabas's description makes cliometric methodology seem absurd. Alas, even the comfort of complacency was denied by Coats's comments: he argues that initial excesses of cliometrics have been overcome and that Schabas's account is 'misleadingly selective'. This reviewer feels compelled to seek a more accurate representation elsewhere.

Kincaid's chapter provides a superb evaluation of the new institutionalist theories of the firm. These theories undertake to explain the specific features with which the organisation of production has evolved, by providing a functional interpretation of their survival value in a competitive world. Such features as hierarchical control, vertical integration, separation of management and ownership, and long-term employment

relationships are alleged to fulfil functions of cost minimisation or risk management or, most broadly, profit maximisation. Although the context differs from macroeconomic causation, Kincaid shares with 'Professor Hendry's methodology' the view that 'successfully deducing predictions from a model counts for little unless it can be shown that no other model predicts or predicts as well those same facts' (p. 212). Especially enlightening is Kincaid's derivation of his 'moral' from Bayesian confirmation theory for, unlike the purely objective criterion of white noise, Bayesian probabilities introduce subjectivity. A model must be 'reasonable given what else we know'. Kincaid finds that the institutionalist theories, while suggestive, have not yet been well-confirmed. He extends his critique:

> There is an unfortunate tendency—in this work and in economics in general—to slip from plausible claims about a model qua model to claims about having explained the real world.... No matter how elegant and mathematically tractable a model is, simply postulating it and seeing its consequences provides no real explanation.... [T]f work on the firm is representative—and I suspect it is—much more weight seems to be given inside the profession to model development than empirical specification and confirmation. (pp. 230–231)

The final chapter, Little's examination of computable general equilibrium (CGE) models, in some ways brings the book full circle. Although Little and Taylor (in his commentary) agree that CGE models are not econometric, in the sense that they are meant to describe particular economies rather than to discern causal regularities, we once again have a case where the philosopher grants more validity to economic theory than the economist believes appropriate. Little, for example, states that 'Economists are highly confident in the underlying general equilibrium theory ... an equilibration of supply and demand through market-determined prices' (p. 261). In fact, any economist is aware that confidence in continuous market clearing is held only by those at the extreme *laissez-faire* end of the policy spectrum. Such market clearing would effectively eliminate most pains of structural adjustment associated with such shocks as the removal of protection from foreign competition. As Taylor puts it:

> Contrary to the thrust of Daniel Little's chapter, CGE models are not primarily set up in 'neutral' fashion to explain the data or numerically explore the possible repercussions of policy changes. Rather, they are designed as quantified illustrations of their designers' conceptions of the economic world. (p. 271)

In particular, these models affirm 'orthodox international trade theory's strong bias in favor of liberalizing trade' (p. 275).

Despite the cultural differences between philosophers and economists, there are benefits in liberalising trade between them. The respective comparative advantages in scientific methodology and economic theory, when they generate exchanges at the level of this book, undoubtedly confer real gains to both groups. Unfortunately, one artificial barrier to trade remains to be mentioned: the proof reading of Kluwer Academic Publishers is appalling, to the detriment of understanding in several chapters, most seriously those of Woodward and Schmidtz.

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