

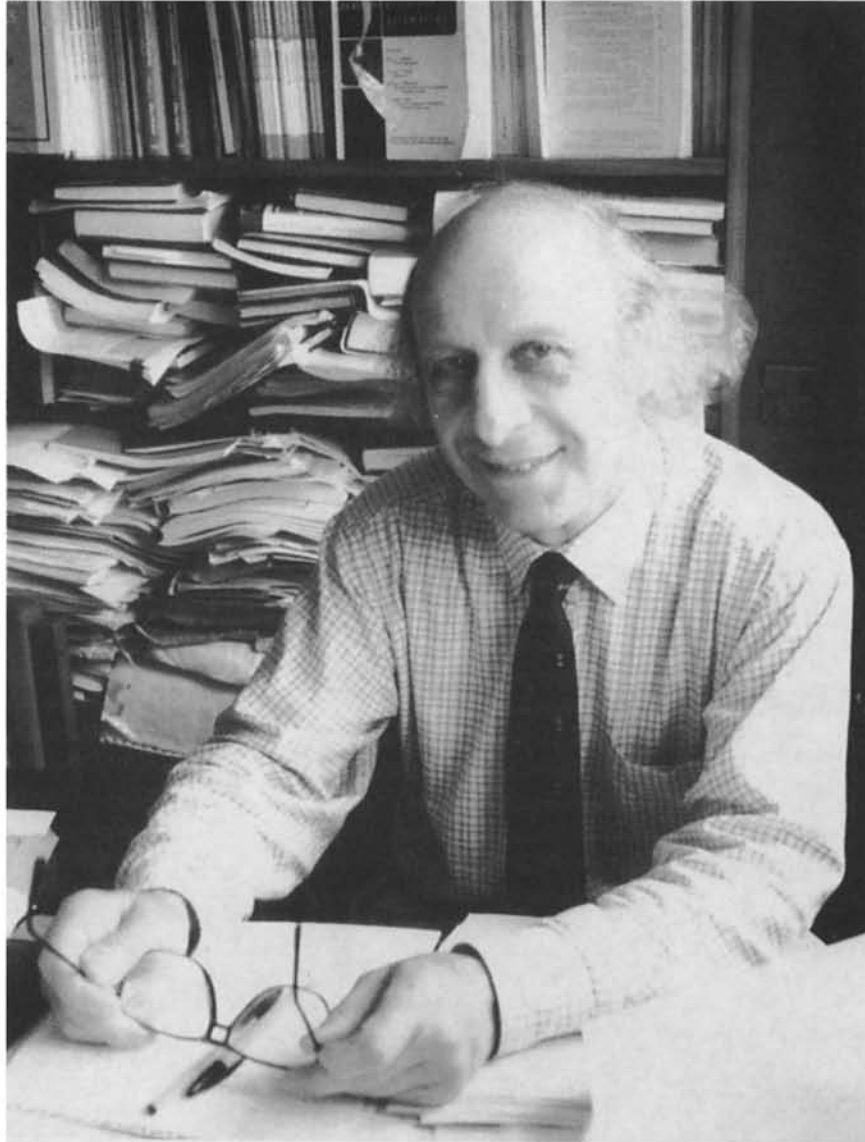
Markets, Unemployment and Economic Policy

Essays in honour of Geoff Harcourt
Volume two

Edited by
Philip Arestis, Gabriel Palma
and Malcolm Sawyer

Routledge Frontiers of Political Economy





Geoff Harcourt
Source: Tony Jedrej, Cambridge Evening News

MARKETS, UNEMPLOYMENT AND ECONOMIC POLICY

Geoff Harcourt has made substantial and wide-ranging contributions to economics in general, and to post-Keynsian economics in particular. In this volume more than forty leading economists pay tribute to and critically evaluate his work.

In particular, contributions focus on:

- the methodological foundations of economic policy-making;
- theoretical and applied analyses of comparative economic systems;
- specific issues in economic policy, including privatization and unemployment.

Contributors are drawn from several countries and represent a wide range of schools in economics.

Philip Arestis is Professor of Economics at the University of East London.

Gabriel Palma is Lecturer in Economics in the Faculty of Economics and Politics, University of Cambridge.

Malcolm Sawyer is Professor of Economics in the School of Business and Economic Studies, University of Leeds.

ROUTLEDGE FRONTIERS OF
POLITICAL ECONOMY

1. EQUILIBRIUM VERSUS UNDERSTANDING
Towards the Rehumanization of Economics within Social Theory
Mark Addleson
2. EVOLUTION, ORDER AND COMPLEXITY
Edited by Elias L. Khalil and Kenneth E. Boulding
3. INTERACTIONS IN POLITICAL ECONOMY
Malvern after Ten Years
Edited by Steven Pressman
4. THE END OF ECONOMICS
Michael Perelman
5. PROBABILITY IN ECONOMICS
Omar F. Hamouda and Robin Rowley
6. CAPITAL CONTROVERSY, POST-KEYNSIAN ECONOMICS
AND THE HISTORY OF ECONOMIC THEORY
Essays in Honour of Geoff Harcourt Volume One
Edited by Philip Arestis, Gabriel Palma and Malcolm Sawyer
7. MARKETS, UNEMPLOYMENT AND ECONOMIC POLICY
Essays in Honour of Geoff Harcourt Volume Two
Edited by Philip Arestis, Gabriel Palma and Malcolm Sawyer

MARKETS, UNEMPLOYMENT AND ECONOMIC POLICY

Essays in Honour of Geoff Harcourt
Volume Two

Edited by
Philip Arestis, Gabriel Palma
and Malcolm Sawyer



London and New York

First published 1997
by Routledge
2 Park Square, Milton Park, Abingdon, Oxon, OX14 4RN

Simultaneously published in the USA and Canada
by Routledge

270 Madison Ave, New York NY 10016

Transferred to Digital Printing 2005

© 1997 Selection and editorial matter, Philip Arestis, Gabriel Palma and Malcolm
Sawyer; individual chapters © the contributors

Typeset in Times by Solidus (Bristol) Limited

All rights reserved. No part of this book may be reprinted or
reproduced or utilized in any form or by any electronic,
mechanical, or other means, now known or hereafter
invented, including photocopying and recording, or in any
information storage or retrieval system, without permission in
writing from the publishers.

British Library Cataloguing in Publication Data

A catalogue record for this book is available from the British Library

Library of Congress Cataloging in Publication Data

A catalogue record for this book has been requested

ISBN 0-415-13390-4

CONTENTS

<i>List of Contributors</i>	xi
<i>Foreword by Joe Isaac</i>	xiii
<i>Geoff Harcourt: A tribute by Richard Goodwin</i>	xv
<i>Introduction by Philip Arestis, Gabriel Palma and Malcolm Sawyer</i>	xvi
 1 HORSES FOR COURSES	 1
<i>Tony Lawson</i>	
 2 ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY	 16
<i>Jochen Runde</i>	
 3 MARKETS, MADNESS AND MANY MIDDLE WAYS	 30
Some reflections on the institutional diversity of capitalism	
<i>Ha-Joon Chang</i>	
 4 EFFICIENT REDISTRIBUTION IN A GLOBALLY COMPETITIVE ECONOMY	 43
<i>Samuel Bowles and Herbert Gintis</i>	
 5 THE FEARS OF ECONOMISTS	 56
<i>Fred Gruen</i>	
 6 UK PRIVATIZATION	 68
Effects on households	
<i>Catherine Waddams Price and Ruth Hancock</i>	
 7 INEQUITY AND ACCESS TO PRIVATE EDUCATION	 80
An overlapping generations model with altruism	
<i>Paul Madden</i>	

CONTENTS

8	THE ACCOUNTANT IN A TURBULENT AGE Accounting conventions and the failure of healthy companies <i>Geoff Meeks and Gay Meeks</i>	91
9	THE ECONOMIC RATE OF RETURN AND THE ACCOUNTANT <i>Geoff Whittington</i>	97
10	CORPORATE FINANCE AND CAPITAL ACCUMULATION <i>Laurence Harris</i>	109
11	HOW FREE SHOULD TRADE BE NOW? <i>Hugh Stretton</i>	121
12	THE COMPETITIVE WEAKNESSES OF THE UK ECONOMY <i>Michael Kitson</i>	133
13	POST-STALINIST SYSTEM REFORM IN CHINA AND RUSSIA Contrasts and implications <i>Peter Nolan</i>	150
14	SOCIALISM Goals and efficiency <i>Bruce McFarlane</i>	165
15	UNDERSTANDING EFFECTIVE DEMAND Capitalism versus socialism <i>Edward J. Nell</i>	178
16	THE EFFECTS OF CHANGES IN AGGREGATE INCOME SHARES ON AGGREGATE DEMAND <i>Trevor Stegman</i>	195
17	MACROECONOMICS AND THE DISTRIBUTION OF INCOME <i>Tony Atkinson</i>	207
18	PROVISIONAL EQUILIBRIUM AND MACROECONOMIC THEORY <i>Victoria Chick and Maurizio Caserta</i>	223
19	EQUILIBRIUM, PATH DEPENDENCE AND HYSTERESIS IN POST-KEYNESIAN MODELS <i>Amitava Krishna Dutt</i>	238

CONTENTS

20	EXPLORING 'THE ORIGINAL KALECKI MODEL T' Simplifications and generalizations <i>K. Vela Velupillai</i>	254
21	THE T-F-M APPROACH TO PRODUCTION ANALYSIS AND THE ANALYSIS OF STRUCTURAL CHANGE <i>Michael A. Landesmann</i>	265
22	LIMITS TO RELATIVE PRICE MOVEMENTS <i>Ian Steedman</i>	284
23	THE MYTHS OF FREE BANKING <i>Mervyn K. Lewis</i>	290
24	DEFINING AND MEASURING UNEMPLOYMENT IN THE UNITED KINGDOM Updating the recent controversy <i>John Wells</i>	299
25	HIGH YOUTH WAGES CAN GENERATE GENERAL AND PERSISTENT UNEMPLOYMENT <i>Murray C. Kemp, Ngo Van Long and Koji Shimomura</i>	315
26	LABOUR MARKETS AND COMPETITION AS AN EVOLUTIONARY PROCESS <i>J. Stan Metcalfe</i>	328
27	THE LABOUR MARKET OF FEAR <i>Peter Riach</i>	344
28	WORKERS' RIGHTS AND ECONOMIC FLEXIBILITY <i>Ugo Pagano</i>	354
29	CHANGES IN THE NOTIONS OF UNEMPLOYMENT AND WHAT THAT MEANS FOR THE POOR <i>Frank Wilkinson</i>	364
30	UNEMPLOYMENT PROSPECTS FOR MODERN CAPITALISM <i>John Cornwall and Wendy Cornwall</i>	377

CONTENTS

31	THE STOCK MARKET AND THE CORPORATE SECTOR A profit-based approach <i>Anwar Shaikh</i>	389
32	EXPANDING EMPLOYMENT IN THE GLOBAL ECONOMY The high road or the low road? <i>Ajit Singh</i>	405
33	WHAT ARE THE CONSTRAINTS ON THE PURSUIT OF KEYNESIAN MACROECONOMIC POLICIES? <i>Malcolm Sawyer</i>	419
34	POST-KEYNESIAN ECONOMIC POLICIES FOR 'WORLD PROSPERITY' <i>Philip Arestis</i>	431
35	CORPORATE RESTRUCTURING IN EUROPE AND THE REGULATION OF COMPETITION <i>Andy Cosh and Alan Hughes</i>	443
36	WHEN WORKERS SAVE NOTHING AND CAPITALISTS CONSUME EVERYTHING Was Kaldor right about why Latin America had such poor savings performance? <i>Gabriel Palma</i>	461
37	GROWTH AND EMPLOYMENT An interpretation of some fundamental transformations in the OECD countries <i>Ferdinando Targetti</i>	483
38	ECONOMIC GROWTH AND THE BALANCE-OF-PAYMENTS CONSTRAINT REVISITED <i>John McCombie and Tony Thirlwall</i>	498
39	ECONOMIC POLICY IN POST-APARTHEID SOUTH AFRICA <i>Jonathan Michie</i>	512
	<i>Geoff Harcourt: a bibliography</i>	526
	<i>Index</i>	535

CONTRIBUTORS

Philip Arestis, University of East London, United Kingdom
Tony Atkinson, Nuffield College, University of Oxford, United Kingdom
Samuel Bowles, University of Massachusetts, United States of America
Maurizio Caserta, University of Catania, Italy
Ha-Joon Chang, University of Cambridge, United Kingdom
Victoria Chick, University College London, United Kingdom
John Cornwall, Dalhousie University, Canada
Wendy Cornwall, Mount Saint Vincent University, Halifax, Canada
Andy Cosh, University of Cambridge, United Kingdom
Amitava Krishna Dutt, University of Notre Dame, United States of America
Herbert Gintis, University of Massachusetts, United States of America
Fred Gruen, Australian National University, Australia
Ruth Hancock, King's College, University of London, United Kingdom
Laurence Harris, School of Oriental and African Studies, University of London, United Kingdom
Alan Hughes, University of Cambridge, United Kingdom
Murray C. Kemp, University of New South Wales, Australia
Michael Kitson, University of Cambridge, United Kingdom
Michael A. Landesmann, Johannes Kepler Universität, Linz, Austria
Tony Lawson, University of Cambridge, United Kingdom
Mervyn K. Lewis, University of Nottingham, United Kingdom
Ngo Van Long, McGill University, Canada
John McCombie, University of Cambridge, United Kingdom
Bruce McFarlane, University of Newcastle, Australia
Paul Madden, University of Manchester, United Kingdom
Gay Meeks, University of Cambridge, United Kingdom
Geoff Meeks, University of Cambridge, United Kingdom
J. Stan Metcalfe, University of Manchester, United Kingdom
Jonathan Michie, University of Cambridge, United Kingdom
Edward J. Nell, New School for Social Research, New York, United States of America

CONTRIBUTORS

Peter Nolan, University of Cambridge, United Kingdom
Ugo Pagano, Università di Siena, Italy
Gabriel Palma, University of Cambridge, United Kingdom
Catherine Waddams Price, University of Warwick, United Kingdom
Peter Riach, De Montfort University, Leicester, United Kingdom
Jochen Runde, University of Cambridge, United Kingdom
Malcolm Sawyer, University of Leeds, United Kingdom
Anwar Shaikh, New School for Social Research, New York, United States of America
Koji Shimomura, Kobe University, Japan
Ajit Singh, University of Cambridge, United Kingdom
Ian Steedman, Manchester Metropolitan University, United Kingdom
Trevor Stegman, University of New South Wales, Australia
Hugh Stretton, University of Adelaide, Australia
Ferdinando Targetti, Università degli Studi di Trento, Italy
Antony Thirlwall, University of Kent at Canterbury, United Kingdom
K. Vela Velupillai, Queen's University of Belfast, United Kingdom
John Wells, University of Cambridge, United Kingdom
Geoff Whittington, University of Cambridge, United Kingdom
Frank Wilkinson, University of Cambridge, United Kingdom

FOREWORD

Joe Isaac

A Festschrift which runs into two volumes hardly needs a preface, especially as each volume has an introduction giving a copious account of Geoff Harcourt's life and work. But I could not refuse the honour and pleasure occasioned by the invitation to write the foreword to this volume.

One of the more satisfying rewards of teaching is to encounter students who show the potential of outclassing their teacher in academic ability and achievement. Geoff was one such student in the batch of outstanding students who passed through the University of Melbourne in the early postwar years.

For the younger teachers of economics brought up on *The General Theory*, it was a time of infectious excitement and optimism. There was virtual unanimity on what was to be done: most of us were 'wet' economists at the time; high unemployment, poverty, social and health deprivation were to be relics of the past. 'The Welfare State' was within easy reach, as governments in many countries and of various persuasions, fortified by the community's strong sense of collective responsibility manifest during the still recent Second World War, pledged commitment to it. In retrospect, many of us might have been starry-eyed politically.

This rosy view was sustained for two decades, after which it waned and was overtaken by the philosophy of individualism and the wisdom of allowing the market to operate largely unrestrained by government intervention. Despite a generally high level of chronic unemployment, increased inequality in the distribution of incomes and the creation of a substantial underclass in many countries, economic policy has come to be judged more by the deregulatory processes being adopted and less by the outcomes of the policy. This is now the tenor of mainstream economic thinking.

It will be evident from most of Geoff's writings that he is not in this mainstream; rather, he has been warning, Cassandra-like, against it. For much of his academic life, Geoff has gone against the fashions of contemporary economic thought, at least on matters relating to the efficiency and fairness of the market mechanism and the prevailing phobia about government intervention. This is not surprising. He does not hide his values behind any

FOREWORD

pretence of positivism, and his policy prescriptions must be considered in the context of his values:

to make the world a better place for ordinary men and women, to produce a more just and equitable society. In order to do that, you have to understand how particular societies work and where the pockets of power are, and how you can either alter those or work within them and produce desirable results for ordinary people, not just for the people who have power. I see economics as very much a moral as well as a social science and very much the handmaiden of progressive thought.¹

True to the Cambridge tradition of his mentors he regards the task of the economist to be fruit-bearing, as well as light-bearing.

The large number of contributors to the two volumes of the *Festschrift* in his honour, is testimony to the fact that his contribution spans a very wide area of economics, in the fields of both theory and policy. Nearly all the papers in this volume deal with some part of his work. This is a remarkable achievement in an age of specialization. It testifies also to the high regard, esteem and affection with which he is held by the international academic community.

Sustaining his academic achievements are his person qualities – integrity, courage, generosity, informality, a sense of humour, and extraordinary energy.

As one who was privileged to have known him and has followed his career from the very threshold of academic life and, perhaps, even to have nudged him slightly in the direction he ultimately took, I bask in his success, and wish him many more years of productivity. Perhaps he might care to tackle econometrics, the one missing item in his extensive repertoire.

NOTE

- 1 Hamouda, O. (ed.) (1986) *Controversies in Political Economy. Selected Essays of G. C. Harcourt*, New York, New University Press, pp. 4–5.

GEOFF HARCOURT: A TRIBUTE

For anyone working in the economics faculty at Cambridge, as I used to, Geoff was a central figure. Almost always to be found in his office, he was so genuinely friendly.

With a very broad range of interests in economic analysis, he was willing to provide help and advice to anyone wishing to prepare work for publication. He knew most of the faculty and was always ready to be consulted, ready to provide advice and information about whom to consult about any particular economic issue and about who might be helpful on specific subjects. Though steadily working himself, he was always ready to discuss any aspect of particular issues of economics. I always felt free to drop by his office and discuss with him whatever topic I was interested in working on. He always responded with useful suggestions. He never gave one the feeling that one was interfering with his own interests or projects.

I always felt he constituted a kind of centre to consult about who might be helpful on a wide range of topics. I never felt that I was unduly impinging on his time for work on his own projects. I almost never came to the faculty, for any purpose, without stopping at his office and having a pleasant and often highly informative communication with him on a variety of topics in economics.

Richard Goodwin
University of Siena, Italy

INTRODUCTION

This is one of two volumes published to celebrate the major contributions Geoff Harcourt has made to the discipline of economics and to post-Keynesian economics in particular. During his illustrious career, Geoff has gained enormous respect and admiration from colleagues and friends of varied persuasions, as is amply evidenced by the contributions to these two volumes.

Geoff Harcourt, 'an Australian patriot and a Cambridge economist' in his own words,¹ was born in Melbourne on 27 June 1931. At school he wanted to be a vet – he has always loved birds and other animals – and mostly did science. He took some economics only to 'make up the numbers', as he puts it, but did very well at it, and very badly at physics, so that luckily for us and unluckily for all the animals in the world, he proceeded to do economics at the University of Melbourne. It was Cambridge-oriented economics that he studied there, which he has loved ever since.

It was clear, even at that early stage in Geoff's academic life, that great things were going to happen in the future. His undergraduate dissertation was a clear pointer to the future. Oligopolistic price theory, with firms having as their objective the 'desire for secure profits as much as maximum profits', was integrated with the macroeconomic system of Keynes's *General Theory*, in an attempt to study the behaviour of Australian companies during the Great Depression. Effective demand, especially investment, pricing under conditions of imperfect competition, applied economics and more, were all there. The main ingredients of his undergraduate dissertation were clearly visible in his master's degree dissertation, although its theme was a radically different one, namely a pilot survey of income and saving in Melbourne.

In late July 1955 he and Joan married, 'lived happily ever after', and left Melbourne in mid-August for King's College, Cambridge – where else, indeed? Geoff's Ph.D. turned out to be a study of the economic implications of using historical cost accounting procedures for price formation in a period of inflation, which inevitably entailed implications for measuring income for dividend and tax purposes. It was at this time that Geoff acquainted himself with Robinson's *The Accumulation of Capital*, then newly published. Indeed, he read a paper on the main propositions of the book to three consecutive sessions of the research students seminar, chaired by Robin Marris. Joan Robinson attended the third session. She actually did not think much of that group of students with the exception of 'a chap called Harcourt'. *The*

INTRODUCTION

Accumulation of Capital provided the *core* which has inspired Geoff's contributions to economics, and his teaching. This is not difficult to understand now, perhaps, given the influence on both Joan Robinson and Geoff Harcourt of the writings of Smith, Ricardo, Marx, Marshall, Keynes, Kahn, Kalecki and Sraffa.

Geoff returned to Australia after his Ph.D. at Cambridge, to take up his first lecturing job in the Department of Economics at the University of Adelaide. It was there that he met Eric Russell, his greatest mentor and friend in Australia. On Geoff's own admission he learned a great deal from Eric, including theory, applied economics and policy as it relates to the real world, and how to teach undergraduates economics. He lectured on Kaldor's economics and wrote a critique of Kaldor's theories of distribution and growth as they had been developed by that time. That critique concentrated on the pricing behaviour of the consumption and investment goods sectors, and focused essentially on the full-employment assumption adopted by Kaldor. It essentially argued that the full-employment assumption required strong conditions on pricing behaviour in the two sectors in order for the distributive mechanism to work and for Kaldor's growth models to behave in the intended way. Geoff argued that it was better to drop the un-Keynesian full-employment assumption in favour of keeping the distribution mechanism, a more realistic and relevant aspect of Kaldor's theoretical framework. At about this time he wrote 'The accountant in a golden age', published in 1965 (*Oxford Economic Papers*), which is perhaps his best-known paper after the 1969 *Journal of Economic Literature* survey.

In 1963 Geoff returned to Cambridge on a year's study leave. His interest in oligopolistic industries continued and these aspects of his work were the central focus of the paper published in the *Economic Record* (1965) on the determination of employment and the distribution of income in a two-sector model in the short period. That paper was presented at a seminar the audience of which included, among others, all the famous Cambridge economists of the time. It was in a sense the apotheosis of his enduring concern with, and maturity of his interest in, the idea of oligopoly, which had occupied him since his undergraduate years. This concern is further reflected in his often-quoted paper with Peter Kenyon in *Kyklos* (1976). Not surprisingly, Geoff has come to view the *Economic Record* contribution as his favourite theoretical paper. The seminar must have been a resounding success, for soon after it he was offered a lecturing job in the faculty which he accepted for only three years – he did not wish to let Adelaide University down – and before he knew it, he was elected to a Fellowship in Trinity Hall. The paper he presented at that seminar provided the impetus for a stream of important and influential contributions, with a distinctly strong post-Keynesian thread running through them. These included the choice of technique papers, the best known of which appeared in the *Economic Journal* in March 1968.

Geoff has always been concerned with making the assumptions of the

INTRODUCTION

problem in hand explicit and with bringing out the limitations as well as the illuminations of the relevant analysis. An excellent example of this is his *Economic Activity* (with P. H. Karmel and R. H. Wallace, 1967), a book based on a lecture course on Keynesian economics and one that Geoff considers as 'a clear and unpretentious account of the "state of the art" at the time'. The time was the end of 1966 and Geoff was leaving Cambridge for Australia. That was also the era of the Vietnam War and Australia's role in it. Those events and Geoff's political involvement had a profound impact upon him, especially upon his views on the relationship between ideology and analysis. No longer could they be separated in his writings and teaching.

In the midst of all that political fervour, Geoff 'took time off' to write his first piece on the capital controversy for the *Journal of Economic Literature*. The favourable feedback on that paper encouraged Geoff to push on within the area and no fewer than four further papers materialized. The then editor of Cambridge University Press was very impressed by that vast output and asked Geoff to put them together in a book. The first draft was ready, in Geoff's words, 'in two months flat (out!)'. Published in May 1972, under the well-known title *Some Cambridge Controversies in the Theory of Capital* (with subsequent editions in most major languages), the book contained two themes. The first was a critique of the concept of using price as an indication of scarcity in distribution theory, and the second was a methodological critique of utilizing differences to study change. A year's study leave in 1972-3 was spent at Clare Hall as a Visiting Fellow. During that time Geoff gave a number of seminars on the capital debate book, with his afterthoughts being published in *Oxford Economic Papers* in 1976, with the apt title, 'The Cambridge controversies: old ways and new horizons – or dead end?'. It was also then that he formed a close friendship with Tom Asimakopulos (they had already met at Cambridge in the 1950s), which lasted until the latter's premature death in May 1990.

Geoff returned to Adelaide University in 1973. The golden age of capitalism was coming to an end by then, soon followed by the stagflation of the 1970s and the monetarist era. Australia did not stand outside the trend. In 1975 the government there introduced monetarist policies accompanied by confrontationist attitudes between government, capital and labour. Although Geoff had already spent some time drawing out the policy implications of the capital theory debate, the Australian experience gave him a new platform on which to formulate his economic policy views more cogently. At the heart of those ideas was a set of economic policies designed to reduce inflation slowly while maintaining high levels of employment and external balance. Furthermore, redistribution through public-sector policies was seen as the quid pro quo for trade union acceptance of incomes policies. Fiscal and monetary policies were to aim at influencing the level and the rate of growth of economic activity. Nationalization of key industries including financial intermediaries would boost investment, and a fixed exchange rate adjusted

INTRODUCTION

from time to time would ensure external balance. When the Australian Labor Party (ALP) was returned to power in 1983, the Accord between trade unions and the government must have owed a great deal to those ideas propounded by Geoff as the economist on the ALP's National Committee of Enquiry in 1978, in the form of background discussion papers. They are still on Geoff's agenda and he has taken them a step further recently in a number of contributions.

In the mid-1970s Geoff developed a new interest in writing intellectual biographies. He has written a steady stream of them, which he has admitted to enjoying enormously – a clear indication of his love of and concern for fellow human beings. Some of this material may very well form the nucleus of the major task he first began when he returned to Cambridge in 1982 (to a teaching post in the Faculty of Economics and Politics and a Fellowship at Jesus). It is to write the intellectual history of Joan Robinson and her circle, and to show the connections between their contributions and those of the classical economists, Marx, and Keynes, as well as of contemporaries whom Joan Robinson and her circle influenced. The project is not finished yet, for good reasons which have to do with Geoff's continuing concern with a number of developments both in economic theory and in economic policy in the real world. But above all, it is a '*Second Edition*' of *The General Theory*, which he is co-editing with Peter Riach and hopes to bring out in 1996, sixty years after the publication of *The General Theory*, that has taken up much of his time. This is an exciting publication which is intended to tell us what Keynes would have written in the late 1930s had he known what those who succeeded him were to contribute on a number of aspects of *The General Theory* in the post-war period. 'Putting Keynes back at the forefront of the debate' is the essential purpose of the book. And just to show the world that he has not run out of steam as a surveyor of the passing scene, Geoff published a paper entitled 'Reflections on the developments of economics as a discipline', a piece about the Nobel Prize winners up to Debreu. It was published in the *History of Political Economy* in 1984.

Since he last returned to Cambridge in 1982, Geoff has been at the centre of the teaching of and research in macroeconomics. He has also been instrumental in the development of the graduate programme in the Faculty of Economics and Politics, and has systematically been the most popular Ph.D. supervisor.

We have tried in this introduction to offer an inevitably short summary of Geoff Harcourt's academic life and work. Just as Mark Perlman has done recently in a preface to a selection of Geoff's essays, we have tried to look into Geoff's four groups of contributions.²

1 works analysing contemporary economic theoretical problems;

2 works synthesizing states of debates in economic theory;

INTRODUCTION

- 3 works having a distinctly biographical flavour and pertaining to various contemporary economics; and
- 4 works pertaining to economic and allied social policies.

Of course this convenient list does not pretend to cover the full range of material covered in Geoff's massive output of books and papers.

We are delighted, but not surprised, that on 13 June 1994 Geoff was awarded one of the highest Australian honours of Officer in the General Division of the Order of Australia (AO), for 'service to economic theory and to the history of economic thought'. Well done Australia! And yet Geoff still flourishes as a Reader at Cambridge (he is Reader in the History of Economic Theory, *ad hominem*), and enjoys himself as ever. Our greatest regret, and his, we are sure (though he has never shown it at all), is that despite his achievements, national and international recognition, and his vast contributions to the discipline of economics, he has been denied a full professorship – although his old university in Adelaide did bestow upon him in 1988 the title of Professor Emeritus. This, of course, continues the famous Cambridge tradition of being less than generous in awarding professorships, as, for example, the failure to so honour Piero Sraffa, Maurice Dobb, Richard Goodwin, Luigi Pasinetti and others, or the late concessions of professorships to Joan Robinson and Nicky Kaldor.

The editors of this book are exceptionally grateful for Geoff's help, encouragement and friendship throughout the years. The speed by which *constructive* comments are generously provided on manuscripts and papers, the breadth and depth of them, his unfailing availability at the other end of the telephone, his writings and contributions to conferences from which we, and so many others, have benefited are just some of the ways in which we have gained through knowing him. We are honoured to have had a long association with so great a figure as Geoff Harcourt. Many have benefited from his warm hospitality to visitors to Cambridge. The two volumes we have put together are intended to mark our enormous respect and admiration for such a great friend. We are sure that these sentiments are completely shared by all the contributors to the two volumes, and indeed by many others who would have liked to have contributed but unfortunately were unable to do so.

There is another aspect of Geoff's skills that makes him almost unique in academia. He was a player of Australian Rules football in an amateur capacity. That he was still playing such a physically demanding game at the age of 47 is no mean feat. As befits an Australian, he was also a keen player of cricket until a recurring back injury and then his four recent brushes with death put him out of the game – only temporarily, we are delighted to say. Undeterred by the back injury, he ran the Cambridge half-marathon in the mid-1980s, and every day now he goes on a long bike ride. Despite his recent illness, his resilience is such that he is as sparkling as he has ever been. Geoff often jokes about those incidents, but then, being intelligently funny is yet

INTRODUCTION

another characteristic of his good nature. This ability adds even more to his humane approach, not just to his economics but also to his fellow human beings, especially his friends.

No introduction of this kind should come to an end without at least a mention of the quiet, and yet dynamic and effective, person who we know has played a central and critical role in supporting what Geoff has managed to achieve in his life. This is, of course, Joan, who has been a reliable and tremendously supportive partner in Geoff's life. She has also been an extremely good and warm friend to all of us, all these years. She, and Wendy, Robert, Timothy, Rebecca, their respective partners and the recent addition of a grandchild (thanks to Wendy and Claudio Sardonì), complete a very happy family indeed.

Nor should we fail to mention Geoff's religious and political convictions, which are central to his economics. It is very pleasing to see how well these three aspects – religion, politics and economics – mesh in his recent paper, 'A "modest proposal" for taming speculators and putting the world on course to prosperity'. They combine with his unfailing compassion for his fellow women and men to give expression to his real feelings about humanity.

Special thanks must go to the contributors for their willingness to respond to our comments and suggestions with forbearance and good humour. Thanks are also extended to the secretaries of the Department of Economics at the University of East London, June Daniels and Christine Nisbet, and the secretary of the School of Business and Economic Studies at the University of Leeds, Eleanor Lynn, for their generous assistance. Finally, Alan Jarvis and his staff, as always, have provided excellent support throughout the period it took to prepare both volumes.

Philip Arestis, Gabriel Palma and Malcolm Sawyer

NOTES

- 1 All the quotes from Geoff Harcourt in the introduction are from his entry in P. Arestis and M. Sawyer (eds) (1992) *A Biographical Dictionary of Dissenting Economists*, Aldershot: Edward Elgar, pp. 232–41.
- 2 The four groups are quoted from the preface (written by Mark Perlman) to G. C. Harcourt (1995) *Capitalism, Socialism and Post-Keynesianism*, Cheltenham: Edward Elgar.

HORSES FOR COURSES

Tony Lawson

INTRODUCTION

All economists adopt a methodological stance of some kind. These stances are manifest not only in implicit research criteria but also in the advice they offer each other and many of the questions they ask. Familiar examples of the sort of questions and advice that I have in mind include: 'What's your model?'; 'Have you tested it?'; 'Don't think about it, just do it'; 'Avoid discussions about the use of mathematics'; 'Be rigorous'; 'Does it have micro-foundations?'; 'Does it have a (unique) solution (equilibrium)?'; 'Does it support interventionist (*laissez-faire*) policy conclusions?' And while various expressions of this sort are regularly employed by Geoff Harcourt, there is one recommendation that I have heard (or noticed?) him use rather more often than any other. It takes the form of the slogan 'horses for courses'. Its usage in the scientific-methodological context is not unique to Geoff; but, perhaps because I have had the opportunity to interact with him so frequently, I associate it with Geoff before anyone else. In fact, although many who hear this slogan usually give it an immediate nod of approval (which is an interesting phenomenon in itself) I cannot think of any other person who so regularly employs it.¹

But if the slogan, as a methodological criterion or stipulation, does have a good deal of immediate intuitive appeal, it also requires some unpacking. As far as I am aware there is nowhere this has already been done. My objective here, then, is to suggest a specific interpretation. Once this has been achieved it is obviously of interest to check whether such an apparently general or abstract slogan carries methodological bite. I conclude that it does by examining its implications with regard to an issue which Geoff has himself recently addressed and which is currently as significant as any facing the discipline: the usefulness to economics of mathematical methods.

HORSES FOR COURSES

How then are we to interpret 'horses for courses' in the context of scientific practice? Of course there are many possible translations, and Geoff sometimes interprets the slogan in different ways. The exercise which follows must

be seen as rather exploratory. My aim is to provide a translation which at least preserves those aspects which seem both essential to it and likely explanations of its apparent immediate intuitive appeal. This appears to be the minimal requirement of any translation that can be sustained.

In the light of these considerations there are several aspects of the slogan that warrant attention. The first is that it *is* a stipulation. After all, the statement is not of the form "any old horse will do", i.e., whatever the course. It follows that whatever the precise form in which it translates into the scientific context, it does so as a directive or criterion of some sort. This is obviously desirable if repeating the slogan is to make any difference to anything.

A second and fundamental feature is that there is some *matching* going on, and it is a matching of some form of agency or action to its conditions of action. The directive is basically that for a given course, a horse ought to be selected according to its suitability in the light of existing conditions (length of course, firmness of terrain, existence, number and/or height of fences, etc.); or for a given horse, the owner or 'racer' ought to select a course such that the perceived conditions give the horse a comparative advantage. In short, horse is matched to course, or vice versa.

The most obvious way for this feature to be carried over into the scientific context is as the requirement that scientific investigatory practice and the feature of reality to be investigated be in some sense tailored to one another. This clearly necessitates obtaining insights into the nature of the objects of enquiry, as well as an awareness of the metaphysical presuppositions of given methods and procedures. In short, this feature of the stipulation necessitates an attention to ontology, and presupposes a realist orientation.² It represents an explicit negation of the epistemic fallacy, i.e. of the erroneous belief that ontology can be reduced to epistemology, that questions about being can be rephrased as questions about knowledge (of being). It distinguishes method from its object.

This second aspect of the 'horses for courses' slogan is, I think, its most essential one. Certainly it is the most essential feature of the interpretation that I am suggesting here. But there is also a (slightly distinct) third feature that warrants emphasis, one which perhaps contributes most to its immediate appeal. This feature is signalled by the apparent ability of the slogan to express much through little – by way of three words, to be precise. This latter result is achieved because the analogy drawn implicitly relates to a (familiar) whole situation: the race course and all that surrounds it. Most obviously, an essential aspect of horse-racing is the goal of winning, which typically means choosing (or riding or owning) the horse which in any given race comes first.³ In other words, the slogan in question appears to convey, as an additional essential aspect of it, something about the objective or purpose of the event, as well as a method or strategy. And in the scientific context, especially if a realist orientation is implied, this presumably translates to the goal of

HORSES FOR COURSES

illuminating (revealing, explaining, understanding) some feature of reality. Object and method of enquiry are 'matched' to one another under the intent of illuminating the former.⁴

Thus, I suspect that Geoff is rather lax and gets it not quite right when sometimes (albeit only sometimes) he suggests that the slogan translates into something like "how you do it depends upon what the purpose is" (e.g. Harcourt 1996: 6). This interpretation (which is barely a stipulation anyway) matches procedures of action to purposes rather than to conditions of action, and thereby neglects that an additional and apparently essential feature of the slogan, and a likely major explanation of its intuitive appeal, is precisely that the purpose is already implicated: to 'pick' the horse which comes first.⁵

In short, I suggest that the most compelling translation of 'horses for courses' in the scientific context pertains to the (usual) situation where the accepted goal is to illuminate (reveal/explain/understand) some feature of reality, and takes the form of the directive that where given methods, techniques or procedures are to be employed, the objects chosen for analysis be of such a nature that the methods appear capable of illuminating them; or where definite aspects of reality are to be illuminated, the methods and procedures followed be fashioned to insights available concerning the nature of such material.

THE USE OF MATHEMATICS IN ECONOMICS

Can this methodological horses for courses (henceforth MHC) directive actually make any difference to anything? This is an important question, because there is obviously no isomorphic relation between a theory of ontology and any set of methods or procedures. Although the noted epistemic fallacy is avoided, the stipulation remains at a high level of generality.⁶ Despite this, it is easy enough to provide an initial indication that MHC can bear important implications by considering one of Geoff's own papers. Although many economists express definite, often strongly held, views on the use of mathematics in economics, Geoff is one of the few to have written an entire paper on the topic (Harcourt, 1995 [1993]). In this paper Geoff, amongst other things, surveys a range of prominent assessments on the usefulness of mathematical formalism to economics. The problem, though, is how to choose between them. Towards the end of his paper Geoff acknowledges that he has presented an array of different views, but without making any definite selection. He writes: 'So where does this leave us? Clearly, to take a weighted average of such divergent views would be a cop out.' Geoff, though, does not really take the issue further,⁷ other than acknowledging that 'mathematics can be a good servant but, even more, a bad master' (1995: 19). My aim here is to reinforce Geoff's latter intuition by explicitly bringing to bear the version of 'horses for courses' argued for above. Certainly this strategy supports implications that are reasonably clear-cut.

CRITERIA OF METHOD SELECTION

Amongst the 'divergent views' explicitly examined and quoted by Geoff,⁸ both the assessments of the usefulness of mathematical formalism for economics and the criteria (or implicit stipulations) employed in making these assessments are discernable in statements taken from Marshall, Keynes, Samuelson, Boulding, Debreu, Koopmans, Mirrlees, Chichilinsky, Hahn and Stone. In consequence, it is possible to divide this group into those who do, and those who do not, accept MHC as interpreted here in making such assessments. In fact only three of those listed explicitly and unambiguously emphasize a need to match method to the nature of the object of study in order to illuminate the latter – namely Marshall, Keynes and Boulding. The others draw on criteria or objectives which are either too generally stated to be interpreted here (such as 'scientific advancement'), or rely upon specific criteria and objectives that are noticeably different.

Thus Samuelson makes an unelaborated reference to 'advancing the science' and also invokes the avoidance of 'depraved' types of 'mental gymnastics';⁹ Debreu emphasizes procedures which permit sounder judgements of relevance, the ability to give 'ready answers' to new questions through reinterpreting 'primitive concepts', deeper understandings of problems formulated, rigour, the intellectual need of economists for rigour, simplicity and generality, the facilitation of efficient communication and thinking;¹⁰ Koopmans emphasizes the efficiency of establishing logical links between premises and conclusions, explicitness of assumptions, a reasoning process that is not intruded upon by 'associations clinging to words';¹¹ Mirrlees mentions 'explicitness of assumptions, attention to detail, and rigour' (Mirrlees, 1978: 15–17, quoted in Harcourt, 1995: 15); Chichilinsky singles out clarity, a strong foundation, and 'desired [mathematical] advance in areas which are of great importance for intellectuals and for those whose lives depend on it'¹² (1990: 16); Hahn emphasizes understanding, honesty, modesty, excitement, beauty, the avoidance of being 'enslaved by slogans and shibboleths of practical men and women' (Hahn, 1985: 28, quoted in Harcourt, 1995: 15), a need to establish precise definitions of problems and necessary conditions for definite results; while Stone draws attention to the nature of current practices in a number of social sciences, efficiency in analysing and comparing theories of complex systems, the reduction of generality in models, a need to gain insight into subjects where concepts are vague and information is imprecise, and an interest in understanding reasoning processes behind effective (as opposed to ineffective) decisions.¹³

In all these cases (whatever the accuracy of the claims put forward¹⁴) the suitability or relevance of methods and procedures to the specific nature of the material that is to be investigated is never explicitly invoked in formulating a criterion. The 'matching' of one with the other is not an issue. Only for Marshall, Keynes and Boulding is this an explicit consideration.

HORSES FOR COURSES MAKING A SELECTION

But so what? What do we learn from subdividing the contributions referred to by Geoff in this manner? The answer is simply that a systematically different conclusion is reached concerning the relevance of mathematical formalism to economics according to whether or not MHC as interpreted here is invoked. Specifically, while those who fail to question explicitly whether formalistic methods are capable of illuminating social material, infer that the application of mathematical formalism to all areas of economics can only be beneficial, those who employ the criterion draw more or less the opposite conclusion.

Thus, while the former group express only positive views on the use of mathematical methods in economics, Boulding finds, for example, that 'mathematicians themselves set up standards of generality and elegance in their expositions which are a bar to understanding' (Boulding 1948, 1971: 236, quoted in Harcourt 1995: 12); Marshall concludes that 'the application of exact mathematical methods to [the few facts which can be expressed in numbers] is nearly always a waste of time, while in the majority of cases it is positively misleading; ... the world would be further on its way if it had never been done at all' (Pigou 1925: 422, quoted in Harcourt 1995: 6); and Keynes writes of 'symbolic pseudo-mathematical methods of formalising a system of economic analysis' concluding that 'too large a proportion of recent "mathematical" economics are mere concoctions which allow the author to lose sight of the complexities and inter-dependencies of the real world in a maze of pretentious and unhelpful symbols' (Keynes 1936, 1973: 297, 298, quoted in Harcourt 1995: 9).

In short, if we decide to choose between the 'divergent views' brought to our attention by Geoff using his own 'horses for courses' stipulation as a criterion, then the conclusion seems clear. *Ceteris paribus*, support should be given to those who conclude in favour of severely restricting the use of mathematical formalisms of the sort that are regularly found in economics. It is this, is it not, that Geoff is really telling us?

METHODOLOGICAL 'HORSES FOR COURSES' MORE DIRECTLY APPLIED

Even so, it must be admitted that no direct argument for or against mathematical economics has actually been made here. Any conclusion against the use of specific mathematical methods is conditional upon the soundness of MHC as well as the legitimacy of the arguments made by Keynes, Marshall and Boulding in support of their noted conclusions. Of course, I suspect that few would really want to reject the (realist) MHC criterion explicitly, even if many regularly overlook it. Moreover, the observation that 'assessments of leading economists on the validity of extending the use of certain formalisms

in economics turn upon whether or not the MHC criterion is invoked' is a partial regularity that *prima facie* suggests that something systematic is going on. A (conjoint) hypothesis which can straightforwardly account for it, certainly, is that (i) the statements selected by Geoff adequately capture the assessments of the various authors; and (ii) these authors have reasoned correctly, so that while the mathematical formalisms in question have numerous attractive aspects, they are not particularly appropriate to the understanding of social reality.

All the same, a direct argument still needs to be made;¹⁵ the case for limiting the use of formalism in economics so far rests merely on the authority of the cited critics. Let me, then, examine more closely at least one of the arguments of one of the 'antagonists'. I focus on Keynes, with whose writings I am more familiar. As Geoff is a major figure in the post-Keynesian tradition, this seems the appropriate example to consider anyway. The question I want to pursue is whether Keynes' reasoning for resisting the encroachment of formalism of the sort found in economics stands up to inspection. For reasons of space I restrict myself to Keynes' assessment of the appropriateness of econometric methods.¹⁶

METAPHYSICAL PRESUPPOSITIONS

I note, first, that formalistic methods of the sort traditionally used in economics presuppose regularities of the form 'whenever event (type) *x* then event (type) *y*'.¹⁷ This formulation can be interpreted generally to include both the probabilistic relationships which characterize econometrics and the deterministic law-like statements ('axioms' or 'assumptions') which are essential to mainstream theorizing. Let me also note that, outside astronomy, most of the event regularities of this sort uncovered in science have been produced in situations of experimental control. At the same time, experimental results are frequently applied outside the experimental situation where event regularities are no longer found. Now the only adequate explanation of this situation of which I am aware interprets reality as structured and open. That is, the confinement of most event regularities, but not of the applications of scientific knowledge, to situations of experimental control can be rendered intelligible if it is acknowledged that the world is (i) structured, in that actual events and states of affairs are produced by equally real underlying structures, mechanisms, powers and tendencies, and (ii) open, in that actual phenomena are typically conjointly determined by numerous often countervailing mechanisms. For, on this conception the noted observations can be explained by seeing the achievement of the well-controlled experiment as the insulation of some fixed or relatively stable causal mechanism from the action of countervailing factors so that the mechanism of interest can be empirically identified. The event regularity so uncovered, in other words, relates the 'triggering' conditions of some mechanism and the way it acts. But the

HORSES FOR COURSES

mechanism itself, when triggered, acts inside and outside the experimental conditions, thereby explaining how it is that experimentally determined results can be applied in non-experimental contexts. Thus gravity acts as much on the pen in my hand as on the object falling with constant acceleration in a vacuum. Laws, on this conception, refer not to event regularities produced in experimental situations but to the causal mechanisms they reveal. In short, event regularities are not the scientific object but a human contrivance which allows underlying causal mechanisms to be revealed.

ATOMISM AND ISOLATIONISM

In the absence of meaningful possibilities of experimental control in the social domain, this analysis bears the consequence that event regularities of relevance to economics can really only be expected if and where sufficiently stable mechanisms spontaneously act in relative isolation. In fact, even the relative isolation of an individual with a constant intrinsic structure does not yet guarantee the production of an event regularity. For the individual may be so structured that, even given an identical set of initial conditions, a range of outcomes remains possible. Restrictions must operate to ensure that but one reaction is possible. In other words, the individuals of analysis must, for all intents and purposes, be atomistic. Certainly, it is a conception of atomistic individuals that is most easily reconciled with these preconditions. Now, the second (sufficiency) requirement for an event regularity, i.e. that of relative isolation, does not necessitate that each individual acts in complete isolation, so long as all other operative factors are either constant in their action, or at least orthogonal to the action of the primary mechanism in question. In other words, if the mechanism or individual of interest cannot be examined in the insulated conditions of experimental control, the hope must be that it acts in a stable environment.

In short, this discussion indicates that a significant reliance upon the formalistic methods of mainstream economics can be rational, accepting MHC, only where there exists something like grounds for supposing that the analysis is concerned with atomistic factors which operate in a homogeneous environment.¹⁸ And, of course, this is far from the typical social situation. It thus follows that use of the sorts of formalistic methods currently dominant in economics must be highly circumscribed.

Now if this argument is most developed in recent realist contributions, the essentials of it are not novel. Indeed, they constitute precisely Keynes' grounds for rejecting the (by now familiar) method of econometrics more than fifty years ago, as set out in his initial response to an invitation from the League of Nations to review Tinbergen's work on business cycles:

There is first of all the central question of methodology, – the logic of applying the method of multiple correlation to unanalysed economic

LAWSON

material, which we know to be non-homogeneous through time. If we are dealing with the action of numerically measurable, independent forces, adequately analyzed so that we were dealing with independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action . . . In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation . . .

To proceed to some more detailed comments. The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant. There is no reason at all why they should not be different every year.

(1973: 285)

These sorts of comments are repeated throughout the late 1930s by Keynes and come to a head in 1939 in the eventual review of Tinbergen's book:

Put broadly, the most important condition is that the environment in all relevant respects, other than the fluctuations in those factors of which we take particular account, should be uniform and homogeneous over a period of time. We cannot be sure that such conditions will persist in the future, even if we find them in the past. But if we find them in the past, we have at any rate some basis for an inductive argument . . . [The] main *prima facie* objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment.

(1973: 316)

Consider, too, his earlier comment on Edgeworth, which I reproduce from Geoff's paper once more:

Mathematical Psychics has not, as a science or study, fulfilled its early promise . . . When the young Edgeworth chose it, he may have looked to find secrets as wonderful as those which the physicists have found since those days. But this has not happened. The atomic hypothesis which has worked so splendidly in physics breaks down in psychics. We are faced at every turn with the problems of organic unity, of discreteness, of discontinuity – the whole is not equal to the sum of the parts, comparisons of quantity fail us, small changes produce large effects, the assumptions of a uniform and homogeneous continuum are not satisfied.

(Keynes, 1993, CW, X 1972: 262, quoted in Harcourt 1995: 9)

In short, conditional upon accepting MHC, the case against the usual types of

HORSES FOR COURSES

mathematical formalism found in economics seems forceful, as Keynes realized some time ago.

FINAL COMMENTS AND QUALIFICATIONS

I have indicated one possible, and I think compelling, way in which the slogan 'horses for courses' translates into the scientific context. Certainly I think it is the interpretation of the original slogan that can most easily explain its apparent intuitive appeal to economists, retain features that appear essential, and sustain methodological bite. Nevertheless the interpretation is somewhat tentative. And some may be quite unhappy with it. If so, I hope I might at least succeed in inducing an explicit and reasoned alternative from those who find it objectionable.

I have also indicated how my interpretation of this stipulation can make a difference by applying it to the question of whether or not certain familiar mathematical methods are appropriate in economics. Although the conclusion reached on this is largely negative I have not (of course) suggested that *all* arguments made against the use of formalism in economics are acceptable to anyone who advocates MHC – any more than I have suggested that criteria employed by those advocating mathematical formalism are irrelevant. Certainly bad arguments for or against the use of formalism (arguments from authority, or according to the sorts of policy options that are supported) cannot be endorsed from this (realist) perspective. At the same time, features such as clarity, rigour, and beauty can be accepted as often desirable and/or important. From the point of view in question the latter mostly pragmatic criteria must be seen as mainly insufficient in the social scientific context; the objective includes the illumination of reality.¹⁹

It is only fitting that I leave the final word on the use of mathematical methods in economics to Geoff. He acknowledges in his 1995 paper that as a 'fledgling economist' he felt the need to know about mathematical economics and like many others was often seduced by it in some form. Where exactly, then, has his 'horses for courses' reasoning on this subject taken his own thinking?

Now that, more and more, we are coming to realise that qualitative as well as quantitative change is the essence of economic processes, it is not clear that traditional mathematical techniques are the appropriate ones to capture this, even in an illustrative manner. Of course, we must continue to try to do so but we must remember that there are other, often more appropriate, languages to be used in economics as well. Keynes sensed, many years ago, that the philosophers had so refined their formal logic that they had cut the umbilical cord that connected their self-contained and consistent systems with the world they were trying to illuminate. Today, we are in danger of doing this too in economics,

LAWSON

because of an overemphasis on the use of mathematics, more to the exclusion or at least the down playing of other, more traditional, forms of analysis in economics.

(Harcourt 1995: 22)

ACKNOWLEDGEMENT

For helpful comments on an earlier version I am grateful to John Davis, Aly Fischer, Joëlle Patient, Mark Peacock, Steve Pratten, Pat Northover and Jochen Runde.

NOTES

- 1 It does on occasion also crop up in Geoff's written contributions. See, for example, Harcourt (1992, 1995, 1996).
- 2 This characterization seems acceptable, perhaps essential. For Geoff is a key figure in the post-Keynesian tradition, and those in this tradition who have questioned its nature have tended to support an explicitly realist formulation. See, for example, Arestis (1990, 1992) and Dow (1990, 1991).
- 3 Although a horse considered to have a good chance of winning may only be backed if the 'odds' seem attractive.
- 4 Of course, certain people sometimes have reasons for going to the race course other than backing a horse which will come first. It is conceivable that a jockey may even purposely avoid winning, as part of a wider attempt to defraud. Sometimes, as annually at Ascot, an apparent objective is to be seen dressed in some outlandish or 'attractive' attire, and so forth. Similarly economists may have their own alternative goals or agenda. These may relate to pleasing those in authority, demonstrating mathematical prowess, arranging data in such a way as to suggest support for some predetermined and preferred result. Some criteria accepted by economists may equally relate to questions of fashion and notions of elegance. But a large part of the appeal of 'horses for courses' is that the *primary* goal is connoted, and so I think this must be recognized as an essential aspect of it, something to carry over in drawing an analogy. Thus I infer the primary scientific goal of illuminating (revealing/understanding/explaining) some feature of reality must be an essential feature of its scientific analogue. Certainly I would expect an acknowledged post-Keynesian such as Geoff to accept this is the essential scientific goal.
- 5 Horses are used for an array of activities, ranging from ranching to breeding. The horsey slogan to express the weaker interpretation on occasion offered by Geoff would thus have to be something like 'horses according to the objective in question'. If this were the message it would seem to be at least as advantageous to stick with the formulation 'how you do it depends upon what the purpose is'.
- 6 Of course the 'How you do it depends upon what the purpose is' interpretation is even more general, with significantly less – if any – methodological bite. See below.
- 7 Perhaps this is one of those occasions on which Geoff implicitly falls back on the weaker interpretation of the slogan, i.e. 'How you do it depends upon what the purpose is'. Indeed, as I have already noted, it is difficult to see how this latter 'stipulation' could really make much difference to anything.
- 8 Let me emphasize that I am concerned here only with those statements to which

HORSES FOR COURSES

Geoff himself refers. My aim, at this point, is to see if we can get a bit further in choosing amongst them.

- 9 Samuelson writes: 'The laborious literary working over of essentially simple mathematical concepts such as is characteristic of much of modern economic theory is not only unrewarding from the standpoint of advancing the science, but involves as well mental gymnastics of a peculiarly depraved type' (Samuelson 1948: 6, quoted in Harcourt 1995: 11).
- 10 Debreu writes:

the subject of an axiomatic analysis in which primitive concepts are chosen, assumptions concerning them are formulated, and conclusions are derived from those assumptions by means of mathematical reasoning disconnected from any intended interpretation of the primitive concepts. The benefits . . . have been numerous. Making the assumptions of a theory entirely explicit permits a sounder judgement about the extent to which it applies to a particular situation. Axiomatization may also give ready answers to new questions when a novel interpretation of primitive concepts is discovered . . . Axiomatization, by insisting on mathematical rigor, has repeatedly led economists to a deeper understanding of the problems they were studying, and to the use of mathematical techniques that fitted those problems better. It has established secure bases from which exploration could start in new directions . . . Rigor undoubtedly fulfils an intellectual need of many contemporary economic theorists, who therefore seek it for its own sake, but it is also an attribute of a theory that is an effective thinking tool. Two other major attributes of an effective theory are simplicity and generality . . . Simplicity makes a theory usable by a greater number of research workers. Generality makes it applicable to a broad class of problems.

The axiomatization of economic theory has helped its practitioners by making available to them the superbly efficient language of mathematics. It has permitted them to communicate with each other, and to think, with a great economy of means. At the same time, the dialogue between economists and mathematicians has become more intense.

(Debreu 1984: 274–5, quoted in Harcourt 1995: 13)

- 11 Koopmans writes:

The appropriateness of mathematical reasoning in economics is not dependent upon how firmly or shakily the premises are established. Let us assume for the sake of argument that the attempt to establish premises or at least to explore their implications is worthwhile, that is, economics itself is worthwhile. In that case the justification for mathematical economics depends merely on whether the logical link between the basic premises economists have been led to make and many of their observable and otherwise interesting implications are more efficiently established by mathematical or by verbal reasoning.

(Koopmans 1954: 378, quoted in Harcourt 1995: 13, 14)

And further:

that the mathematical method when correctly applied forces the investigator to give a complete statement of assuredly non-contradictory assumptions has generally been conceded as far as the relations of the assumptions to the reasoning is [*sic*] concerned. To this may be added that

LAWSON

the absence of any natural meaning of mathematical symbols, other than the meaning given to them by postulate or by definition, prevents the associations clinging to words from intruding upon the reasoning process.
(Koopmans 1957: 172–3, quoted in Harcourt 1995: 14)

- 12 In a piece on 'markets and democracy', Chichilinsky writes:

the connection between [the two] is not at all clearly understood ... [It] has indeed been analyzed by literary means, but it has not been logically or mathematically analyzed in the context of a well-defined model ... [.] the foundation which is needed. Since both the theory of markets and ... of social choice have been mathematically formalised ... not an impossible task ... we need a logical foundation and a mathematical edifice to build upon these areas which are the daily concern of many people across the world ... Political economy must build an abstract and general mathematical thinking. It is the only way to assure clarity, a strong foundation and the desired advance in areas which are of great importance for intellectuals and for those whose lives depend upon it.

(Chichilinsky 1990: 27, 39, quoted in Harcourt 1995: 15, 16)

- 13 Except in a few obstinate pockets of resistance, the use of mathematics in the social sciences is now generally accepted. The reason is not to be found in the outcome of any high-flown philosophical battle but in a number of simple facts. In the first place, many branches of the social sciences are obviously, one might almost say aggressively, quantitative; demography and economics are clear examples of this. In the second place, while theories about the complex systems which are the subject matter of the social sciences can be expressed verbally, their analysis and comparison are greatly helped by formulating them mathematically. In the third place, the application of such theories must remain very general unless the terms in their relationships can be quantified. In the fourth place, mathematics provides a means of obtaining insight even into subjects whose concepts are rather vague and where precise information is hard to come by. Finally, in the social sciences we are interested not only in a description of what happens and of how the different parts of the social system are related, but also in the rational processes that lie behind effective as opposed to ineffective decisions; to a large extent these processes too can be formulated and analyzed mathematically, so that our decisions may eventually come to rest a little more on knowledge and a little less on guess work than they do at present.

(Stone 1996: 1, quoted in Harcourt 1995: 18, 19)

- 14 And it is easy enough to show that many are not accurate – see, e.g., Dennis (1994, 1995).
- 15 For I suppose it might be suggested (erroneously) that the phenomenon in question is equally well explained by a conjecture along the lines that good mathematicians by definition advocate ever extending its application in economics; poor ones who find mathematics difficult resist this – with the automatic, if implicit, stipulation that all critics be ignored. There is no doubt that this sort of reasoning is often heard or alluded to. It is of course untenable. Ignoring the obvious circularity in deciding who is able to do mathematics well, it is not even obvious that mathematics is any more difficult than social theory of a critical sort. But more to the point, people who are skilled in the use of something usually recognize its limitations; it is the poor crafts-people who never realize that there

HORSES FOR COURSES

are any. Yet most of those who employ mathematical formalism in economics seem hardly to have an inkling that any formal system is limited in its scope of application. Some, though, have reflected on the matter. Although to quote an 'authority' is not in itself an argument, such is the esteem in which economists seem to hold the mathematical abilities of von Neumann, that the following assessment (quoted by Geoff once more) may prove salutary:

As a mathematical discipline travels far from its empirical source, or still more, if it is only a second or third generation only indirectly inspired by ideas coming from reality, it is beset with very grave dangers. It becomes more and more purely aestheticising, more and more purely *l'art pour l'art*... [T]here is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its course, will separate into a multitude of insignificant branches, and that the discipline will become a disorganised mass of detail and complexities. In other words, at a great distance from its empirical source, or after much 'abstract' inbreeding, a mathematical subject is in danger of degeneration.

(Quoted in Harcourt 1995: 23)

The fact of the matter, however, is that it is not obvious that mathematical economics has *at any stage* been 'inspired by ideas coming from reality'.

- 16 For a contribution which examines Keynes' assessment of the usefulness to economics of mathematical methods more widely, see O'Donnell (1990). Despite appearances, O'Donnell's and my own interpretations of Keynes on these matters are not so different. O'Donnell is keen to demonstrate that Keynes' opposition to mathematical methods was not *a priori*, but depended on the context. This is precisely my own position. The question that remains fundamental, though, is what is the social context in which the current methods of mathematical economics have relevance?
- 17 Let me be clear about this. Mathematical economists tend to represent any conditional formulation of the 'whenever this then that' sort in question by a functional one, such as $y = f(x)$. Now functionality and conditionality are not the same thing. Mathematical economists are in error in making a conflation here. The import of this recognition is that there is much more to the claims of mathematical economics than (pure) mathematics (which I take to be the science of operations) and its results. Moreover, it is easy enough to show that contemporary mathematical economists fail to formalize most of what they profess to deal with (for example, intentions, choice, dispositions and beliefs). In short, it can be fairly said that contemporary mathematical economics is logically incoherent and unrigorous (Dennis 1994, 1995). But these failings of the project are not my concern here and do not undermine my assessment of the metaphysical presuppositions of their typical formulations. For whether the formulations of mathematical economists are (as at present) largely incoherent, or even supposing they were rather more rigorously produced, their relevance in either case presupposes that event regularities of the noted form are in evidence. In short, I am concerned here only with the tenability of the metaphysical presuppositions of contemporary mathematical economics. Although logical inadequacies are a further argument against that project, they do not undermine the assessment of the metaphysics of that project that I am making.
- 18 Of course an event regularity may come about by chance even if these conditions do not hold. Nothing can rule out such a possibility. But while such an eventuality seems *a priori* unlikely, and is certainly not in evidence, economic modellers need more than mere hope in chance occurrences. In order to proceed

LAWSON

systematically they need ways of theorizing such sought-after regularities. And for this the assumptions of atomistic individuals acting in isolated or homogeneous environments are obviously compelling.

- 19 Nor do I wish to suggest that all conceivable mathematical methods must be incapable of aiding the illumination of social phenomena. However, it would seem that methods that are capable of contributing to an understanding of social reality must at a minimum be non-deductivist, and in fact, given the nature of social reality, employ an intentional logic (and so be capable of accommodating dispositions, beliefs and intentions, etc.). I am not aware of such a system being available but I doubt that developments along these lines can be ruled out *a priori*.

REFERENCES

- Arestis, P. (1990) 'Post-Keynesianism: a new approach to economics', *Review of Social Economy*, Fall 1990, 48(3): 222-46.
- (1992) *The Post-Keynesianism Approach to Economics: An Alternative Analysis of Economic Theory and Policy*, Aldershot: Edward Elgar.
- Boulding, K. E. (1948) *Economic Analysis*, revised edition, New York: Harper.
- Chichilinsky, G. (1990) 'On the mathematical foundations of political economy', *Contributions to Political Economy*, 9: 25-41.
- Debreu, G. (1984) 'Economic theory in the mathematical mode', *American Economic Review*, 74(3): 267-78.
- Dennis, K. (1994) 'Formalism in economics', in Hodgson, G., Samuels, W. and Tool, M. (eds) *The Elgar Companion to Institutional and Evolutionary Economics*, Aldershot: Edward Elgar, vol. I: 251-6.
- (1995) 'A logical critique of mathematical formalism in economics', *Journal of Methodology*, 2(2): 181-99.
- Dow, S. C. (1990) 'Post-Keynesianism as political economy: a methodological discussion', *Review of Political Economy*, 2(3): 345-58.
- (1991) 'Post Keynesian school', in Mair, D. and Miller, A. (eds) *Comparative Schools of Economic Thought*, Aldershot: Edward Elgar.
- Hahn, F. H. (1985) *Money, Growth and Stability*, Oxford: Basil Blackwell: 10-28.
- (1986) 'Living with uncertainty in economics', *Times Literary Supplement*, August: 833-4.
- Harcourt, G. C. (1992) 'A Post-Keynesian comment', *Methodus*, 4(1): 30.
- (1995) 'On mathematics and economics', *Capitalism, Socialism and Post-Keynesianism: Selected Essays of G. C. Harcourt*, Aldershot: Edward Elgar. (Page references to mimeo, Cambridge, 1993.)
- (1996) 'How I do economics', in Medema, S. and Samuels, W. (eds) *How Should Economists do Economics*, Aldershot: Edward Elgar (forthcoming).
- Jevons, W. S. (1871) *The Theory of Political Economy*, London: Macmillan.
- Keynes, J. M. (1933) *Essays in Biography*, London: Macmillan, CW, X (1972).
- (1973) *The General Theory and After. Part II Defence and Development*, (edited by D. Moggridge) London: Macmillan.
- Koopmans, T. (1954) 'On the use of mathematics in economics', *Review of Economics and Statistics*, 36: 377-79.
- (1957) *Three Essays on the State of Economic Science*, New York: McGraw-Hill.
- Mirrlees, J. A. (1978) 'Review of Malinvaud (1977): *The Theory of Unemployment Reconsidered*', *Economic Journal*, 88 (349): 157-9.
- O'Donnell, R. M. (1990) 'Keynes on mathematics: philosophical foundations and

HORSES FOR COURSES

- economic applications', *Cambridge Journal of Economics*, 14: 29–47.
- Pigou, A. C. (ed.) (1925) *Memorials of Alfred Marshall*, London: Macmillan.
- Samuelson, P. (1948) *Foundations of Economic Analysis*, Cambridge, Mass.: Harvard University Press.
- Stone, R. (1966) *Mathematics in the Social Sciences and Other Essays*, London: Chapman & Hall.

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

Jochen Runde

Geoff Harcourt advocates a pluralist, or what he calls 'horses for courses', approach in his writings on the methodology of economics. I suspect that this attitude is partly one of temperament. For, as everyone knows, Geoff is something of an exception in a discipline not generally noted for the generosity of spirit displayed between its competing factions. Certainly I have never known him but to emphasize the positive aspects of any book, paper, or talk, whatever the methodological or ideological inclinations of its author.

Yet Geoff is clearly no supporter of an 'anything goes' approach to economic analysis. As he sees it, the main methodological alternatives are two:

One is axiomatic, for example, as Frank Hahn often says, let us see how far the assumption that the world is populated by 'greedy people' will take us. The other starts by observing behaviour, institutions, 'stylized facts' and then constructs simple models incorporating the essence of the observations in order to try to explain the original observations *et al.* Debreu, Arrow, Hahn are outstanding proponents of the first approach, Kaldor, Kalecki, Joan Robinson – also Keynes, Marx and Smith –, of the second.

(Harcourt 1995)

Geoff is of course well known as a champion of the second, 'stylized facts' approach. The many arguments he has brought to bear in this role, both theoretical and metatheoretical, are more than can be considered here. But one that recurs in his writings, one that is hinted in the passage quoted above, is that the way the world is should condition the way we think about it, where this includes our methods of explanation. As he puts it at the end of a recent paper, '*always* the guiding principle must be the economics of the problem and its importance and relevance, not what economic problem can we fit to any fancy technique that we have come across' (see also Harcourt 1992, 1993). I should like to use the present opportunity to expand on this point by

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

developing a distinction that is often glossed in economics, between abstraction on the one hand and idealization on the other. I shall argue, first, that the highly idealized nature of contemporary economic theory is closely bound up with its commitment to a particular method of explanation and, second, that the viability of this method is not something that can be settled *a priori*, that is, independently of the nature of what is to be explained.

ABSTRACTION AND IDEALIZATION

Although abstraction and idealization are often treated as the same thing in economics, I should like to argue that they are in fact quite different.¹ I shall interpret abstraction as the process of individuating and focusing on an aspect or aspects of some concrete phenomenon of interest, with the aim of concentrating attention on factors that are considered essential to it, for a particular purpose or from a particular point of view, while temporarily relegating factors that are deemed inessential into the background. Idealization in economic theory, in contrast, tends to take the form of postulating limit or ideal types and/or analysing economic phenomena as if they or their component parts exist and operate in isolation from the involvement or interference of aspects of the situation in which they arise.

Abstraction

The notion of abstraction is in many ways a natural and familiar idea. Certainly it is something that we are engaged in all the time. Just as it is impossible for the economist to comprehend the totality of any complex economic phenomenon all in one go, so it is not possible for us to comprehend the totality of whatever situation we are in at any particular moment. Our waking moments are spent constantly shifting and altering our depth of focus on different aspects of the situation we are in. When driving a car, for example, we might focus momentarily on the movements of a pedestrian, abstracting temporarily from the scenery and the conversation of our travelling companion. At other times we focus on the actions of other motorists or the speedometer, driving automatically until our attention is called away again. Yet it is surprisingly difficult to give a precise account of what abstraction consists in, perhaps not least because it seems to be a largely subconscious activity. There nevertheless seem to be three features that are fundamental to it. I shall briefly consider each in turn, bearing in mind that they will tend to be in play concurrently and tend to influence each other.

What is abstracted from, in any situation, is the totality of the given or concrete. But exactly what is abstracted will depend on the questions being asked by the person making the abstraction. The first feature of abstraction, then, is that it is always interest-relative. Take the topic of social rules and conventions – for example, one that has recently been the subject of

RUNDE

discussion from a variety of angles in a variety of disciplines.² In some places this work focuses on questions such as the ways in which social conventions contribute to social order (or disorder), passing over questions of their emergence and the social and psychological factors that govern their reproduction. In other places it is precisely these factors that are the focus of interest, leaving aside the impact of convention following on the broader social community. While the focus is on different aspects in each case and the abstractions achieved are to this extent interest-relative, then, this does not mean that what is abstracted is subjective in the sense of expressing or corresponding to nothing real.

Second, abstraction never occurs in a vacuum. The mere fact that it is possible to identify and raise questions about some phenomenon already presupposes at least some knowledge about it. So when a researcher asks a question about something, this will already be from the vantage point of accumulated experience and within a framework of existing theories and conceptions (which suggests that the object of scientific research is generally to transform and sharpen existing conceptions of phenomena). By the same token, the process by which some phenomenon of interest is individuated and, if the aim is to understand that phenomenon, the factors that give rise to it, will also depend on existing theories. To continue with the above example, it seems to be widely agreed that conventionality is closely bound up with notions of community and the public nature of conventional behaviour. This suggests that 'picking out' the conventions followed in some community will involve abstracting from what the people do alone in the privacy of their own homes, for example, or when they are asleep. And if the object was to explain how members of that community come to follow conventions, the focus might be on 'focal points' and the force of precedent, the sanctions that non-conformists face or the need people have to 'belong', rather than such factors as local climatic variations or the particular diet followed in the community concerned.

Third, what is abstracted at any particular time also depends on the level of abstraction adopted. For example, if we were interested in why a certain convention is followed in a particular region, we would in all probability have to enquire into highly specific context-dependent features of the community concerned. In contrast, if we were aiming to provide an explanation of social conventions that applies pan-culturally, we would want to be looking at features of convention-following that apply independently of any particular social context. Variations in the level of abstraction may thus help to parse the essential from the non-essential and to determine the space-time extension of the analysis. Abstractions that set spatial limits to the focus taken will limit the factors that can be taken into consideration. Abstractions that set temporal boundaries to the focus taken will set limits on the histories of any particular factors invoked. Finally, it is worth noting that the most general aspects of a phenomenon, those which accompany it most often, need not be the most

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

relevant to its identification and explanation. The apparent fact that conventions are often arbitrary in the sense that they have conventional alternatives, save on bounded rationality and lend stability to social systems may all be general features of conventions, for example, but may well not be relevant when it comes to explaining how it is that people come to follow them without explicitly agreeing to do so.

Idealization

I suggested above that idealization tends to take one of two broad forms in economic theory. The first of these is the use of limit types, 'entities, aspects or situations which are characterised by some feature that is perfect, complete or absolute in some (limiting) sense' (Lawson, forthcoming). Economic theory abounds with examples of limit types: perfect competition, complete preference orderings, perfect foresight, infinitely lived agents, perfectly divisible goods and common knowledge of rationality, to name just a few. Of course it is possible that instances of the kind of limit types postulated in economic theory may be real possibilities. For example, there may be situations in which decision-makers do in fact have completely ordered preferences over some relevant domain, such as when choosing from a simple menu in a restaurant. Typically, however, examples of this kind are the exception rather than the rule. The limit types so characteristic of economic theory might more generally be labelled fictions in the sense that they are the product of a (usually deliberate) transformation or deformation of something real into something that is a mere idea.

The second form of idealization, often flagged with the *ceteris paribus* assumption, is the practice of analysing phenomena as if existing and operating in isolation. I shall follow Mäki (1992, 1994) and call idealizations of this kind theoretical isolations.³ Theoretical isolations are typically achieved by (or are some combination of) assuming away entirely, assuming away changes in, or assuming away interdependencies between, factors that accompany and may be causally relevant to some phenomenon.⁴ Again, contemporary economic theory is replete with examples. Some instances of the first variety include partial equilibrium models and two or three sector 'economies'. Two familiar examples of the practice of assuming away changes in factors that may be causally relevant to some phenomenon of interest are the assumptions of constant tastes and fixed technology. Finally, some examples of the practice of assuming away interdependencies between different aspects of the phenomenon of interest include the separability assumptions on which the subjective expected utility theorem is based (Runde 1995a), perfect competition and, again, the representative agent 'economy'.

The two broad forms of idealization described above are closely related and may well overlap.⁵ The assumption of perfect competition, for example,

is at once a limit type and an assumption that rules out strategic interdependencies between individual agents.

Abstraction versus isolation

One way of thinking about the distinction between abstraction and idealization is that whereas abstraction involves looking at the same real phenomenon from different points of view, idealization typically involves transforming that phenomenon or one or more of its aspects into something that exists only in the realm of ideas. But this way of putting it, while perhaps not overly contentious so far as limit types are concerned, must be qualified in the case of isolations. This issue may be looked at in terms of why particular isolating assumptions are made.

There are two main possibilities. The first is the situation in which isolations are made on ontic grounds, where the phenomenon of interest or its component parts are treated as if acting in isolation because the excluded factors are deemed to be causally irrelevant to the phenomena concerned (or to be acting on them in a constant way).⁶ Isolation may here be interpreted as serving to parse the 'greater' causes of some phenomenon from its 'lesser' causes and, to the extent that this is so, there may be no great distance between abstraction on the one hand and isolation in the other. The second possibility is the situation in which isolating assumptions are made on the grounds of analytical convenience in general or mathematical tractability in particular, which, as the above examples suggest, may lead to phenomena being analysed as if acting in isolation from factors that are known to be causally relevant. In this case isolation, no less than the use of limit types, leads to the deformation of the phenomenon of interest into something that exists only in the realm of ideas (such as two-sector or single-agent 'economies'). The difference between abstraction and isolation is again marked here: to abstract an aspect of some phenomenon is not to treat that aspect as if existing and operating in isolation from the context it is abstracted from.⁷ Or alternatively, to abstract from specific aspects of some phenomenon is not to treat that which is temporarily out of focus as something that is assumed out of existence.

The distinction between abstraction and isolation is crucial when dealing with phenomena whose system properties do not consist of separable properties of parts of that system. And this will of course often be the case when dealing with social phenomena, the properties of which cannot always be comprehended without reference to their other parts and, often, particular histories. This does not of course mean that features or aspects of an internally related set of structures or processes cannot be considered individually at a moment in time, merely that they cannot be treated as isolated phenomena existing outside of time. It does mean that the comprehension of such structures and processes will generally involve examining them from different

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

levels and planes of abstraction, sometimes focusing on their particular features and passing over the general, sometimes passing over the particulars and focusing on the general. The point is that aspects that characterize some phenomenon at one level or plane of abstraction cannot be dismissed as inconsequential when statements are made about it at other levels or planes of abstraction.

WHY IDEALIZATION?

Given that idealization in economic theory almost always involves the postulation of entities or states of affairs that manifestly do not occur in the world, the question arises as to why economists resort to them at all. This question becomes all the more pressing in view of the fact that economics is routinely criticized on just this point, for making unrealistic assumptions, for concentrating on fictitious 'toy' economies, and so on. The standard response to charges of this kind is to point out that theories and models are necessarily descriptively false, simplifications, exaggerations, isolations, approximations and so forth. We have already noted that it is not possible to make sense of complex social phenomena by attempting to comprehend them in all their aspects in one go. But we have seen that the role of abstraction is precisely to look at phenomena from a particular point of view, focusing on some of their aspects while temporarily leaving others out of focus. Why then, given that abstraction already does this particular job, does contemporary economic theory put so much weight on idealizations?

I should like to argue that the answer to this question is closely connected with the acceptance of an unwritten and largely unchallenged rule in orthodox economic theory, that the only way to arrive at a precise 'scientific' theorization of some economic phenomenon *X* is to deduce a statement about that phenomenon (the *explanandum*) from a set of premises that contains statements about initial conditions and at least one law necessary for the deduction (the *explanans*).⁸ By 'law' I mean a statement of an association or regularity that always or almost always holds, and which may simply be *a priori* ('agents have preferences' or 'firms maximize profits') or arrived at on an empirical basis. Such laws are not only a precondition for, but also the goal of, deductivist explanations. For if we can 'explain' some phenomenon by showing that it is logically entailed by a set of statements about initial conditions and laws, then that 'explanation' has the same logical form as a statement of a law (namely, 'whenever these conditions, then that outcome'). It turns out, however, that if the deductivist ideal is to be in any way achievable, then at least three strong closure conditions have to be met.⁹

The first condition is that the elementary units referred to in the *explanans* (individual consumers or firms, for example) must be organized or constituted in such a way that they always behave in the same way under the same circumstances. One way of ensuring this is to require that the elementary units

exhibit a constant internal structure. The relevant condition is therefore sometimes called the intrinsic condition for closure. Intrinsic constancy of the elementary units of analysis is, however, not sufficient to ensure determinacy at that level (and therefore, in most cases, at the aggregate level). This requires a further reducibility assumption, namely that the individual units of analysis be constituted so as to behave in the same unique way in the same circumstances.

The second condition that has to be met is that the phenomenon of interest must conform to some principle of composition that ensures that the behaviour of the elementary units referred to in *explanans* translates into a determinate outcome (or set or spread of outcomes) at the aggregate level. This is called the aggregational condition for closure and, in general, requires a detailed specification of the rules that govern the interaction of the elementary units of analysis. Again, if the aim is to deduce a unique outcome, this time at the aggregate level, the aggregational condition must be specified in a way that is strong enough to ensure reducibility.¹⁰ Finally, the third fundamental condition that has to be met, the extrinsic condition for closure, is that the *explanandum* phenomenon is effectively isolated from the factors not explicitly taken into account in the analysis itself. The extrinsic condition for closure is met if all extrinsic factors are either physically isolated from the system, or impact on the system in a constant way. In terms of probabilistic formulations the intrinsic condition for closure is met if the extrinsic factors impinge on the system in a way that is constant on average and is not correlated with those explicitly taken into account in the analysis.¹¹

In the following section I shall consider some implications of proceeding on the basis of the assumption that these conditions are met. For the moment, I merely want to suggest that the main reason why contemporary economic theory is shot through with fictions of the kind discussed above is that they are necessary to meet the three closure conditions necessary to achieve explanations on deductivist lines.¹² A few examples will suffice to show what I mean. Perhaps the most familiar idealizations used to satisfy the intrinsic condition for closure are the rational utility maximizer and the black-box theory of the firm. In both cases the 'agents' are specified such that they are largely devoid of internal structure and always behave in the same (unique) way in the same circumstances (the utility maximizer is assumed to have fixed tastes, for example, represented by 'well behaved' indifference curves that ensure determinacy). Some familiar examples of the idealizations used to satisfy the aggregational condition for closure include the hypothesis of perfectly competitive equilibrium, the various solution concepts of game theory and the practice of side-stepping the aggregation problem altogether by modelling an economy in terms of the maximization of a representative agent's utility function (see Hoover 1993: 696–8; Kirman 1992). And finally, the extrinsic condition for closure is typically met by the (often unstated) assumption that the phenomenon being explained is operating in isolation

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

from any potentially disturbing factors, that is, that all causally relevant factors have been included in the set of premises from which the phenomenon is being deduced, or that the disturbing factors impact on the system in a constant way.

THE METAPHYSICS OF CLOSURE AND ISOLATION

I have already noted that the use of the deductivist method of explanation is largely unquestioned in the economic theory literature and, consequently, that the assumptions on which it rests are rarely examined. In the previous section I argued that one of the key characteristics of deductivism is its reliance on *a priori* and/or empirical laws, both as a condition for and the goal of (deductivist) explanation. It would therefore seem that proponents of the deductivist method are committed to a particular metaphysics, namely that laws or regularities of the postulated kind exist. Following Lawson, this metaphysical position might be called 'regularity determinism' (that for every event or state of affairs y there exists a set of events or states of affairs $x_1, x_2, \dots, x_n$, such that y and $x_1, x_2, \dots, x_n$ are regularly conjoined under some formulation), or its probabilistic analogue 'regularity stochasticism' (that for every event or state of affairs y there exists a set of events or states of affairs $x_1, x_2, \dots, x_n$, such that y and $x_1, x_2, \dots, x_n$ are regularly conjoined under some set of 'well behaved' probabilistic formulations).

As economists know all too well, however, the problem is that regularities of either of the above kinds are rarely found in the social world. That this is so should not be surprising because the kinds of phenomena that they are interested in – price and interest rate levels, unemployment, the organization of industries and firms, and so on – are typically the product of an ever-changing mix of causal mechanisms that may at different times amplify, impede or override others. Indeed economists are often very good at giving *ex post* rationalizations of predictive failures in terms of the operation of some unanticipated causal mechanism. I gave the formal conditions that have to be met to ensure an event regularity in the preceding section. In practice (and ignoring astronomy) these conditions are typically only met by actively intervening in the world – such as the complex set of isolations that leads to the light turning on (almost) every time I flick the switch. The paradigm case, of course, are situations of experimental control or 'material isolations', where a causal mechanism is 'sealed off' from disturbing phenomena so as to ensure a one-to-one relationship between that mechanism being triggered and some set of its effects. The main alternatives open to defenders of the deductivist approach, from this perspective, seem to be three.

The first is to argue that deductivist economic theory has value despite the paucity of deterministic or 'well-behaved' probabilistic laws in the social world and despite the fact that it does not seem to be well-suited to deriving

RUNDE

conclusions that relate in any obvious way to anything to be found 'out there' in the economy. Hahn, for example, turns defence into attack by claiming a virtue of formal theorizing that it

leaves the practitioner with the suspicion that, what I suppose was once a programme for economics, may be impossible to carry out ... It is not just that there are many variables and complex interactions: it is that the constraints on what is possible seem much weaker than is the case with physical processes ... A theorist then will be surprised if there are 'laws of economies,' in the sense of propositions holding universally, waiting to be discovered.

(1985: 26–7)

But the whole game then changes. If economic theory is in fact not based on deterministic or 'well-behaved' probabilistic laws, and if the 'explanations' produced, while having the logical form of statements of such laws, are not even expected to have any obvious empirical counterpart, then the bearing of such theory on its subject matter will at best be oblique. In particular, it would then seem necessary to justify it, as Hahn in fact does, on grounds other than its actual or potential success in predicting and/or explaining real economic phenomena (see Runde 1996).

But most economists would probably like to see a firmer empirical connection between their models and the world than Hahn might be prepared to countenance. The second possibility, then, is to attempt to intervene in the social world and make the phenomena under investigation more like that presupposed by the deductivist approach. Recent years have witnessed growing interest in attempts to do just this, to test the predictions of microeconomic models in controlled experimental settings. This approach involves attempting to recreate the conditions specified by the assumptions of a particular model in a laboratory setting, by specifying an 'environment' (tastes and technology), an 'institution' (the language by which agents communicate, the order in which they move and the rules under which messages become contracts and thus allocations) and behaviour (that agents are utility maximizers, choose as if they are risk averters, and so on). Usually it is the assumptions about agent behaviour that are tested, their not being falsified lending to their support given the environment and institution posited. The findings of this literature – for example, that institutions matter, that people often manage to optimize in market interactions without consciously attempting to do so, that less information may be better than more (see, for example, Smith 1991, 1994) – are certainly interesting, often replicable and usually at variance with standard neoclassical theory. But from the perspective of the position developed in the current paper, the value of this work lies not so much in finding regularities but in creating the conditions under which causal mechanisms can be empirically identified. As Elster puts it: 'Laboratory experiments have the great value of isolating and controlling

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

factors so that we can see the mechanisms in their pure form, but they are of limited help in explaining the tug of war between mechanisms that is the rule in social life' (1989a: 216). The point is that the causal mechanisms identified in the laboratory will, if triggered, also exercise their powers outside of it, whether or not the regularities that aided their identification in the laboratory are also manifest outside it (see also Cartwright 1989: chapter 4).

The third and possibly most common argument is that the deductivist models that are currently on offer should be regarded as preliminary steps on the path towards progressively more complex and general models that will in future approach the deductivist ideal. The idea here is that empirical success will eventually be achieved by the method of successive approximation or what Mäki (1994: 151–2) calls 'horizontal deisolation' (the process of adding factors at a particular level of abstraction – sometimes by relaxing the idealizations that helped to neutralize them – to arrive at a 'more comprehensive, more encompassing picture of the causal nexus of the phenomena under consideration'). Again, however, this approach presupposes that the phenomena it is applied to are of a particular kind. It assumes, often without argument, that the phenomena of interest are such that they can be studied as an amalgamation of separable closed systems, in effect that they are decomposable into separable components whose (predictable) effects can then subsequently be aggregated to arrive at an outcome for the system as a whole.¹³

CONCLUDING REMARKS

I have characterized orthodox economic theory in terms of its adherence to the deductivist mode of explanation and argued that the limit types and isolations it employs are typically not abstractions of real social phenomena, but idealizations designed to shore up the closure conditions needed to facilitate deducibility. What, then, if the social world is after all open and highly internally related? If the unspoken assumption that significant aspects of the social world can be comprehended as if they were closed systems were to be rejected, this would seem to call for at least considering the possibility of adopting an alternative model of explanation. This is a subject that would lead beyond the scope of the present paper, although on the basis of the arguments given above, there would seem to be a strong case for an approach that emphasizes abstraction over isolation, as well as the more context-specific 'horses for courses' orientation that Geoff Harcourt favours (see Hamouda and Harcourt 1988: 25). I should merely like to note that there *is* such an alternative, namely the view that to explain some phenomenon is to give information about its causal history, or where a type of phenomenon is being considered, to give information about the types of causal mechanisms that produces it.¹⁴

RUNDE

ACKNOWLEDGEMENT

I am grateful to Paul Anand, Jörg Bibow, Johan Fedderke, Geoff Hodgson, Eckehard Rosenbaum, Arnis Vilks and Marianna Vintiadis for their helpful comments on earlier drafts of this paper.

NOTES

- 1 This section draws heavily on the manuscript of Tony Lawson's forthcoming book *Economics and Reality*, particularly on the subject of abstraction. I have not provided detailed references because of the provisional nature of the manuscript when I had access to it.
- 2 For example, Giddens (1984), Gilbert (1992), Hayek (1973), Lewis (1969) and Orléan (1989).
- 3 I shall, however, not follow Mäki in using the term 'isolation' to cover what I call abstraction (and what he calls 'vertical isolation').
- 4 Mäki defines theoretical isolations as occurring where 'a system, relation, process or feature, based on an intellectual operation in constructing a concept, model, or theory, is closed off from the involvement or impact of some other features of the situation' (1992: 325). Theoretical isolations may be thought of as the thought-experiment analogue of material isolations, that is where 'a real system, relation, process, or feature, based on a causal intervention in the processes occurring in the world, is materially isolated from the involvement or causal interference of some other real entities' (p. 325).
- 5 It may be argued that there is a third form of idealization that is neither a limit type nor an isolation, namely when economists resort to purely fictitious entities such as the Walrasian auctioneer, or Friedman's money-dropping helicopters. I shall pass over idealizations of this kind here.
- 6 See Simon (1969: 101–3).
- 7 I am therefore reluctant to follow Cartwright (1989: 187) in interpreting the difference between idealization and abstraction as one between *changing* factors or properties and *subtracting* factors or properties. On the present account, subtracting properties or features is a form of idealization where the phenomenon of interest is internally related with the factors that are 'subtracted' from it.
- 8 This characterization is easy to confirm by paging through any intermediate or advanced theory text (see Hahn (1985) for an explicit statement). Lawson (forthcoming) regards what he calls deductivism – essentially the deductive-nomological model of explanation (Hempel 1965) – as *the* method of orthodox economic theory. Of course most economic theories do not meet the strict requirements of the model since, amongst other things, they are not based on exceptionless laws. They must therefore be seen as better or worse approximations to full deductive-nomological explanations. It is in recognition of this fact that Hausman (1992) characterizes contemporary microeconomic theory as 'inexact'. But the 'inexactness' of the empirical laws on which a (sound) deductive structure is founded does of course not disturb the logical integrity of that structure. As Hahn puts it: 'It seems of things which are logically true that they are also true. Of course, in economics these are contingent truths – contingent on the truth of axioms' (1984: 6–7). What the recognition of such 'inexactness' does is to qualify such structures as explanations/predictions of real economic phenomena, something Hahn appears to give up on anyway (see below).

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

- 9 The present account of these conditions builds on the one given in Lawson (1995a) and supersedes the one given in Runde (1996).
- 10 This reducibility condition is obviously not met in models with multiple equilibria. But models of this kind are no less in the deductivist paradigm for that: the difference is that they are underdetermined, in the sense that the *explanantia* are insufficient to generate unique outcomes.
- 11 See Lawson (1995b) for a discussion of probabilistic versions of the deductivist approach.
- 12 This line of argument is by no means new. See Sen (1986), for example, who discusses how the standard notions of rationality, maximizing and equilibrium are used to circumvent the 'choice problem' (i.e. shoring up the intrinsic condition for closure) and the 'interaction problem' (i.e. shoring up the aggregational condition for closure).
- 13 Mäki himself recognizes that

the legitimacy of strong isolations in general can be questioned altogether on the basis of organicist metaphysics. It is possible to hold an organicist view of the constitution of the economy according to which the nature of an element is dependent on its interrelations with other elements. This is the stance adopted by some institutionalist economists who subscribe to what they often call 'holism'. By this they mean the idea that the primary and undistorted object of study in economics should consist of 'organic' social wholes as intertwined sets of institutional structures. Accordingly, the behaviour of separate individuals or markets or even a narrowly conceived 'economy' is not a legitimate object of analysis ... This point relates to a major problem involved in the method of isolation as used in studying social and economic phenomena. This is the question whether the causes of economic phenomena are combined 'mechanically' or 'chemically', to use J.S. Mill's phrases. When causes combine 'mechanically', their effects can be 'added up' like vectors, and the outcome is an additive 'sum' or 'resultant' of the effects of those causes taken singly. On the other hand, when causes are combined 'chemically', some qualitatively novel, emergent outcomes ensue ... It is easier for the method of isolation to deal with the domain of 'mechanics' than that of 'chemistry'. No wonder, therefore, that standard neoclassical economists do their work most of the time as if economics were 'mechanics'. The challenge they are requested to meet concerns the relative adequacy of the 'mechanical' versus the 'chemical' metaphysics and of the methods respectively supported by them in the study of the economy.

(1992: 348–9)

- 14 For more on this alternative, see Lewis (1986), Lipton (1991), Miller (1987), Elster (1989b), Runde (1995b) and especially Lawson (forthcoming).

REFERENCES

- Cartwright, N. (1989) *Nature's Capacities and their Measurement*, Oxford: Oxford University Press.
- Elster, J. (1989a) *The Cement of Society: A Study of Social Order*, Cambridge: Cambridge University Press.
- (1989b) *Nuts and Bolts for the Social Sciences*, Cambridge: Cambridge University Press.

- Giddens, A. (1984) *The Constitution of Society. Outline of the Theory of Structuration*, Oxford: Polity Press.
- Gilbert, M. (1992) *On Social Facts*, Princeton, N.J.: Princeton University Press.
- Hahn, F. (1984) *Equilibrium and Macroeconomics*, Oxford: Basil Blackwell.
- (1985) *Money, Growth and Stability*, Oxford: Basil Blackwell.
- Hamouda, O. F. and Harcourt, G. C. (1988) 'Post Keynesianism: from criticism to coherence?' *Bulletin of Economic Research*, 40: 1–33.
- Harcourt, G. C. (1992) 'The legacy of Keynes: theoretical methods and unfinished business', in Sardoni, C. (ed.) *On Political Economists and Modern Political Economy: Selected Essays of G.C. Harcourt*, London: Routledge, 235–49.
- (1993) 'Mathematics: what should non-mathematicians know? On mathematics and economics'. Paper presented at the Science and the Human Dimension Conference, Jesus College, Cambridge 3–5 September 1993.
- (1995) 'How I do economics', manuscript, Cambridge University.
- Hausman, D. M. (1992) *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.
- Hayek, F. (1973) *Law, Legislation and Liberty, vol. 1: Rules and Order*, Chicago, IL: Chicago University Press.
- Hempel, C. (1965) *Aspects of Scientific Explanation*, New York: Free Press.
- Hoover, K. D. (1993) 'Causality and temporal order in macroeconomics or why even economists don't know how to get causes from probabilities', *British Journal for the Philosophy of Science*, 44: 693–710.
- Kirman, A. P. (1992) 'Whom or what does the representative individual represent?' *Journal of Economic Perspectives*, 6: 117–36.
- Lawson, T. (1989) 'Abstraction, tendencies and stylised facts: a realist approach to economic analysis', *Cambridge Journal of Economics*, 13: 59–78.
- (1995a) 'A realist perspective on contemporary "economic theory"', *Journal of Economic Issues*, 29: 1–32.
- (1995b) 'The "Lucas Critique": a generalisation', *Cambridge Journal of Economics*, 19: 257–76.
- (forthcoming) *Economics and Reality*, London: Routledge.
- Lewis, D. (1969) *Convention: A Philosophical Study*, Cambridge, Mass.: Harvard University Press.
- (1986) 'Causal explanation', *Philosophical Papers, Vol. II*, Oxford and New York: Oxford University Press.
- Lipton, P. (1991) *Inference to the Best Explanation*, London: Routledge.
- Mäki, U. (1992) 'On the method of isolation in economics', *Poznań Studies in the Philosophy of the Sciences and the Humanities*, 26: 317–51.
- (1994) 'Isolation, idealization and truth in economics', *Poznań Studies in the Philosophy of the Sciences and the Humanities*, 38: 147–68.
- Miller, R.W. (1987) *Fact and Method: Explanation, Confirmation and Reality in the Natural and the Social Sciences*, Princeton, N.J.: Princeton University Press.
- Orléan, A. (1989) 'Mimetic contagion and speculative bubbles', *Theory and Decision*, 27: 63–92.
- Runde, J. (1995a) 'Risk, uncertainty and Bayesian Decision Theory: a Keynesian view', in Dow, S. and Hillard, J. (eds) *Keynes, Knowledge and Uncertainty*, Aldershot: Edward Elgar, 197–210.
- (1995b) 'Assessing causal explanations', unpublished paper, Cambridge University.
- (1996) 'Keynesian methodology', in Harcourt, G. C. and Riach, P. (eds) *Keynes's General Theory Second Edition*, London: Routledge.

ABSTRACTION, IDEALIZATION AND ECONOMIC THEORY

- Sen, A.K. (1986) 'Prediction and economic theory', *Proceedings of the Royal Society*, London, A 407: 3–23.
- Simon, H.A. (1969) *The Sciences of the Artificial*, Cambridge, Mass.: MIT Press.
- Smith, V. (1991) 'Theory, experiment and economics', in Smith, V. (1991) *Papers in Experimental Economics*, Cambridge: Cambridge University Press: 783–801. Reprinted from the *Journal of Economic Perspectives*, Winter, 1989.
- (1994) 'Economics in the laboratory', *Journal of Economic Perspectives*, 8: 113–31.

MARKETS, MADNESS AND MANY MIDDLE WAYS

Some reflections on the institutional diversity of
capitalism

Ha-Joon Chang

INTRODUCTION

Many of us know and respect Geoff Harcourt for his role in those highbrow theoretical debates of modern economics such as the Capital Controversy, but throughout his career he has always been more than an ivory tower economist and has been constantly engaged in many policy debates. His active engagement in policy debates partially reflects the fundamentally policy-oriented nature of the Keynesian economics that he has studied and taught all his life, but also reflects his concerns, as a dedicated social reformer in the Christian socialist mould, about inequality, injustice, instability, and conflicts that still rule our societies.

In the last few years, Geoff Harcourt has written a number of articles which synthesize his previous works in various areas of economic policy and sketch out what he sees as a more just and rational alternative to the current orthodoxy of neo-liberal free market 'madness' – such as his second Donald Horne lecture, entitled 'Markets, Madness and A Middle Way', delivered in 1992 in Australia, and the 'sequels' to the piece, such as 'Macroeconomic Policy for Australia in the 1990s' and 'A "Modest Proposal" for Taming Speculators and Putting the World on Course to Prosperity'. This essay aims to take his discussions in these papers of a 'middle way' one step further, and argues that recognizing the existence of a number of 'middle ways' and trying to understand each of them better will enable us to think about some interesting theoretical and empirical issues which have received inadequate attention until now.

THE RISE AND FALL OF THE 'MIDDLE WAY'

The notion of the 'middle way', or the 'third way' as it is also known, has been with us since the establishment of the socialist economic system

MARKETS, MADNESS AND MANY MIDDLE WAYS

following the 1917 October Revolution in Russia. The challenge posed to the capitalist system by the socialist system was not simply that it claimed to have different objectives – ‘fuller’ and more ‘rational’ use of resources, more equitable income distribution, more equal life chances (through universal provision of education, health, etc.), and so on – but it was also that it tried to achieve its objectives on the basis of principles of coordination which were entirely different from those prevailing in the capitalist system. The socialist system denied the role of profit motives in the accumulation of resources and their allocation amongst alternative uses (‘production for use value rather than for exchange value’), substituted the ‘anarchic’ capitalist coordination of the market with a more conscious and more ‘orderly’ coordination through central planning, and in some cases (notably in the former Yugoslavia) tried to replace the hierarchical management of the capitalist enterprise with a more democratic and participatory form of management.

To the advanced capitalist countries during the interwar period, which were failing in a spectacular way to achieve full capacity utilization, full employment, economic stability and economic growth, such challenge was indeed formidable – especially when combined with the growing strength of labour movements and left-wing political parties in their own societies. One response to this challenge was, of course, the reassertion of the old liberal policy agenda through the adherence to the now defunct *laissez-faire* doctrines (a balanced budget, the Gold Standard, etc.), but there were also various intellectual and political movements that wanted to save capitalism from itself through institutional reforms and the introduction of more centralized coordination (if not outright physical planning) by the government. The group of British economists around John Maynard Keynes, the American New Dealers, and the Swedish social democrats such as Gunnar Myrdal are the well-known representatives of such movements.

These reformers acknowledged, in various degrees, that the increasing importance of large-scale organizations in the modern economy (be they large firms or labour unions) requires a higher degree of conscious centralized coordination than was the norm in the *laissez-faire* phase of capitalism. In this vision of reformed capitalism, an enlightened government works with large organized groups in order to overcome the instability, stagnation, and inequality of the market economy. It is a vision of the world where neither the ruthless competitive struggle between small players, coordinated through the anonymous forces of a free market, nor mechanical bureaucratic management by a planning hierarchy dominates. For these reformers, the fundamental shifts in the political balance of power and the changes in the institutional set-up of capitalism made it necessary to strike a class compromise and to increase centralized coordination, but not to the point of killing off entrepreneurial spirit by abolishing private property and of abolishing the principle of market coordination altogether.

Although this vision of the middle way did make some progress in the interwar period, especially with the launch of the New Deal in the USA and the beginning of over half a century's unbroken Social Democratic rule in Sweden, its full realization had to wait until the end of the Second World War. The new economic and political orders established in the advanced capitalist countries after the War emphatically rejected the model of *laissez-faire* capitalism that had failed so spectacularly during the interwar period. The political discrediting of the traditional liberals whose cherished system had failed to deliver prosperity and stability, not to speak of preventing the rise of extremist political forces and then finally the War, allowed the emergence of the so-called 'corporatist' regimes, which allowed a power-sharing between the reform-minded centre-right parties and the non-revolutionary centre-left parties backed by strong organized labour.

Needless to say, there were a number of influential liberal intellectuals, such as Mises (1929), Hayek (1944), Friedman (1962), Buchanan and Tullock (1962), who saw in this new 'collectivist' political and economic order a grave threat to 'free society' (for a fascinating analysis of these arguments from a historical perspective, see Hirschman 1991). They moreover argued that there is no middle way, as, given the interdependence between different policy areas, a government which is serious about achieving the aims of its policies will have to extend the boundaries of its intervention to the point where the economy will become fully planned from the centre in all but name (the so-called slippery-slope argument). These criticisms were to become influential later, but not just yet.

During the first quarter century after the Second World War, the corporatist regimes in the advanced capitalist economies used a wide range of policy tools to change capitalism into what they perceived as something more rational, stable, and 'kind'. Aggregate demand management, the welfare state, public enterprises in strategic industries, indicative planning, and (in some countries) active selective industrial policy through various forms of subsidies and protection were the measures – unknown or unacceptable to the supporters of the old liberal doctrine – but actively used by most advanced capitalist countries during this period. (Shonfield (1965) is a classic work discussing the evolution and the operation of this policy regime.)

This period also saw the rise of interventionist policy regimes in the newly independent developing capitalist countries. In many developing countries, the governments took a very active role in order to pull their economies out of their places in the traditional international division of labour, which condemned their countries to the disadvantageous role of primary product exporters. This structural shift, it was thought, called for industrialization. And for this purpose, these governments used a wide range of measures, in various degrees, such as investment planning, large-scale public investments in infrastructure and heavy industries, tariff protection, quantitative restrictions on trade, controls on foreign investments and technology transfer, and

MARKETS, MADNESS AND MANY MIDDLE WAYS

in some cases – notably the East Asian newly industrializing countries (NICs) – export subsidies.

During the following three decades, often dubbed the 'Golden Age of capitalism', the advanced capitalist countries achieved unprecedented levels of economic growth, stability, and equity simultaneously on the basis of such an interventionist policy regime (see Marglin and Schor 1990; Cairncross and Cairncross 1992). Most developing countries also achieved rates of output growth and industrialization which were far beyond what they had ever achieved before, and even in excess of what the developed countries had achieved in their earlier stages of development. Thanks to the success of interventionist policy regimes across the world, the middle way became established as the organizing principle of most capitalist economies during this period. To many, it indeed seemed to provide a happy medium between the suffocating totalitarianism and inefficiency of communism and the systemic instability, stagnation, and inequality of *laissez-faire* capitalism.

However, the notion of the middle way has become distinctly unpopular more recently. Economic performances deteriorated across the world after the 1970s – partly thanks to the very success of the earlier economic developments (e.g., drying up of surplus labour in the advanced countries, saturation of the domestic market for many import substitution industries of developing countries) – and the political consensus that had bolstered the early postwar policy regimes was now seriously challenged. Following this collapse of the Golden Age after the first oil shock, a new politico-economic doctrine known as neoliberalism soon emerged as the dominant economic and political ideology (see essays in Chang and Rowthorn, 1995, for some critical evaluations of neoliberalism).

According to the neoliberals, the existence of what they see as a grossly over-extended state not only threatens personal freedom and introduces arbitrariness in the activities of the government (Mises, Hayek, Buchanan), but also opens the door for the appropriation of the state apparatus by sectional interest groups, including the politicians and the government bureaucrats themselves (Stigler, Niskanen, Olson). Rejecting the corporatist philosophy of the Golden Age, which was based on the notion of 'antagonistic cooperation' between organized groups (capital, labour, farmers, etc.), the neoliberals called for the curtailment and restraint of the state activities and the weakening (if not total disbandment) of those corporate groups, which to them were little more than covert forms of cartels advancing 'special interests'.

Although very few of them actually advocate a full return to the 'nightwatchman state' in the classical mould, the neoliberals emphatically reject the notion of the middle way, the pursuit of which to them has been the source of many current economic and social ills. They argue that the over-extension of the state and the ever-increasing state regulations (often

implemented due to the pressures from special interest groups) blunt the economic incentives to work hard and try new things, thus creating inefficiencies and rigidities in the economy that harm its performance. Therefore, in order to revitalise the economy, they argue, it is necessary to restrain the state and liberate individual choices and initiatives from its suffocating grips, through policies such as privatization, deregulation, and budget cuts (for a typical statement of such a policy agenda, see Giersch 1986). Privatization is regarded as necessary in order to restore the profit motive as the motor force behind efficiency and productivity growth. Deregulation, it is argued, would free the entrepreneurs from the straitjacket of government regulations and give them more chances to exercise initiatives and take risk. Tax cuts and a balanced budget (which, together, amounts to budget cuts) are recommended in order to stop the draining away of resources from the productive and efficient private sector to the unproductive and inefficient public sector, to improve the incentives to work hard and invest, and to allow people to exercise more choices in deciding their lifestyles.

IS THERE STILL A MIDDLE WAY?

The neoliberal revolution started in the late 1970s in the advanced capitalist countries which were traditionally more open to liberal ideas, such as the US and the UK – although a few years before them, under General Pinochet, Chile had embarked on the same path, only with more ruthlessness and speed. It then spread to other advanced countries throughout the 1980s – albeit its popularity in these countries was less than that in the US and the UK. Many developing capitalist countries maintained their early postwar policy regime of state-led industrialization (somewhat misleadingly called import-substitution industrialization) until the early 1980s, thanks to the recycled petrodollar, despite deteriorating internal and external economic conditions since the mid-1970s. However, they finally had to succumb to the pressures from inside and outside to restructure their economic policy regimes, when international finance dried up for most of them following the Mexican default in 1982. The spread of neoliberalism reached its peak when the ex-socialist countries of Eastern Europe and the former USSR decided to ditch the ‘second way’ and fully embrace the neoliberal doctrines in their most radical forms.

As is now becoming clearer, the neoliberal experiments have rarely delivered what they promised. The neoliberal policies in the US and the UK, the advanced capitalist economies which made the most ‘progress’ in this regard, may have produced some short-term efficiency improvements in some areas, but on the whole have failed to improve the long-term performance of the economy – and all these at the costs of increased inequality in income distribution, higher unemployment, and increased instability of the macro-

MARKETS, MADNESS AND MANY MIDDLE WAYS

economy (at least partly due to the increase in speculative financial activities following financial deregulation). Neoliberal reforms in most developing countries also have failed in general to improve their long-term economic prospects, and have sometimes even led to disastrous results – as seen in the early 1980s economic crisis in Chile or the current Mexican crisis. And what has been happening in many former socialist countries since they embraced the most naive form of neoliberalism testifies to the limitations of the neoliberal policy package, especially in its extreme form – steep decline in the level of activities, rising unemployment, decaying of public services (law and order, health, basic R&D), increasing income inequality, and in some countries, the general disintegration of the basic social fabric.

It is the neoliberal doctrine which created this deplorable state of the world that Geoff Harcourt has recently set himself to criticize. In those papers that we mentioned at the beginning of this article, he delivers some damning criticisms of a doctrine which glorifies individualistic competition at the cost of almost all forms of cooperation, encourages unproductive speculative activities over productive entrepreneurship, is willing to lay idle manpower and capacity in an almost obsessive pursuit of low inflation, and actually damages the long-term growth prospects of the economy by excessive cuts in public spending on education, infrastructural investments and R&D in the name of fiscal prudence and widening of individual choices.

The resulting society resembles increasingly the one which Geoff Harcourt and many of his generation of economists, and before them people like Keynes who were their inspirations, have devoted their professional and personal lives to reform. High unemployment, increasing social and economic inequality, reductions in the long-term growth potential of the economy due to the neglect of investment, research, training and education, and other features of this 'brave new world' are exactly the features of capitalism which these economists wanted to eliminate through the construction and development of the middle way, based on class compromise, social consensus, and commitment to long-term productive investment in human and physical assets.

Geoff Harcourt rightly feels indignant at the current situation, which has created so much unemployment, inequality, and the incentives for unproductive entrepreneurship, in the name of increasing efficiency and growth, both of which frequently have failed to materialize. He outlines a proposal to steer our economies away from what he sees as a madness and back to a sensible middle way. He argues for the restoration of full employment and growth as top policy objectives and the provision of appropriate policy measures. He also calls for the restoration of productive, committed, long-term-oriented entrepreneurship as the driving force behind capitalism. This, according to him, requires macroeconomic measures that will reduce uncertainties which hamper long-term productive investments, on the one