Book Reviews

Ritson, C. and Harvey, D. (eds) (1991), **The Common Agricultural Policy and the World Economy,** CAB International, Wallingford. Pp.354, ISBN: 0 85198 688 9.

This is a collection of articles dealing with various aspects of the Common Agricultural Policy (CAP) of the European Community (EC) by leading United Kingdom (UK) agricultural economists. The topics covered include the history of the CAP and its impact on consumers, the food industry, the countryside, and relations with developing countries, the United States (US), the Mediterranean countries, and the world in general. There is also some discussion of CAP reform and alternative support measures.

The book is a useful introduction to the history of the CAP and how it works. Andrew Fearne gives a detailed 49 page history of the CAP, and other contributors (Ritson, Thomson, Hubbard and Harvey) discuss the nature of the CAP and related issues. Contributions from Buckwell, Lingard and Hubbard, Josling, and Swinbank and Ritson, deal with various international impacts of the CAP, including world trade, developing countries, the US, and the Mediterranean countries, respectively.

The most original and up-to-date contribution is that of the trans-Atlantic Tim Josling, on US-EC relations and the CAP. He explains the relative positions adopted, and discusses the progress of the Uruguay Round up to the end of 1991.

Some very specific coverage is given to UK-related aspects of the CAP. This includes a contribution from Martin Whitby on the environmental impact of the CAP on the UK countryside, and from Charles Capstick on 'British Agricultural Policy Under the CAP'.

The focus on the view from the UK is the book's major limitation, since it is Germany and France which are the CAP's main financier and producer of surpluses respectively. However, to some extent there is less of an open debate in Germany and France on the negative consequences of the CAP. There is a relatively short contribution from Fearne on the CAP decision-making process, which deals only with the supranational level and not the national forces which drive the CAP. Public choice theory is not applied in the analyses undertaken: Hubbard and Ritson refer (p. 298) in this respect to 'a failure of the academic work to catch up with the changed policy environment'. The contributions to the volume are in the UK tradition of providing historical background and economic appraisal of policies and alternatives, rather than political economy.

There is no discussion in the book of the effects of German unification, and little sense of the EC 'big picture' into which the CAP fits, or of the impact of future structural changes such as widening the EC policy mix, giving more power to the European Parliament, and expanding the Community. No advance intimation is given of the reform measures agreed upon this year with respect to cereal, dairy and beef prices, and a shift from price support towards direct income support, and the contributions in the volume do not greatly assist in understanding the factors and processes behind the reform. Nevertheless the book is still possibly the most up to date CAP primer available, and will remain a key UK reference.

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Workman, J.P. (1986), **Range Economics,** Macmillan Publishing Co., New York. Pp. xvi + 217, $US34.50, ISBN: 0-02-948810-9.

The title 'Range Economics' may be partially misleading to potential Australian purchasers seeking a specialist text on 'rangelands' economics. This arises through the different interpretations of what might constitute 'range' production in North America and that traditionally applied to this country. That is, the concept of 'range' and the effective scope of the book should be locally widened to include all extensively grazed pasture lands, including those of the 'high rainfall' and 'wheat-sheep' zones, where broad-acre agriculture is not pursued because of environmental limitations or

landholder preferences. This would be important because, in many respects, the principals and examples used are more appropriate to the latter regions than to the semi-arid and arid regions which make up the Australian rangelands. A more appropriate title than 'range economics' might then be 'extensive livestock production economics'.

The book was written in response to a need perceived by the (American) Society for Range Management (SRM) for a range economics textbook for senior level university undergraduate students studying range management, property managers and persons whose research or professional interests encompassed management of extensive grazing lands. An important part of the SRM's brief was that such a book was to provide a state-of-the-art summary of established economics principles and concepts relevant to range management. As such it is not intended to be used as a text for students formally studying economics or agricultural economics per se, although some of the material may be of interest to them.

The author states in the Preface that the "book is intended for use as a guide in performing economic analyses of range [property] improvements and management practices. It is not meant to be a review of range economics research results nor a catalogue of range management practices expected to prove profitable. Usefulness of these latter approaches would be very short lived due to changes in prices of products and inputs" (Pp. xv-xvi).

As a result, the final product is essentially a theoretical treatise covering microeconomic optimisation principles of production theory, time preference and benefit-cost analysis for private and public land-based investments. Each of these subjects, in turn, is presented at a reasonably elementary, albeit comprehensive, level. Material requiring a higher level of economic or mathematical understanding is sensibly presented in appendices or identified in references and reading lists at the conclusion of each chapter.

There are ten chapters that may loosely be placed into five basic groupings of subject area; viz

(1) Background:

Definitions and historical development of the 'science' of range economics vis a vis range management and agricultural economics. Purpose and content of the book. (Chapter 1)

The concept of economics as a science dealing with the efficient allocation of scarce resources among competing uses. Tradeoffs in alternatives for both private and public land managers. Absolute and comparative advantage concepts as an explanation for grazing rangelands. Structure and operation of a 'typical' livestock production unit and statements of income and net worth. (Chapter 2)

(2) Supply and Demand:

Definitions of supply and demand and concepts underlying the shape and position of supply and demand schedules (or curves). Market equilibrium and determination of prices. Shifts versus movements within supply and demand schedules (or along curves). Factors that 'shift' supply and demand schedules (or curves). (Chapter 3)

(3) Production Theory:

Definition of factors of production (inputs) and products (outputs) of range production systems. Production functions, variable versus fixed factors and the 'law of diminishing returns'. Introduction to the production economics problem trilogy - what, how and how much to produce? (Chapter 4)

Optimal intensity of production (how much to produce). Marginal concepts and their role in optimisation. Factors affecting optimum levels of production and the impact of change in those factors. Short versus long run production decisions. Allocation of a scarce resource between enterprises. (Chapter 5)

Optimum combination of inputs (how to produce). Minimum cost combinations for fixed levels of output. Maximum production for fixed levels of outlays on inputs. Profit maximising production levels. (Chapter 6)

Optimum combinations of outputs (what to produce). Interactions between alternative land uses - competitive/complementary/supplementary and antagonistic. Biological and economic interpretations of land use interactions. Profit maximising output combinations. Calculation of 'prices' for outputs with no formal market. (Chapter 7)

(4) Time Preference:

Present versus future values of lump sums. Factors responsible for positive rates of time preference - inflation/risk/opportunity cost. Compounding and discounting formulae - lump sums/annuities. Internal rates of return. Capitalisation of perpetual flows of annual income. Sinking funds for replacement of long lived capital items. Calculation of effective rates of interest. (Chapter 8)

(5) Benefit-Cost Analysis:

Analysis of private investment decisions for property improvements. Investment appraisal criteria - net present value/benefit-cost ratio/internal rate of return. Information needs for project appraisal. Valuing forage gains from alternative land improvements. Budgeting. Expected project life. Expected project costs. Choice of interest-discount rates for private investment appraisal. Disagreement between alternative investment criteria and normalisation procedures for their resolution. Optimum combinations of projects with limited capital. Modified internal rate of return calculations for non-uniform cost and benefit flows. (Chapter 9)

Analysis of investment decisions on public lands. Economic efficiency versus equity goals in public decision making. Justification versus analysis in public investment appraisal. Quantifiable versus non-quantifiable benefits and costs. Priced and non-priced values. Cost-effectiveness and non-market pricing procedures. Choice of interest-discount rates for public investment appraisal. Opportunity cost versus social discount rates. Adjustment of discount rates for risk and inflation. (Chapter 10)

Each of the Chapters is well structured and proceeds to cover the material selected in a reasonably clear and logical manner - a trademark of Workman's,which no doubt reflects a long teaching career as much as a desire to get the basic economic concepts across to students who are not specialising in economics (eg. agricultural science or natural resource management), practical range managers and administrators. In that regard, the inclusion at the end of each chapter of a practical application of the theoretical concepts covered is particularly appealing.

For the serious Australian student of economics or agricultural economics, however, the theoretical material is identical to that found in most of the basic microeconomics or production economics textbooks to which he/she would be exposed in first and second year of training. Nevertheless, some students might find the anecdotal material and applied examples contained in the book to be interesting, or convenient in having a reasonably broad range of basic material condensed in one reference. However, the quoted $US list price would make the book an expensive purchase whose justification would be difficult on those grounds alone.

One obvious distraction of the book is that it is clearly written for an American audience and so the examples used or surrounding anecdotes are specific to US circumstances. While this might be somewhat off-putting to Australian readers, local equivalents or alternative examples can usually be thought up and the principles espoused applied. A more serious shortcoming, shared by some texts of local origin, is the absence of any attempt to really come to grips with either the extreme variability in production and prices or the ecological feedback mechanisms characteristic of 'range' production systems in most countries, including the US. This is disappointing given its specific recognition by the author in the introduction to the book.

That is, while the principles taught are useful as principles, all of the analyses presented are 'static' and unlikely to be truly optimal for the 'dynamic' environment under which real world 'range' management decisions must be made. It is almost axiomatic to 'range' management practioners that there is no such thing as a static optimum in the 'range' environment. The book might then usefully have included some material on sensitivity

analysis, decision theory under uncertainty and making allowances for risk. Alternatively, its absence may be excused on the ground that the profession has yet to find a method of expressing these concepts in a manner that is both understandable and convincing to practical managers.

Despite some of the reservations made above, I would still recommend the book for the libraries of universities and government departments which have an interest in 'range' production systems or the administration of 'rangelands'.

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Davis, J.S., Oram, P.A. and Ryan, J.G. (1987), **Assessment of Agricultural Research Priorities: An International Perspective,** Australian Centre for International Agricultural Research in collaboration with the International Food Policy Research Institute, Canberra.

With the increasing budget spent by national and international donor agencies on agricultural research in developing countries, there is increasing pressure to ensure that this money is being spent effectively. One approach to this problem, and the one adopted by Davis, Oram and Ryan, is to determine the returns on investment to agricultural research on specific crops in particular regions of the developing world. This assessment is intended to assist such donors as the Australian Centre for International Agricultural Research (ACIAR) in formulating their programs. However, as at least one recent paper testifies (Harvey 1988), this is a contentious subject for developed, let alone developing, countries.

If the investment made in research in a developing country is successful in increasing yields, the extent to which consumers and producers gain or lose depends on the slope of the demand curve. Ignoring redistributional effects, or implicitly assuming that the compensation principle is actually practised, the national net benefit of this research can then be assessed.

The concern of national agencies is only likely to be with national impacts. International donor agencies, on the other hand, are likely to be interested in the wider implications of initial research investment. In particular, two additional factors (at least) need to be considered:

- Where the research concerns a commodity that is exported or imported, any subsequent increase in production in the target country will also affect producers and consumers in other trading countries, depending on the respective supply and demand relationships.
- Since publicly funded research findings often fall into the public domain, research results obtained in one country may be applicable in others, and have (what the authors term) spill-over effects from the original research investment. Whether this spill-over is an externality, in the usual sense of the term, is a moot point.

Thus, to assess the return on donor agency research investment, three major factors need to be considered:

(1) Supply and demand of the commodity in target and non-target countries, including international trading relationships,
(2) The size of research spill-over, and
(3) The probability of research success, the level of adoption and the time lags of adoption in all affected countries.

This can then be discounted to produce a present value return on investment.

The determination of (1), though onerous, is conceptually straightforward. The estimation of (2) and (3) is far more controversial.

The way in which the authors estimate research spill-over is based on the area of crop in the same agro-climatic zone as the national crop that is initially being targeted. There are a number of objections one can raise to this. The first that immediately comes to mind is that climatic homogeneity may not necessarily be the crucial factor. Infrastructural differences, for example, could be far more important in the degree to which technology can be transferred from one country to another. A second objection is that the type of research undertaken will affect the degree of spill-over.

Thus, the development of (say) novel pesticide application technology, could be applicable across a very wide range of climates and crops. The most appropriate form of research for which an agro-climatically based estimate of spill-over does appear relevant is classical biological control, involving the introduction and release of predators or parasites of specific pests; a major advantage of this approach being the limited adaptation required.

Assessing the third major factor - the probability of success - is also, necessarily, subjective. The authors assume that the probability of success in research can be expressed as a sigmoid function of current research intensity, measured here as the percentage of the gross value of production spent on research. The probability of success will be increased most if donors can increase research effort to the region where probability increases exponentially.

This means of assessing research success, as with most of the technical details of this work, is ripe for discussion. Indeed, this inevitably will be the case for any attempt to produce a rigorous means of setting research priorities. However, there are a number of broader issues that are not discussed by the authors in any detail and yet which are fundamental to the type of approach they advocate. These issues fall under three headings - objectives of donors, donor spill-over, and targeted research.

Objectives of Donors

As well as the economic return on investment, on which this analysis focuses, donors are likely to take other objectives into account when investing in agricultural research in developing countries. For instance:

- Some research, such as Desert locust research, may be more concerned with reducing disasters, than with producing a high return on investment per se. Thus, the benefit or pay-off to this research may be more appropriately expressed as the opportunity cost of disaster relief, rather than a return on investment.
- A major research objective of many developing countries is to minimise foreign exchange requirement.
- The re-distributive effect of new technology within producers in a country is often of major concern. The re-distributive effects of "Green revolution" research in favour of the richer farmers is well documented.
- Donors are increasingly concerned with the environmental impact of agricultural research projects, and particularly the hazards associated with pesticides.
- Successful research projects rarely have a discrete and additive effect. More often they are an integral part of a development process, each phase of adoption reinforcing previous adoptions and channelling developments along a particular pathway. Thus, to paraphrase Ciriacy-Wantrup (1971), albeit in another context, "The emphasis of this approach is on ... establishing base levels rather than on locating peaks, on not entering dead-end streets and on keeping direction rather than on computing the shortest distance, and on mobility and adaptability of productive factors rather than on their optimum combinations".

The analysis the authors propose might also be used to address what one might call the size of the "spillback effect". Some national donor agencies, under pressure from their own producers, are anxious not to support research on crops in developing countries with which they are in direct competition. It would seem this type of analysis could provide some indication of the extent to which domestic producers suffer as a result of aid policies.

Donor Spill-over

An apparent danger in advocating and using the type of analysis advocated by Davis, Oram and Ryan in setting research priorities, is that all donor agencies will tend to target on the same crops and regions. Indeed, with similar exercises currently being implemented in the UK, for example, this seems to be taking place already. One consequence is that the apparently poor return on investment in such "minor" crops as tef in Ethiopia and spice crops in various countries, would imply zero donor investment in research, despite the crucial importance of these crops to local economies and societies.

Undoubtedly the authors are aware of these additional factors and would argue that by concentrating on one aspect, return on investment, they are not denying that other factors need to be accounted for in the final decision. '

Nevertheless, experience with other cost-benefit exercises suggests the overbearing "testimony" of statistics may well subsume these other considerations in reality. Thus, I would beg to suggest that, rather than developing a more detailed return on investment model, future effort would best be directed at determining a rigorous yet appropriate means of incorporating these broader considerations.

Targeted Research

In devoting analytical effort to donor funding decisions, one might also pose the question - is this emphasis the one that will produce best results? Given the problems of the analysis, as indicated above, the role of scientific peer review, as well as the frequently over-riding importance of political factors in determining the allocation of research funding, perhaps greater return would be obtained by attempting to improve research management. One approach, aimed at increasing the probability of successful implementation of research results, would be to prescribe a consensus, interdisciplinary, targeted research strategy, building on the adaptive research philosophy. But herein lies a paradox. While research targeted and adapted to the local problem increases the probability of local success, it also reduces the degree of spill-over to other situations; another serious problem to contemplate!

In stimulating a much longer review than I had intended, it is clear that Davis, Oram and Ryan's book provides food for thought. It certainly made me think about aspects of the problem of assessing research priorities to which I have not given adequate attention in the past. Thus, as a discussion document, I believe it provides a valuable role in pointing out to donor agency decision makers some of the factors they should be considering. In terms of an operational decision support tool, however, I fear we have a very long way to go.

References

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Braden, John B. and Kolstad, C. D. (1991), **Measuring the demand for environmental quality, Contributions to economic analysis,** 198, North Holland, Amsterdam. US$69.50.

The State of Illinois requires economic studies of proposed environmental regulations. The difficulties that confront these studies have led the Illinois Department of Energy and Natural Resources to commission reviews of the state of the art in benefit valuation. This book is the latest such review.

The editors are John Braden and Charles Kolstad of the University of Illinois. They set themselves the objective of a broad assessment of progress on techniques to value environmental goods and services. Their secondary objectives are to assess the relative strengths of the methods, and to evaluate preferences for important classes of environmental goods and services. They have targeted graduate students and practitioners.

The contributors (and their chapter topics) are John Braden, Charles Kolstad and David Miltz (Introduction), Raymond Palmquist (Hedonic Methods), Kerry Smith (Household Production Functions), Richard Carson (Constructed Markets), Maureen Cropper and Myrick Freeman (Environmental Health), Philip Graves (Aesthetics), Nancy Bockstael, Ted McConnell and Ivar Strand (Recreation), Richard Adams and Tom Crocker (Materials Damages), and Alan Randall (Total and Non Use Values). The editors have written the second chapter on Environmental Demand Theory, and the final chapter on Summary and Conclusions.

The first part of the book (up to and including Carson's chapter) addresses theory and methods, beginning with the mathematical chapter two on demand theory. This skeletal review of theory integrates uncertainty into demand, emphasises

that quantities can usually be observed but prices rarely can, and provides a common foundation for all the subsequent chapters.

The task of modelling consumer demand becomes more complex when access to different goods and services differs. The household production function approach, which specifies the relationships between environmental services and private goods, is one way to address this complexity. Apart from application through the travel-cost method, this approach remains conceptual due to data deficiences. Hedonic methods recognise that goods are not homogenous and differ in many characteristics, some of which can be defined as environmental qualities. Palmquist's chapter sets out the hedonic theory, and the econometric issues involved in applying it.

When environmental commodities are not bought and sold, markets must be constructed and Carson reviews the contingent valuation method to do this. The theory of applying willingness to pay and willingness to accept questions is summarised and representative studies (including Australian ones) are reviewed. Carson presents useful criteria for designing the market for the questionnaire and so avoiding bias. They include theoretical accuracy, policy relevance, and practical application.

The second part of the book concerns methods for valuing classes of environmental effects, and starts with a chapter on environmental health. Changes in the life support capacity of the environment can change the incidence of disease, pollution, human activities and life expectancy. Logically, and in reality, the monetary values for improvements in human health are measures of changes in the support system. Cropper and Freeman assess the valuation of such improvements through the human capital (or earnings) and willingness to pay approaches. They justify the role of economic concepts in these cases. If people are rational and if their preferences are taken as the basis of value measures, their willingness to consent to trade-offs between small changes in the probability of mortality or morbidity is the basis of valuation. This consent, and the veil of ignorance over precisely who is affected, justifies a focus on the changes in risks rather than on life or death per se. The utility functions on which to base valuations, and the value estimates derived to date, are well reviewed.

The chapter on aesthetics, by Graves, is shorter than most of the others, more applied than most, and introduced as carefully as the rest. But it fails to link its introductory emphasis on the inseparability of aesthetic benefits, and material impacts, to the methods of analysis. Cleaner city air is healthier and more pleasing to look through. Aesthetics is, or should be, special because it concerns only such sensory impacts, separated from material impacts and other non use values. Contingent valuation and hedonic methods are reviewed, with the conclusion that the former is more useful because of its flexibility. This applied review is good, as it is in all the other chapters, but the discussion of the separability of aesthetic utilities from other utilities could usefully have been developed.

The valuation of recreation has made advances over the years and many of these contributions have come from the University of Maryland. Accordingly Bockstael, McConnell and Strand, all contributors and all from this institution, wrote the recreation chapter. Their material is entirely theoretical-slightly unexpected in this second part of the book.

The environment could be classified into systems that are beyond human management (such as climate and topography) and systems that can be managed (presumably the rest). Adams and Crocker concentrate on agricultural systems, biological systems and physical assets (such as buildings). Damage or improvements to these systems imply welfare changes, and the value of these changes should be assessed. The authors take a broad context starting from the role of natural science information, and the inter-temporal and risk dimensions of material effects. They proceed through methods of programming, simulation and hedonics, before they survey the empirical studies. Their assessment for air pollution leads them to conclude that producers tend to gain from increases in air pollution, surplus changes are less than biological changes, and increases affect productivity and the demand for inputs. Changes in air pollution have a different effect on different agricultural regions, and alter international trade.

Randall's review of the conceptual basis of total and non use values is thorough and well dove-tailed to his review of empirical studies and to his reviews of contingent valuation as a method to assess such values. While everything has an existence value, that value may approach zero when existence is not scarce. When uncertainties are great, and if reversal of a decision is costly, uncertainty must explicitly be considered.

The editors' concluding chapter attempts to review the last two decades of work and the previous ten chapters of their book. The authors have met their objective of a broad assessment of progress in the valuation of environmental goods and services. Their success largely reflects the careful organisation of the flow of ideas in the chapters, and the integration of the structure of the chapters. They conclude that there is a diverse portfolio of analytical methods and a rich inventory of applications. The methods produce reasonable estimates of demand and value, they say, and do so with regularity and consistency.

This book is an up-to-date review of the state of the theory. It is thorough and well documented. Hopefully, the publishers can price it so that it can reach the wide audience it deserves.

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