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Regional Research in Rangeland Utilization

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Regional research in the economics of rangeland use is a very new undertaking. At the present time there is no completed project that can be examined and evaluated with detached judgment. Therefore, this paper will give something of the background of the present research program in this field, some of the accomplishments to date, some of the problems, and a few suggestions for future research on the economics of rangeland utilization.

The economics of use of rangeland has been the subject of a considerable body of literature. Some of the brochures prepared for circulation in England in the 1870's were essentially monographs on the economics of rangeland use. These were prepared specifically for the purpose of attracting English capital into the developing livestock industry of the West. They were successful in accomplishing this purpose, but they bore almost no relationship to reality. Many of the English financiers became bitterly disappointed by the realities of the business, but some of them hung on and made fortunes which helped England to finance two world wars.

In the last 50 years much writing has been done about public land. The work of E. O. Wooten showed the consequences of lack of management on the public domain. He showed the effects on the range itself, but he dealt only lightly with the effects on the people who make their living from rangeland. Scattered studies on ranch management and range economics have been made by Youngblood, Adams, Burdick, Pingrey, Saunderson, and others, over the last 30 years. Despite some excellent pieces of research in the past, analytical economic studies are needed to inquire into the problems and principles of utilization of rangeland.

Recent background

The present program of regional research on use of rangeland has several parents. In 1948 the Farm Foundation sponsored a land-tenure workshop at Centennial, Wyoming, where much thought and attention was given to the problems of rangeland tenure. There was much public discussion of rangeland tenure at that time, as evidenced by articles in the public press which told of friction between ranchers and the public land-managing agencies. Congressional committees and administrative department inquiries were especially active in the immediate postwar years.

Also, about 1948, Dean Eckert, who was then Head of the Agricultural Economics Department, Montana State College, proposed to the Western Agricultural Economics Research Council that the Council sponsor a research program to inquire into the public-land questions of the West. The Council asked Dr. Kelso to survey the research needs in agricultural economics outside the field of marketing. Kelso obtained opinions on production economics research needs from about 50

agricultural leaders in the West. The consensus was that research should be done on the economics problems of rangeland improvement and on the economics of water resource development. The Council then asked Dr. Thomas of Utah State Agricultural College to recommend action. Dr. Thomas called a meeting of a number of interested people at Logan and prepared a statement for the Council recommending the establishment of two standing committees, one on the economics of range resource development and one on the economics of water resource development. The "Range Committee" initially was composed of Maurice Kelso, Howard Mason, and George Blanch. Membership on the Committee subsequently has been changed and broadened. Both the "Range" and the "Water" Committees were implemented by a grant of funds from the Farm Foundation to the Council.

At about the same time, a research project was undertaken cooperatively between the Bureau of Agricultural Economics and the University of California to inquire into tenure problems of rangeland in northeastern California. Also, the Oregon and Idaho experiment stations started some research into the economic problems of rangeland improvement.

The California research project revealed some interesting information, but it was perhaps more important because of the insight it gave into other problems not studied directly. We learned from the project, for example, that land tenure as such was perhaps not as serious a problem on rangeland as we had thought initially. We learned that the economic questions involved in rangeland improvement were uppermost in the minds of ranchers and public-land administrators and that the competition for use of rangeland, chiefly between big game and livestock, was generating serious problems.

Shortly after the passage of the Research and Marketing Act of 1946, the Western experiment station directors approved a regional research project on improvement of rangeland. This project was proposed by range-management technicians and, although it was approved, no money was allocated to it initially. As a result of the Western Agricultural Economics Research Council's interest in the economics of rangeland improvement and the attempts made by the Council to finance a regional project in range economics, the Western directors approved a regional economics project as one phase of the earlier project submitted by the range technicians. Subsequently the range technicians resubmitted and obtained approval of a new regional project, W-25 Ecology and Improvement of Brush Infested Rangeland.

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The present project

The present regional research project on the economics of range improvement was activated in July 1953. It is financed in part by RMA funds and designated as W-16 in the numbering system. The work already under way in Oregon and Idaho on the economics of range improvement was incorporated into the regional project. A number of other experiment stations had closely allied research projects, such, for example, as the one in Montana on the economics of water spreading on rangeland.

The present regional project on the economics of range improvement is designed to do three things: (1) examine the economic factors that affect range improvement in several of the major range types, with emphasis on range reseeding and brush control; (2) explore the economics of range production and improvement in the questionable dryland farming areas along the western edge of the Great Plains; and (3) develop measures of range productivity that would be meaningful for economics research. This project is 2 years old, and some published results should be available shortly. Most of you are familiar with the present regional program of research in range economics, and I shall not go into it in greater detail. Neither shall I offer any critique of it. The project is still too new and has too much ground yet to cover for a critical review.

The present W-16 project (The Economics of Range Improvement) and other closely related research dealing with rangeland and range problems in the W-25 regional project and in others in state and federal research agencies should be a beginning rather than an end. In the years ahead we, as economists, have an obligation to use the tools of our science to bring about more effective use of one of the West's greatest resources.

Opportunities in rangeland research

We also have an opportunity to develop much more useful research now than has been possible in the past. This opportunity grows out of the increasing availability of technical knowledge about range resources, improved techniques of measurement that can be applied to range technology and economics, and a growing awareness of the need for economic interpretations of range research.

We should pursue our present regional research project on the economics of range improvement as vigorously as funds and personnel will permit to achieve the full usefulness of this work. The present project should be extended into several other major range types which have not yet been considered in the study. Specifically, these are the pinon-juniper and the desert shrub types of the Southwest. Also, despite some excellent work by the Montana station, we should extend the phase dealing with range production in the questionable dryland areas to other parts of the Great Plains farming-grazing transition zone.

In addition, I suggest that further work on the economics of rangeland utilization should be along three major lines--one dealing with the improvement and use of native range as distinct from the present emphasis on reseeding and related practices, one dealing with the economics of ranch operation as distinguished from range management, and the third dealing with the complex problem of the economics of multiple land use.

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Analytical tools

Our present work on range reseeding and brush control is primarily an incremental approach to a single enterprise, but part of the future work should be "firm studies." These studies would take into account the cost and returns of specific parts of the firm. However, they should also be integrated studies of the entire ranching business. They would complement the work already done or under way. They would make such

work more meaningful because it is within the framework of operating ranches that economic decisions are made on range improvement as well as on other production problems. Studies of this kind would delve into the various opportunities for improved operation and management of the entire ranching business rather than just the range feed supply.

Although such studies can make excellent use of the ordinary tools of analysis, the analytical job would differ in some characteristics from customary farm-management studies. Ranch management, in which native vegetation is used as a primary resource and in which harvesting of the resource can be done only by grazing livestock, places some unique limitations on practical ranch operations and presents some unique problems of analysis. In farm-management studies we often can sharpen our analysis by considering crop production and livestock production as two separate businesses. Their combination on a farm is a vertical integration of enterprises each of which might exist alone or at least can be analyzed separately. In ranch-management studies the crop production (range forage) usually has no use or value except as it is grazed. Therefore the whole complex of management variables with respect to the livestock are inseparable from the production of the forage. For this and other reasons the input-output relationships in ranching are less well known and much harder to measure than these same relationships in the farm economy. In addition, the ranching firm characteristically has a very high proportion of its resources in fixed assets with relatively fixed costs of operation. Opportunities for adjustments in response to economic conditions are much narrower in ranching than is common in farming. Further, a rancher does not always have close control over the resources he uses. Despite these characteristics of ranching, the ordinary tools of analysis that agricultural economists use will help us to understand and solve ranch-management problems. Better data and analysis are always needed.

In addition to future studies of ranch management, we need some good economics research on problems of multiple land use in range areas. Economists can help to improve the judgments and management decisions that determine uses and combinations of uses of rangeland. Economic science must plow some new ground in this area. Techniques of measurement and techniques of analysis must be developed that will give us better understanding of the combinations of values and the competition for resources between different major interest groups in our society.

The work of John Hopkin and a few others recently is commendable. They are trying to use the marginal rate of substitution technique to determine the combinations of uses of rangeland that will be most economical. In principle, this approach to the problem is sound, but as a practical matter we do not yet have sufficiently accurate data to use it with confidence. Marginal analysis and the marginal rate of substitution technique is useful when inputs and outputs of competing factors can be measured rather precisely and in comparable terms. We have neither precise measurement nor measurement in comparable terms for attacks on the problems of multiple use of rangeland. For example, the income to society from game has not yet been measured satisfactorily in dollars and cents. Until it is so measured this approach to the multiple-use problem will be elusive. It would appear to be well worthwhile to collect specific data as to areas, numbers, values, and other features to give a better understanding about particular multiple-use questions when

conflicts arise. A pragmatic solution to each specific conflicting situation as it arises may give us eventually a better qualitative understanding of the essence of this problem, even though it may not provide us with empirical tools and precise scientific measurements.

Whether future regional research on utilization of rangeland emphasizes enterprise studies, ranch-management studies, or broader aspects of multiple use, we must improve measurement and analytical techniques. Dr. D. F. Beard, Head, Forage and Range Section, Crops Research Branch, ARS, recently put this at the top of a list of research needs for pasture and range. The range-management technicians themselves are aware of the need to improve their tools of observation. A committee of the American Society of Range Management in cooperation with the National Research Council is preparing a handbook on research methods, which will include a chapter on research methods in the economics of rangeland use. This effort will attempt to outline what we now know about methods of economic research in rangeland, but it will not develop any new methodology. Development of new tools must come from research itself.

Place of regional research

Perhaps we should inquire as to how the major fields of research which I have suggested for future work might be approached as regional studies. The questions of ranch management are to some extent local questions in the same way that questions of farm management are local. No two ranching situations are exactly the same, and conclusions drawn in one local study may not be applicable to local areas elsewhere. Nevertheless, a regional approach to these studies would have some advantages. Techniques of analysis would be applicable everywhere, and some new techniques need to be developed. In addition, some of the principles of management, even specific decisions, will apply generally throughout major range-type situations.

To be effective future research on multiple land use economics must be performed on a regional basis. The influence of multiple uses extends over wide areas and certainly beyond state lines. One has only to glance at the newspapers in recent years to realize that snow falling on rangeland in Wyoming is quite important to housewives in Los Angeles. Further, the deer that summer in California and winter in Nevada may be hunted by a man from Oregon, so that the costs and incomes of the different combined enterprises involved in multiple land use extend over wide areas. Regional research, then, is a practical approach to studies in this area.

Research in range economics, perhaps more than any other phase of agricultural economics research, can profit from a team effort. Ecologists, animal husbandmen, foresters, game technicians, and others, as well as economists, have a contribution to make to the analysis of range and ranching problems. Range technicians consistently have met with the present technical committee on range improvement. The W-16 and W-25 committees have held one joint meeting, to the advantage of both groups. As future regional research is undertaken in range economics, definite plans should be made for participation of other technicians.

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