PRIORITY SETTING AND FUNDING OF AGRICULTURAL RESEARCH

M. Petit[1]

[1]Institut National Agronomique Paris-Grignon, France.

Agricultural research funding amounts and modalities have been profoundly transformed in recent years in many countries. For instance, competitive funding has become the norm rather than the exception in most countries and funding sources have been greatly diversified. In addition, in some major countries, the private sector has become a major actor In agricultural research, both as a funder of research and as a provider research results. This poses a major challenge to public research institutions, which remain the major agricultural research actors and employers of researchers in most countries. With these changes in funding, formal procedures for research priority-setting and resource allocation have been developed and become quite popular in research financing organizations. Many researchers are not very happy with theses developments. They often see such formal procedures as purely bureaucratic, the source of much time lost and diverted from productive work on real research tasks, only to satisfy administrators. Most researchers ignore the economic concepts underpinning most formal priority-setting procedures. When they know them, they are often skeptical about the reliability of the data used in the quantitative models implementing these concepts. All of this casts serious doubt in the research community on the added value of these exercises. Thus, it is appropriate in an international meeting of grassland researchers to reflect on these developments, to assess their impact on research and to discuss how researchers can best cope with them.

The purpose of this paper is to raise these questions and to suggest how they can be approached. As any paper written for an international meeting, a major challenge is to say something meaningful, i.e. avoiding general platitudes, while remaining relevant to a broad audience of researchers coming from many very diverse countries. This challenge is particularly acute here because every researcher has some personal experience with the subject, an experience which indeed has often been painful. So everybody has some view on the general trends characterizing the phenomena under review in this paper. But, at the same time, the real situation varies a great deal from country to country, and even within country; and these differences are very important in terms of the implications which they have for deciding on how best to cope with these developments on a day-to-day basis. Faced with this dilemma, I believe that research institutions and individual researchers need, first, to understand the forces which have produced the changes in funding modalities and priority setting procedures, then they must be able to critically assess these new ways of funding research. This will require a better appreciation of the value and limitations of the economic models involved . The first two parts of this paper will directly address those two needs. Finally, we will examine how these developments may affect livestock and grassland research, a domain where complex interactions are numerous and important and pose a major challenge to economic modellers. In this perspective, special attention will be given to the interactions between grazing, livestock production and the environment. These interactions are indeed critical as they touch on many essential aspects of grassland management. Thus they deserve high priority among research topics; yet they are difficult to incorporate into quantitative economic models.

I- Trends and pressures for change

Data on **funding for agricultural research** in the world are surprisingly difficult to obtain. Much effort has been devoted to that purpose by the International Service for National Agricultural Research (ISNAR) since 1984 and, more recently, by the International Food Policy Research Institute (IFPRI), two Centers of the CGIAR. Two somewhat surprising results have come out of this work. First, total public agricultural research expenditures have increased significantly world-wide, more than doubling for instance in twenty years, from $7.3 billions in 1971 to $ 15.0 billions in 1991 (all of these expressed in constant 1985 dollars) for the 153 countries from which the authors succeeded in collecting the necessary data (Alston et al., 1999). Secondly, and perhaps more surprising yet to many readers, the growth during the 1971-1991 period was more rapid in developing than in developed countries. It increased from $ 3 billions to $ 8 billions in the 131 developing countries of the same authors' sample.

Of course developing countries had much catching up to do; and they still lag far behind in terms of "research intensity", the ratio of agricultural research expenditures over agricultural GDP. That ratio is commonly greater than 2% in OECD countries, reaching as much as 4% in some, and less than 0.5% in developing countries. Given this huge lag, it is indeed worrisome to note that in both developed and developing countries the growth of total public agricultural research expenditures had been faster in the 70s than in the 80s. An additional source of concern arises from the well-documented difficulty faced by many research institutions, particularly in developing countries: the inability to correct a very inappropriate resource mix, skewed in favor of personnel costs, leaving too little for operating expenditures per researcher. As a result, it is not surprising that many individual researchers in public institutions feel that they are under-funded and when this is not the case, they at least feel that they are too much pressed into fund raising activities. The following analysis of the institutional pressures on public research institutions suggests that this situation will probably not improve in the foreseeable future.

Another feature of the current research funding situation has been **the growth of the private sector**, both as a provider of research and as a funder. Alston et al. estimated that for OECD countries as a whole, "privately performed agricultural R and D represented 49.4% of total agricultural R and D in 1993. (*Op. cit.)* Most of that research was directly funded by the private sector, which in addition provided significant funding to researchers in public institutions through numerous contracts. The rapid development of biotechnology research has greatly accelerated this growing involvement of the private sector, particularly in some developed countries leading several of us, when I worked for the World Bank, to caution against the danger of a " scientific apartheid" between developed and many developing countries.

Many of the trends, just discussed, characterizing the evolution of the funding environment for many public research institutions reflect major pressures which must be understood if effective coping strategies are to be designed and implemented. Admittedly, situations vary greatly from country to country. Yet, several commonalities can be identified. For instance, the single most important driving force behind many changes has probably been the **profound re-examination of the proper role of governments** and government institutions in the promotion of economic and social development, or more broadly in the pursuit of the public good. In particular, with the widespread collapse of socialist regimes and, as a result, of socialism as a reference social doctrine, a very wide consensus has developed in most countries of the world that serious constraints limit the ability of governments to intervene effectively in pursuit of the public good. Besides, in attempting too much to improve on market mechanisms in areas where they are not very effective

substitutes, governments have very often jeopardized their own ability to be effective in the domains where market mechanisms cannot be effective. So the slogan is much less “get rid of governments” than “govern less to govern better”. This ideological context must imperatively be born in mind when assessing the pressures exerted on public research institutions.

It is this context which explains why many public officials, politicians and senior civil servants, brought to power thanks to their challenge to the old order dominated by large government bureaucracies, have attempted to eliminate, or at least profoundly reform, these bureaucracies, including public research institutions. And indeed, one must admit that many of these research institutions had become quite bureaucratic, as illustrated by such symptoms as promotions exclusively on seniority and not on merit or the impossibility to fire staff, particularly redundant and ineffective research support staff recruited through patronage or for other political expediency. In addition, many public research institutions have become quite large and, in the process, they have lost the flexibility and nimbleness which characterized them when they were first launched, and often made them exciting places to be working in. These subjective feelings have contributed to the raising of many questions on the wisdom of investing scarce public funds in institutions which did not seem to deliver the good, in terms of research results having a real impact in farmers’ fields.

A few economic concepts can help here to formulate an intellectual framework for reflecting rigorously on the proper role of governments. In an earlier paper on this topic (Petit and Gnaegy, 1999) written with S. Gnaegy, I have suggested that three types of “market failures” could provide us with a tentative list of legitimate government interventions for the provision of rural services, including agricultural research. These are monopolistic power, externalities and the provision of public goods. In each case, private costs and benefits will differ from social costs and benefits and market mechanisms will not ensure the socially optimum amounts of goods and services. Government intervention may be legitimate to correct such failures of the market mechanisms. This is not the place to fully develop the intellectual framework alluded to above. It will be sufficient to say that such a framework can be intellectually satisfactory as it can underpin rigorous analyses. But the main difficulty stems from the fact that most real life cases involve these theoretical concepts in complex and interrelated ways. For instance, in the case of agricultural research, some but not all the benefits of research can be privately appropriable, making the products of research a mixture of public and private goods. Similarly, externalities are often hard to identify and specify precisely. These two points are critical for conceptualising the appropriate relative roles of the public and private sectors in agricultural research. In most cases each sector has some role to play, but the appropriate mix will vary from situation to situation. Furthermore, assuming one knows what the optimal mix is in a given situation, implementing it will involve delicate management of contradictory pressures. The current controversy on patenting the results of biotechnology research provides an important illustration of such contradictions. Recognition of property rights, IPRs in this case, is well recognized as socially desirable because it helps provide private economic actors with the necessary incentives. But equity issues, regarding benefit sharing with traditional communities which contributed to the selection and conservation, over many generations, of the genetic resources utilized by biotechnology firms can not be simply ignored in this debate. In addition, patents may enhance the provision of private goods, such as varieties for a commercial sector, and totally ignore other concerns of a public good nature, such as crops for the subsistence sector or environmental costs and benefits.

To conclude here on the relative roles of the private and public sectors in agricultural research, two main points stand out: 1) In most cases both sectors have a role to play. Note that this is not new; in the area of farm mechanization for instance, the prominent role has been played by the private sector in many countries for many years, the public sector

having often found a small complementary niche to ensure the provision of public goods related to safety for example. What is new, particularly because of the scientific explosion in biology which has led to the development of biotechnology, is the closeness of the interrelationships between the two sectors, calling for the emergence of new partnerships. 2) Inventing those new partnerships has turned out to be very difficult. In fact, after having been for several years in the business of promoting them, I do not know of any case which is fully satisfactory, in terms of fulfilling the legitimate fundamental objectives of the various partners.

In addition to redefining the appropriate role of the public sector, much **pressure has been exerted on many public institutions to reform themselves**. Sometimes, these pressures have been very strong and led to radical reforms. Agricultural research institutions have not been spared. In some cases, the resulting changes have been traumatic. Perhaps one of the most visible and symbolic illustrations of this reform movement was the breakdown of the famous Cambridge (UK) Plant Breeding Institute, one of the "crown jewels" of public agricultural research in the world, and the privatisation of its crop improvement activities. It is indeed in the UK, under Mrs Thatcher, and subsequently, through "ideological contagion" , in several Commonwealth countries –notably New Zealand and Australia-, that the pressures for institutional reforms have been the greatest and the reforms most profound[1]. But the same pressures exist world-wide; and it is critical to understand their root causes.

The well known case of New Zealand, a country which is perceived as having done all the reforms promoted by the IMF and the World Bank for developing countries, without the pressures of these Bretton Woods institutions, can be very illuminating in this regard. In 1984, a new Labour government was elected at a time when the country was facing a major economic crisis: high budget deficit (9% of GDP), high inflation, slow economic growth, one of the lowest per capita income among OECD countries after having had one of the highest. Radical reforms were needed and the political climate for such reforms was ripe. The new government rapidly undertook a series of reforms including, of course a macroeconomic stabilization program. For the purpose of this paper however, the reform of the public sector is the most relevant component of that package. As expressed by Bale and Dale, "the government wanted the ethic of value for money and customer service to take its place alongside the ethic of probity and stewardship in the public sector" (Bale and Dale, 1998). Taking their inspiration from modern developments in economic theory (public choice, principal-agent, transaction costs) and the related public management literature, reformers attempted to replicate in public institutions the types of incentives structure faced by people working in well performing private enterprises. "The approach had five characteristics:

- Establishing clear lines of accountability between government ministries and their departments
- Defining performance in an unambiguous and measurable way
- Delegating authority to chief executives
- Establishing incentives that reward or punish results relative to the agreed outcome
- Reporting and monitoring performance" (*Op. Cit.)*

These prescriptions appear sensible for large bureaucracies and the first one, in particular, may be very appropriate to clearly delineate the responsibilities of public research institutions vis-à-vis the government. But anyone having experience working in a good and

[1]**Beattie, A.** (1998). " The United Kingdom" and Henzell E., Crawley M., Johnson R., and Wallis E. "Australia and New Zealand" in Persley G. (ed.) Investment Strategies for Agriculture and Natural Resources: Investing in Knowledge for Development, Wallingford, CABI Publishing.

dynamic research institution will immediately feel upon reading these characteristics that, however well intended and reasonable they may be, they do not capture the essential ingredients of what makes a research institution successful. They indeed risk creating too formal an environment and stifle creativity and imagination, the two most critical ingredients of good research. But, whatever their merits, the main point for our purpose here is that this explicit formulation in the case of New Zealand reflects quite faithfully the types of pressure for reform exerted on many public agricultural research institutions in the world. Personally, I would add that given the many malfunctioning research institutions which I have seen in the world, these pressures for reform are generally legitimate, even though they are often resented by research managers and even, somewhat paradoxically, by individual researchers who are the first victims of the institutional failures the reforms attempt to address.

II- Emphasis on priority setting

Recent emphasis on formal priority setting procedures results from the same pressures for institutional reform discussed just above. Concerns for transparency and accountability have been the main driving forces in this case. In large bureaucracies, transparency and accountability are difficult to achieve because the public services provided are not always well defined and results are difficult to measure in terms consistent with expected outcomes. For very different reasons, the same applies to agricultural research. Expected outcomes, defined and negotiated before a research activity is undertaken, are always difficult to specify precisely. So, they are either couched in very broad terms (e.g. improving farmers' incomes), not directly related to the research activity to be funded, or expressed in unrealistic indicators of progress (the famous "milestones"). All of this contributes to undermine the credibility of research institutions and individual researchers in the eyes of the middle level officials, in public agencies funding research, who are responsible for agricultural research institutions and, as a result, who have to defend the budget allocations for these institutions in front of their superiors.

The result is pressures for evidence of past impact, for clear statements of purpose and expected outcomes as well as evidence that the proposed research should indeed receive high priority among all the interesting research projects one could think of a priori. A testimony from the past Chairman of the Finance Committee of the CGIAR, who I was from 1993 to 1998, may be useful here. Very significantly, most of the pressures for greater budget transparency and accountability, emphasizing "value for money", during that tenure, came from government representatives of the UK and Australia. But other government representatives on or around the Committee, which after all reports to the whole Group at large, willingly followed on the same ideological path. Realistically, which government representative could have spoken in favor of less transparency and less accountability?

As a consequence, the Technical Advisory Committee (TAC)of the Consultative Group on International Agricultural Research (CGIAR) endorsed the development and use of **transparent, quantitative priority assessment frameworks** that would allow a clear linkage to be made between research priorities and resource allocation to research activities. This was also a response to the fact that the nature of the CGIAR's funding changed substantially, in recent years. The share of a typical Center's budget that was unrestricted in nature, i.e. funds made available to the Institute that could be expended in any way in pursuit of the Institute's goals and mandate area declined significantly in a few years. By 1999, this had fallen to less than 50%, the balance being made up of project and programme funds that are restricted in the sense that they are directed towards very specific activities and outputs, for which research teams are held directly responsible. A more formal and transparent research planning process, integrating impact assessment, priority setting and resource allocation, is viewed as a way to

ensure the integrity of the overall effort, in particular its continued focus on the overriding goal of the CGIAR: poverty eradication through research for an environmentally sustainable agriculture. A very good example of such an effort from a CGIAR Center is given by Thornton et al. (2000). This publication provides excellent illustrations for several points made in this section of this paper. A pioneering work on a similar situation was reported in Kelley et al. (1955).

Economic concepts and approaches, here again, can provide us with the necessary comprehensive intellectual framework to design, and also assess, formal priority setting procedures and methods. As stated by Alston et al. in the book, which is the authoritative reference on the subject (Alston et al., 1995): "Agricultural research is an investment in the production of knowledge that must compete with other activities for scarce resources." They further boldly add: "agricultural research evaluation and priority setting are economic problems". Many other disciplines would legitimately claim that they also have something to contribute and that, very obviously, the evaluation of research, for instance, requires knowledge in the disciplines where research is being done. But once more, the merit of the economic approach is that it provides us with a comprehensive framework where the economic benefits of research can be weighed against its economic costs. The main issues then relate to the conceptualization of these costs and benefits and to the methods of measuring them. On the first point, the matter is relatively clear. For costs, the opportunity principle is critical. What counts is what other uses the resources devoted to support agricultural research could be put to. This issue is important for public funds which, because of the problems inherent in tax collection everywhere, have an opportunity cost which is greater than their face value. For benefits, one must ideally capture the benefits from a particular piece of research to society, as a whole throughout time. Immediately, this however raises thorny difficulties regarding the distribution of such benefits among social groups, across countries and across generations, with the attending arbitrariness of the choice of the appropriate discount rate reflecting how societies assess trade offs through time. Economic theory has long recognized these difficulties and forged the concept of economic surplus to deal with many of them. Thus, it is appropriately that Alston et al. wrote: "There is really no substitute for the economic surplus model" (*Op. Cit.*., p. 493).

The economic surplus, a concept invented at the end of the 19th century, has been proposed to capture the aggregate benefits derived by both consumers and producers from a change affecting the production or the consumption of some product, such as a technical innovation leading to a reduction in the cost of production. Economists can demonstrate, under a set of specific assumptions, that the total welfare gain to society, resulting from the change being considered, can be measured as a change in the area of a triangle formed by the demand and supply curves of the product on the appropriate market. Most of the underlying assumptions relate to the competitive nature of the markets involved. Classically, non-economists find that approaches in terms of economic surpluses are far-fetched; in other words, they find that such approaches are very demanding in terms of heroic assumptions and of data. As a result, these approaches are neither very credible nor even very legitimate in their eyes. Such skepticism is fully understandable. Indeed, economists would be the first to recognize, and even to stress, that aggregating all welfare gains from a given technical change in a single measure assumes, implicitly or explicitly, that the gains and losses of each individual producer and consumer can be added together, i.e. giving the same weight to the gains and losses of the poor and of the rich for instance. We know that many public policies (e.g. social welfare policies or the progressive income tax) are based on a radically different assumption. Yet, the truth of the matter is that in the absence of any alternative comprehensive framework, approaches in terms of economic surpluses are the only rigorous reference, providing a yardstick to assess empirical analyses of specific, concrete situations.

This is really what Alston et al. meant in the above citation. In addition, regarding the evaluation of the distribution of welfare gains arising from a given technical change, be it envisioned *a priori* or considered *a posteriori*, it is usually fairly easy to distinguish between producers' and consumers' gains and losses and even to make distinctions among categories of producers and of consumers and to evaluate these gains and losses in empirical studies.

But providing quantitative estimates of economic surpluses, defined as areas of so-called "welfare triangles", requires mathematical specification of the supply and demand curves, which among other things requires a clear definition of the appropriate market (i.e. a less than trivial task) and quantitative estimates of the relevant parameters of the functions represented by these curves, as well as of the shifts in the relevant supply curves resulting from the technical change being considered. Estimating these parameters is itself very demanding in terms of mathematical specifications of the economic models involved and in terms of data. In particular, estimating the shifts in supply requires to predict *a priori*, or to assess *a posteriori*, the quantitative impact of some piece of research on the supply curve of a product impacted by that research[2].

As already suggested, economic surplus approaches can be conducted both *a priori* and *a posteriori;* i.e. both for priority setting and for impact assessment. In the first instance, various pieces of research being contemplated are ranked according to the net benefits they can yield to society, measured in terms of economic surpluses minus research costs. For impact assessment, estimates of material impacts, in terms of yield increases for instance, can be economically valued in an economic surplus framework. Admittedly, the latter is not done very frequently, analysts being happy when they have been able to provide reliable, quantitative estimates of physical impact. But the main point for our purpose is that this approach in terms of economic surplus provides a comprehensive and logically consistent framework and a rational basis for the allocation of scarce research resources and for assessing the efficiency and effectiveness of their use. This is indeed a very attractive feature of the approach for those who administer scarce public funds and are accountable for their use. Furthermore, the very existence of this intellectual framework legitimizes the demands addressed to research institutions and individual researchers regarding clarity of purpose, identification of outcomes, definition of milestones, assessment of progress in reference to theses milestones, and evidence of impact. Returning to priority setting *per se*, the concern for impact has led many research institutions to incorporate measures of the potential impact of alternative future research investments into a priority setting framework from which a defensible and marketable portfolio is derived. But the difficulties alluded to above for implementing full-fledged economic surplus approach for setting research priorities have led to the invention of **many alternative or substitute approaches**. Admittedly, they are not necessarily mutually exclusive and a number are often used in combination. A few are listed below for illustrative purposes:

- Informal or ad hoc: The simplest but least intellectually satisfying.
- Precedence: The previous year's funding is used as the base for the subsequent year, with incremental changes only. Practical but inimical to change.
- Peer review. Individuals or groups are asked to subjectively assess alternative proposals and rank them based on their preferences. It is pragmatic but does not really address the concerns which have led to the pressures for change.

[2]This is often done by asking specialists for their judgements on the matter and, through iterative confrontations, trying to reach a consensus view among specialists (Delphi methods).

- Congruence: Research resources are allocated across research areas in proportion to the value of agricultural production. Relatively simple but does not really address the hard questions.
- Scoring method: These are used when there are multiple objectives specified for research. These objectives are translated into a set of criteria. Weights are assigned to the criteria based upon the judgements of decision-makers, using Delphi methods, and these are used to create a weighted composite index for each alternative. These indices are used to rank alternatives for determining priorities.
- Economic surplus: (as briefly described above).
- Benefit-cost analysis: This approach is usually used in association with the economic surplus method. The costs of undertaking the research and the time required, as well as the "social discount rate" are employed to estimate the discounted net present value of the stream of economic surpluses from the innovations arising from the research. Net benefit-cost ratios are then calculated. Instead of using economic surplus estimations, s budgeting procedures which are less demanding of data and market parameter estimates, can be used.
- Econometric approaches: These are generally used to statistically estimate the aggregate impacts of past national research investments on the basis of equations linking index numbers series on input and output growth, including research, education and extension investments. The estimated equations may cover growth accounting, cost or profit functions, supply functions and production functions. They are all demanding in terms of time series data and economic skills, as well as time.
- Mathematical programming: This is the approach of choice of economists for implementing the economic surplus approach, as it allows an optimisation of the research portfolio subject to resource constraints. Of course, it is very demanding in terms of specification and data.

Hopefully, this list will suffice to illustrate that in choosing a method, one is always confronted with a dilemma between the search for simplicity at the cost of oversimplification, and a more intellectually satisfying approach, very demanding in terms of data, economic skills and heroic assumptions. Thus, the main lesson of this section of the paper is that formal procedures for research priority setting are in to stay even though the exercise is full of pitfalls. Thus, it is essential that agricultural researchers, who are not economists, become cognizant of the economic concepts -mainly those associated with economic surplus- providing the intellectual framework for these procedures, and aware of the limitations as well as relative merits of the various methods used. The temptation to ignore the concerns underlying the pressures for the adoption of these complex procedures should be resisted because ignoring these concerns is a luxury that few research institutions will be able to afford.

III- Implications for grassland researchers

The challenges faced by the grassland research community as a result of the changes and institutional pressures discussed above are very daunting. This research community is directly impacted by the new research funding arrangements and financial strategies of research funding agencies. This community is also under the same pressures as other agricultural researchers to demonstrate past impact of research, to identify clear purposes for new research activities as well as expected outcomes. In addition the claims that these new

proposed activities deserve high priority will have to be substantiated. These challenges are formidable because grassland research does not lend itself easily to the same framework of commodity research suitable for crop research. Typically several products and services are involved. As a result implementing an economic surplus approach requires multi-market analyses, involving what economists call cross-elasticities, for which robust empirical data are scarce. In addition, grassland utilization often involves complex interrelationships with the resource base, which must be seen within an ecological system perspective. This complicates by several orders of magnitude the development of quantitative models capable of taking these important and complex interactions into account. Indeed, these interactions are often not known with sufficient precision to be readily amenable to quantitative modelling. In this context, grassland researchers may be at a disadvantage to demonstrate quantitative impact and to implement sophisticated priority setting procedures.

The complexity of the interactions involved can usefully be illustrated on the case of the interaction between livestock and the environment in livestock grazing systems. The topic is important because livestock are often taken as scapegoats in the environment debates, as illustrated by the following two quotations: "livestock have been criticised for damaging the environment in a number of ways" (FAO, 1995) or "livestock have been charged with wholesale devastation of African rangelands and irreversible destruction of soils" (Winrock International Institute, 1992). As a result of these perceptions and also because of the complexity of the interactions involved in livestock projects, many development aid agencies are reluctant to get involved in activities supporting livestock production. Yet given the generally high income elasticities of the demands for many livestock products, there is no doubt that livestock development can greatly contribute to the fight against poverty. In addition, because of these high elasticities livestock development takes place, even in the absence of government attention and donor support; and this tends to be done in ways that ignore externalities, i.e. ways which have a high chance of being destructive of the environment. Keenly conscious of this dilemma, several senior livestock specialists from key development agencies took the initiative a few years ago to launch an inter-agency study of the relationships between livestock and the environment, in order to clearly identify the main problem areas, analyze them and, on that basis, to propose solutions (Steinfeld etal. (s/d). The authors convincingly argue that livestock are not the problem but what humans do with them may be. Hence, it is not surprising that the solutions they eventually propose relate to a wide array of human activities from economic policy reforms to technological change, through infrastructure development, institution building etc.

The main environment concerns addressed by the authors are grazing and overgrazing, livestock and deforestation, crop-livestock interactions, livestock and nutrition surplus and waste from processing. The fist two of these concerns are relevant for grazing systems, i.e. those of greatest interest to grassland researchers. The analysis stresses the complexity of the interactions involved in these systems leading to a set of interesting conclusions. These can be summarized as follows:

- Rangelands are increasingly seen as global resources for livestock production, but also eco-tourism, carbon sequestration and biodiversity conservation.
- The conversion of tropical rainforest into ranching is more and more widely recognized as economically, socially and environmentally wrong.
- Promoting the multiple use of grazing areas will require new educational, financial and institutional instruments. This will require major changes of attitudes vis-à-vis pastoralists and ranchers in the public at large, and among "rangeland and pasture agronomists", too many of whom "think only in terms of increasing levels of beef and milk productions".

- Major economic policy changes are needed: 1) to give proper value to the multiple benefits of rangelands and to share the costs of providing these benefits, 2) to correct the price distortions, such as fertilizer subsidies, which encourage environment degradation, 3) to recognize the links between various eco-systems, and 4) to address the underlying causes behind population pressure and poverty.

Interestingly, each one of these conclusion has important consequences for setting up a research agenda in support of the technological, institutional and policy changes which are explicitly or implicitly advocated. Most of that research agenda has to be couched in a broad system perspective and the research should often be of a multi-disciplinary nature. For an interesting example of a broad system perspective applied to the problems of rangeland management see Ison R. and Russell D. (2000). In principle, both features can be accommodated in a formal quantitative procedure for research priority setting but the challenges, which then need to be faced, are often daunting. Multiple objectives do not fit easily into an economic surplus framework. Modelling complex interrelationships among many variables is very difficult. Besides, the fact that many of these relationships are not known precisely forces analysts to make heroic assumptions, which undermines the credibility of the exercise. Finally, specifying the expected outcome of a given research activity impacting a complex system is of course hazardous. This however does not mean that grassland researchers will be exempted from the pressures to become more transparent and more rigorous in their research planning process, including impact assessment, priority setting and resource allocation. Coping with these pressures will undoubtedly be challenging. In some cases, quantitative exercises will be required. The difficulties discussed above suggest that, in such cases, it would be wise to select fairly broad research themes, possibly of a multidisciplinary nature, as units of analysis. In all cases however, there is no doubt that significant efforts will be warranted to specify clearly, at least in qualitative terms, the purpose of the research proposed, the type of result which can be expected and the consequences for the systems involved (i.e. possible outcomes). Such an attitude seems to me critical for the agricultural research community to regain its social credibility and, thus, be able to cope individually and collectively with the pressures profoundly affecting research funding and demanding more transparent and rigorous research planning processes.

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