THE WORLD BANK OPERATIONS EVALUATION DEPARTMENT

The CGIAR at 31:An Independent Meta-Evaluation of the Consultative Group on International Agricultural Research

Thematic Working Paper
Global Public Goods From the CGIAR: Impact Assessment

Bruce Gardner, University of Maryland

Director-General, Operations Evaluation: Gregory K. Ingram
Acting Director: Nils Fostvedt
Task Manager: Uma Lele, Senior Adviser, OEDDR

This paper is available upon request from OED.

2003
The World Bank
Washington, D.C.

ENHANCING DEVELOPMENT EFFECTIVENESS THROUGH EXCELLENCE AND INDEPENDENCE IN EVALUATION

The Operations Evaluation Department (OED) is an independent unit within the World Bank; it reports directly to the Bank's Board of Executive Directors. OED assesses what works, and what does not; how a borrower plans to run and maintain a project; and the lasting contribution of the Bank to a country's overall development. The goals of evaluation are to learn from experience, to provide an objective basis for assessing the results of the Bank's work, and to provide accountability in the achievement of its objectives. It also improves Bank work by identifying and disseminating the lessons learned from experience and by framing recommendations drawn from evaluation findings.

OED Working Papers are an informal series to disseminate the findings of work in progress to encourage the exchange of ideas about development effectiveness through evaluation.

Contact:
Operations Evaluation Department
Partnerships & Knowledge Programs (OEDPK)
e-mail: eline@worldbank.org
Telephone: 202-458-4497
Facsimile: 202-522-3125
http:/www.worldbank.org/oed

Abbreviations and Acronyms

CEP	CIMMYT Economic Program (CGIAR)
CGIAR	Consultative Group on International Agricultural Research
CIAT	Centro Internacional de Agricultura Tropical (CGIAR)
CIFOR	Center for International Forestry Research (CGIAR)
CIMMYT	Centro Internacional de Mejoramiento de Maïz y Trigo (CGIAR)
CIP	Centro Internacional de la Papa (CGIAR)
DGF	Development Grant Facility (World Bank)
Embrapa	Brazilian Agricultural Research Corporation
EU	European Union
FAO	Food and Agriculture Organization of the United Nations
GPG	Global public good
IAEG	Impact Assessment and Evaluation Group (CGIAR)
ICARDA	International Center for Agricultural Research in the Dry Areas (CGIAR)
ICLARM	International Center for Living Aquatic Resources Management (CGIAR)
ICRAF	International Center for Research in Agroforestry (CGIAR)
ICRISAT	International Crops Research Institute for the Semi-Arid Tropics (CGIAR)
ICW	International Centers Week (CGIAR)
IFAD	International Fund for Agricultural Development
IFPRI	International Food Policy Research Institute (CGIAR)
IITA	International Institute of Tropical Agriculture (CGIAR)
ILCA	International Livestock Center for Africa (CGIAR)
ILRAD	International Laboratory for Research on Animal Diseases (CGIAR)
ILRI	International Livestock Research Institute (CGIAR)
IPGRI	International Plant Genetic Resources Institute (CGIAR)
IRRI	International Rice Research Institute (CGIAR)
ISNAR	International Service for National Agricultural Research (CGIAR)
IWMI	International Water Management Institute (CGIAR)
NARS	National agricultural research systems
NGO	Nongovernmental organization
NRM	Natural resource management
ODA	Official development assistance
OECD	Organization for Economic Cooperation and Development
OED	Operations Evaluation Department (World Bank)
R&D	Research and development
SINGER	System-wide Information Network for Genetic Resources (CGIAR)
SPIA	TAC Standing Panel on Impact Assessment (CGIAR)
TAC	Technical Advisory Council (CGIAR)
TSR	Third System Review (CGIAR)
UN	United Nations
WARDA	West Africa Rice Development Association (CGIAR)
WTO	World Trade Organization

Director-General, Operations Evaluation	:	Mr. Gregory K. Ingram
Director (Acting), Operations Evaluation Department	:	Mr. Nils Fostvedt
Task Manager	:	Ms. Uma Lele

Contents

Figure

Preface

This is one of five thematic working papers by independent scholars prepared as part of the meta-evaluation of the Consultative Group on International Agricultural Research (CGIAR) conducted by the Operations Evaluation Department (OED) of the World Bank. The report, entitled *The CGIAR at 31: An Independent Meta-Evaluation of the Consultative Group on International Agricultural Research*, is available on OED's external Web site: http://www.worldbank.org/oed/gppp/. The thematic working papers are: C. B. Barrett, "Natural Resources Management Research in the CGIAR: A Meta-Evaluation," C. K. Eicher and M. Rukuni, "The CGIAR in Africa: Past, Present, and Future," B. Gardner, "Global Public Goods from the CGIAR: An Impact Assessment," W. Lesser, "Reviews of Biotechnology, Genetic Resource and Intellectual Property Rights Programs," and D. J. Spielman, "International Agricultural Research and the Role of the Private Sector."

The report on the CGIAR is part of a two-phase independent review by OED of the World Bank's involvement in global programs. The first phase has been published: *The World Bank's Approach to Global Programs: An Independent Evaluation, Phase 1 Report* (OED, Washington, D.C., 2002). The second phase, due in fiscal 2004, involves case studies of 26 programs, of which the CGIAR is one. The inclusion of the CGIAR evaluation in the OED review of the Bank's global programs was requested by the Development Grant Facility (DGF) and Bank Management in June 2001, and endorsed by OED's global program advisory committee.

While the focus of the meta-evaluation is on the Bank and the strategic role it has played and ideally will continue to play in the future in ensuring the CGIAR's development effectiveness, the thematic and country working papers and the country background papers focus on the different components of CGIAR activities that determine impact, including country perspectives. In addition to informing a broader understanding of the policy and technical context of CGIAR implementation, the papers provide a tool for assessing the performance and impact of the whole CGIAR partnership; this, in turn, provides a critical context for gauging the impact and value added of the Bank's participation in the program, the primary objective of the CGIAR meta-evaluation.

All five thematic working papers are based on extensive reviews of CGIAR's own evaluations as well as other related scholarly literature and discussions with relevant stakeholders. Four of the five thematic working papers were extensively peer-reviewed by knowledgeable external experts. A list of working and background papers and peer reviewers for the working papers is provided in Annex 2.

In addition, four country case studies on Brazil, India, Colombia, and Kenya provide developing country perspectives on the CGIAR. Two of the four – a study on India, written by Dr. J. C. Katyal and Dr. Mruthyunjaya, and a study on Brazil, by Jamil Macedo, Marcio C.M. Porto, Elisio Contini, and Antonio F.D. Avila – are issued as country working papers. The other two – C. Ndiritu, "CGIAR-NARS Partnership: The Case of Kenya" and L. Romano, "Colombia Country Paper for the CGIAR Meta-Evaluation"– are available on request.

The CGIAR was the first program providing global public goods to receive grants from the Bank's net income. Although the program has an impressive tradition of self-assessments, System-level evaluations have been few and far between. An exception, the Third System Review (TSR), was carried out in 1998, 17 years after the previous System-level review. OED determined that a meta-evaluation would most effectively assess CGIAR performance and inform OED's overall review of the Bank's involvement in global programs. In brief, the objectives of the meta-evaluation were three-fold:

- Evaluate implementation of recommendations in the 1998 TSR review
- Identify issues confronting the CGIAR from a forward-looking perspective
- Draw lessons for overall Bank strategy on global public policies and programs

The meta-evaluation report is in three volumes. *The Overview Report (Volume 1)* addresses strategic questions regarding the organization, financing, and management of the CGIAR as these have affected research choices, science quality, and the Bank's relationship to the CGIAR. *The Technical Report (Volume 2)* explores the nature, scope, and quality of the System's scientific work, assesses the scope and results of the reviews, and analyzes the governance, finance, and management in the CGIAR. *The Annexes (Volume 3)* provide supporting materials and are available on request.

Uma Lele
Senior Advisor Operations Evaluation Department
Leader, CGIAR Meta-Evaluation Team and Global Program Evaluation Teams

****

Bruce L. Gardner, who obtained his Ph.D. in Economics from the University of Chicago, serves as Distinguished University Professor and Chair of the Department of Agricultural and Resource Economics at the University of Maryland. He previously held positions as Assistant Secretary for Economics for the U.S. Dept. of Agriculture, a Fellow, and later President, of the American Agricultural Economics Association. Dr. Gardner has been a member of numerous international delegations and missions, primarily in Eastern Europe, and has published extensively on U.S. agricultural policies.

Summary

1. Attempts to estimate the returns to research on crop varietal improvement have generated a huge body of benefit-cost and rate-of-return studies. These are practically unanimous in finding returns well above the returns attainable from alternative uses of public funds. The studies do not address the separate contributions to yield increases of basic scientific work, development of particular new varieties, innovations in farming methods, training of scientists, or institutional development, but typically view the research system as a whole, a "black box" process through which research funds are transformed, after substantial lags, into increased productivity in farmers' fields. While the studies can legitimately be criticized on several grounds, they provide convincing evidence that, taken as a whole, the crop-breeding CGIAR Centers have generated extraordinarily high returns to investment.

2. *First caveat: Returns are likely to have been less high in recent years.* The evidence is weak on the extent to which the high rates of return have been maintained on research since the mid-1980s, inevitably given the long lags between research activities and results in the field. The limited evidence on returns to more recent research as compared to earlier efforts, summarized in Alston et al. (2000), reveals no decline in the rate of return to agricultural research in recent years. But even the most recent studies are assessing the results of research that largely occurred a decade or more ago. Evidence of recent slowdown in rates of increase in crop yields in many countries may mean that a decline in returns to research is now occurring.

3. *Second caveat: Attributing the role of NARS*. It remains unclear how much of observed productivity gains are properly attributed to the CGIAR Centers, for one reason because if the international agricultural research Centers (IARCs) had not undertaken their research efforts, the fallback would not be no research at all, but rather that done in national agricultural research systems (NARS) and other public and private entities. The relationship between the CGIAR and the NARS has changed significantly since the CGIAR founding in 1971. For one group of countries, mainly large ones such as Brazil, China, and India, the situation has changed from one in which the CGIAR is the mentor of NARS to the CGIAR as partner of NARS. For a second group of countries, notably smaller, poorer ones in Africa and South Asia, where both NARS and the private sector are weak, the CGIAR still has something more like its traditional leadership role in agricultural research. In either case it proves impractical to separate out the contributions of the IARCs and NARS convincingly. Rather, the research findings have to be considered products of their joint activity.[1] Returns to germplasm research have been so high that the benefits greatly exceed the costs of both the IARCs and NARS together. Nonetheless, the changed relationships suggest that a withdrawal or reduction in activity in CGIAR would now be less crucial for the NARS than was formerly the case, although the CGIAR is undoubtedly highly beneficial in many cases.

4. *Third caveat: Increasing role of private sector research*. The issue was posed as follows by Ingram (2001): "…[T]he argument that certain research should be subsidized underlies the CGIAR. Yet today, the bulk of agricultural research has moved from (subsidized) public institutions to private firms" (p. 229). He goes on to suggest that the enhancement of intellectual property rights is behind this shift. The literature on returns

to agricultural research has not fully come to terms with this issue.[2] While the opportunities for private agricultural research are growing, the experts who commented for this report uniformly believe it would be a considerable mistake to infer that publicly-funded agricultural research could be wound down without substantial net social loss. But recent developments serve to reinforce the idea that future consideration of public investments in agricultural research should be more forthright in taking into account not only the promise of the CGIAR's research agenda, but also the extent to which that agenda falls into the realm of applied science where private-sector activities are equally or more promising avenues of progress.

5. *Fourth caveat: Some activities related to crop varietal improvement have lower returns*. Many examples are suggested in the literature and in interviews, but there is no consensus on what specific activities have been disappointing. Longstanding efforts in development of scientists' skills and capacity building in developing countries are central to what the CGIAR has done and is doing, but as separable programs it is unclear they have the high returns that germplasm research has had.

Recently Expanded Areas of CGIAR Activity Have Unknown Returns

6. Donors have been attracted to having the CGIAR move into new environmentally focused areas like global warming, or at socially focused activities like empowerment of women or landless workers generally. While highly promising examples of improved sustainable productivity increases through new farming methods have been identified, evidence for generally high rates of return has not been documented, nor even widespread informal observation of major improvements comparable to what can readily be observed for crop yields using new varieties. Impact studies to date provide little quantitative evidence about net benefits or the rate of return to the new agenda – even in the case of policy research where considerable efforts have been made in IFPRI.

7. *Caveat*: An absence of documentable high returns should not be taken as evidence of low returns. Many experts consulted for this report expressed concern that too great a focus on impact assessment based on demonstrable returns will skew CG activities and harm the funding of some extremely valuable research efforts. The lags between research efforts and ultimate socio-economic impacts are so long and variable, and the outcomes so uncertain, and some of the benefits so fundamentally unquantifiable, that the whole benefit-cost approach to impact assessment approach is suspect in the view of some experts.

Expanded Objectives of CGIAR Research Are Likely to Have Reduced LARCs' Capabilities in Addressing the Traditional Objective of Productivity Improvement

8. More fundamental than a shift to new areas is a shift to broader objectives, notably sustainable poverty reduction. A general reason for doubt about returns to research aimed at such a broad objective, and an objective only indirectly related to crop productivity and farming methods, is the likelihood of getting into projects where neither the scientific nor the practical plant-breeding expertise that characterized the CGIAR's past successes are central. Beyond this, a focus on immediately reaching the poor, or increasing the role of poor farmers in setting the research agenda, runs the risk of

diverting efforts within the traditional CG agenda away from the scientifically most promising paths for productivity improvement.

9. *First implication: Refocusing of CGIAR research*. IFAD (2001) exemplifies the view that a re-emphasis upon research directed at yield improvement is needed. This recommendation is based on past achievements of the IARCs, coupled with observed reductions in real spending on core germplasm research, and evidence that the rate of yield growth since roughly 1990 has slowed in many developing countries.[3]

10. *Second implication: Restructuring of impact assessment*. Because returns are so difficult to estimate for recently emphasized areas of research, evaluation of future research alternatives has to be based on *ex ante* judgments of likely returns. One approach is to look for particular "success stories," and support research and related activities that appear similarly promising. This has been suggested particularly in the area of policy research. Another approach is to convene panels of experts and get their judgments, as is often done in using peer-reviewer panels in the awarding of competitive research grants. But either approach has drawbacks. They are subject to limits of specificity of the items evaluated and subjectivity in the process. More fundamentally, these methods do not work for comparing alternatives across types of research (e.g., agricultural research versus human health research) as benefit/cost evaluations can do.

11. *Third implication: Role of donors in research agenda and funding*. Donors will ultimately decide both funding and agenda in any case. But it is widely believed that donors' decisions have become too driven by their countries' politics and fashionable opinion, and that this had directed CGIAR efforts away from what the IARCs are best at doing. The question then is how to win the confidence of the donors in a funding mechanism that will allocate resources to Centers and activities where the global public good returns really are highest. A possible answer is to set up a general goal of the CGIAR, along the lines of the Strong Report, to pursue sustainable reductions in poverty through agricultural research, and then empower a "super-TAC" to run a competitive grant program that would fund multiyear research programs to the most promising, among the broadest possible set of scientists and/or institutions.

12. The three preceding main points and caveats require more detailed, nuanced discussion. The sections below take up that challenge as follows. After an introduction, Section 2 provides an overview of impact assessment issues, with a focus on the idea of global public goods from the CGIAR as the analytical basis for justifying international donor support, as opposed to funding from national governments or the private sector. Section 3 reviews the findings of a large literature of studies of rates of return to agricultural research. Section 4 considers criticisms and justifications of those studies, most importantly the ones noted under "caveats" in the preceding summary. Section 5 discusses the economic and social impacts of agricultural research, and how they relate to rates of return, with an emphasis on poverty reduction. Sections 6 and 7 develop the implications for priority setting and management of the CGIAR, and the chief questions remaining that might most usefully be investigated in further CGIAR assessment.

13. The approach taken in this thematic working paper, by virtue of which it is part of a meta-evaluation, is to rely principally on existing assessments of research and other

activities of the CGIAR System and of individual Centers, including both CG-commissioned studies and independently generated assessments by outside observers. The survey is meant to be representative of the evaluative efforts made and as comprehensive as possible in covering both in-house CGIAR and external assessments; but the large number of studies and the lack of a comprehensive data base, especially for assessments by outside scholars, mean that gaps in coverage are likely. A secondary source of assessments is views expressed in personal interviews and responses to questions given by people both inside and outside the CGIAR System. Some of these interviews and responses were arranged as part of the overall OED meta-evaluation process, and others were initiated by the author in order to clear up or expand upon statements in the published record.

---

1. Moreover the jointness appears to be complementary rather than competitive (or, in other jargon, the research sector exhibits economies of scope): that is, low national commitment of resources to NARS reduces the ability of IARCs to contribute to productivity growth.

2. In one of the few published studies of the facts and their implications, Pray and Umali-Deininger (1998) estimate that multinational companies as of 1985-90 were spending $36 million annually on agricultural research in developing countries, but even by 1995 the percentage of all agricultural research expenditures accounted for by private companies was low in these countries, ranging from 5 percent in Peru to 36 percent in Ecuador (with Brazil at 8 percent and India at 16 percent).

3. An alternative reading of the situation is that crop improvement research is continuing, so what the yield slowdown implies is diminishing returns to research efforts; and if the marginal returns are less, the appropriate investment strategy is to spend less, not more, on these efforts.

1. Introduction

1.1 One of the great post-World War II economic success stories is productivity increases for internationally traded agricultural commodities. Improvements have been sufficient not only to defeat earlier fears about feeding a rapidly growing world population, but beyond this to drive world commodity prices ever lower in real terms even as food demand more than doubled. International agricultural research Centers have been credited with a major role in this success story, so much so that the CGIAR model has been seen as more promising than the alternatives available for research outside the bounds of its original activities.

1.2 Nonetheless, questions have been raised about the value-added of the CGIAR System as it operates today, especially with respect to the expanding range of activities beyond crop varietal improvement that CGIAR Centers have increasingly undertaken. Even in the area of crop varietal improvement, national agricultural research systems (NARS) and private-sector enterprises have expanded their capabilities and scope of agricultural research and development activity in recent years, especially NARS in the largest developing countries. The frontier areas of research in applied biotechnology raise particularly important issues of the comparative advantage and appropriate role of the CGIAR as compared to NARS and non-governmental organizations both profit-seeking and non-profit.

1.3 In recent circumstances, has the CGIAR contribution kept pace? How certain can we be about the net value added that the international Centers have generated, as compared to the counterfactual situations in which they were much smaller, or had not been created, or at some point had been allowed to lapse? Looking to the future, is greater or less investment in the CGIAR by its donors warranted? What research areas offer the greatest promise for delivering global public goods in the future? And, more narrowly, what is the return to investment from the viewpoint of the World Bank in the context of its portfolio of global public programs?

1.4 Answers to these questions have been of scientific interest to economists and historians for many years, and of practical interest to the CGIAR and to donors. Particularly in the context of its Special Panel on Impact Assessment (SPIA), the CGIAR has undertaken an ambitious program of impact analysis. A most impressive amount of thoughtful and data-rich investigation has been carried out. The present paper is primarily devoted to a review of the findings and limitations of such investigations, principally by economists and other social scientists who have attempted to quantify the consequences of agricultural research. It particularly focuses on estimating the economic value of what has been accomplished by the CGIAR, how confident we can be in those estimates, and what the implications are for future investment in the CGIAR System.

1.5 The structure of this paper and tenor of the discussion is premised on the assumption that the main purpose of this paper – and the meta-evaluation of which it is a part – is to help the World Bank determine the place of the CGIAR in the Bank's portfolio of investments in global public goods. To have a place in this portfolio, an investment should ideally not only generate a high rate of return, but also provide returns that could not reliably be captured by private-sector investors (and so provide insufficient incentives for private sector investment), and, moreover, could not reliably be captured by national government investment. The latter criterion is particularly limiting in working against investments that might otherwise be viewed as quite promising ones – for example, improved crop varieties that are tailored to a particular

country or region within a country. Since practically all crop varietal improvements are so tailored, the ideal of pure global public goods has been tempered to obtain a realistic assessment of CGIAR contributions. The main tempering principle is that even though germplasm research has less than global application in typical cases, it is nonetheless the case that the research would not be undertaken in the absence of the CGIAR, either by NARS or private sector entities, because of research capacity and funding constraints and limited ability to pay in low-income countries. This subject is elaborated below.

2. Approaches to Impact Assessment

2.1 The impacts of primary interest are measured in terms of economic returns. Economists' research on returns to agricultural research typically estimates all the returns (not just public-goods generated), whether those returns are captured by private sector interests or not, whether accruing to the rich or to the poor within the developing world, and whether realized in a particular country or not (although a recently burgeoning but still empirically primitive literature addresses effects on poverty alleviation and national "spill ins" and "spill outs").

2.2 There are good reasons to count all the returns in deciding on the desirability of an investment, even if the focus is on global public goods, as argued in section 4 below. While the World Bank's approach (see Cooper, 2001; World Bank, 2001, Annex 2) treats public goods in the classical Samuelsonian sense (characterized by nonrivalry and nonexcludability, as epitomized by a radio signal that is no less available for others because I use it, and which I cannot be prevented from using), it is important to recognize that none of the investments the Bank will be considering are pure public goods in that sense. Instead the typical case is that some users cannot be excluded or charged for some uses of the goods or services produced. This complication is important because, even for research findings that have a large public benefit that cannot be captured by the researcher, it will nonetheless often pay for the private sector to invest just to get the benefits that can be captured. Kealey (1996), for example, emphasizes the role of private-sector providers (farmers and estate managers as well as suppliers of agricultural inputs and services) of agricultural research findings throughout history.

2.3 It is nonetheless to be expected that private-sector investment in research will not be carried as far as the level at which the added (private and social) benefits fall to the level of the cost of funds invested, and this is the fundamental reason for public institutions getting involved.

2.4 In the area of increasing agricultural productivity, the traditional focus of the CGIAR, genetic knowledge underlying improved seed varieties and innovative crop management methods has clear public-good properties. Although opportunities are increasing for private companies to establish and defend property rights and capture economic returns from such innovations, it is nonetheless expected that CGIAR work aimed at increasing agricultural productivity will continue to generate public-good benefits. Three other of CGIAR's major areas of activity – environmental protection, fostering biodiversity, and improving policies – also have strong public-good features.

2.5 The next two sections review existing assessments of CGIAR institutions' contributions to global public goods. The review in this paper, like the impact assessments that have been carried out, concentrates on the agricultural productivity-enhancing activities that the CGIAR has undertaken. This focus reflects the facts that work on environmental protection, fostering biodiversity, and improving policies is relatively recent, and that impact assessment – even quantification of the baseline situation into which innovations are introduced – is very difficult. The agricultural productivity area has been most thoroughly studied, but nonetheless assessments that have been carried out typically note that impacts are quite difficult to estimate. Some scholars indeed have doubted the credibility of such estimates in even the most well-worn areas of measurement.

2.6 Different evaluations have looked at different measures of impact. There are also differences among evaluators in what kinds of impacts are seen to be most important. Some of the main possibilities include the following (not all of which are independent of one another):

2.7 Direct impacts:

- Additions to practically usable scientific knowledge
- Development of new or improved seeds or other farm inputs
- Implementation of policies conducive to technology development and adoption
- Improvement of national research capabilities (especially well-trained scientists)
- Strengthening of national capabilities in policy analysis and development

2.8 Welfare-increasing results:

- Growth of agricultural productivity
- Increases in farmers' well-being
- Improved food security
- Improved resource and environmental sustainability
- Fostering rural development
- Contribution to poverty reduction

2.9 It is difficult enough to estimate research impacts on the most directly observable items – papers or patents produced, citations of a program of work, yield increases on experimental plots, adoption of new varieties, etc.; yet for impact assessment to be useful in making recommendations about CGIAR operation and funding, more is needed. We require information about further consequences that can be assigned a monetary value, which can then be weighed against the costs of the research. Important aspects of value added for such indicators are having a *net* measure that accounts for:

- the cost of what was accomplished,
- the prospect that if the CGIAR institutions had not carried out an activity that generated impact X, some other publicly funded institution or profit-seeking enterprise would have, and
- (from the World Bank's viewpoint) the prospect that if the Bank had not supported the CGIAR activity, some other donor would have.

- a value-added measure that is *commensurate* across component impacts, which is typically the stage at which we give monetary value to achievements. The difficulty is that we will not in practice be able to aggregate all impact indicators. For example, we may have real income gains, poverty reduction gains, and environmental gains as individual items, without having convincing ways to place commensurate values on them so as to add them up.[1]

2.10 An alternative way to categorize CGIAR outputs is by "programme thrusts" of the CG, as for example in the Danish review, which looks for results in the following six areas (Danida, 1999):

(a) Increased productivity of resources committed to food production
(b) Improved management of natural resources
(c) Improvement in policy environments in developing countries
(d) Capacity building in NARS
(e) Germplasm conservation
(f) Building linkages between NARS and other components of the global agricultural research system

2.11 But in considering how to measure results in these areas, one is led again to the issues previously discussed in this section.

3. A Survey of Selected Impact Studies

3.1 This section considers papers that include, and usually have as their main purpose, assessment of the effects and effectiveness of various aspects of CGIAR activities. The studies reviewed fall into three categories, by provenance:

- System-wide assessments commissioned by the CGIAR, in recent years the responsibility primarily of the Impact Assessment and Evaluation Group (IAEG) and its successor, the Standing Panel on Impact Assessment (SPIA). These include a few early assessment efforts but focus mainly on those produced since 1995. The studies include Anderson, Herdt, and Scobie (1988), Heisey, Lantican, and Dubin (1999) [unclear if this was commissioned by CGIAR – no sponsor listed in the front-matter], Dixon (1999), Alston and Pardey (1995), Evenson (2000), Evenson and Gollin (2001), Waibel (1999), Maredia, Byerlee, and Anderson (2000), Dalrymple (2001), Pingali (2001), CGIAR (1997a), CGIAR (1997b), CGIAR 1998a, 1998c), CGIAR (1999), CGIAR(2000), and CGIAR (2001). Some of these studies, notably ones carried out under the auspices of IAEG and SPIA, have evaluated the impact assessment process itself. Discussion of these papers is taken up in a more explicitly evaluative section following this primarily descriptive one.
- Impact assessments of programs in individual Centers, including Kumar and Rosegrant (1994), Jha and Kumar (1998), Manyong, Dixon, et al. (2000), Monyong, Kling, et al. (2000), Anderson, Moscardi, and Pardey (1994), Raab, Denning, and Cruz (1998), Ryan (1999a and 1999b), Paarlberg (1999), Farrar (2000), Morris (2001), Hazell and Haddad (2001a, 2001b), and CGIAR (1998b).

- (3) Impact studies by institutions or scholars outside of the CGIAR, some of them at the behest of donors, including Hayami and Herdt (1977), Pasour and Johnson (1982), Fox (1985), Dalrymple (1985, 1986), Farrington and Martin (1988), Lipton and Longhurst (1989), Brennan (1989), Pray et al. (1991), Byerlee and Moya (1993), Kealey (1996), Anderson and Dalrymple (1999), Byerlee (2000), Alston, et al. (2000), Asian Development Bank (2000), Otsuka (2000), Ingram (2001), Danida (1999), IFAD (2001), and Alston (2002).

3.2 The following discussion of these studies below is not organized by provenance, but rather by type of impact considered. The first and by far most thoroughly investigated impacts are effects on agricultural productivity growth (and its economic consequences). Second are studies of impacts of research aimed at goals other than productivity growth, a miscellaneous category including environmental improvement attributable to innovative natural resource management and government policy changes attributable to analyses carried out at the International Food Policy Research Institute (IFPRI). Third, and briefest, are impacts of CGIAR capacity-building efforts. Most of the discussion below deals with productivity-enhancing results of crop improvement research, as its track record is the longest, best documented, and most thoroughly assessed in prior reviews.

Research on Improvements in Crop Varieties and Farming Methods

3.3 The first comprehensive attempt to assess the impact of the CGIAR was that of Anderson, Herdt, and Scobie (1988). They drew upon many prior piecemeal studies and made a most admirable attempt to pull the available information together into a comprehensive whole. Key elements of their assessment include the following:

- Vast areas of wheat and rice planted to high-yielding varieties in developing countries. AHS take from Dalrymple (1986) an estimate that by 1982 high-yielding wheat was being grown on 48.5 million hectares, and AHS estimate that by 1986 high-yielding rice was being grown on 76 million hectares. These areas amount to half and 58 percent, respectively, of the total wheat and rice area planted in the countries using those varieties. AHS estimate that high-yielding varieties brought about an increase of 40 million tons of wheat and rice annually as of the mid-1980s. This estimated increase is over and above the output generated by complementary inputs such as fertilizer and irrigation, whose use increased in tandem with the adoption of high-yielding varieties. AHS estimate that new varieties thus "made it possible to meet the food grain demands of perhaps 500 million people," (p. 7). Alternatively, one could say – although AHS do not – that at mid-1980s world prices of about $150 per ton, the value of output, net of increased input costs, generated by high-yielding wheat and rice varieties was $6 billion annually.
- 200 new varieties of maize and many new varieties of cassava, potatoes, beans, sorghum, and other food crops. But no estimates of acreage planted to new varieties, or output effects.
- Large collections of germplasms: seeds, roots, and cuttings. But no estimate of the value of this resource.
- Many new and improved farming techniques. But no quantification of extent or value.

- More than 16,000 scientists trained in CGIAR Centers by 1983, and publications and other materials and research methods that have strengthened national agricultural research institutions (NARS) in developing countries. But again no quantification of the value added to scientific capacity was attempted.

3.4 A question about each of these impacts is how confident we can be that they are net impacts of the CGIAR. AHS note that new varieties have been produced in collaboration with NARS. Pray et al. (1991) provide evidence that substantial parts of growth in new varieties in India is attributable to research carried out by the private seed industry, and likely this is not an isolated case. In the instance where large gains are best documented, the increases in wheat and rice yields, AHS do not attempt to parcel out credit to the CGIAR and other public and private institutions.

3.5 Another complication that can blunt the benefits attributable to new varieties is that yield increases are often accompanied by increased purchased inputs or other resource use. AHS attempted to adjust for costs of added inputs with new varieties (although how they did this is not clear). Some have argued that increases in unpriced or underpriced input use, and consequent reductions in water quality or environmental amenities should also be factored in. A counterargument, however, is that if yield increases had not occurred, a larger land area would have to be cultivated in order to produce the same agricultural output, that the added area would be largely marginal lands, and that the environmental damage likely from this expansion of cultivated acreage at the expense of forested or other uncultivated land would be more significant that any problems associated more intensive use of the smaller cultivated areas under higher yielding crop varieties. Evidence that environmental costs of improved varieties are widespread or quantitatively significant appears to be absent in any case.

3.6 Despite the preceding caveats it appears that any reasonable assessment will give a substantial share of the credit for improved varieties to the CGIAR, and any substantial share will make the world's investment in the CGIAR a hugely profitable one.[2] The world's total contributions to the CGIAR Centers was about $150 million annually circa 1980.[3] Giving CGIAR credit for half of the yield gains for just wheat and rice, and supposing the benefits were only half the annual $6 billion above (and allowing no benefits at all for the other crops and services listed), an investment of $150 million a year that generates over $1 billion per year has to be reckoned a great bargain.

3.7 Huge net gains may be expected from investments that triggered innovations in developing countries that "have spread more widely, more quickly, than any other technological innovation in the history of agriculture…" (Dalrymple, 1985). Nonetheless, post-Green Revolution innovations in other crops over the last two decades have not been anything like so dramatic, so the world may still ask of the CGIAR what it has done for it lately. More to the point for today's investing institutions, based on currently available evidence of recent innovations, what is the expected payoff from future investment?

3.8 Anderson and Dalrymple (1999) provide a summary evaluation including more recent history. They document increases in crop production and yields in developing countries, but do not attempt to estimate the portion of the increases attributable to the CGIAR. They note that over 50,000 or about one-third of developing country scientists have received training in CGIAR programs. A&D present more detailed data for estimated impacts of individual

Centers. The wheat area planted to varieties having CIMMYT-related germplasm increased from an estimated 41 million hectares in 1990 to 51 million hectares in 1997, with the additional wheat production generated by the use of CIMMYT varieties valued at an estimated $1.8 billion in 1997.[4] They similarly report an estimated value of additional CIMMYT maize germplasm of $1 billion in 1997.

3.9 Jha and Kumar (1998) estimate high net returns for IRRI and national rice research (jointly, without claiming to sort out the particular contribution of either IRRI or national research separately) in the particular case of India, drawing primarily on state-level data analyzed by Kumar and Rosegrant (1994). They report estimates of internal rates of return for eight states, ranging from 32 percent in Karnataka to 74 percent in Utter Pradesh. One may ask whether it is possible the returns should be attributed more to IRRI or the national efforts. The returns take into account the costs of both; and we can at least say that the joint effort is highly profitable.

3.10 Heisey, Lantican, and Dubin (1999), update earlier work of Byerlee and Moya (1993) to estimate the impacts of international wheat breeding research in the developing world. They report that in the 1990s the area planted in developing countries continued to expand, to "just over 80 percent of all wheat area in 1997" (p. 8-1). CIMMYT's wheat program, as coordinated with NARS and ICARDA, made over half the spring bread wheat crosses released in the developing world since 1985. They estimate yield gains averaging 0.2 to 0.4 metric tons per hectare for all improved wheat, and value the benefits of international wheat breeding at $2 to $8 billion annually, depending on assumptions used, resulting from an investment of "perhaps 100 to 150 million dollars" (p. 8-3). They consider a wide range of assumptions about wheat production technology, particularly substitutability among inputs, and what would happen in the counterfactual situation of no international wheat breeding effort, but some key features of their estimating methods remain opaque. (Issues in proper estimation of economic impacts are discussed below.)

3.11 Morris (2001) estimates the yield increases and resulting economic benefits to developing countries attributable to the maize breeding program of the international Centers (essentially CIMMYT) over the 1966-1998 period. This program is modest in size relative to NARS and private-sector investment, as compared to wheat and rice research, so the difficulties in separating out the CGIAR's effects and specifying the counterfactual yield path that would have occurred absent the CG programs are even more important to the assessment. Morris has a reasonable discussion of attribution difficulties and handles the very substantial residual uncertainty by considering a range of effects as follows: yield increases as little as 15 percent or as much as 45 percent due to adoption of CIMMYT-derived maize varieties, with a contribution of CIMMYT germplasm to the increases of as little as 25 percent or as much as 75 percent. The result is an estimate of economic benefits of the CIMMYT maize breeding program of between $167 million and $1,503 million annually (Morris, 2001, Table 17).

3.12 Morris also recognizes and attempts to account for difficulties in estimating the costs of the maize breeding program. The principal difficulty is that maize research is to a significant but not precisely quantifiable extent a joint product with wheat research and other activities and CIMMYT. Considering alternative possibilities of cost allocation – all of which are ultimately arbitrary for joint products – Morris place the cost of the CIMMYT maize breeding program as low as $8 million or as high as $18 million annually. What is striking is

that even if we take the highest plausible costs and the lowest plausible benefits, we still have an annualized benefit/cost ratio of 167/18 = 9.3, implying a truly phenomenal rate of return (although precise estimation of a rate-of-return equivalent would require assumptions about the time path of benefits and costs). [5]

3.13 Studies on commodities other than the internationally traded cereals are fewer and less well developed, but tell a similar story. Manyong, Dixon, et al. (2000) estimate the impact of improved cassava from the International Institute of Tropical Agriculture, with benefits accruing from yield increases on about two million hectares on which new varieties were adopted, mainly in the 1990s. They estimate a 49 percent increase in average yield on this area, with an annual economic value of about $400 million as of 1998.[6] Similarly, Monyong, Kling, et al. (2000) estimate about $500 million in economic gains in 1998 due to improved IITA maize varieties (identified separately from CIMMYT varieties) in 11 African countries.[7] The data on which the yield gains in adopted areas and the size of those areas are based are not well documented in these studies – the main source is questionnaires sent to crop-breeding experts in each country, not on-the-ground surveys of areas planted or yields. But even if the economic benefits are only half the estimated amount, and the 1998 benefits persisted for only five years, the returns to investment in research in these two crops would exceed the aggregate IITA budget for 1971-1990 by five times.[8]

3.14 Other impact assessments have generated similarly large net benefits as compared to costs. Pingali (2001) summarizes and interprets earlier reviews of 17 pre-1990 CGIAR impact studies from Echeverria (1990) and later ones focused on particular outcomes such as fertilizer use, machinery adoption, pest management, and agriculture as an engine of economy-wide growth. The rates of return, for research in rice, wheat, maize, potatoes, beans, cassava, and cowpeas, range from 17 to 332 percent annual rates (p. 5). Even allowing for various possibilities of overstatement of effects, a body of research that has generated billions of dollars of additional product annually as result of expenditures of less than $20 million each year (on average during the 1970s and 1980s) at CIMMYT, or $100-150 million for the CGIAR altogether as Heisey et al. estimate, indicates an extraordinarily high social rate of return on investment.

3.15 Such high rates are the rule rather than the exception in agricultural research studies dating from the original 1950s work of Griliches (for example, his 1958 *Journal of Political Economy* paper) on returns to U.S. hybrid corn research. Anderson and Dalrymple cite work of Pardey and his colleagues at IFPRI that, in a review of rates of return to individual CGIAR Center research, find rates of return comparable to these high rates (see Anderson and Dalrymple, 1999, p. 55). They also review other attempts to document individual Center accomplishments, notably for ICRISAT and CIP, in higher-yielding varieties of other commodities, better methods of disease or pest control, and other outcomes such as training scientists. Some of these estimated results are impressive but not quantified as to value.

3.16 Alston and Pardey (1995) earlier reviewed impact assessment and broader research evaluation efforts in the CGIAR. They did not however come up with pointed conclusions criticizing then-current practice or with proposals for good practice in the future. Overall, their review and the recent reports from the Centers confirm the view one obtains from the broader literature that evidence of high rates of return has been confirmed many times for the traditional cereal crop variety improvement efforts.

3.17 Nonetheless, a recurring weak point in most studies is estimating the contribution made to improved yields specifically attributable to the CGIAR. Evenson (2000) addresses an aspect of this question by trying to sort out the effects of CG institutions from those of NARS. He did this principally by asking how much of NARS-generated improved varieties could be attributed to genetic material that originated in international research Centers. He estimated a net effect intended to compensate for a possible "crowding out" effect, according to which scientific effort is not undertaken in NARS if effort that would substitute for NARS efforts is carried out in the international Centers.

3.18 The way in which the net effect is estimated is not presented in sufficient detail for me to understand it completely, and as presented (principally in Appendix C) is not fully convincing. The dependent variable is called "the number of scientist man years of CGI effort in the NARS program" (p. 9) in the text, and "NARS CGI investments" in the appendix table (Table C2), which is defined quite differently at the bottom of p. 18 and calculated, apparently, as "CGI Research Intensities" in Table C1. Still the regressions of Table C2 appear to use the man-years-in-NARS dependent variable. The resulting estimate is that CGIAR germplasm, at the average levels of the variables, increased NARS efforts by 19 percent – any "crowding out" is overwhelmed by a positive effect of availability of CGIAR germplasm. Since the CGIAR share of total NARS and CGIAR expenditures is quite small, 10 to 20 percent as estimated on p. 19, this is an impressive finding.

3.19 However, the finding turns on adding the effect of two regression coefficients, one of which is an interaction coefficient multiplying the log of CGIAR germplasm by the log of crop area in each country. Because the straight-up germplasm variable is only marginally significant at best, and the cross-effect not much more so, the statistical significance of the joint effect is questionable (the appropriate F-test statistics are not reported). More importantly in my view, there is no strong reason for this interaction term (and no other) to appear in the regression, and the evident extensive specification search that occurred, with respect both to inclusion of variables and functional form, make it crucial that the findings are robust to specification.

3.20 Evenson goes on to estimate that improvements in crop varieties planted account for half to three quarters of productivity gains in the crops considered (wheat, rice, maize, barley, beans, potatoes, lentils, cassava), and with further aggregate econometric work, and not unreasonable assumptions about CGIAR/NARS interaction, estimates that "The actual rates of CGI [crop germplasm improvement] produced approximately 1.2 percent productivity growth per year. Growth accounting studies show that for rice in Asia, the combined impact of CGI and non-CGI research, extension, markets, and infrastructure was roughly 1.9 percent per year. The reduced CGI scenario calls for reducing this by .5 percent" (p. 12). Evenson and Gollin (2001), however, suggest a more important role for the CGIAR in a broader context, stating that "Our counterfactual calculations suggest that something like 50 to 60 percent of the Green Revolution might have been achieved without the IARC's (international agricultural research Centers). The use of "might have," however, indicates a lack of confidence in this estimate and the grounds for it are not presented by Evenson and Gollin.

3.21 In the economic simulations presented in Evenson (2000), the absence of CGIAR output is taken to reduce total factor productivity growth by 0.45 percent annually in South Asian rice, as an example. For all the crops considered, the resulting commodity price decreases range from 27 to 41 percent in 1970-1995 using IFPRI's IMPACT (International

Model for Policy Analysis of Agricultural Commodities and Trade) framework. Presumably the price effects result from the 0.45 percent annual TFP growth difference cumulated over the 25 years, although I didn't find this stated explicitly.

3.22 Taking the midpoint of the 27 to 41 percent range, and cutting that in half to represent an average year in 1970-1995, gives a conservative estimate of a17 percent net cost of production reduction attributable to CGIAR research. With about 500 million tons of relevant crop production at an average value of $100 per ton, the gains are worth roughly $8.5 billion annually, far in excess of the roughly $200 million annually spent on the CGIAR over this period (my calculation not Evenson's). These results are similar in magnitude to the very high ones taken earlier as implied by the original study of Anderson, Herdt, and Scobie (1988) and the later ones of CGIAR (principally CIMMYT and IRRI) cereal crop breeding research.

Beyond Research on Production of Cereals

3.23 When one moves to other commodities and to CGIAR efforts in areas less directly related to agricultural productivity, the evidence available on impact shrinks tremendously – indeed hardly exists for substantial areas of CGIAR activity. Recent discussion of impact within the CGIAR indicates a concern with estimating these impacts.

3.24 The First External Review of the System-wide Genetic Resources Programme (SGRP) contains a chapter assessing the performance of the SGRP since its creation in 1994 (CGIAR 1998c, Chapter 4). The section on "accomplishments," however, finds few concrete achievements. Most promising appears the System-wide Information Network for Genetic Resources (SINGER), but while this program was to provide access to users by 1997, it appears that only first steps have been made and "Potential users, particularly among developing country NARS, have commented on the inaccessibility of information on genetic resources…"(p. 38). Impact appears to have been essentially nil, which would be acceptable for a complicated effort begun in 1994 and reviewed in 1998; but more troubling is that one doesn't see the program on a path to results that will justify the $5.2 million spent on the SGRP during 1995-1998.[9]

3.25 CGIAR (2001) contains summary papers by individual Centers on their impact assessment activities that begin to place some of the broader issues on the table. A brief summary of each of these reports follows:[10]

- CIMMYT: the huge body of work (175 papers, mostly from the 1990s) on adoption and impact of CIMMYT varietal research are cited. Some notable quantitative estimates of returns have been quoted above.
- CIP (International Potato Center): claims that an "impact Czar approach to ex-post evaluation has worked reasonably well" (CGIAR 2001, Annex 2, p. 39). Seems to be a qualitative story-telling rather than quantitative approach, and no rate-of-return estimates are cited. [11] But Fuglie and others of their staff have published estimates of high returns to potato research elsewhere. Walker (2000) summarizes ten impact case studies of improved potato varieties and pest control technologies in ten different countries that have estimated rates of return ranging from 27 to 202 percent.

- ICARDA (International Centre for Agricultural Research in the Dry Areas): Situation summarized by the section headed "Data: a prerequisite." No quantitative return estimates reported or cited.
- ICLARM (International Centre for Living Aquatic Resources Management): One case study cited which found farm income raised about 65 percent with the adoption of integrated rice-fish farming in Bangladesh.
- ICRAF (International Centre for Research in Agroforestry): Mentions interviews with African stakeholders and establishing database in benchmark watersheds, no quantitative assessments cited or reported.
- ICRISAT: Mentions database on varietal adoption and farm-level surveys. Discusses linkage of impact assessment with priority setting. No quantitative studies cited but for ICRISAT there have been some.
- IFPRI: Discusses major effort to measure the benefits of policy-oriented social science research, and the use of policy research in the policy process. IFPRI has been responsible for many rate-of-return studies and a major meta-analysis of rate-of-return studies. This work has generated contributions to creating methods of impact assessment that other Centers may use (more detail on IFPRI below).
- IITA (International Institute of Tropical Agriculture): Rates of return from an economic surplus model of gains from biological control of cassava green mite in Benin, Ghana, and Nigeria are presented, ranging from 100 to 117 percent. This looks rather like an *ex ante* calculation of expected results rather than an estimate of actual results, however.[12] Some 54 papers are cited on impact and adoption studies, but their titles and checks of a few indicate few quantitative rate-of-return estimates of the type reviewed above. However, the two cited earlier that did undertake economic return estimates implied very high returns.
- ILRI (International Livestock Research Institute): Mentions three completed economic impact studies of ILRI-related technology, but cites no findings. The reports are said to contain "important lessons for the future delivery and dissemination of ILRI research products" (p. 81).
- IPGRI (International Plant Genomic Resources Institute): The nature of IPGRI's output does not lend itself well to traditional impact assessment, as it does not generate findings applicable agricultural production either directly or with further identifiable development. This Center has nonetheless has undertaken interesting attempts to measure the value of germplasm conservation and other public goods it provides; but as yet with no quantified results.
- IRRI: A rich history of quantitative impact assessment is cited, going back to Hayami and Herdt's classic work (1977). Recently IRRI has moved beyond estimates of returns to varietal research, which is responsible for some of the high rates of return to improved rice varieties cited above, to conduct impact assessments of innovations in farming methods. An example is described, of a contour hedgerow-based farming system to control soil erosion, using data from 74 adopters and 56 nonadopters. Economic return estimates were not reported, but the discussion suggests they were disappointingly low. Note however that five IRRI projects, only one of which involves an improved rice variety, are reported in Asian Development Bank (2000) to have generated an average internal rate of return of 83 percent (p. 22).
- ISNAR (International Service for National Agricultural Research): This is another Center whose on-the-ground economic impact is inevitably going to be difficult if not

impossible to estimate with confidence. ISNAR worked on methods of impact assessment to be used by NARS and other CGIAR Centers. The impact of developing impact assessment methods is a second-order task like that confronting evaluation of IFPRI's research program. ISNAR has not attempted to estimate benefit/cost or rate-of-return estimates for such work.

- IWMI (International Water Management Institute): Here again we are not dealing with crop varietal research. IWMI is widening its range by considering impacts of its work on other scientists, on "international policies and discourse" (p. 107), individual governments' policies, and human resources. But no findings about impact were reported.
- WARDA (West Africa Rice Development Association): Ten varietal rice adoption studies are cited, all in West Africa, as well as a comprehensive review of the impact of improved rice varieties in West Africa. Under "conservative" assumptions, the value of rice production is estimated to have been increased by $93 per hectare (p. 113). The specific source for this estimate was not cited, so one cannot check the methods used to arrive at this estimate – and without estimates of area planted or costs, a rate-of-return estimate cannot be made.
- There were no reports on impact assessment from the International Center for Tropical Agriculture (CIAT) or the Center for International Forestry Research (CIFOR).

3.26 The Third System Review, referred to as the "Strong Report" (CGIAR 1998a) afforded an opportunity to make a first attempt to come to grips with the question of evidence on what the expanded CGIAR agenda has achieved or is in process of achieving. The Review did not however take up this challenge. Rather it "focused its attention and its work primarily on these issues it considers of highest priority for setting the direction for the CGIAR System as it moves into the next century and in ensuring that it has the resources and capacities to fulfill its mission in an increasingly complex and changing world." (CGIAR 1998, p. 38). The Report does point to "a universally acknowledged record of success," states that "Investment in the CGIAR has been the single most effective use of overseas development assistance (ODA), bar none," and that "These results have been possible because the CGIAR has been totally focused on agricultural science…" (p. 36).

3.27 Even if one is prepared to accept the past successes of the CGIAR as common knowledge, the Strong report can be faulted for lack of serious attention to the question whether the high-payoff achievements had been maintained in the period since the previous System-wide review (which after all raised some alarms). Moreover, one would think in the context of the 1998 review that it would be thought crucial to establish that CGIAR's success is being maintained after the focus has been diffused to cover environmental sustainability, biodiversity, poverty reduction, and other goals beyond increased agricultural productivity, especially because the science to be brought to bear in pursuit of these goals is not predominantly the agricultural science at which the CGIAR has performed so well.

3.28 The Strong Report sees as a past weakness of the CGIAR the neglect, recognized in the 1980s, of efforts to move beyond concentrating on productivity improvements alone "to integrate the dimensions of ecological and social sustainability with that of economic viability." (p. 51). More recently that neglect has been remedied, which the Strong Report applauds. But again, no substantive achievements in these directions are cited, or apparently

were even sought. The Report does regret a perceived insufficiency of progress on the traditional objective of productivity improvement in low-yield and high-risk ("less-favored") areas. This quite limited and informal impact critique could be the basis for questioning the expanded agenda. An alternative conclusion could be a recommendation for further concentration on research more directly aimed at less-favored areas, even if research resources have to be redirected from more favored areas. The Strong Report takes an easier alternative with a call for additional resources to pursue the new agendas without squeezing the areas of traditional success.

Environmental Impacts

3.29 Waibel (1999) provides one of the few systematic attempts to assess CGIAR research in an area other than crop varietal improvement. He focuses on efforts in integrated pest management (IPM). He notes good acceptance of CGIAR research findings on IPM in the science community but has less apparent impact "with partners involved in implementing IPM field programmes" (p. 39). But he finds heartening examples of success of IPM programs in crisis pest management situations, and points to studies indicating environmental gains in replacing chemical with biological control methods. But he finds rate-of-return estimates less than solid, citing alternative views of an extension program in Kenya (not a CGIAR program) in which a rate of return estimated at over 100 percent could well have actually been zero (p. 28). Waibel's conclusion is that the rate of return to investments in IPM has been "in the order of magnitude of 15 to 40 percent" (p. 41) – though I have to say I could not see how he came up with this estimate.

3.30 The Standing Panel on Impact Assessment (CGIAR 2000b, Appendix A) addressed the vexing question of the state of knowledge on the environmental impacts of yield-increasing research. One hypothesis about environmental damage could, if correct, substantially reduce the rates of return to germplasm research that existing studies have found: namely, new varieties result in increased fertilizer use and more irrigation, and the former contributes to water quality degradation and the latter to salinity. The report draws on several prior studies to estimate that about 25 million hectares of cropland as of 1998 had been abandoned because of salinity. Of course some and perhaps most of this would have occurred had there been no improved varieties. Water quality problems due to over-fertilization could not be identified as a significant problem. But, it is estimated that in the absence of CGIAR-generated new varieties, land in crop production would have been 230 million hectares greater (p. 14). Discussion of uncertainties in all these estimates ends with a convincing case that added cultivation of marginal lands without improved varieties would have had negative environmental effects that far outweigh negative environmental effects that improved varieties may have caused. Thus the CGIAR should feel free to continue to focus on productivity-enhancing research.

3.31 The CGIAR's First System-wide Review of the "ecoregional approach," which is "conceptually holistic, combining human and technical dimensions and linking productivity and natural resource management concerns" was undertaken in CGIAR (1999 – quotation from p. xix). But the progress in estimating impacts in any quantified way was disappointing. With respect to economic impact the Report states: "The Review Panel was unable to obtain hard data on value added" (p. xx). They did not report soft data or other evidence on economic effects either.

Social Science and Policy Research

3.32 One nontraditional area where considerable effort has been spent on impact assessment is social science research. An important body of social science research for the CGIAR's purposes is the explanation of technology adoption. CGIAR Centers have not been at the frontiers of this research, which has been primarily the province of academic economists and sociologists (see Sunding and Zilberman (2001) for a review). But applied work in adoption of new varieties is central to many studies of the benefits of research on agricultural technology improvements.

3.33 An area where social science research has taken on a major role within the CGIAR is policy research, particularly at IFPRI.[13] Policy research can be undertaken in a variety of ways and on many topics. The main alternative approaches are methodological and applied. Research on methods of policy analysis has focused on ways to adapt and extend standard welfare economics (mainly benefit/cost analysis and social rate-of-return estimation) for purposes of agricultural and rural development policy work. Applied research has sought to assess existing and proposed policy alternatives, to estimate the effects of policies. The applications fall into two main categories of policies: those of national governments and policies at the international level (for example, international trade agreements); and the national policy work falls into two main sub-categories, policies of developed (roughly OECD) countries, and policies of developing countries.

3.34 The appropriate role of policy research in the CGIAR has been a matter of debate since IFPRI brought into the CGIAR in 1977. A principal issue has been whether IFPRI would duplicate types of analysis carried out by FAO. The Technical Advisory Committee has stressed that IFPRI should give emphasis to the problems of developing countries and more general global analysis should be mainly in support of such policy research. A related issue was IFPRI's location in Washington, stemming from a concern about perceptions of a privileged position and undue donor influence.[14]

3.35 In addition to the quality and relevance of the research products themselves, a closely related issue is capacity building in policy research capabilities in developing countries. A refrain from many in developing country NARS, governments, and NGOs is that more even than policy advice, what is needed is better capabilities within developing countries to develop their own policy analysis and advice.

3.36 One of the main issues in evaluating the CGIAR policy research program is assessing the extent to which the program is giving sufficient priority to the areas of policy research where its comparative advantages are greatest, including both the generation of research products and policy analysis capacity building.

3.37 *Impact of research findings.* The main areas of IFPRI's work that are candidates for having valuable effects are (i) applied quantitative economics of world agriculture, such as the projections, as in the "2020" project, of future food supply/demand prospects and estimation of demand systems in developing countries; (ii) economic analyses of policies that affect nutrition and food security, rural poverty, agricultural investment, the adoption of new technology, or other aspects of rural economic growth in developing countries; (iii) more fundamental economic research in development of methods of analysis (such as computable

general equilibrium models) and in data generation through surveys of farmers and others (iv) direct involvement with policy analysis units in developing countries.

3.38 One of the most difficult issues facing IFPRI is how far and what directions to go in topic area (iii), as discussed further below. A specific area of methodological work that is central to the CGIAR is methods of measuring benefits and costs of research and the implied rates of return to research. IFPRI has helped developed and carry out impact assessments of individual IARCs and of the CGIAR System as a whole. This work is especially notable in the case of assessment of impacts on poverty, and on the topic of spillover gains developing countries have received as a result of germplasm research in IARCs. IFPRI has also carried out assessments of Centers' own impact assessments, which might be labeled meta-reviews.[15]

3.39 IFPRI has also done notable research on how to evaluate its own products. In 1997 and 2001, IFPRI held conferences in which ideas from a broad range of social scientists were solicited on the question of how to measure the benefits of policy-related social science research. Some of these papers contained estimates of impact as well as discussion of ways to measure such impacts. The principal areas of policy research that appear most promising in generating net social gains are those that influence policy decisions by national governments or international institutions. Increases in incomes or other social indicators that result from appropriate policy decisions can then be in part credited to the findings and influence of the research.[16]

3.40 IFPRI's projections of future supply/demand conditions globally and in the developing world have been notably sober, fact-based, and reasonable (e.g., Islam 1995). The recent projections, properly, have underlined the unlikelihood of worldwide food shortages. The result at the margin is to give support for emphasis on broad-based rural development (education, health, infrastructure) as a means to increase incomes and well-being in rural areas, as compared to emphasis on research aimed at increased agricultural productivity. However the review of IFPRI's 2020 Vision Initiative by Paarlberg (1999) suggests that IFPRI has tended to hedge the point. Paarlberg summarizes IFPRI's central message as that while the global food situation may appear safe, tremendous problems of food insecurity and hunger remain and that "Removing this suffering by 2020 will require, first of all, policies designed to improve the performance of developing-country agricultural sectors" (Paarlberg 1999, quoting Pinstrup-Anderson 1995). But what these policies might be, in particular the role played by agricultural research, is left unstated. This is not to deny that both areas of investment (agricultural research and broader rural development initiatives) can yield high rates of return. The question is rather what IFPRI has contributed to the policy debate. As an example of a positive assessment, Farrar (2000) reported on a broad review of IFPRI's food subsidy research. He does not estimate quantitative impacts of the work but makes the case well that IFPRI's long series of research efforts on food subsidies made it the recognized world source of expertise on the subject. Their signature policy recommendation was less emphasis on attempts to hold market prices down to benefit consumers and more emphasis on targeted subsidies directed to needy buyers (which tend to increase rather than reduce market prices), and this became quite broadly accepted wisdom. The bottom-line value of IFPRI's specific contribution is however not estimated – and is probably not estimable.

3.41 *Policy advice and capacity building.* In evaluating the impact of policy research the most helpful evidence available is qualitative rather than quantitative, as embodied in case

studies of IFPRI impacts on policy decisions in developing countries. These are spelled out in several detailed papers. For example, Ryan (1999a) investigated the returns to IFPRI's rice market and policy research in Vietnam. The work was well integrated with Vietnam's Ministry of Agriculture and Rural Development, thus minimizing (but not eliminating) the outreach problem of getting an audience for the findings. IFPRI made a large number of generic and specific policy recommendations, among them recommendations to liberalize rice trade both internally and externally. Vietnam did in fact relax rice export quotas and internal restrictions on rice trade. Increased rice exports under these policy changes were estimated by Ryan to benefit Vietnam to the tune of about $60 million per year during 1996-2001. Taking a "conservative" view of IFPRI's causal role in the policy change – which attributed to IFPRI a speeding up of the policy change sufficient to credit IFPRI with the $45 million in gains realized during 1996-1997 – Ryan obtains a huge benefit/cost ratio of at least 45 since the policy research cost just under $1 million.

3.42 A quite different picture emerges from Ryan's (1999b) painstaking review of IFPRI's 10-year program of work in Malawi. Despite very substantial efforts in capacity building, with many tangible products in papers, training, and advice, the measurable impacts of the kind estimated for Vietnam were essentially nil in Malawi. Credit is given for engagement in the food policy process at the beginning of the 1990s, but IFPRI's presence was not apparent in more recent important policy debates. He cites "the adverse perception that IFPRI is an expensive organization, which is too close to donors, too Washington-centric, and too possessive of the data bases it generates" (p. 35). He concludes by quoting with approval an interviewee who says "it seems IFPRI may have concentrated too much on data collection and too little on building solid linkages with the policy environment in Malawi. Links with the donor community are not a substitute for this, as staff turnover and changing priorities are not conducive to sustainability" (p. 36).[17]

3.43 Assessment of IFPRI's support for capacity building in policy analysis and social science research to support such analysis is an area where disagreements among those consulted for this report are notable. Some give high marks to what IFPRI has done to improve the analytical capacity of government analysts and others involved in policy in Africa, especially in the period up to the early 1990s. Others are critical of what has been described as a Washington-centric or stop-and-go characteristic of IFPRI involvement in African policy issues and education. They point to a lack of good policy, movement towards good policy, or even of serious consideration of proposals for policy reform in much of sub-Saharan Africa as evidence of a regrettable lack of appropriate commitment by IFPRI, a "retreat to Washington." At the same time, IFPRI's proponents point to IFPRI's ongoing research on the economics of African agriculture, reasonable advice to and training of personnel from African governments, and note that no other outside government, international agency, or NGO can claim even that much influence or effect.

3.44 Similarly, the 2020 Vision Project's efforts in regions of Africa have received both praise as innovative initiatives, and blame as activities peripheral to and largely divorced from IFPRI's core research program "as an afterthought or to placate the critics amongst its donors," as one person put the matter.

3.45 In evaluating applied policy research one should not underestimate the problems caused by the fact that the analysis and recommendations about a policy issue will often

remain controversial.[18] Potential gains here arise from the contribution IFPRI's work makes to the quality of the debate – keeping it focused on facts and analysis, with well-defined objectives. On this score some of IFPRI's work, going back to the papers in Mellor and Ahmed (1988) and onward through the food subsidy research that Farrar covers, ranks quite well. In other areas, however, such as general equilibrium and trade modeling, some of the papers appear to have pursued methodological tangents at the cost of less focus on the policies themselves and estimation of their effects.

3.46 *1998 EPMR.* IFPRI underwent its third External Programme and Management Review in 1997-1998. The Review saw IFPRI's impact in terms of changes in policies, generated through "two types of output: directly through the policy relevant information it provides; and indirectly through capacity strengthening…"(CGIAR 1998b, p. 36). Given the long time lags and difficulties of attribution in policy change, the Review looks to "anecdotal evidence of impacts, or at least evidence that the results indicated by IFPRI's work are consistent with policy changes that actually have or are taking place" (p. 36); and for measurable "intermediate" indicators – usefulness and extent of written output and quality and relevance of work produced by IFPRI clients (this last an indicator of success in capacity strengthening activities). The Review Panel later noted that in getting written output to the point of usefulness "the outreach function becomes a critical one" (p. 37). So one wants to see the circulation (how many and to whom) of printed and electronic findings and documentation of presentations at policy forums of various kinds.

3.47 The Review noted attendance at several policy training events in Africa and surveys carried out by IFPRI or its readership, e.g., that of the 13 percent of 3356 readers of the "2020 vision" *News and Views* newsletter who responded, 74 percent called the 2020 vision documents useful or very useful (p. 40). Overall, the Review did not find a great deal in the way of quantitative intermediate indicators of impact. Pardey and Christian (2002) have since the review attempted to provide a more quantitative account of the amount of written output from IFPRI, and moreover to compare IFPRI output with that of other national and international institutions that focus on economic policy analysis. They document an impressive and increasing amount of printed output. The only cautionary note is that citation counts give little evidence of use of IFPRI products by developing country social scientists.

3.48 The programs of IFPRI's four research and one outreach division are reviewed separately, with generally positive assessments of the quality and quantity of work done, but noting unevenness of output among the divisions (without singling out any one specifically for criticism).

3.49 The EMPR made four sets of recommendations. One set is on impact assessment where continuation of efforts to nail down impacts is encouraged. Another recommends adjustment of the research agenda to re-emphasize water issues and take into account developing countries' interactions with the world economy. These are quite innocuous recommendations. More fundamental are the recommendations (i) on increasing relevance by taking developing country concerns more fully into account and by diversifying the staff to include more with policy as well as research experience, and (ii) improving outreach by better integration and mutual reinforcement of research and outreach activities. The TAC, in its commentary on the EMPR, elaborates implications for the research program, suggesting there should be more country-specific, focused research (as opposed to large-scale multi-

country efforts) and that IFPRI should become more actively involved in "emerging policy debates" (p. vi).

3.50 These concerns temper the reception of IFPRI's generally well regarded 2020 initiative. The EMPR reviews this effort separately, and while supporting various activities in that program, urges IFPRI to keep the 2020 activities in perspective within IFRPI's "main mission of carrying out high quality policy research and transferring its experience and expertise through capacity strengthening activities" (p. 31). This statement should be seen in the context of the EMPR's recommendations on relevance and outreach, reflecting the concerns expressed above that IFPRI is too Washington-centric and with too much emphasis on economic research leading to technical publications in professional journals.

3.51 The IFPRI response (IFPRI 1998) to the EMPR's recommendations in areas (i) and (ii) above was accepting on (ii) but less so on (i). IFPRI agreed with the need to become more proactive on outreach. But the response suggests a structural problem. It states: "While most of IFPRI's outreach activities have been and will continue to be undertaken within the research divisions as integral parts of research projects, we visualize that the Outreach Division will play the dual role of supporting such outreach activities while undertaking other outreach activities that are more appropriately done outside the research divisions, such as capacity strengthening and information dissemination that cut across divisions" (p. xiii). The statement presumes that outreach already is an integral part of research projects, suggesting no need for change. But comments we have received suggest a lack of sufficient integration of IFPRI's research with on-the-ground policy issues and capacity strengthening in developing countries, precisely when the research is directed at a particular issue, i.e., is division-specific. So changes in the Outreach Division will not serve to improve matters sufficiently.

3.52 IFPRI's response on area (i) says that IFPRI's priorities already are "heavily influenced by developing countries' needs," and that IFPRI already has senior researchers with significant policy experience. The response goes on to agree to take steps in these directions but evidently do not see a problem where the EMPR does.

3.53 The tenor of the preceding evaluations suggests that the most important issues concerning IFPRI are not the quality or quantity of its work, but rather its priorities in research topics and outreach programs. IFPRI has generated good research in many areas, notably policy analysis of market-distorting policies in developing countries, relationships among economic growth, environmental improvement, institutional design, and poverty reduction including issues in nutrition and health, and methods of impact assessment. But in other areas their priorities can be questioned. It is arguable that IFPRI devotes too high a priority to (i) world supply-demand projections and (ii) technical pursuits such as developments in CGE modeling and simulations, and not enough to (iii) applied policy analysis of developing countries, carried out in those countries with IFPRI guidance and (iv) analysis of the damage being done to developing countries by industrial countries' agricultural subsidy policies. Despite the popularity of work in area (i) and other 2020 activities with donors, and of work in area (ii) with the most highly qualified IFPRI professional staff, to serve its mission best some tempering of emphasis in these areas might be warranted.

3.54 With respect to both areas of emphasis and style of publication, the issue is not absolute quality but comparative advantage given the other institutions at which economic

research is carried out. Supply-demand projections are well handled in national government economic agencies and private firms. CGE modeling is well handled in a few universities (e.g., Purdue) and government agencies. On the other hand, there are huge gaps in the analysis of particular policies in developing countries, and the opportunity to make a significant dent in that analytical agenda in joint work with developing country economists in their own setting is one that should get absolutely top priority. Analysis of industrial country policy effects on developing countries is perhaps a less obvious priority for IFPRI, but the issue is sufficiently important and sufficiently ignored by other economic institutions with IFPRI's credibility, that the value-added of more work on this topic, especially with the new round of agricultural trade negotiations in the WTO, is likely to be high.

Capacity Building

3.55 The contribution of the CGIAR in training scientists in NARS, providing institutional infrastructure and organizational advice, and hopefully synergistic joint research and information dissemination activities with NARS and others in developing countries has been mentioned in the preceding discussion of IFPRI and at several earlier points with respect to other Centers. Practically every Center has counted many developing country scientists as having received valuable experience, knowledge, or materials as a result of training by or joint research with CGIAR scientists.

3.56 Attempts to place a value on CGIAR's capacity building activity (taken to encompass all the areas just outlined) are surprisingly scarce relative to the effort devoted to valuing crop varietal improvements. SPIA sees the need for substantial additional effort in this area, and is undertaking "an assessment of the CGIAR's impact on scientific capacity strengthening of NARS" (CGIAR 2000b, Section 4.7, p. 11). The evidence on scientists trained at, jointly working with, and publishing with IRRI, CIMMYT, and other crop research Centers is impressive, and the national recognition of the value of the CG contribution to this buildup of human capital is also heartening. Raab, Denning, and Cruz (1998), for example, provide telling data and anecdotal evidence for IRRI's influence in Southeast Asia. However, a recent more general review of CGIAR training activities found plenty of evidence that training occurred but no citable results of that training (Dixon 1999).

3.57 A reasonable place to look for evidence of impact in capacity building is the work of ISNAR, in view of the centrality of information transfer and synergy with NARS in its mandate. ISNAR (2001) reports results of some substantial impact assessment activity. Mackay and Debela (2001) summarize some main findings to date. They classify ISNAR output as products (notably publications) and services, such as training and advisory missions. They tally a total of 1,026 reports and publications in 1991-1996 (p. 13) and through interviews and questionnaires find convincing evidence that many in NARS and elsewhere in developing countries have found ISNAR activities valuable. They do not provide a sense however of how valuable ISNAR output is viewed as being as compared to other products or services that might be delivered through international assistance. An overall assessment was carried out in the recently completed 4th EPMR (CGIAR, 2002). That report concluded that "overall, the impact of ISNAR's work has been modest" (Summary, pages unnumbered), and offers options for the future, all of which involve a substantial phasing down of ISNAR's activities.

3.58 The value of capacity building looks particularly crucial in the case of sub-Saharan Africa, where productivity improvements have been less widespread and NARS are generally weak. Ryan's (1999b) review of IFPRI's activities in Malawi spells out a substantial history of attempts at capacity building that after ten years came essentially to nothing. Eicher (1999) provides a broader-based view of CGIAR shortcomings in African capacity building, and notes that the budgets available to the African CGIAR Centers are far below the substantial sums required to "combat human capital degradation in Africa" (p. 53). His discussion as well as Ryan's points to substantial economies of scale over the range of currently feasible commitments, in terms of funds and effort spent at a given point in time, and in sustained efforts over many years – a pessimistic possibility as it suggests the CGIAR may be in no position to create a sustainable size of research institutions in more than a few small countries. Thus it may be not only that returns to CGIAR capacity-building activities are not so high as for CGIAR research itself, but moreover the comparative advantage of CGIAR (as compared to institutions of higher education, international agencies, or NGOs) is just not there once we move away from training that emerges as a joint product of collaborative research between CGIAR and NARS scientists.

3.59 The capacity building situation fits with the view that the IARCs and NARS should in general be viewed as complements not substitutes. It is not just that the impact of CGIAR is not diminished when there are strong NARS; more crucially for the poorest countries, it is impossible for the IARCs to make up for the lack of effective NARS (or in the case of policy research, an effective national policy unit) in countries where national commitment of resources is too small.

3.60 One area where disagreements among those consulted for this report is notable is the assessment of capacity building in the area of policy analysis and social science research to support such analysis. This pertains primarily to IFPRI. Some give high marks to what IFPRI has done in improving the analytical capacity of government analysts and others involved in policy in Africa, especially in the period up to the early 1990s. Others are quite critical of what has been described as a Washington-centric or stop-and-go characteristic of IFPRI involvement in African policy issues and education. The 2020 Vision Project's efforts in regions of Africa have received both praise as innovative initiatives, and blame as activities peripheral to and largely divorced from IFPRI's core research program "as an afterthought or to placate the critics amongst its donors," as one person put the matter.

3.61 Several recent papers reporting perceptions from the viewpoint of developing country NARS are helpful in sorting out CGIAR impacts in recent experience. These NARS are called upon to document successes of agricultural research, and this means disentangling to the extent possible their own contributions from those of the IARCs. This might be thought to provide an incentive for NARS to downplay the importance of the CGIAR, but to the extent the NARS do in fact rely on inputs from and partnerships with the CGIAR, it is in the interest of NARS to acknowledge this and keep the inputs and partnerships going. So the apparent motives of evaluators are appropriately mixed - which might not make a difference in any case, but if interests ever matter they are most likely to matter where the hard data are scarcest.

3.62 The paper by Romano (2002) on the Colombia NARS in the CGIAR context introduces several substantive points and recommendations. Many IARCs have worked with Colombian institutions, with the longest and most fruitful relationship that between CIAT and the

Colombian Agricultural Institute (ICA). Romano cites nine studies estimating the usual high rates of return to crop varietal research on rice, barley, soybeans, wheat, potatoes, sorghum, and oil palm, with rates of return ranging from 12 percent (wheat) to 87 percent (rice).

3.63 The highest of these estimated rates of return, the 87 percent estimate for rice, is from the classic study of Scobie and Posada T. (1978). This is actually one of the lower of the range of estimates they provide, with different assumptions about supply and demand elasticities. The S&P study shows both the problems and ultimate impressiveness of rate-of-return studies in the NARS/CGIAR context. The problems go beyond those of benefit-cost analysis in that to estimate the internal rate of return (the interest rate that just equates the stream of discounted benefits and costs) one has to specify the whole time path of costs and returns, with appropriate discounting. Scobie and Posada T. make benefit and cost estimates for 1957-1974, and just assume that the 1974 costs and benefits continue unchanged until 1986, when both stop. This of course is an arbitrary procedure, as is the use of a 10 percent opportunity cost of capital for discounting. Also, S&P count only Colombian NARS costs, omitting CGIAR costs even though rice developed from IRRI varieties made up an estimated 60 percent of Colombia's rice area sown.

3.64 Thus, one could shoot many analytical holes in the calculations. Nonetheless, a close look at the underlying figures dispels doubts quite effectively. The estimated gains to Colombian producers and consumers together is $6.1 billion Colombian dollars in 1957-1974. Varying the assumed supply and demand elasticities changes the distribution between producers and consumers tremendously (indeed the producers as a whole are made worse off by the new varieties if demand is inelastic); yet the sum of gains to producers and consumers is much less sensitive, although under the most extreme assumptions considered it is reduced by half. On the cost side, the Colombian NARS during the period covered spent $ (Colombian) 63 million. The IARC costs left out are an estimated $C 19 million in CIAT. S&P do not provide comparable IRRI costs but if they are as large (unlikely, and certainly not true for IRRI's marginal costs since IRRI just provided germplasm of varieties they had already developed), then the omitted CGIAR costs add up to $C 38 million. So, if we take the worst case the $6.1 billion benefits would be reduced to $3 billion, and the costs would be increased to $101 million. We still get $30 of benefits for each $1 of costs (and the costs include costs of capital valued at 10 percent per annum). In short, we have a repeat of the story from Griliches onward – no matter how you slice it the rate of return is enormous.

3.65 Why is it so? Essentially it is a matter of leverage: activity in a small market (as measured by costs) for research generating significant effects in a much bigger commodity market. If an innovation costing $X reduces costs of production of a commodity worth $100X by 10 percent or more (and to get adopted it will probably have to reach this threshold), then a fairly robust estimate of the overall GDP gains is $10X. The ease of passing this test is what ought to give one pause about making much of possible declines in the productivity of research in recent years. And, when we read of productivity effects such as Romano reports on p. 17 for tomatoes, plantains, cassava, milk, and pigs, even when we don't have the full benefit-cost study we can be fairly confident that the rates of return are or will end up being high (assuming only that the productivity effects are roughly correct).[19]

3.66 The Brazil paper (Macedo, Porto, Contini, and Avila, 2002) tells a similar story to that of Colombia, but with a more definite trend in the evolution of CGIAR/Embrapa cooperation.

Again we have extensive documentation of fruitful use of CGIAR findings and resources from several IARCs. most notably CIMMYT, CIAT, ICRISAT, and CIP. Again we have many detailed studies of particular crops and aggregate Embrapa programs that find phenomenally high rates of return. It is also well documented that a large amount of the genetics in some key new varieties (rice, wheat, maize, and beans) is derived from CGIAR germplasm. But there is a clear trend toward the percentage of CGIAR genetic material declining during the 1990s (Macedo, Porto, Contini, and Avila, 2002, Figure 2, p. 19). The Brazil paper also emphasizes the role of capacity building and institutional strengthening due to CGIAR activity, with 875 Brazilians receiving training at CGIAR Centers in 1971-2000. Here too the activity is declining in the 1990s, with Brazilian trainees falling from a peak of 223 in 1986-90 to 99 in 1996-2000 (Ibid., Table 1, p. 14). The Embrapa scientists are now much more highly qualified on average than two decades ago, and arguably are on a par with CGIAR scientists.

3.67 In short, for the same reasons as discussed above with respect to Colombia, the historical net benefits of the CGIAR in the NARS context are enormous in Brazil and amply reward the donors' investments. In Brazil, however, the case for continued high returns is more attenuated. The CGIAR contribution now is seen more in collaboration with Embrapa in joint work in other countries, most notably the Lusophone (Portuguese speaking) countries of Africa. More generally, future cooperation between the CGIAR and Brazilian NARS is seen as the use of CGIAR resources (to which Brazil now contributes) in leveraging NARS activities in both research and scientific training. The returns from these agricultural R&D activities in Brazil are, even if not as high as formerly, still likely to be quite substantial and repaying donors' investments handsomely. However, the CGIAR does not appear to be as crucial as formerly in achieving those returns.

3.68 The paper on the Indian perspective on CGIAR effectiveness (Katyal and Mruthyanjaya, 2002) does not discuss rate-of-return estimates but one nonetheless gets a good sense that the impacts discussed are on the order of magnitude of IRRI and CIMMYT impacts that generated very high estimated rates. The paper later (circa p. 26) also gives testimony to a lack of yield and TFP increases in the 1990s which suggests recently declining rates of return to germplasm innovations. Also the indication of NRM returns of 10-15 percent to the farmer (p. 43), which may imply that net returns could be quite low (because as I read the presentation, the estimate does not subtract the costs of the research). The paper provides concreteness on some cropping practice innovations and gives reasons to expect significant rates of return, even if not quantified, for crops such as chickpeas where fully quantified evidence is scarce.

3.69 On the key issue of CG/NARS interaction, Katyal and Mruthyanjaya do not attempt to share out credit for successes in India (which is probably impossible in any case), but it is clear the Indian opinion is that the CG has had a crucial contribution even after the initial "green revolution" work, and continuing today. The key contributions are germplasm development and stocks, and scientific expertise in the CG that the Indian NARS draws on. But it is also clear that the NARS contribution is essential, and more so now than initially.

3.70 On the broader research agenda, Katyal and Mruthyanjaya recognize significant contributions of IFPRI and ISNAR, but appear guarded in their view of ISNAR, saying that it has contributed little of substance and with an objectionable "directive style" (p. 25). Even

with IFPRI the assistance to India seems to be more in the line of what could be done rather than what has been done.

3.71 With respect to the comparative advantages of the CG vis-à-vis Indian NARS, according to Katyal and Mruthyanjaya the IARCs are looked to for biotechnology, near-frontier science and scientists, expertise on information management, participatory methodology (seen as quite important), and "new agenda" expertise, e.g. some social science, policy analysis, and GIS; and the CGIAR has advantages of less "bureaucratic rules" (p. 16) and above all a way to supercede the budget crunch that much hinders the Indian NARS.

3.72 More unexpected, perhaps, are the areas where the paper sees disadvantages of the CGIAR as compared to NARS. Tying the story to global public goods, Katyal and Mruthyanjaya say that NRM "should be the activity of NARS, not IARCs" (p. 46) and elaborates that "IARCs should give more and explicit attention to improving productivity ...than issues like environment and equity which involve local sensitivities." This can be interpreted as meaning environment and equity are not GPGs in the relevant (to India) sense. This view should be taken seriously. Is an activity a GPG just because the donors think so? K & M also advise less emphasis on the eco-regional approach in CGIAR (p. 47). With respect to scientific personnel, the NARS should provide supporting scientists beyond a few top international experts (much cheaper), while on the other hand the training and human capital development aspects of the CGIAR are clearly wanted. This is where not only genetic scientists but also IFPRI's experts are given a high status as a source of CGIAR comparative advantage.

3.73 A paper on CGIAR/NARS partnership in Kenya (Ndiritu, 2002) reinforces the points that the CGIAR's substantive contribution to gains in agricultural research in Kenya have been substantial, but that the relationship has evolved in ways that raise questions and that the future relationships between IARCs and NARS should evolve into somewhat different partnerships, more nearly partnerships of equals. Ndiritu notes promising beginnings when Africa-based IARCs, and notably ILRI, played a key role in coordinating a series of meetings where "The idea was to craft an agenda that would close the felt gap between the NARS and the CGIAR Centers" (pages unnumbered). Numerous training and collaborative research efforts have been undertaken. Kenya is particularly well placed for this with the presence of two IARCs within its borders, and so may not be typical for Africa.

3.74 The African NARS hoped not only for a sharing of the limelight and of responsibilities but also of resources (especially when African states became paying members of the CGIAR), and this effort proved disappointing. Kenya went after international donors directly, and found ISNAR "a great partner" in formulating this approach.[20] Not only is a lack of CGIAR resources felt but in addition the IARCs "often employ and take away the qualified personnel from NARS." Ndiritu also states that Kenya, as other NARS, "questioned what appeared like a policy shift for ISNAR, wishing to align itself more on research rather than services with research playing a supporting role. The mood then (mid-1999s) was ISNAR was not listening to the NARS enough as its partners, choosing to pay attention to the TAC." Although IFPRI is praised for a desire to assist with policy analysis, "IFPRI needs to review its capacity building and partnership very urgently if it is to help the African/Kenyan NARS to negotiate for themselves in the policy platform, which now includes such diverse themes as globalization, world trade, biotechnology…"

3.75 Overall, the discussion of NARS/CGIAR interaction in the Colombia, Brazil, India, and Kenya papers brings out the range, depth, and limitations of CGIAR impact in ways that are not captured in rate-of-return studies. However, as some of the discussion indicates, the successes and failures of capacity strengthening and training do not add up to any kind of case for re-evaluating the estimates of CGIAR success that rate-of-return studies have provided. It is clear however that considering the CGIAR/NARS Centers as a whole could lead to more effective use of agricultural research funds. And, even if these matters are unlikely to make a big difference in estimated benefits relative to costs, they are important in deciding the allocation of donors funds among IARCS and between the CGIAR and other research providers, and in determining which activities are most productively undertaken primarily by NARS and which by IARCs. The NARS' increased emphasis on the IARCs as a source of cutting-edge science also raises an issue beyond the comparative advantages of NARS and IARCs, namely the relative capabilities of IARCs and alternative sources of cutting-edge science such as universities or private-sector firms.

4. Evaluation of Impact Studies

4.1 The preceding discussion has not been free of evaluation, but in this section the credibility and implications of impact studies are considered more systematically. The CGIAR itself has carried out or sponsored, currently through its Standing Panel on Impact Assessment (SPIA) some of the most rigorous questioning of impact estimation methods and evidence.[21] A review of the issues and approach of SPIA, impressive in both breadth and depth is contained in the Proceedings of SPIA's May 2000 Workshop (CGIAR 2001). The following is largely a broad review of SPIA's work and that of independent scholars.

4.2 The question of how much confidence may be placed in estimates of returns to agricultural research has been examined ever since the first estimates were made. Griliches (1979) laid out the key technical problems. Fox (1985) put forth several reasons why the existing literature may overstate the gains, but after considering the corrections that would result if his concerns were met he concluded that the social returns to research in agriculture would likely remain above private market rates of return. Pasour and Johnson (1982) raised questions about the political forces behind agricultural research, and the consequent likelihood of inefficiency and boondoggles. But they did not present quantitative evidence that returns to research are actually lower than commonly estimated.

4.3 Kealey (1996) published a wide-ranging assessment of the economics of research in a general historical context in which he emphasized the role of private-sector contributions to new knowledge and technology. With respect to agricultural research Kealey reviewed a long history of private-sector innovations in Europe, and based on the work of Pray et al cited earlier, ventured the opinion that publicly funded agricultural research in India (presumably covering both NARS and CG Centers) "was not only less good than that of the private sector, it also inhibited that of the private sector by artificially reducing the private sector's market" (p. 56). However, this appears to be a somewhat tendentious reading of the findings of Pray et al., as they note the source of much of the private-sector (seed company) germplasm originated with CG or national institutions.[22] Although Kealey makes a compelling case that private sector entities in agriculture and industry have generated a large fraction of the

technological innovations that have occurred, notwithstanding the public-good character of the knowledge embodied in those innovations, he does not put a dent in estimates of high rates of return to public-sector agricultural research.

4.4 Maredia, Byerlee, and Anderson (2000) focus specifically on the methods used in estimates of returns to public-sector agricultural research and find that "simplistic assumptions about lags, costs, and supply shifts, together with failure to account for spill ins [effects of research carried out at one institution upon output credited to a different institution whose research impacts are being assessed], have biased estimated rates of return, usually upward," and that focusing the estimation of returns on "winners" has furthered this bias (p. 26).

4.5 Alston (2002) helpfully reviews a large body of literature on spillovers. He cites a number of studies in which accounting for spillovers reduces the estimated rate of return to research – most shockingly Schimmelpfennig and Thirtle's (1999) estimate that the rate of return to public agricultural R&D in the US and EU falls from 60 percent when spillovers are neglected to 10 percent when they are taken into account. However, when it comes to CGIAR research, the literature Alston reviews finds the predominant spillovers yielding benefits to developed countries from CGIAR crop varieties, e.g., Brennan (1989). These studies estimate that in the cases of Australia and the United States, their national gains from spill ins more than compensate those countries for all their contributions to the CGIAR. Since gains in developed countries are omitted from the CIMMYT and other studies cited earlier, including spillovers would have increased the (global) rates of return to CGIAR research that were quoted earlier.

4.6 Alston and Pardey (2001) make similar criticisms, noting that some biases lead to overstating returns and other biases to understating returns but that on balance "we expect that the tendency to overestimate has predominated" (p. 143). Yet the 1,772 estimates they review found a median rate of return to agricultural research and development expenditures of 44 percent, with a median rate of return of 40 percent in 62 estimates of returns to research at IARCs (p. 143). They provide no evidence, and do not argue, that any plausible correction of the estimates would result in rates of return that would make agricultural research a worse investment than competing uses of public funds.

4.7 Evenson (2001) reviews more than 100 studies estimating rates of return to research, almost entirely focusing on yield increases, paying attention to spillover and selection-of-winners bias. The studies generally estimated high rates of return – 82 percent of the studies found a rate of return to agricultural research of over 20 percent, which would argue for a considerable expansion of research effort. The studies show no evidence of a decline in the rate of return over time, indicating that recent research has been just as effective as research done in earlier decades. He also finds public and private research to be equally productive, and that more basic ("pre-invention") research on average generates greater returns than more applied research such as developing new crop varieties. He finds the very highest estimates of rates of return incredible, but finds the average estimates believable. Since these average rates of return are still quite high, in the neighborhood of 40 percent annually, Evenson's review has to be considered a strong endorsement of a high social return to agricultural research in the CGIAR, the NARS, and in the private sector – but he does not separate out estimates of CGIAR returns from those of NARS. He also, in this study, does not address the value-added issue of the extent to which a reduction in research spending by the CGIAR might trigger increases in spending by other agricultural research institutions.

4.8 Alston, et al. (2000), made a Herculean effort to assess the literature on rates of return to investment in agricultural research and development, and attempt to get at the questions of how returns to research in international Centers compares to that of NARS, and whether the rate of return to research has declined over time. They considered 292 studies containing 1,886 estimated rates of return. While the median annual rate-of-return estimate falls within the 40-60 percent range that they refer to as "conventional wisdom" about these returns, it is striking that only 21 percent of the estimates they reviewed fall within this range. This suggests either huge variation in success of research efforts or else huge errors of estimation, and the fact that so many of the studies work with similar aggregated data make it uncomfortably likely that huge errors in the estimates are a significant part of the picture. The approach Alston et al. take to interpret this mass of estimates is "meta-analysis," defined as "a statistical review of research studies in a particular area of scientific inquiry" (p. 3).

4.9 Although the range of estimated rates of return is large, so that precision is unlikely, it is nonetheless notable that less than 15 percent of the observations give an estimated annual rate of return of 20 percent or less. This suggests an overwhelming likelihood that the true average rate of return (what could be expected from a randomly chosen investment in agricultural research) is in excess of rates of return expected from private profit-seeking investments (opportunity rates), unless there is a quite strong systematic upward bias common to all or most of the estimates. Possibilities of such bias have been mentioned earlier, but authors who have attempted to quantify such bias have not found any that are plausibly sufficient to knock the average estimated rates of return below, or indeed anywhere near, the relevant opportunity rates.

4.10 In their econometric analysis of the estimated rates of return, Alston et al. looked for effects of characteristics of the researcher, the research evaluated, and the method of estimation used. With respect to possible bias associated with the researcher, they find that analysts affiliated with international research Centers or funding bodies estimated rates of return about 20 percentage points higher, other things equal, than analysts affiliated with universities. But another statistically significant effect is that self-evaluations come up with estimated rates of return that average about 20 percentage points lower than independent assessments, other things equal.[23] With respect to the method used, estimated rates of return are significantly lower for certain specifications of lag length, supply shift, and benefit calculation; but it is unclear which are more appropriate methods, the ones giving the higher or lower returns. It is notable though that estimates published in refereed journals give significantly lower rates of return (point estimate of 16 percentage points lower) than estimates in nonjournal publications.

4.11 With respect to type of research evaluated, estimated returns are significantly lower for investments in extension, and in fisheries and forestry, as compared to crops or livestock. More pertinent to bias is that estimated returns are lower when a whole research program or institution is evaluated as opposed to a single research project – suggesting a tendency to select winners in the single-project evaluations. This effect is quantitatively quite large, with an average rate of return over 40 percentage points lower for program-level assessments. It is also notable, however, that some factors thought likely to cause an upward bias in estimated rates of return had no significant effect in the Alston et al. analysis. Whether an analysis considered spill ins (effects of research done elsewhere) or spill outs (effects of research assessed on outcomes elsewhere), in neither case had a significant effect on the estimated rate of return. Likewise, some studies attempted to adjust for effects of distorting commodity subsidy

programs, taxation, exchange-rate distortions, or environmental impacts; but Alston et al find no significant effects of attention to these matters on estimated rates of return to research.

4.12 On an issue that is not pertinent to possible flaws or biases in estimating methods, but which is substantively important, Alston et al find no significant effect of earlier or later dates at which research outcomes are measured. Thus there is no support in the body of studies they review for the idea that the returns to agricultural research are lower in recent years than they were in the glory days of the green revolution. However, even the latest-dated outcomes reflect the product of research that occurred years earlier, and we really have no evidence from these studies on returns to research conducted after 1990.

4.13 Because the outcomes of the CGIAR are not pure public goods, the issue arises as to whether all the returns (including those captured by private suppliers of inputs embodying new technology like seed companies, and those captured within the country in which a new variety is primarily used) should be counted, or alternatively only the public-good component of the returns (nonexcludable, nonrivalrous spill outs realized in a country other than the one where the research is carried out)? It might seem obvious that the correct answer is the former. Suppose investment of $10 million in program A generates $50 million in private, appropriable benefits to the home country plus $5 billion in nonexcludable spill outs (as genetic information spreads, say), while investment of $10 million in project B generates no private, appropriable benefits in the home country but $20 billion in nonexcludable spill outs (as published knowledge about a method of genetic manipulation, say). Isn't it obvious that project A is a preferred investment in the World Bank's portfolio? The argument for the answer being No is that we can confidentially expect the private sector to undertake program A, but not B; so the Bank's funds are more effectively used for it GPP purposes in project B.

4.14 I nonetheless believe that the first, "obvious" answer is correct in the case of investment in the CGIAR. Practically speaking, we have good evidence of past high returns, and plausible reasons for expecting future high returns, from agricultural research of the kinds that the CGIAR has carried out. We have no good reason to believe that, absent the CGIAR, this research would continue, much less grow in volume and scope as the high rates of return suggest it should. Our ability in practice to sort out projects of type A above from type B is so limited that the distinction cannot be applied anyway; better to let a preference for type B be known, but let the scientists follow their own sense of what agricultural research is most promising. A final and most important, practical point that has emerged in the Team's interviews is that while national public goods may reasonably be expected to be captured by the largest and most solvent developing countries, this is far from the case in smaller, poorer countries. The rationale for Bank investment here departs from the pure public good criterion; it is rather (a) the existence of economies of scale in national public good provision through research; and (b) capital lending markets constrained not to provide loans for poorer countries whose credit worthiness is insufficient to finance NARS whose expected returns would nonetheless more than justify the cost of capital.

4.15 Dalrymple (2001) has pushed further in attempting to assess CGIAR contributions to global public goods in areas other than agricultural productivity, notably environmental improvement and poverty alleviation. Of course, productivity increases generate either improved farm incomes or lower food prices, or both, which in and of themselves can be expected to reduce poverty. And yield increases, by requiring less cropped area to meet

world food demands, make a real contribution to environmental goals. To go beyond these results the CGIAR institutions can search for new varieties or production technologies particularly well suited to small-scale as opposed to large scale production, or to low as opposed to high use of purchased inputs such as pesticides and fertilizers in the production process. Dalrymple (pp. 13-15) cites several assessments that are more pessimistic about the returns to investment in these areas as compared to the benefits reaped from the longer-standing style of research focused first and foremost on yield increases. None of this however has been quantified even crudely.

4.16 Dalrymple also discusses the issue, almost completely neglected in impact assessments until recently, of the extent to which privately funded research, through seed companies or other agribusiness enterprises, could today profitably take over the research functions that CGIAR (and NARS) institutions formerly served so well. Enhanced national and international protection of property rights in innovation has made this takeover increasingly feasible.

4.17 In 1996-1997, the CGIAR's Impact Assessment and Evaluation Group (IAEG) carried out a review and synthesis of *ex post* assessments of effects of individual international agricultural research Centers completed during 1980-1996 (CGIAR, 1997). They looked at 265 documents submitted by Centers and funders of Centers, of which 87 were found to contain sufficient evidence of effects of Center activities to warrant review. Of the 112 activities reviewed, 60 involved germplasm development and 25 crop management technologies; only a few covered less applied activities (13 in institution building, 4 in germplasm banking, and 3 in "basic research"). The review was quite rigorous in the sense of asking for high standards of evidence for claims of Center impacts. The quality of the assessments was generally found wanting, so much so that the synthesis of findings contemplated in the IAEG's project was not carried out. The report concludes that "the credibility of the linkages between IARC activities and the outcomes reported in the documents is often difficult to assess because of insufficient information on methodology and alternative explanations for observed effects. For this reason, a synthesis of evidence on effects across the documents is not defensible" (CGIAR, 1997a, p. 4). Indeed, the report did not attempt to synthesize findings, presumably because there was an insufficient mass of such assessments.

4.18 A subsequent report reviews a subset of 11 best-practice Center impact assessments with a view to "compile those claims that met a high standard of plausibility to learn what could be said with confidence about Center effects in general" (CGIAR, 1997b, p. ES-1). The conclusions are chastening. The assessments convincingly demonstrate that "the products of the IARCs are used by scientists and institutions engaged in missions similar to those of the IARC" (for, example, ILRI reports that a publication on the discovery of mini chromosomes in trypanosomes was cited in 124 papers and influenced the work of several laboratories); and they present good evidence that IARCs "produce products that are used by agriculturalists in developing countries"; but there is much less on "the short- and long-term effects of the use of the products on the beneficiaries. In other words, after careful review of the 11 documents, we still know very little about the degree to which the CGIAR is achieving its mission of food security and sustainable agriculture in developing countries" (p. 4-6).

4.19 The IAEG also commissioned the Evenson (2000) study discussed above, which ended up with more confident conclusions of high rates of return to CGIAR research. Applying the rigorous standards of evidence just outlined against that study raises the

question whether its findings of very high returns to CGIAR research might collapse like a house of cards if sufficiently probed. There are essentially two kinds of studies: first, those that estimate acreage planted to an improved variety (or other innovation), estimate the yield gain attributable to the improved variety, and value the aggregate implied output increase at market prices to estimate the economic returns to the research that created the new variety; and second, the much smaller set of studies (though relatively plentiful for developed countries) that econometrically relate productivity growth to lagged research expenditures.

4.20 The first (yield-gain) studies require a lot of careful, detailed empirical work to estimate acreage planted to new varieties, the CGIAR contribution to those varieties, the yield gain actually achieved as compared to what would have been achieved with former varieties, and the extent to which the new variety's yield gain was due to increased fertilizer or other input use rather than improved genetics. The IITA studies cited earlier, to use the most egregious examples, inspire no confidence that accurate estimates of these effects have been obtained. Others are more careful or at least better documented, notably the early CIMMYT studies, one still can easily see how big errors could be made.

4.21 In this context it is troubling when a paper like Chavas (2001) comes along. He finds, using nonparametric representation of aggregate production technology for 12 countries (including Brazil, Burkina Faso, China, India, Madagascar, Mexico, Peru, Poland, Thailand, and Tunisia, as well as two developed countries, France and the US) output increases between 1960 and 1994 can in almost every case be explained by fertilizer and other input increases, and that the data are largely consistent with no technological progress at all in agriculture. This conclusion is dubious, and Chavas himself doubts the finding. Parametric estimation of aggregate production functions or total factor productivity growth in a country's agriculture, the second category of studies mentioned above, generally find high rates of return just as the micro-oriented first category of studies do. But these studies too can be questioned along lines going back to the critique of Griliches (1979). The essence of the critique is that we are dealing with highly correlated and trending time series, a situation in which spurious causality is a constant threat because the analyst simply doesn't have the data to separately identify the effects of research and the effects of the other simultaneously trending variables. And the analyst cannot resort to the helpful expedient of first-difference or more sophisticated time series models with confidence, because the lags are too long and the data series too short.

4.22 Notwithstanding these serious problems of estimation, basic on-the-ground observations are convincing that new varieties really do yield better with the same inputs, and the issue is really, how big is the effect. The extraordinarily high estimates of rates of returns just go to show that even quite small productivity gains on millions of hectares are sufficient to compensate for the really quite small investments in R&D that have been made. The evidence of substantial productivity gains is too widespread to deny, and the continuing declines in real commodity prices to all-time historical lows is corroborating market evidence. Even after adjusting for spillovers, biases, world price declines due to developed-country productivity growth and commodity subsidies, it seems clear that technological progress associated with improved varieties in the developing world is real and substantial, and even after considering alternative ways in which those varieties might have been generated in the absence of the CGIAR institutions, one cannot plausibly deny the CGIAR substantial credit for the productivity gains.

4.23 Impact assessments of CGIAR activities beyond those leading to increased agricultural output have faced tougher problems from the beginning. MacKay and Horton (2002) and Horton and Alexaki (2002) review achievements and prospects in the area of capacity building, or capacity development to use their preferred term. These ideas are thoughtful but address impact in the sense of the existence and extent of results only – no prospect of valuing the results or estimating the values relative to costs. This literature exemplifies an important distinction between impact assessment and benefit/cost analysis. Impact analysis can be well done and demonstrate significant results, but still fall short of providing sufficient information for the World Bank or other donors to use in allocating their global public good budgets.

5. Economic and Social Benefits and Costs

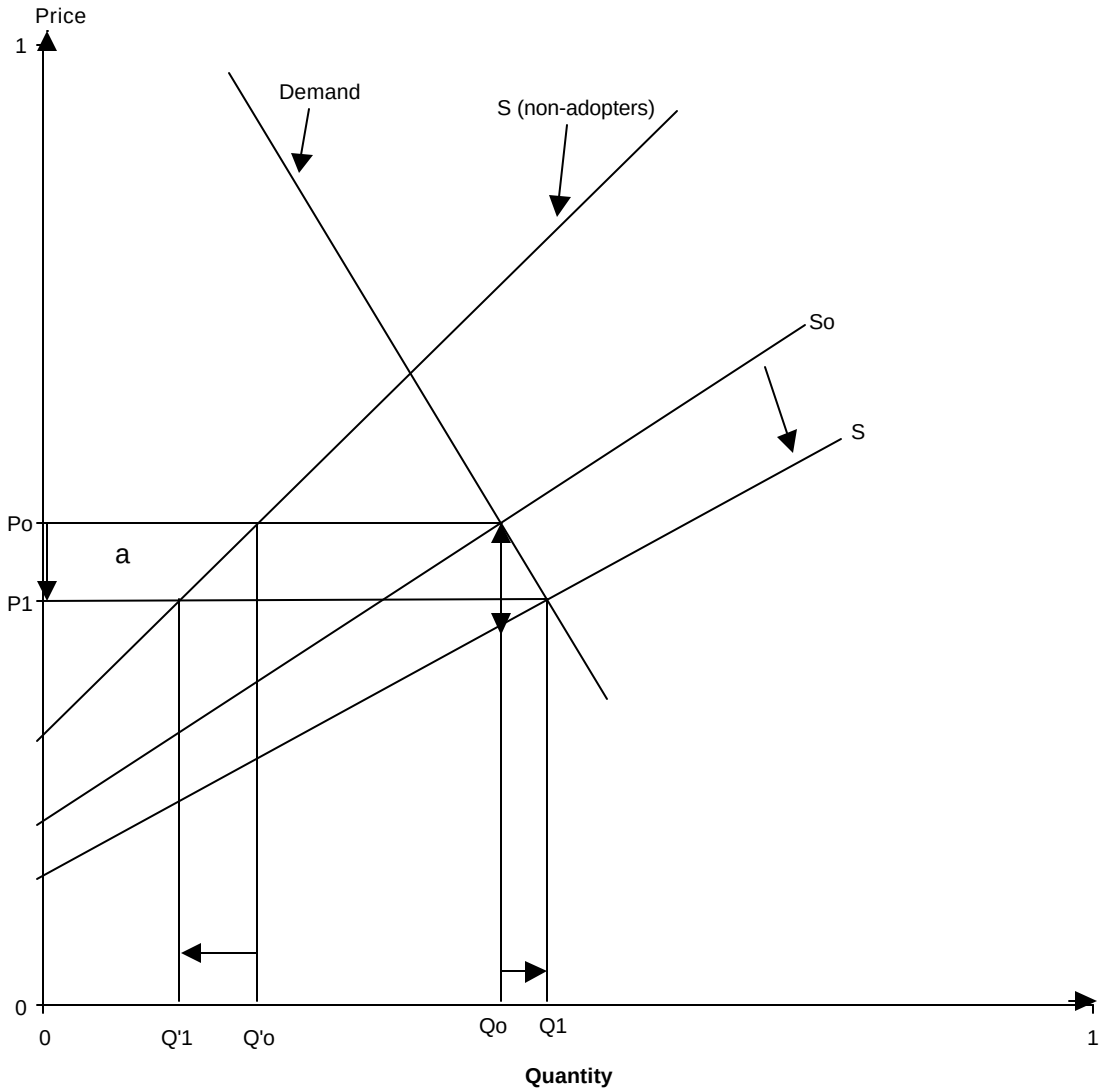
5.1 Productivity gains and reduced commodity prices are not the end of the story. In the discussion above, the typical procedure used to reach economic conclusions followed several steps: (1) assume percentage total factor productivity gains translate to the same percentage reduction in average cost of production, and (2) take the reduction in average cost multiplied by the quantity of output as the economic gain to society. A defect in assumption (1) is that while an improved crop variety may require 10 percent less of the aggregate of all inputs, it may change the mix of inputs used, increasing the demand for some (irrigation equipment, say), and if these inputs are inelastic in supply their prices may rise significantly. Such input price rises would reduce the cost savings generated by the innovation and the economic gains available to farmers and consumers.[24] The main problems with approach (2) are choosing the appropriate quantity by which to multiply the cost reduction, and the question of how market prices of commodities change when the average cost of production changes. Attempts to become more sophisticated in dealing with these problems in turn generate problems of their own, and the result is a large literature of analytical problems and proposed solutions.

5.2 Figure 1 illustrates the main issues, and provides a sense of likely errors introduced by simplifying assumptions used to obtain quantitative economic impacts. An improved crop variety, when adopted, shifts the supply function from S_0 to S_1, reducing costs by the distance of the double-headed arrow at pre-innovation market quantity Q_0. The approach to measuring the returns to the innovation taken in the earlier calculations was to multiply that distance (the length of the double-headed arrow) times the quantity

5.3 produced using the improved variety ($Q_1 – Q'_1$). This calculation could just as well have used the pre-innovation quantities ($Q_0 – Q'_0$) – the former gives an overstatement and the latter an understatement of the true gains. Also, the calculation assumes the vertical distance between the supply curves is constant, i.e., that the innovation generates a parallel shift in the supply curve.

5.4 Given the situation shown in Figure 1, a better approximation of the value of the innovation would be the area between S0 and S1, to the right of Q'_1 and to the left of the demand curve. This better approximation takes into account the elasticity of demand and the fact that the cost reduction differs at different output levels. However, given our knowledge of supply and demand elasticities, and of departures from a constant shift in supply, the error introduced in the earlier calculations are unlikely to introduce significant bias.

Figure 1. Economic Effects of Productivity Increase



5.5 More important than the size of the overall gain in debate about the impacts of new technology are issues concerning the distribution of gains. As drawn in Figure 1, the product price falls from P_0 to P_1, a decrease almost as large as the cost reduction. So buyers of the commodity reap most of the gains. Producers who do not adopt the new technology see no cost reduction, but they also receive the lower price P_1; so they clearly lose (in Figure 1 they lose the producer surplus labeled *a*). Even the producers who adopt the improved variety may lose, depending on the nature of the supply shift and the elasticity of demand. However, most analyses conclude that producers most likely gain from cost-reducing technical change for the main agricultural commodities – and they will never lose if demand is elastic enough. For more detailed discussion of this and related issues see Alston and Pardey (1996, Ch. 5).

5.6 Analysis based on commodity supply and demand functions has the shortcoming that it does not separate out the gains and losses of different contributors to the production process, i.e., owners of the factors of production. Farm income is the returns to the factors of production that farmers own, principally their labor, land, and capital assets, and whether

technical change increases their income depends on how technical change affects those returns. Technological change may be biased in such a way as to reduce labor requirements (e.g., mechanization), land requirements (e.g., improved irrigation methods), or the use of purchased inputs (e.g., seeds engineered to be pest resistant). It is possible that the set of technological changes that occur during a given time period may be factor-neutral in the sense of reducing the need for all inputs proportionally. One reason why there is no sure forecast of whether farmers will gain or lose is that we cannot be sure whether any factor bias in technical change will increase or decrease the use of farm-owned inputs.

5.7 A second reason for uncertainty is that even if technical change is factor neutral we cannot be sure what the effects on factor returns will be. Factor neutrality results in the demand for both farm-owned and purchased inputs moving in the same direction, but we don't know if that direction is up or down. The key variable in determining that direction is the elasticity of demand for farm products. If an improved crop variety involves a 10 percent decline in all input requirements and hence a 10 percent decline in the cost of a producing the crop, and that induces a less than 10 percent decline in demand for the product (inelastic demand), then there will be a net reduction in the demand for inputs and aggregate farm income will decline. But if product demand is elastic, then more of both farm-owned and purchased inputs will be used, tending to increase their returns, and farm income will rise. This last scenario is most likely when farm products are either exported or imported relatively freely.

Income Distribution and Poverty

5.8 Poverty reduction has gained increased prominence as an explicit target of global public goods. Several ideas about the linkages between agricultural productivity growth and poverty reduction are controversial. More fundamental is the linkage between overall economic growth in a country and poverty in that country; in that context the issue about agricultural productivity growth is how it contributes to economic growth generally as well as specifically to incomes of the rural poor. Timmer (2001, p. 19) puts it bluntly: "the only long-run hope of the poor is to live in a growing economy." But as he also says, economic growth is not always sufficient to reduce poverty significantly.

5.9 Agricultural productivity growth as well as appropriate policies and investments in human capital can easily be seen as valuable in the growth process, but the connection to poverty is not fully straightforward. It may seem paradoxical that, to the extent that the poverty of greatest concern is urban poverty, the evidence and analysis points to a substantial contribution to poverty reduction through CGIAR research through improved food security and reduced food costs. To the extent that the poverty targeted is that of rural residents, the picture is less clear. CGIAR documents tend to fudge the issue by pointing out, correctly, that the rural poor are food consumers too, and in this respect they gain just as the urban poor do. Nonetheless, the rural poor who generate the means to obtain what nonfood consumption they have through agricultural products they produce are vulnerable to be made worse off, on net, when commodity prices fall. And even if poor farmers gain as a group, the subset among them who do not adopt improved varieties will see there incomes reduced.

5.10 It can be argued that these are the farmers who, with economic growth, are precisely the ones who should move into nonagricultural employment, and in so doing their interests as consumers in reduced food costs will come to outweigh their losses as relatively inefficient

producers. But this view may also be viewed as too harsh, and certainly one has to take into account the adjustment costs and the pain of losses that forces exiting farmers to adjust, especially in economies with weak or inaccessible nonagricultural economies. The review of literature by Hazell and Haddad (2001a, p. 9) lists the following ways that research that leads to improved technologies can benefit the poor in developing countries:

> (1) increased consumption of own-farm produced commodities
> (2) greater agricultural employment opportunities for landless workers
> (3) opportunities to migrate to other agricultural regions
> (4) nonfarm economic growth stimulated by agricultural growth
> (5) lower prices facing the poor who buy food products
> (6) improved access to high-nutrient crops
> (7) empowerment of the poor through capacity for collective action and reducing vulnerability to economic shocks via asset accumulation

5.11 The first and fifth of these are the most straightforward economic consequences of productivity growth, and the main documentable source of gains that can be with high confidence linked to improved crop varieties and other outcomes of agricultural research. The second and third sources of benefit depend on technical progress increasing rather than decreasing the demand for labor in agriculture. This is likely to occur in many developing country contexts, especially when internationally traded commodities are involved, as discussed earlier. But the data-based evidence for this is scant as compared to the wealth of evidence on yield improvement and declining food prices.

5.12 The fourth item on the Hazzell-Haddad list addresses a source of economic gains from agriculture research that has not been seriously dealt with in recent literature. Convincing evidence would involve documentation of agricultural sector growth as an engine of overall economic growth. There are hopes for this outcome in Brazil and elsewhere in Latin America, with tropical exportable crops in parts of Africa, and perhaps in Central Asia. Hazzell and Haddad cite studies of India that show substantial multiplier effects of farm income increases as farm households buy more nonagricultural goods (e.g., Hazzell and Haggblade, 1991) and that agriculture contributed to industrial growth in some areas (Rangarajan, 1982). But given the inescapable observations about new varieties and farming practices, and associated yield increases, the evidence on these productivity increases as a source of overall economic growth is notably thin, and surely is insufficient to build a case for agricultural research on this ground over and above the direct agricultural effects.

5.13 The sixth item is quite likely to be important, and perhaps increasingly important in the future as innovations such as "golden rice," based on increased capabilities to target specific genetic improvements to meet specific nutritional problems (see Graham and Welch 1996, or Bouis, Graham, and Welch 2000). But the evidence on actual nutritional improvement, beyond general increase in dietary intake as food prices fall and real incomes increase, is not cited by Hazzell and Haddad.

5.14 The seventh and final channel for poverty reduction through agricultural research comprehends two quite distinct ideas. The first is "empowerment" of the rural poor, meaning primarily in this context their influence on the research prospect. It is of course likely to improve the quality of field tests to obtain detailed views from farmers trying out new

technologies of how innovations are working and what might be done to improve the usefulness of innovations, as Hazzell and Haddad argue citing Farrington and Martin (1988). But empowerment, if it means more than opinion polling or information gathering, implies some ability of farmers to actually determine the research agenda. There is no evidence apparent that this is even a good idea.[25] In any case the argument for empowerment being a consequence of research, and hence an additional benefit of that research, is not even argued for. The issue of empowerment through asset accumulation reducing vulnerability to economic shocks is a rather different thing; this result turns on incomes and hence wealth of poor farmers being increased by improved varieties.

5.15 The International Fund for Agricultural Development's report on the challenge of ending rural poverty (IFAD 2001) places agricultural research in the context of other means to accomplish this end. Its priorities include access to education and health care, legally secure entitlements to assets (land and water especially), and access to markets and microfinance in addition to productivity increasing technology (especially for food staples). The report takes seriously the objections raised by some that technological innovations have advantaged the relatively prosperous and well situated more than the poor, and hedges its discussion of conservation and environmentally aimed innovations, and commercially oriented technical progress such as in exported crops. It therefore comes as all the more powerful a judgment when the report includes crop variety improvements as perhaps the key factor in rural poverty reduction to date, and points with alarm to declining rates of increase in yields of staples (cereals, roots, and tubers) in the developing world generally and particularly in Africa (IFAD, Chap. 4). Hence the strong recommendation of the report for enhancement and refocusing of agricultural research effort on yield improvement (not distinguishing NARS and CG Centers, presumably meaning both).

5.16 Otsuka (2000) reinforced the emphasis on yield and supplemented this with a recommendation that, for purposes of poverty reduction, research should focus on labor-using as well as yield-increasing innovations. This advice can be questioned on the grounds that focusing on labor-using ideas is likely to have a cost in terms of searching for the innovations that increase productivity the most *tout court* (essentially because adding an additional constraint can only reduce one's capabilities of reaching the larger objective). Otsuka supports his argument with one of the few empirical studies of impact to focus directly on poverty, by consideration of landless laborer households as compared to land-holding farmer households in Southeast Asia. Among many interesting findings from a cross-sectional comparison of countries and villages within countries, he finds that "land income is positively associated with MV [modern varieties of rice] adoption and availability of irrigation, but labour income from rice production and income from other sources are not clearly correlated with technology and environmental factors" (p. 455). At the same time, he finds strong support for Hazzell and Haddad's third factor listed earlier, to the point that he finds essentially complete equalization of wages across production environments through labor migration.

5.17 A further telling fact is that in a related study that is able to analyze improvement in farm laborer wages over time (in an IRRI study of the Philippines), Estudillo, Quisumbing, and Otsuka (2000) find that the increase in agricultural wages is attributable to an increase in labor demand in the nonfarm economy. Analytically, the points made by Otsuka and the related literature can be represented by an elaboration of Figure 1 in which the gains to producers (producers' surplus is disaggregated into rents received by owners of land,

suppliers of family and hired labor, and suppliers of other inputs, all of which factors of production underlie the production technology represented by the supply curve of Figure 1.

5.18 In that disaggregated context, the division of gains from new technology among landowners, workers, and other input suppliers depend not only upon the shift in factor demand induced by the innovation (which is the focus of most discussion of how to orient new technology so as to favor the poor) but equally upon the elasticity of factor supply. Indeed, the thrust of the Otsuka et al. literature is that in the case of earnings of landless laborers the elasticity of supply dominates the factor demand effects. The reason is that, almost surely in the long run and to a substantially so even in the shorter run of one or two years, the supply of agricultural labor is highly elastic, so that whether an agricultural innovation increases labor demand or not makes little difference. What matters is rather shifting the labor supply curve upwards by changing the productivity of the workers themselves (in a range of economic activities) as for example by improving worker skills or health, and so shifting the supply curve of labor upwards (increasing the opportunity cost of supplying one's labor to agriculture).

5.19 With respect to the wage issue as well as others, the tenor of discussion concerning new technology and poor or small farmers' well-being has striking parallels between the debate in developing countries and in the industrial countries. In the U.S. case, the Land-Grant Universities have been criticized on much the same grounds as the international Centers and NARS in the developing country context. Cochrane (1993) summarized his discussion of the evolution of U.S. agricultural technology as follows:

> "Thus it turns out that rapid and widespread technological advance in American agriculture from 1920 to 1990 worked to the advantage of two groups: (1) urban consumers and (2) the small, select group of farmers who were in the technological vanguard. For the rest, the agricultural development process based on rapid and widespread technological advance has been a nightmare." (p. 388)

5.20 The U.S. Agricultural Research Service and Land Grant University research programs have come under extensive criticism as causal factors in these events, forcefully stated in Hightower (1973). The complaint is not that public agricultural research has been ineffective, but rather that the effects have been largely undesirable — that the technology resulting from research efforts advantaged large corporate farms and agribusiness enterprises, and actually worsened the situation of family farmers while reducing the quality of the food consumers eat. In the particular case of the mechanical tomato harvester, part of the research effort at the University of California that inspired Hightower, Schmitz, and Seckler (1970) brought the human costs of displaced workers into their accounting. Even after their adjustments, however, the annual rate of return they estimated was over 35 percent. That and other efforts to broaden cost-benefit analyses of research, most recently by bringing environmental elements into the picture, have not altered the conclusion that rates of return are high. More relevant with respect to poverty reduction is a general increase in the incomes of farm relative to nonfarm households, and of the relative incomes (but not farm product sales) of small relative to large farm households. Both phenomena are mainly attributable to improvements in the nonfarm labor market and integration of the farm and nonfarm labor markets (Gardner 2000), and this is what if fact saved "the rest" from Cochrane's nightmare scenario. The implication for both developing and OECD countries is that focusing research on innovations that do not save labor is not necessary in addition to being a possibly damaging constraint on research.

5.21 Nonetheless, in both the developed and developing country contexts, economic investigations addressing the distributional question of which farmers and what other economic interests are helped most by agricultural research remain primitive. An example of a topic on which our knowledge remains seriously incomplete is evident in conflicting advice on the commodity focus of CGIAR research that would best serve the cause of poverty alleviation. IFAD (2001) and other experts point to the need for a concentration of effort on staple food crops. Yet Hazzell and Haddad (2001b), in their paper explicitly addressed to what the CGIAR can best do to enhance its ability to reach the poor, recommend "Expanding its mandate to include cash crops and non-ruminant livestock where those are important to the livelihoods and nutrition of smallholders and farmers in less-favored lands" (p. 45).

5.22 The issue of productivity growth and poverty impact resulting from adoption of high-yielding varieties in more- as opposed to less-favored areas has been investigated for regions of India and China by Fan and Hazzell (2001) and Fan, Hazzell, and Haque (2000). They find that both productivity and poverty are as strongly affected in desired ways in less-favored (or "low-potential") as in more favored areas, and conclude that "more investment should now be channeled to less-favored areas in India" (Fan and Hazzell, p. 1219). However, their results show only the high-yielding varieties have desirable results in those areas, and only assume agricultural research can efficiently develop high-yielding varieties for those areas.

5.23 An additional complicating factor in returns to agricultural research in developing countries is the interaction between research and farm commodity taxation or support programs. Criticism has been made of spending on research to boost production in the OECD countries at the same time commodity programs try to reduce production (e.g., by acreage set-asides as the US and EU have implemented), or dump surplus production on world markets. Several studies by agricultural economists have indicated that in this situation research spending that would otherwise have a high rate of return becomes a waste of the taxpayers' dollars (see Alston, Edwards, and Freebairn 1988). Still, Alston and Pardey (1996) conclude that even the highest commodity program distortions in developed countries have not changed the story of high social returns to publicly supported agricultural research. More serious for developing countries is the fact that currently the OECD countries are subsidizing both research and commodity production, giving a double whammy to commodity prices and thus reducing the returns to agricultural research in developing countries.

5.24 Mundlak, Larson, and Butzer (1999) and Fulginiti and Perrin (1993) are among those who have estimated that reduced commodity prices in developing countries significantly retard technology adoption and productivity growth. The papers in Ingco and Winters (2001) quantify the price effects and some of the losses developed countries impose upon developing countries. It will be a sad day if OECD countries cut back on funding for CGIAR research on the grounds that the returns to adoption of new technology have fallen, when it is in substantial degree the commodity policies of the OECD countries that are responsible for the fallen returns. As will be discussed later under priority setting, it is surprising that IFPRI has chosen to devote relatively little of its research effort to evidence and analysis in this area.

6. Discussion of Impact and Implications

6.1 Throughout the recent efforts at impact assessment in the CGIAR there is a strong undercurrent of concern that too great a focus on demonstrable returns will skew CG activities in ultimately unrewarding directions and harm the funding of valuable research efforts. The lags between research efforts and ultimate socio-economic impacts are so long and variable, and the outcomes so uncertain, and some of the benefits so fundamentally unquantifiable, that the whole impact assessment approach becomes suspect. What then are funding agencies asked to do, short of turning over to scientists the keys to the vault?

6.2 We can consider other items on the list of outcomes in section 2 above. One such that is used in funding basic science in universities is to evaluate research on the basis of what other scientists say about it. The issue then about CGIAR research now being carried out and in prospect is the degree to which it is considered good science in the judgment of scientific peers. This is indeed what happens in Center reviews. Why shouldn't the funding agencies satisfied with this level of approbation? Here one has to admit the importance of trust. Are the reviews too much a mutual back-scratching game? Our interviews suggested some resentment that some Center administrators took upon themselves an inappropriate role in attempting to influence what reviewers saw, and then what they said, and exhibited too much of an attitude that any criticism was an affront. This leads to a search for "objective" indicators, which rate-of-return estimates aspire to be, but over a too limited sphere of measurement.

6.3 A general reason for doubt about returns in the environmental/social areas is that neither the scientific nor the practical plant-breeding expertise that characterized the CGIAR's successes increasing agricultural productivity are demonstrably in place or can readily be put in place to give the CGIAR its former advantages in the newer areas. At the same time, other means of international coordination of investment in global public goods are seen by many as unpromising given, for example, the record of UN-related bodies or the once-hyped International Commodity Agreements. Some even propose expansion of the CGIAR beyond the currently discussed agenda. One expert not long ago opined that "It is a sad sign of inertia or lack of imagination that the CGIAR has not set up a research institute with as its central concern the development of biological and other new energy technologies to benefit poorer rural people." (Chambers, 1983, p. 182).

6.4 The discussion heretofore has said nothing about the efficiency of research in CGIAR, and the possibility that while agricultural research is a promising investment, means other than the CGIAR as currently constituted might be a better vehicle for such investment. The bureaucratically induced inefficiency of other international institutions is a reason for preferring the CGIAR. But more broadly, as long as we can see hundreds of millions in research expenditures generating scores of billions in economic benefits, it seems churlish to worry about costs of research or the organizational details of the research enterprise. This pertains principally to crop varietal research of the past. Taking cognizance of currently competing research entities and emerging elements of the current research agenda that are not so obviously socially profitable, the idea that bureaucratic or sclerotic inefficiencies in CGIAR might be a problem begins to get more attention.

6.5 While many impact indicators are highly favorable, they do not cover much of what the CGIAR now does, and do not get at the net value added of the CG in a convincing way.

So if one comes at the issue skeptically, one comes away unconvinced that it is crucial to maintain, much less expand, funding of the CGIAR. At the same time, the limitations of impact analysis are so great, in that really sensational scientific breakthroughs could easily escape notice over the observable time frame, that it seems unduly patronizing for outsiders to pretend they know how to allocate agricultural research funds and how much to allocate. If we see agricultural research as generally desirable, as the longer term record gives us reason to do, we may be inclined to give way to the approach of letting the scientists themselves decide not what to do, and to cut funding or close down an activity only in the case of clear malfeasance or incompetence.

6.6 Nonetheless, what may legitimately stick in the craw of a funding agency in this context is giving the allocative authority to a subset of all the scientists who could possibly be involved, given the self-interest that this subset will have a hard time resisting. A way of dealing with this is to make the allocation of funds genuinely competitive among the broadest possible set of scientists. In the case of the U.S. in many areas of research, this has been done by allocating funds to competing scientists rather than to institutions. Perhaps it is time to try this approach to international agricultural research. The donors would fund something like the CGIAR Secretariat, with something like the just-abandoned TAC as a priority setting mechanism, and that body would award grants competitively in response to requests for proposals from any scientist anywhere, the winners selected according to peer review of the capabilities of the scientist and institution he or she is associated with to carry out the research successfully.[26]

6.7 Some caveats about competitive funding require serious consideration. Some competitive grants programs become too short-run focused, a particular danger in biological research where long-term research programs are prevalent. The granting process itself can become drawn out and burdensome; transactions costs may be considerably less with a mechanism like the CGIAR as currently structured, with just the modest competitive element introduced with the Challenge Program. In such a strongly top-down character of granting decisions, care must be taken that the research program not neglect real research needs as seen by the developing countries

6.8 An alternative to across-the-board competitive funding is to allocate core funding to the CGIAR as currently structured, sufficient to support a continuing crop varietal and other productivity-enhancing research at or near current levels.[27] A new, more widely dispersed competitive funding mechanism would be used for addressing the expanding research agenda.

7. Priority Setting

7.1 Priority setting gets plenty of attention in the CGIAR. Three practical issues that may seriously impinge upon the productivity of CGIAR have emerged in the course of the Team's review. The first is whether over-attention to the nuances of what donors are willing to fund has diverted effort from scientific research that has potentially high but uncertain returns, and returns which even if high will be realized only after a long gestation period.[28] The second is whether more influence of developing country farmers themselves should be felt in the research agenda. The third is whether the recent apparent diminution of the authority of TAC through the introduction of the seemingly weaker "Science Council" will tend divert priorities away from the criterion of scientific promise. (A fourth issue is whether the CG is

already devoting its resources so intensively to self-assessment and priority setting for the future that that effort is itself detracting from the Centers' proper scientific business. However, this effort, while large, does not appear unduly large as compared to the planning and management budgets of other comparably complex enterprises; and the fact that only a relatively few projects appear to generate the bulk of returns while the great majority turn out to be "dry holes" indicates that devoting a large amount of effort to determining what projects to undertake is a good idea.[29])

7.2 Beyond these general issues there are specific issues about the detailed priorities of individual Centers. For example, it is arguable that the social value of IFPRI's research would be higher if focused more pointedly at analyzing developing country agricultural price support policies, with the aim of quantifying the extent to which those policies drive down world prices and retard investments in agriculture that embody improved technology and hence enable developing countries to feed themselves and compete in international markets. A candidate for a corresponding reduction of effort would be in world food supply-demand baseline projections, where other national and international institutions already are heavily involved with less of an axe to grind than a CGIAR institution's interest in uncovering future food shortages (although in fact IFPRI's projection work appears quite unbiased, or at least not at variance with findings of other economic researchers – but again the fact that there exist these other researchers diminishes the need for IFPRI's research).

7.3 Byerlee (2000) provides a nice discussion of priority-setting issues, with a focus on the objective of poverty alleviation. He distinguishes "supply-led" and "demand-led" approaches to priority setting, features of the former being scientists' judgments, yield, and economic benefit considerations, with the latter characterized by donor preferences (competitive funding), national or regional politics, and local commodity groups or farmer committees. With respect to particular approaches to poverty alleviation, Byerlee emphasizes research aimed at staple commodities, because (i) lower prices paid for them benefit poor consumers (following arguments going back to Scobie and Posada, 1978) and (ii) (properly less emphasized by Byerlee) because staple commodities are often produced in less-favored areas by poorer producers who use them for consumption.

7.4 Yet recall Hazzell and Haddad's recommendation quoted earlier that CGIAR pay more attention to the possibility of alleviating poverty by increasing the profitability of growing cash crops. By opening up priority in poverty fighting to livelihoods, one can even see research on coffee, cocoa, or cotton as possibly selected for poverty-fighting efficacy. Sorting out which path would do most for the poor when market income generation is taken into account suggests quite different priorities, and economists' capabilities to make solid recommendations here require empirical information that is typically not available.

7.5 Meyers (2001) encapsulates the view of FAO (2001) as follows:

> "Agricultural growth brought about by an increase in agricultural productivity, a dynamic export sector or a switch to higher value crops will raise profitability of farming and thus the incomes of those involved directly in the beneficiary subsectors: land owners, agricultural workers. It may also benefit those subsectors upstream or downstream primary production…The extra income from agricultural growth creates

> demand for locally non-tradable goods…Through such a process a virtuous circle is created, with agricultural and rural non-farm income growing." (Meyers, p. 14)

7.6 One could even push a step further and ask whether research that increased the demand for farm products in non-food uses would be poverty-alleviating or poverty-creating? Consider for example research, ongoing in many countries, to reduce the cost of converting grains or other crops to energy sources such as ethanol or other biofuels. Success at this endeavor would increase farmers' returns but would increase the cost of food to consumers by diverting crop supplies out of the food chain. Incomes of the poor could well be reduced by successful research in this area; but it is possible also that the effect in reducing poverty among growers would offset the losses to poor food consumers.

7.7 Byerlee (2000) also endorses research that targets labor-intensive commodities and technologies, citing Lipton and Longhurst (1989). This is a questionable idea, for reasons argued earlier with respect to labor-using technologies. Targeting research at labor-intensive commodities is dubious even under the premise that one wants to avoid decreasing the demand for labor. Improved technology for these commodities is especially likely to be labor saving, *ceteris paribus*. But more fundamentally, searching for technologies that use a lot of labor runs a substantial risk of being counterproductive over the long-term (which is the term at which research typically operates). As the discussion above of Otsuka's paper indicates, evidence is lacking that agricultural labor demand is an effective route to improving labor earnings in agriculture, counter-intuitive as that may seem.

7.8 There appears to be a real difficulty stemming from agriculturalists' thinking being stuck in the tracks of a too-stubborn agro-centrism. This narrowness of view appears most explicitly in the Editorial (2000) that opens the volume in which Byerlee's and Otsuka's papers appear. In arguing that agricultural research compares well with alternatives for reducing poverty, education is dismissed as a competitor because it "promotes migration from rural areas" (p. 381). What this dismissal leaves out of account is that when poverty reduction is our objective, we ought to focus first on poverty without reference to sub-categories of the poor, even when we are addressing rural issues. Then we will be open to the possibility that the most promising approach for rural poverty alleviation, especially for the landless, involves education, health, and growth in the general economy coupled with freely functioning labor markets.

7.9 More generally, one can make a reasonable case for priority setting that does not attempt to tailor agricultural research to fit any complicated economic model of poverty reduction, because too much about the parameters and equations of such a model is too uncertain. It is preferable more simply to focus where the scientific prospects for productivity improvements are greatest *tout court*. The case for continued support of agricultural research can then be made, with more solid factual support as well as with more economic honesty, on the grounds of the astonishing improvements in total factor productivity that have been achieved through improved agricultural technology stemming from CGIAR activity. Osmani (1998), after critically reviewing a literature he finds "obsessed with the question of whether or not growth has led to greater inequality and poverty," concludes "Perhaps the time has come to spend more effort in exploring the other question of why growth has been too slow in the first place" (p. 208). The strength of the CGIAR has been its contributions to overcoming the premise of that question, and despite implementation of desires to broaden the CG Centers beyond it, evidence that this has been a good idea is quite lacking.

7.10 Looking within alternative approaches to productivity-increasing research, however, some added complication in priority setting is becoming increasingly important in light of the literature reviewed for this paper. The core of the complication is the belief that the global public goods idea should be more explicitly introduced into the priority-setting process. CGIAR activity should be focused on investments that, other things equal, are less likely to be undertaken by the private sector.

7.11 The public-goods focus places a somewhat difference light on the question of training and capacity building in NARS as a CGIAR output. While high returns are nowhere near as well documented as for new knowledge applied to agricultural production, the informal evidence is substantial that that these outputs have been highly productive. But, are they global public goods? Not by the standard definitions. Those who are trained can capture some of the benefits of their training through higher wages. Boosts to NARS by definition are not global. Nonetheless there is a reasonable argument (in addition to the argument that aid to the poorest countries is in and of itself a global public good); namely, scientific training of developing country personnel, and other enhancement of the intellectual capital stock in those countries, has been and will increasingly be to a substantial degree joint products with CGIAR research – once a CG Center is doing the research the additional cost of certain training/capacity building is small or nil.

7.12 Within the set of global public goods, further refocusing should give priority to those activities for which evidence of high rates of return is most persuasive. This includes research on improved crop varieties and other productivity-increasing innovations. With respect to the expanded agenda discussed earlier, procedurally, what is needed is a benefit/cost test. The CG has done some of this under the heading of *ex ante* impact assessment. The pitfall of this approach, when carried out by the research institution itself, it the risk that it will contain too high a ratio of hope to reasonably expected achievement. The recommendation of this paper is more basically to make some form of benefit/cost analysis central to priority setting. Admittedly this approach is made difficult to apply by the revealed preference of donors to fund their favorite areas of activity. So one's appeal here is as much to the donors as to the CGIAR itself.

8. Agenda for In-Depth Assessment

8.1 A capsule summary of what is well established, what is probable, and what is largely unknown about the key issues in CGIAR impact is as follows:

- Well established:
    1) The social rates of return to investment in improved cereal crop varieties derived from CG Centers have been enormous.
    2) The CGIAR has played a key role in the development and current scientific capabilities of NARS in many countries.
- Probable:
    3) Rates of return to continued "maintenance research" on cereals production and on other crops, and related joint products in training and capacity building in NARS, remain above the cost of capital.

4) These returns have a sufficient public-good component that it makes sense for CGIAR research aimed at agricultural productivity improvement to remain an important component of a global public goods portfolio.
5) The public-good benefit is significantly reduced from former years in some countries because (a) NARS and (b) private-sector provision of agricultural research are in some countries now better equipped to be an efficient provider of agricultural R&D; so the composition of CGIAR activities is important – notably there should be greater focus on Africa and perhaps South Asia.

- Largely unknown:
  1) Rates of return to parts of the expanded CGIAR agenda – fostering biodiversity, countering global warming, social science research – are highly conjectural. Uncertainty of returns would be fine if the expanded agenda were costless, but not if it is diverting research resources from activities with higher expected returns
  2) The value of an explicit focus on poverty alleviation by the CGIAR is unclear, because it is unclear what that focus tells the CGIAR to do in pursuit of productivity growth, and because to the extent it leads the CGIAR to divert its activities to objectives other than productivity growth it leads the Centers to undertake activities for which other institutions may be better suited, and professionals other than agricultural scientists better equipped.
  3) CGIAR impacts in capacity building and training in developing countries have undoubtedly been significant, but what we don't have is a sense of the value of these impacts, and how the benefits stack up against costs. The comparative advantage of the CGIAR relative to universities or non-CG biological science institutions in capacity building or scientific education is in doubt. Given the uncertainties, it might be productive for CGIAR Centers to compete with other institutions for global public goods funding in agricultural and social science.

8.2 The terms of reference for this meta-evaluation include making recommendations for a possible in-depth review of the CGIAR. The natural set of recommendations from the preceding is to attempt to resolve uncertainties in those statements. Yet in the area of impact assessment, we have the paradoxical situation that while much remains to be learned about the effects of past CGIAR activities, and the prospects for future research and other activities that will generate benefits in excess of costs, a specific agenda for an in-depth further assessment is not immediately apparent.

8.3 The main source of pessimism about what can be achieved with a further in depth review in the area of CGIAR impact is the enormous effort that has already been committed to impact assessment. The data and experience available have already been extensively and intensively mined by investigators both within the CGIAR System and outside it. Significant progress on the many questions that remain unanswered would typically require long-term inquiries – partly because there is in the end no substitute for waiting to see on-the-ground results from what is now and has recently been done in the CG Centers. This is not feasible for even, say, a year-long follow-up assessment to this meta-evaluation.

Endnotes

1. Such multiple impacts underline the importance of avoiding double counting, for example not separating *proximate* or first-round effects from *ultimate* effects (so we do not for example count both improved scientific capacity to carry out yield-increasing research and the producer/consumer gains from yield the resulting yield increases as separate impacts to be added up to get an overall impact measure).

2. It should be noted also that the gains discussed here are only those of developing countries. Alston and Pardey (1996, pp. 275-76) cite evidence that U.S. producers have gained by adopting varieties derived from CGIAR-developed wheat varieties.

3. Data on funding of CGIAR centers is taken from Pardey, Roseboom, and Craig (1999), Table A3.3, which shows in compact form total funding of each of 16 Centers annually, 1960-1997.

4. "Additional" wheat means over and above what would have been produced in the absence of the CIMMYT-related germplasm, which is of course very difficult to estimate. The estimates imply that on average the 51 million hectares in CIMMYT-related wheat yielded about 0.2 tons per acre (roughly 20 percent above developing country average yields) more than would have been the case with non-CIMMYT wheat that would alternatively been used. D&A cite some problems with the estimates, which were made by the Economics Department of CIMMYT, and provide details and make adjustments (see Anderson and Dalrymple, 1999, p. 54). Overall, information about exactly how these estimates were arrived at is scant.

5. Alston et al., following Griliches (1958), present a simple formula for approximate comparison of benefit/cost ratios (B/C) with the internal rate of return (IRR, the interest rate that just equates discounted benefits and costs); namely B/C = IRR/i, where i is the opportunity cost of funds (called by Alston et al. the required rate of return). So if the benefit/cost ratio is 9.3 as in the example given here – not uncommon in agricultural research studies, and actually lower than many – and the rate of (real) opportunity returns is 0.04, the IRR is 37 percent.

6. Calculated from data provided in Table 6 of Manyong, Dixon, et al. (2000).

7. Benin, Burkina Faso, Ghana, Guinea, Mali, Nigeria, Senegal, Togo, Cameroon, Congo, and Chad.

8. While very high, these implied returns do not indicate that extraordinary ratios of benefit/cost estimated for CIMMYT research. The latter generated estimated productivity gains worth billions of dollars annually instead of the hundreds of millions for IITA research, while the aggregated budget for IITA is slightly larger than for CIMMYT.

9. An evident reach for trendiness appears particularly unpromising, notably in the section of accomplishments devoted to gender, "incorporating gender issues into policies and practices related to biodiversity," where it is stated that "Gender provides a useful framework for analyzing and evaluating the respective activities of women and men." (p. 40).

10. My summary statements omit the centers' reports on *ex ante* impact assessment, which for some centers is the bulk of the activity reported. *Ex ante* assessment is based on projection of what the research will accomplish if successful. This is undoubtedly helpful in decision-making in individual centers and in the CGIAR System, but it is only peripherally relevant to consideration of how successful CGIAR research has actually been.

11. This report contains the remarkable statement that "No amount of improvement in methods of evaluation will substitute for human interest stories" (p. 40). This may sometimes be true but the converse is more generally true, and CIP seems a bit cavalier on the point.

12. Coulibay et al. (1998) is cited as the source, but the reference list for the summary paper does not include publication details.

13. The following pages on policy research in IFPRI go into far more detail on a single center than elsewhere in this paper. The main reason is the importance of the subject coupled with the lack of existing literature that estimates the returns to policy research quantitatively, while there exists a lot of qualitative assessment that requires more discussion to address. Uninterested readers may skip to page 29. Also, while policy research has been undertaken at IARC's other than IFPRI, those efforts are not well documented in materials available to the author, and so are omitted from the meta-evaluation.

14. A special team under the chairmanship of a TAC member had felt strongly that IFPRI should move from its location in Washington, D. C., to a developing country to "place its research staff in an environment which would be more relevant to the objectives of the institute, avoid the perception of IFPRI having a somewhat privileged status in the CGIAR and could also protect IFPRI from undue donor influences and demands in its analysis of the world food problems" Baum, p 139.

15. For example, Anderson, Moscardi, and Pardey (1994) evaluated the CIMMYT Economic Program (CEP), criticizing adoption studies done in the CEP as not contributing beyond the findings already available in the literature on wheat and maize (p. 3), finding their farm-level adoption studies questionable, and recommending that CIMMYT cease doing adoption studies – while at the same time commending CIMMYT's global impact assessments, e.g., Byerlee and Moya (1993).

16. Since IFPRI's work led to a recommendation of increased funding for productivity-enhancing research, yet such funding didn't increase and in some years decreased, shall we say that the impact was nil and the research had no economic value? No, because the relevant comparison is not between the recommended policy and actual policy, but rather between the recommended policy and what the policy would have been if the absence of the recommendation. The relevant counterfactual situation could be that, had rate-of-return estimates not been so high, funding for productivity-enhancing research in CGIAR would have been sharply reduced or even phased out. Impact assessments of improved CG crop varieties face a similar problem of identifying the relevant counterfactual – how would crop yields have evolved in the absence of these varieties? In policy-related activity, identifying the relevant counterfactual is significantly more difficult. In any case it is harder to document a favorable impact when the facts are unfavorable.

17. In this paper even some of the compliments are back-handed, e.g., "IFPRI publications are well regarded in Malawi among the few of those interviewed who receive them." (p. 32). Overall, given the provenance of Ryan's paper (commissioned by IFPRI) and his knowing the culture of IFPRI (not unlike academia), where every criticism is likely to be taken as an affront, the Malawi paper is remarkably negative.

18. The Third EPMR (CGIAR 1998b, p. vi) recommends higher priority, even at the expense of work in other areas, on "a more in-depth involvement with policy research and advice on low-income transitional economies about how to institute a functioning market economy…" This is sensible advice, but it should be fully recognized that nothing recommended in these economies will be accepted without fierce controversy.

19. Romano later discusses limitations of rate-of-return estimates. The limitation that has to give one most pause is the omission from rates of return (or benefit/cost ratios) of distributional impacts. In the Colombian case it is quite possible that for some innovations at least, poor farm households were losers. The point about poverty is also given prominence in a comment of Raghav Gaiha on a draft of Gardner's review paper. A major item of debate is the extent to which the CGIAR agenda should be tuned to an explicit emphasis on research that will be used by the poorest farmers, typically is less favored agricultural areas (as opposed to focusing on wherever the prospects for productivity improvement are greatest).

20. Between 1991 and 2000 international funding sources of the Kenyan NARS grew from 19 percent to 61 percent of total funding (and World Bank credit grew to 63 percent of outside funding). Ndiritu also notes that while the CGIAR argues that they spend 40 percent of their budget in Africa, the NARS "feel that most of the money goes back as salaries and not to infrastructural support."

21. The CG's attention to rigorous impact assessment is impressive not only in absolute volume and quality or work, but also in comparison with efforts of national agricultural research systems. For example, U.S. federal and state agricultural research institutions have budgets that total over $2 billion annually, six times the funding level of the entire CGIAR System, but organized efforts at impact assessment in the U.S. system are negligible.

22. In an interview (December 2001), Carl Pray indicated his view that the government of India had indeed hindered private-sector R&D, but that it was not through crowding out. Rather the problem was legal restrictions on domestic and international companies entering the seed business until the mid-1980s. He does believe that in cases like hybrid maize private companies have shown clear competitive advantage and in such cases public research institutions have withdrawn or reduced their efforts.

23. "Other things equal" here refers to effects calculated from OLS multiple regression coefficients, and "significant" refers to t-statistics that give a 95 percent confidence level against a null hypothesis of no effect.

24. It may also be argued that imperfections of competition may result in productivity gains not being realized in cost reductions. In either case – monopoly power in input supply or scarcity of factors that experience demand increases – social gains remain approximately equal to the productivity gains. It's just that the gains go in part to monopolists or input suppliers rather than farmers or consumers.

25. In their rural poverty report, the International Fund for Agricultural Development states that "Where farmers are consulted about their priorities, they often select priorities other than yield." (IFAD, 2001, p. 136). Yet IFAD's recommendation for refocusing CGIAR research to better fight poverty is renewed emphasis on yield.

26. With respect to the U.S., agriculture is notable as a strong resistor to this approach, and it has so far largely been defeated politically by the U.S. NARS (Land-Grant Universities and USDA's Agricultural Research Service).

27. An argument for actually increasing such funding is well expressed in Pardey and Beintema (2001). Their concluding section puts forth the following argument for a greater commitment to publicly funded agricultural research in developing countries: (1) the "South-North gap" [using South and North as labels for developing and developed countries, respectively] in public investment in agricultural research had been shrinking, but no longer is; (2) growth in even the North's stock of publicly generated knowledge is slowing; (3) together with the shifting of the North's research to local environmental and food safety concerns means growth in the pool of knowledge for spillovers to the South is slowing; (4) the even greater slowdown in the growth of science in the South "limits the potential of poor countries to develop locally relevant technologies and tap into Northern knowledge stocks"; (5) while private sector research is increasing, "it covers only a small subset of the needs and is mostly a complement, not a substitute, for continued public and other nonprofit research"; and (6) there is no evidence to bear out the idea of diminishing returns to more recent public research. These points lead to the conclusion that "Reinvigorating support for Southern science is unquestionable the top priority" (all quotations are from p. 22). One may reasonably question assertion (6), but even so the weaker statement still holds that there is no evidence that the returns to investment in public agricultural research have fallen below the cost of capital.

28. It has been argued that even on scientific grounds, the most productive research in the CG centers as well as NARS will be quite practically and relatively short-term oriented. "Implementing and intensifying known technology in plant breeding will be more productive than searching for

'prospects of breakthroughs.' Wholehearted implementation of known technology actually will increase the likelihood of breakthroughs. In at least the short and medium terms, current upward trends in crop productivity can be maintained and amplified if plant breeding at all levels, both professional and on-farm, is given appropriate support in the form of funds and organizational opportunities" Duvick (1995).

29. A concrete examination of "predominance of dry hole" phenomenon emerged from a project-by-project review of agricultural research funded under the U.S.-Israel Binational Agricultural Research and Development Fund (BARD). Just et al. (1988) found that of 208 projects completed after 1979, by 1988 only 20 had some degree of commercial application, and only 5 could be characterized as commercially successful. But these five projects generated net benefits to the U.S. during that period (present value as of 1979) estimated at $521 million from an investment in all of the BARD projects of about $50 million.

Annex 1. References

Alston, Julian. "Spillovers," Presidential Address, Australian Agricultural and Resource Economics Society 46th Annual Conference, Canberra, February 2002.

Alston, Julian M, and Philip G. Pardey. "Research Evaluation Requirements in the CGIAR," Report to the Impact Assessment Task Force of the CGIAR, 1995 (revised December 2001).

_____. *Making Science Pay: The Economics of Agricultural R &D Policy*, Washington, D.C.: AEI Press, 1996.

_____. "Attribution and Other Problems in Assessing the Returns to Agricultural R&D," *Agricultural Economics* 25 (2001): 141-152.

Alston, Julian M., Geoff W. Edwards, and John W. Freebairn. "Market Distortions and Benefits from Research." *American Journal of Agricultural Economics* 70 (1988): 281-288.

Alston, Julian, C. Chan-Kang, M. Marra, P. Pardey, and T. Wyatt. "A Meta-Analysis of Rates of Return to Agricultural R&D," International Food Policy Research Institute, Research Report 113, 2000.

Anderson, Jock R., Robert W. Herdt, and Grant M. Scobie. "Science and Food: the CGIAR and Its Partners," World Bank, Washington, D.C., 1988.

Anderson, Jock R., E. Moscardi, and P.G. Pardey. "Review of CIMMYT Economic Program," CGIAR Secretariat, Report, January 1994.

Anderson, Jock R., and Dana G. Dalrymple. "The World Bank, the Grant Program, and the CGIAR: A Retrospective Review," Operations Evaluations Department, World Bank, Washington, D.C., March 1999.

Asian Development Bank. "Special Evaluation Study on the Policy Implementation and Impact of Agriculture and Natural Resources Research," Operations Evaluation Office, SST: STU 2000-17, December 2000.

Bouis, H., R. Graham, and R. Welch. "The CGIAR Micronutrients Project," *Food and Nutrition Bulletin* 21 (2000): 374-381.

Brennan, J.P. "Spillover Effects of International Agricultural Research: CIMMYT-Based Semi-Dwarf Wheats in Australia," *Agricultural Economics* 3 (1989): 323-332.

Byerlee, Derek. "Targeting Poverty Alleviation in Priority Setting for Agricultural Research," Food *Policy* 25 (2000): 429-445.

Byerlee, D., and P. Moya. Impacts of International Wheat Breeding Research in the Developing World, 1966-90, mimeo, CIMMYT, 1993.

Byerlee, D., and G. Traxler. "National and International Wheat Improvement Research in the Post-Green Revolution Period: Evolution and Impact," *American Journal of Agricultural Economics* 77 (1995): 268-278.

CGIAR, Impact Assessment and Evaluation Group. "Methodological Review and Synthesis of Existing Ex Post Impact Assessments," Report 1, A Review of Documents, October 1997(a).

_____. Report 2, Analysis of Comprehensive Ex Post Studies of Impacts, October 1997(b).

_____. "The International Research Partnership for Food Security and Sustainable Agriculture," Third System Review of CGIAR, Final Report, CGIAR Secretariat, October 8, 1998(a).

_____. "Report of the Third External Programme and Management Review of the International Food Policy Research Institute," TAC Secretariat, SDR/TAC:IAR/98/2, November 1998(b).

_____. "Report of the First External Review of the System-wide Genetic Resources Programme," TAC Secretariat, December 1998 (c).

_____. "Reducing Poverty through Cutting-edge Science: First Review of System-wide Programmes with an Ecoregional Approach," TAC Secretariat, September 1999.

_____. "Impact Assessment of Agricultural Research" Context and State of the Art," TAC Secretariat, Impact Assessment and Evaluation Group, April 2000.

_____. "Report of TAC's Standing Panel on Impact Assessment to International Centers Week 2000," TAC Secretariat, October 2000 (b).

_____. "The Future of Impact Assessment in the CGIAR," Standing Panel on Impact Assessment of the TAC, Workshop Proceedings, TAC Secretariat, October 2001.

_____. Report of the Fourth External Programme and Management Review of ISNAR, Summary, 2002.

Chambers, Robert. *Rural Development,* London: Longman, 1983.

Chavas, Jean-Paul. "An International Analysis of Agricultural Productivity," in L. Zepeda, ed., *Agricultural Investment, Production Capacity, and Productivity,* Rome: FAO, 2001 [downloadable from http://www.fao.org/docrep/003/x9447e04.htm].

Cochrane, Willard W. *The Development of American Agriculture*. Minneapolis: University of Minnesota Press, 1993.

Cooper, R.N. "Financing International Public Goods," in C. Gerrard, M. Ferroni, and A Mody, eds., *Global Public Policies and Programs*, Washington, D.C.: World Bank, 2001, pp. 15-26.

Dalrymple, Dana G. "Development and Adoption of High-Yielding Varieties of Wheat and Rice in Developing Countries," *American Journal of Agricultural Economics* 67 (1985): 1067-73.

_____. "Development and Spread of High-Yielding Wheat Varieties in Developing Countries," U.S. Agency for International Development, Washington, D.C., 1986.

_____. "International Agricultural Research as a Global Public Good: A Review of Concepts, Issues, and the CGIAR Experience," mimeo, October 2001.

Danida (Denmark). "Analysis of Danish Assistance to the CGIAR Centres," Final Report, Ministry of Foreign Affairs, Denmark, August 1999.

Diaz-Bonilla, E., M. Thomas, and S. Robinson. "On Boxes, Contents, and Users: Food Security and the WTO Negotiations," mimeo, IFPRI, 2002.

Dixon, Diana. "An Overview of CGIAR Centres' Training Activities," CGIAR, IAEG Secretariat, October 1999.

Duvick, D.N. "Plant Breeding and Biotechnology for Meeting Future Food Needs," in N. Islam, ed., *Population and Food in the Early Twenty-First Century*, IFPRI, Washington, D.C., 1995, pp. 221-228.

Echeverria, R.G. "Assessing the Impact of Agricultural Research," in R. Echeverria, ed., *Methods of Diagnosing Research System Constraints and Assessing the Impact of Agricultural Research*, ISNAR, 1990.

Editorial (Anonymous), "Assessing the Impact of Agricultural Research on Poverty Alleviation: Some Issues and Priorities," *Food Policy* 25 (2000): 379-388.

Estudillo, J., A. Quisumbing, and K. Otsuka. "Income Distribution in Rice-Growing Villages During the Post-Green Revolution Period," *Agricultural Economics* (2000).

Evenson, R. M. "Crop Genetic Improvement and Agricultural Development," prepared for CGIAR Mid-term Meeting, Dresden, Germany, May 2000.

_____. "Economic Impacts of Agricultural Research and Extension," in B. Gardner and G. Rausser, eds., *Handbook of Agricultural Economics*, New York: Elsevier Science, 2001.

_____. and D. Gollin. "The Green Revolution at the End of the Twentieth Century," TAC Secretariat, April 2001.

Eicher, Carl K. "Institutions and the African Farmer," Third Distinguished Economist Lecture, CIMMYT Economics Program, January 1999.

Fan, Shenggen, and P. Hazzell. "Returns to Public Investments in the Less-Favored Areas of India and China," *American Journal of Agricultural Economics.* 83 (2001): 1217-22.

Fan, Shenggen, P. Hazzell, and T. Haque. "Targeting Public Investments by Agro-Ecological Zone to Achieve Growth and Poverty Alleviation Goals in Rural India," *Food Policy* 25 (2000): 411-28.

Farrar, Curtis. "A Review of Food Subsidy Research at IFPRI," IFPRI, Impact Assessment Discussion Paper No. 12, Jan. 2000.

_____. "The Consultative Group for International Agricultural Research," Case Study for the UN Vision Project on Global Public Policy Networks, mimeo, 2001 (text cited as available in December 2001 at www.globalpublicpolicy.net.

Farrington, J., and A. Martin. "Farmer Participation in Agricultural Research," Overseas Development Institute, London, Occasional Paper 9, 1988.

Fox, Glenn. "Is the United States Really Underinvesting in Agricultural Research?" *American Journal of Agricultural Economics.* 67 (1985): 806-812.

Food and Agriculture Organization of the United Nations (FAO). "Food Insecurity, Poverty, and Agriculture: A Concept Paper," mimeo, 2001.

Fulginiti, Lilyan E., and Richard K. Perrin. "Prices and Productivity in Agriculture." *Review of Economics and Statistics* 75 (1993): 471-482.

Gardner, B. Review of Mellor and Ahmed (1988), in *Economic Development and Cultural Change* 38 (April 1990): 650-652.

Graham, R.D., and R.M. Welch. "Breeding for Staple Food Crops with High Micronutrient Density," IFPRI, Agricultural Strategies for Micronutrients Working Paper 3, 1996.

Griliches, Zvi. "Research Costs and Social Returns: Hybrid Corn and Related Innovations," *Journal of Political Economy* 66 (1958) 419-432.

_____. "Issues in Assessing the Contribution of Research and Development to Productivity Growth," *Bell Journal of Economics* 10 (1979): 92-116.

Hayami, Y., and R.W. Herdt. "Market Price Effects of Technological Change on Income Distribution in Semi-Subsistence Agriculture," *American Journal of Agricultural Economics.* 59 (1977): 245-246.

Hazzell, Peter, and Lawrence Haddad. "Agricultural Research and Poverty Reduction," IFPRI, Food, Agriculture and the Environment Discussion Paper 34, August 2001(a).

_____. "CGIAR Research and Poverty Reduction," TAC Secretariat, SDR/TAC:IAR/01/02, August 2001(b).

Hazzell, Peter, and S. Haggblade. "Rural-urban Growth Linkages in India," *Indian Journal of Agricultural Economics* 46 (1991): 515-529.

Heisey, P., M. Lantican, and H.J. Dubin. "Assessing the Benefits of International Wheat Breeding Research in the Developing World: The Global Impacts Study, 1996-97," mimeo, 1999 [version used here undated but probably 2001 since publications of that year are cited; publisher or provenance of the monograph unstated in the front matter].

Hightower, Jim. *Hard Tomatoes, Hard Times*. Cambridge: Schenkman Publishing, 1973.

Horton, D. and N. Alexaki. "Evaluating Capacity Development in Research and Development Organizations," International Conference on Impacts of Agricultural Research and Development, San Jose, Costa Rica, Feb. 2002.

Ingco, M., and L.A. Winters, eds. *Agricultural Trade Liberalization in a New Trade Round: Perspectives of Developing Countries and Transition Economies,* World Bank Discussion Paper No. 418, September 2001.

Ingram, G. "Important Questions for Evaluation," in C. Gerrard, M. Ferroni, and A Mody, eds., *Global Public Policies and Programs*, Washington, D.C.: The World Bank, 2001, pp. 227-229.

International Fund for Agricultural Development (IFAD). *Rural Poverty Report: The Challenge of Ending Rural Poverty*, Oxford University Press, 2001.

Islam, Nurul, ed. *Population and Food in the Early Twenty-First Century,* Washington, D.C.: IFPRI, 1995.

Jha, D., and P. Kumar. "Rice Production and Impact of Rice Research in India," in Pingali, Prabhu, and M. Hossain, eds., *Impact of Rice Research*, Thailand Development Research Institute and International Rice Research Institute, 1998, pp. 279-291.

Just, R., D. Zilberman, D. Parker, and M. Philips. "The Economic Impacts of BARD Research on the U.S." mimeo, 1988.

Kealey, Terence. *The Laws of Scientific Research.* London: MacMillan, 1996.

Kerr, J., and S. Kolavalli. "Impact of Agricultural Research on Poverty Alleviation," IFPRI, Discussion Paper 56, 1999.

Kumar, P., and M. Rosegrant. "Productivity and Sources of Growth for Rice in India," *Economic and Political Weekly,* December 31, 1994.

Lipton, M., and R. Longhurst. *New Seeds and Poor People*, Baltimore: Johns Hopkins University Press, 1989.

Kackay, R., and S. Debela. "Summary Report: ISNAR's Achievements, Impacts, and Constraints," in ISNAR (2001), pp. 1-35.

MacKay, R., and D. Horton. "Expanding the Use of Impact Assessment and Evaluation in Agricultural Research and Development," International Conference on Impacts of Agricultural Research and Development, San Jose, Costa Rica, Feb. 2002.

Manyong, V.M., A. Dixon, K. Makinde, M. Bokanga, and J. Whyte. "The Contribution of IITA-improved Cassava to Food Security in Sub-Saharan Africa," IITA, Ibadan, Nigeria, 2000.

Manyong, V.M., J. Kling, K. Makinde, S. Ajala, and A. Menkir. "Impact of IITA-improved Germplasm on Maize Production in West and Central Africa," IITA, Ibadan, Nigeria, 2000.

Maredia, M., D. Byerlee, and J. Anderson. "Ex Post Evaluation of Economic Impacts of Agricultural Research Programs: A Tour of Good Practice," presented at a workshop of the Standing Panel on Impact Assessment of the CGIAR's Technical Advisory Committee, Rome, May, 2000.

Mellor, J., and R. Ahmed. *Agricultural Price Policy for Developing Countries*, Baltimore: Johns Hopkins University Press, 1988.

Meyers, William H. "What's the Buzz? Reflections on the World Food Summit – Five Years Later," Department of Applied Economics, University of Minnesota, Staff Paper P01-14, Oct. 2001.

Morris, Michael L. "Impacts of International Maize Breeding Research in Developing Countries, 1966-98," mimeo draft, CIMMYT, October 2001.

Mundlak, Yair. "Production and Supply Response," in B. Gardner and G. Rausser, eds., *Handbook of Agricultural Economics*, New York: Elsevier Science, 2001, pp. 3-85.

Mundlak, Y, D. Larson, and R. Butzer. "The Determinants of Agricultural Production: A Cross-Country Analysis," Policy Research Working Paper 1827, World Bank, Washington, D.C., 1999.

OED. "The World Bank and Global Public Policies and Programs: An Evaluation Strategy," Washington, D.C.: World Bank, July 2001.

Osmani, S.R. "Did the Green Revolution Hurt the Poor? A Reexamination of the Early Critique," in Pingali, Prabhu, and M. Hossain, eds., *Impact of Rice Research*, Thailand Development Research Institute and International Rice Research Institute, 1998, pp. 193-212.

Otsuka, Keijiro. "Role of Agricultural Research in Poverty Reduction: Lessons from the Asian Experience," *Food Policy* 25 (2000): 447-462.

Paarlberg, Robert. "External Impact Assessment of IFPRI's 2020 Vision for Food, Agriculture, and the Environment Initiative," IFPRI, Impact Assessment Discussion Paper No. 10, June 1999.

Pardey, P.G., J. Roseboom, and B.J. Craig. "Agricultural R&D Investments and Impact," in J. Alston, P. Pardey, and V. Smith, eds., *Paying for Agricultural Productivity*, Baltimore: Johns Hopkins Press, 1999, pp. 31-68.

Pardey, P.G., and N. Beintema. "Slow Magic: Agricultural R&D a Century After Mendel," International Food Policy Research Institute, Washington, D.C., October 2001.

Pardey, P.G., and J.E. Christian. "The Production and Diffusion of Policy Knowledge: A Bibliometric Evaluation of the International Food Policy Research Institute," IFPRI Impact Assessment Paper No. 14, January 2002.

Pasour, E.C., Jr., and M. Johnson. "Bureaucratic Productivity: The Case of Agricultural Research Revisited," *Public Choice* 39 (1982): 301-317.

Pingali, Prabhu. "Milestones in Impact Assessment Research in the CGIAR, 1970-1999," CGIAR-TAC, 2001.

Pingali, Prabhu, and M. Hossain, eds., *Impact of Rice Research*, Thailand Development Research Institute and International Rice Research Institute, 1998.

Pinstrup-Anderson, Per. "The Challenge for a 2020 Vision: Extent of Today's Human Suffering and a View Toward 2020," IFPRI, 2020 Vision Conference, June 1995.

Pray, Carl, et al. "Private Research and Public Benefit: The Private Seed Industry for Sorghum and Pearl Millet in India," *Research Policy* 20 (1991): 315-24.

Pray, C., and D. Umali-Deininger. "Private Agricultural Research: Will It Fill the Gap?" *World Development* 26 (1998): 1127-1148.

Pray, C., and K. Fuglie. "Private Investments in Agricultural Research and International Technology Transfer in Asia," USDA, Economic Research Service, Agricultural Economics Report No. 805, Nov. 2001.

Raab, R., G. Denning, and R. Cruz. "The Impact of IRRI's Training Program: A Different Perspective," in Pingali, Prabhu, and M. Hossain, eds., *Impact of Rice Research*, Thailand Development Research Institute and International Rice Research Institute, 1998 pp. 159-191.

Rangarajan, C. "Agricultural Growth and Industrial Performance in India," IFPRI, Research Report 33, 1982.

Ryan, James G. "Assessing the Impact of Rice Policy Changes in Vietnam and the Contribution of Policy Research," IFPRI, Impact Assessment Paper No. 8, January 1999(a).

_____. "Assessing the Impact of Policy Research and Capacity Building by IFPRI in Malawi," IFPRI, Impact Assessment Paper No. 11, December 1999(b).

Schimmelpfennig, D.E., and C.G. Thirtle. "The Internationalization of Agricultural Technology: Patents, R&D Spillovers, and Their Effects on Productivity in the European Union and United States," *Contemporary Economic Policy* 17 (1999): 457-468.

Schmitz, Andrew, and David Seckler. 1970. "Mechanized Agriculture and Social Welfare: The Case of the Tomato Harvester." *American Journal of Agricultural Economics* 52 (Nov.): 569-577.

Scobie, G., and R. Posada T. "The Impact of Technological Change on Income Distribution: The Case of Rice in Colombia," *American Journal of Agricultural Economics* 60 (1978): 85-92.

Timmer, C. Peter. "Economic Growth and Agriculture," Draft for Natsios Report on Foreign Assistance and the National Interest," mimeo, 2001.

Traxler, G., and D. Byerlee. "Economic Returns to Crop Management Research in a Post-Green Revolution Setting," *American Journal of Agricultural Economics* 74 (1992): 573-582.

Waibel, Hermann. "An Evaluation of the Impact of Integrated Pest Management at the International Agricultural Research Centers," CGIAR, IAEG Secretariat, ICW/99/08/c, September 1999.

Walker, T.S. "Reasonable Expectations on the Prospects for Documenting the Impact of Agricultural Research on Poverty in Ex-Post Case Studies," *Food Policy* 25 (2000): 515-530.

Annex 2: List of Working and Background Papers, Authors, and Peer Reviewers

Working Papers

Barrett, Christopher B. 2002. *Natural Resources Management Research in the CGIAR: A Meta-Evaluation.*

> Peer Reviewers: Jock Anderson, Derek Byerlee, Dana Dalrymple, Hans Gregersen, Ted Henzell, John Lynam, Vernon Ruttan, Meredith Soule, Joachim von Braun, Usha Barwale Zehr

Eicher, Carl K. and Mandivamba Rukuni 2002. *The CGIAR in Africa: Past, Present, and Future*.

> Peer Reviewers: Malcolm Blackie, Dana Dalrymple, Bob Herdt, Alain de Janvry, Romano Kiome, John Lynam, Eric Tollens, Geoffrey Mrema, Wilfred Mwangi, Cyrus Ndiritu, Emmy Simmons, Moctar Touré

Gardner, Bruce 2002. *Global Public Goods from the CGIAR: Impact Assessment.*

> Peer Reviewers: Jock Anderson, Dana Dalrymple, Osvaldo Feinstein, Paul William Glewwe, Hans Gregersen, George Norton, Scott Rozelle, Vernon Ruttan, Sara Scherr, Sudhir Wanmali

Lesser, William 2002. *Reviews of Biotechnology, Genetic Resource and Intellectual Property Rights Programs.*

> Peer Reviewers: Ronnie Coffman, John Dodds, Robert Evenson, Brian Ford Lloyd, Anatole Krattiger, Steve Kresovich

Spielman, David 2002. *International Agricultural Research and the Role of the Private Sector.*

Macedo, Jamil, Marcio C. M. Porto, Elisio Contini, and Antonio F. D. Avila 2002. *Brazil Country Paper for the CGIAR Meta-Evaluation.*

Katyal, J.C. and Mruthyunjaya 2002. *CGIAR Effectiveness — A NARS Perspective from India.*

Background Papers (Available upon request)

Ndiritu, Cyrus 2002. *CGIAR-NARS Partnership: The Case of Kenya.*

Romano, Luis 2002. *Colombia Country Paper for the CGIAR Meta-Evaluation.*