

Poverty, Land Degradation, and Rural Research Policy¹

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Abstract

Poverty and land degradation are surely interrelated but to go beyond simplistic generalities proves to be challenging, given the limited understanding of the relationships. Notwithstanding the inherent data and other difficulties, the following syllogism of relevance to work in the CGIAR (CG) is examined at the suggestion of CIAT: “The CG is concerned with poverty; the poor are located in ‘marginal’ lands; therefore the CG investment in agricultural research should be focused in ‘marginal’ areas in order to reduce poverty”. These and related syllogisms are found not to be helpful in informing rural research policy at this juncture, conforming with late-1990s efforts by the Technical Advisory Committee (TAC) of the CGIAR to define a framework to operationalize research relative to these concerns. In fact, research in a variety of settings over a reasonable length of time is needed for cogent research policy analysis to be based on deepened understanding of household decision-making processes relating to resource management and poverty situations, especially in terms of the links to land. Pending such improved information, it is concluded that consideration of degradation and marginality of land per se does not lead to unambiguous and valuable insight to better prioritization of agricultural research that is oriented to reducing poverty and improving food security.

Key words: marginal land, agricultural research, poor people

Introduction

Poverty has become the central concern of the CGIAR. Emphasis on lands of various types has not been a strong CGIAR tradition beyond what has occurred coincidentally in articulating the mandates of the Centers and the locations of their programs. With regard to agricultural lands, many of the terms used are inherently imprecise, and this essay in using them more or less interchangeably does little to reduce the confusion about what is meant by “marginal”, “fragile”, “less-favored”, etc. lands and areas. The essay, as noted in the Abstract, comes to positive conclusions about poverty targeting and negative about land quality in research prioritization.

This value-laden topic seems to bring out personal convictions, so some of mine are noted up front. My first work with Narpat Singh Jodha was in 1972, while we were teaching together at the Indian Agricultural Research Institute, New Delhi. I have admired his insightful works on the Indian Semi-Arid Tropics ever since, and we have occasionally had the chance to share our (including his increasingly mountainous) prejudices about agricultural research and “fragile lands” (e.g., Anderson and Jodha 1994). Many of the perspectives below reflect his work (e.g., Jodha 1998). As Jodha was a member of the TAC Panel that addressed these issues in 1996-97, as is evident from the Report (TAC 1997, hereafter TACML), and as I have an equally long friendship with the Chair of that Panel, Michael Nelson, it will not be too surprising that I have much sympathy for many of the findings, which bear directly on the present effort, and which will propel me to a similar, for some surely frustrating, conclusion.

An unrelated confession is the pleasure I have had in reading the collection edited by Leach and Mearns (1996a), which demolishes effectively many of the environmentally/politically/conventionally-scientific-correct myths about the degradation of African rural environments. An extension of these views (going from endowments to entitlements) is provided by Leach, Mearns and Scoones (1999). Perhaps no essay in the intriguing book collection is more interesting than that of Fairchild and Leach (1996), with the account of native-settler-induced forest generation in the forest-savanna areas of Guinea—a case of population pressure inducing “green” niche creation and nicely complementing the oft-told story of Machakos (Kenya), which is also revisited in this volume by Tiffen (1996).

Near finally in the confession department, is an expression of the fascination I have experienced in coming to terms with some of the recent efforts of other friends Peter Hazell (1998) and Shenggen Fan (e.g., 1998 and the 1997 and 1999 works cited below) in their analyses of variants of the Marginal Lands Hypothesis (Graham-Tomasi 1991) relative to investment in India.

Space or embarrassment should demand an end to overt introductory confessions. But there is one that is definitely personal but perhaps of wider relevance. In my own patchy farming endeavors, I have always been highly conscious of different forms of land degradation apparent to me. Yet my remedial actions (sometimes agonizingly slow) have been highly conditioned by the degree of personal financial illiquidity (a crude surrogate for poverty) that I have perceived at different times. It is thus that I have sustained a strong concern for the private profitability of farmers’ anti-degradation interventions, as is reflected, for instance, in Anderson and Thampapillai (1990).

TAC efforts concerning resource degradation and poverty

The Technical Advisory Committee (TAC) on behalf of the CGIAR System has clearly struggled long and hard with the various research issues surrounding poverty and marginal lands. I believe it concluded that existing data and a good deal of more subjective evidence suggested that further research was unlikely to confer general validity on the propositions that had thus far guided CGIAR decisions about how to deal with these issues. For example, TACML found, from examination of data on poverty incidence in countries with high and low proportions of marginal land, and available national case studies, that there was no evidence that poverty incidence is consistently greater in marginal lands than in lands with higher present use value. Even with a commodity-specific focus (maize), Heisey and Edmeades (1999, pp.13-16) were unable to detect useful associations among poverty, marginality of land, and degradation of the land resource. TACML also concluded that there was little evidence to support a view that the poor are the prime cause of resource degradation on marginal agricultural lands.

TACML ultimately came to the conclusion that the inconsistencies and lack of data on the underlying site-specific forces driving poverty processes were such as to invalidate their usefulness in guiding CGIAR strategy towards poverty alleviation on marginal

lands. Instead, TACML proposed the idea of a “marginal area” as a preferred unit of analysis for use by the CGIAR. The unit was defined by the presence of a high number and high incidence of poor people, who are subject to a relatively homogeneous set of conditions explaining why the poverty prevails, the set including institutions, policy, infrastructure, human capital, biophysical characteristics of land, etc.

TAC apparently accepted the Panel’s broad conclusion that the available evidence did not support a primary focus on marginal lands as an effective method of addressing the CGIAR goals of poverty alleviation and protecting the environment. There were two reasons why such a conclusion might be expected, especially in the light of existing knowledge of poverty. First, many of the poor in developing countries are landless or nearly so, even though presently most live in rural areas. A second reason why a primary focus on marginal lands, or indeed a focus on any attribute of land, may not be effective in addressing the CGIAR goal of poverty alleviation, is that a large number of other factors have been adduced to explain rural poverty (e.g., Jazairy, Alamgir and Panuccio 1992). Another pertinent view is that the importance of different causes of rural poverty varies greatly between nations, and within them. If this is true, then no single factor or indicator, such as marginality of the land resource, is likely to be generally valid for targeting poverty, although much progress is possible in poverty targeting per se, as is emerging from work in progress at IFPRI (Kerr and Kolavalli 1999).

TAC noted that a general lack of data, together with the evolution of TACML’s thinking away from a focus on marginal lands, prevented it from addressing the further request to “Make suggestions on future CGIAR priorities and strategies for research work on marginal lands, including whether the current level of effort is adequate in relation to that devoted to other land types”. Rather, it commissioned some further work on a more specific topic that TACML was unable to examine as fully as it had wished, namely the relationship between rural poverty and land degradation.

The lack of data had prevented TACML from making its hoped-for progress in examining potentials for research gains from inherently marginal lands, implying perhaps that the accounting units being proposed by TACML may be too knowledge-intensive to be operational at this stage. The questions of which types of land are likely to give the highest rates of return to further investment in international agricultural research, and of the optimal balance between the objectives of enhancing the productivity of land, of preventing its degradation, and of restoring already degraded land, remain important in determining the System’s future priorities and strategies, and would be addressed by TAC in that context. The poor in marginal areas in most cases are not poor only because of the biophysical quality of their land resources. Poverty cannot be addressed adequately if a perverse set of policies and institutions keeps people from reaching their potentials and effectively denies them taking advantage of opportunities to better themselves economically.

To try to advance the policy debate, TAC resolved that the CGIAR System should seek improved scientific evidence on (a) the extent and magnitude of the impacts of agriculture, forestry and fisheries on the degradation or enhancement of natural resources

and the consequences for production and food security; and (b) the linkage between poverty and observed resource degradation. Accordingly, in spite of being another friend, Sohail Malik (1998) was asked by TAC to review the latter field. In doing so he had the advantage of access to what is now Scherr (1999), along with earlier efforts at IFPRI (e.g., Scherr and Yadav 1995) to distill the available wisdom (but seemingly excluding Anderson and Thampapillai 1990), including the formidable assembly of conceptual and empirical material put together by Reardon and Vosti (1998).

Malik found that measurement, statistical and causality problems reduce the confidence that can be attached to extrapolations about poverty and land degradation. Evidence from the few available micro-level studies is mixed. The pressures that might induce people to degrade the land include those related to: increases in population; declines in common property resources; increases in interest rates; and lack of technology transfer. He argued that there is, however, considerable evidence that the response to population pressures and market forces is an endogenous process of adaptation towards sustainable behavior, a conclusion that Jodha would presumably agree with as the behavioral norm, although Jodha would also presumably express concern for how the adaptation can be assisted to deal with the unprecedented stresses emerging under globalization, climate change, and high population densities in many (often relatively marginal) areas that do not have such influences in their history. Let me explain these presumptions.

The essence of Jodha's (e.g., 1998) argument is that, in many traditional cases of rural resource management, farm and village families had a strong community stake in the resource base on which they have long been so heavily dependent, over which they had effective local control of their integrated management system, and of which they have had close functional knowledge of the subtleties of sustainable management (including coping with climatic variability). He argues that it is not poverty per se that leads to actions and decisions leading to resource degradation but rather externally generated changes to the managerial environment of the community. His positive spin on this is to use these insights (gleaned from observations in marginal lands of the semi-arid tropics and highlands) to point to remedial approaches to contemporary resource degradation interventions.

The implied actions thus include new mechanisms: to revive the community stake in the resource base (not easy in many countries where governments and NGOs have conspired to diminish such local ownership and responsibility, e.g., Ethiopia as detailed by Hoben (1996); improve the incentives for resource restoration, perhaps through developing niche opportunities for more profitable farm and non-farm enterprises (perhaps including food processing, which may also benefit from targeted international research); and last but surely not least (at least for the present discussion), using new knowledge from rural research to increase agricultural productivity and improve natural resource management, recommendations closely analogous to those of Pandey (1995) for Asian upland rice-based systems. There are naturally special considerations to be taken into account for the theme of new knowledge and resource-based techniques to have direct and targeted relevance for the poor, marginal lands and degraded rural resources, and to set a research agenda that will deliver, and at an appropriate intensity for what are usually neglected or

even orphan crop or livestock enterprises.³ In a way, this is one of the undeclared (but perhaps unanswerable without much more information of a rarely available kind) questions for this conference. Technological research *inter alia* should, according to Lapar, Pandey and Waibel (1999), be focused on innovations that not only reduce soil erosion but directly increase farmers' incomes, embedded in a greater understanding of the dynamics of rural change (in this case the uplands of Asia).

A syllogistic approach to “the problem” in the topic

Logical analysis may help clarify the nature of the topic at hand. The Workshop organizers suggested this first syllogism, based on the Marginal Lands Hypothesis, on the grounds of its oft-cited relevance to directing resource disposition in the CGIAR.

1. The Marginal Lands Syllogism: The CGIAR is concerned with poverty; the poor are located in ‘marginal’ lands; therefore the CGIAR investment in agricultural research should be focused in ‘marginal’ areas in order to reduce poverty.

The logical structure is fine, but the “middle” is demonstrably false, even with the contemporary skimpy data. Poverty analysis has far to go in many aspects, but there are many data to demonstrate that the rural poor are to be found in great numbers in many of the *more* favored agroecosystems (TACML, Malik 1998). It is thus literally not true that most of the rural poor reside in “marginal” areas, so a false middle makes the syllogism false.

The middle is “demonstrably false” in part, as Dana Dalrymple was quick to remind me, for another quite obvious reason: not all of the poor live in the countryside. They are also to be found, in substantial and increasing numbers (a majority by about 2015), in the urban areas, where they spend a large proportion of their income on food. The concern about marginal lands has been focused on the rural poor and will not do much for those who have already left the rural areas; there might, furthermore, well be some question about how much it might do to reduce this flow.

Perhaps alternative syllogistic frames may get more directly at the issue in syllogism 1. Two are offered. The first is an interpretation of a view often expressed in the development literature (e.g., Smith and Østreg (1999, p.4): “In developing countries many poor people are forced to exploit fragile land to secure their own subsistence. This often leads to land degradation, which renders the land infertile for future uses and forces people to move to other fragile land areas.” The second is a view less frequently expressed but is offered as a natural complement.

2. The Poverty-driven Degradation Syllogism: The rural poor degrade the land resource; research reduces poverty; therefore poverty-focused research reduces land degradation.

3. The Degradation-driven Impoverishment Syllogism: Degrading rural resources increase poverty of farmers; agricultural research contributes to reducing resource degradation; therefore resource-management focused research reduces poverty.

In the both of these it is the initial statement that does not sit well with the limited relevant information, so the logical arguments cannot be sustained as rationales for

CGIAR or even national research investment. In the former, the mentioned Jodha (1998) and Leach and Mearns (1996a) observations are that it is not the poor especially who degrade the lands on which they depend. In the second, while the statement may have validity in the longer term, farmers may sometimes do well out of degrading resources for a time if prices, policies and eventual new opportunities allow them or encourage them to take short planning horizons. TACML could not find evidence to support either one of the propositions embodied in syllogisms 2 and 3. Reardon and Vosti (1995) sensibly argue for a richer disaggregation of the real nature of both poverty and environmental attributes, causes, types, etc. to be able to reach worthwhile insights.

Perhaps the causal directions emphasized in these syllogistic representations do not help too much in rationalizing or directing the research agenda per se. A non-syllogistic form may capture the main thoughts:

0. General Idea of the Nexus: There is much poverty in marginal rural areas; resource degradation tends to be greatest in marginal areas; so, for both these good reasons, marginal lands cannot afford to be “left out” by bodies such as the CGIAR.

One of the Fan and Hazell (1999, p. 33) versions of this is: “investments in rural infrastructure, agricultural technology and human capital are now at least as productive in many rainfed areas as in irrigated areas [in India], and they have a much larger impact on poverty.” Further encouraging support for this proposition is provided by the recent analysis of Eastern Indian rice systems by Ballabh and Pandey (1999).

For yet further instance, Heath and Binswanger (1995), in a study of the Colombian situation, argue that rural poverty, inefficient resource allocation and natural resource degradation are *joint* phenomena, usually induced by a common nexus of policy failures, especially those directed to favoring large-scale farmers. Getting the policies right is the first step, and usually this will include appropriate research investment to fuel growth. While they may be joint phenomena, Pender, Scherr and Durón (1999) find in their recent valuable (IFPRI) work in Honduras that better resource management and poverty are different in their associations with the range of factors that cause poverty. Accordingly, they argue for a pluralistic approach that offers various pathways out of poverty and into better resource custody, some of which could be described as niche exploitation.

As Lipton and Ravallion (1995, section 5.2) note, agricultural growth, especially growth and stabilization of staple food production, is likely to benefit poor people. First, most of the poor, including the rural poor, are net food buyers. Ample local food supplies, even in open economies, carry special advantages for them, especially if international or national transport costs of staples are high. Second, while poor people’s entitlements to food (rather than local availability per se) determine their nutritional levels (Sen 1981), poor people’s rural exchange entitlements depend heavily on earnings from growing food. For such reasons, Ravallion and Datt (1996) are optimistic about the potential for pro-poor growth through well designed agricultural interventions, particularly (it must be presumed, a fortiori, by this observer) through poverty-oriented agricultural research.

Poor rural populations in many countries continue to increase. A major part of rural poverty policy, therefore, depends on higher productivity of land already owned, rented, worked or occasionally grazed by the rural poor. Fortunately, a growing body of evidence confirms that rural innovations tend to help the poor (though not generally to reduce inequality) by reducing food prices, raising demand for labor, and often stabilizing farm-specific output. The effects on poor farmers in non-adopting regions, however, may be less advantageous (Lipton with Longhurst 1989). Needless to say, the style and nature of the research undertaken on behalf of farmers operating in marginal areas will, as argued by Farrington and Mathema (1991) likely have profound influence on the types of innovation and their likely uptake by the poor.

Datt and Ravallion (1998) found that the effects of farm yield growth in rural India on various poverty measures, were strong. Using data spanning the period 1958-94, they found that higher real wages and higher farm yields reduced absolute poverty, and that the poor gained in absolute terms from lower relative prices of food. However, this effect was not evident in measures of relative poverty. The bulk of the gains to the poor from higher farm yields and higher real wages were via rising average living standards rather than improved distribution. The gains to the poor from higher average yields were not confined to those near the poverty line but reached deeper. They also found evidence of important indirect channels linking average farm productivity to living standards of the rural poor. There was evidence that real agricultural wages responded positively to higher farm yields, presumably through effects on labor demand, such as due to multiple cropping. There was also a strong link through food prices. While the impact of agricultural growth on food prices was quantitatively small, even small food price changes have large effects on absolute poverty.

How targeting poverty might help

Various frameworks can be invoked to explore poverty targeting. The DFID Poverty Aim Marker scheme, with its intrinsic emphasis on being locally enabling, inclusive, and focused on the key components of rural livelihoods, is one rich and general if yet untested approach (Cox, Farrington and Gilling 1998). The technology represented by GIS and other aids to spatial analysis certainly will be important in trying to do some of this work in practice (e.g., Wood et al. 1999, and Wood in this Conference session). An oblique reference can also be made here to the conference presentation of Byerlee (1999). One liberal interpretation of his framework is that poverty-impact-oriented research is what might be styled as a “4C” multiplicative relationship of:

Cache = magnitude of benefits relative to costs of research, times

Cogency = poverty incidence in a target group relative to the whole domain, times

Commitment = dependence of the poor population on agriculture, and times

Capture = research impact for a target group relative to the whole domain.

Clearly, this is an information-intensive C-set. In Byerlee's numerical explorations for his favorite country Pakistan, he is surprised at the robustness of the implications for research planning to alternative assumptions about the Cogency issue of poverty, and the marginality of the land resource (in this case using Provinces as a surrogate). Once again,

the bluntness of research as an instrument for poverty alleviation (Alston, Norton and Pardey 1995) is indicated.

By way of seeking another prism to examine this issue, let us examine a simplified two-domain model of research allocation. The reality of crop yields being influenced by research is well established in a large literature, interestingly distilled by Alston and Pardey (1996), and there is growing evidence that increasing crop yields reduce poverty (e.g., Ravallion and Datt (1999)). From the same “data-rich laboratory” that is India also comes the good news that such poverty alleviation impact of research works across agroecosystems of widely varying agricultural potential, even to the point where Fan and Hazell (1999) argue that the situation now (perhaps reflecting diminishing marginal returns in the more favored areas) is such that returns to research investment in the more marginal areas are greater in economic impact and poverty alleviation impact than in the areas more favored by both Mother Nature *and* (it is presumed by this observer)⁴ past research resource allocators. This view resonates with the observations of Heisey and Edmeades (1999, pp.32-33) in their exploration of how to bring poverty targeting to maize research. Although there are some non-comparabilities, the Fan-Hazell implication does not, however, accord with the Ravallion and Datt (1999) results, which show that the poverty reduction effects of increasing yields are less in those States with lower initial farm productivity. It possible that findings similar to those of Fan and Hazell might eventually emerge from the other notable data-rich laboratory, China, but that analysis has yet to be done.

Consider a two-domain setup with the transparently simplified features as follows:

The change (D) in poverty (P_i for region i) is conceived as a linear and additive function:

$$DP_i = a_i.DA_i + b_i.DB_i, \quad (1)$$

where a and b are the respective multipliers for agricultural income A and non-agriculture B , and let $i = 1$ for the favored region, and $= 2$ for the unfavored/marginal region.

Changed agricultural income arises from a surplus change driven by a supply-shifting K :

$$DA_i = p_i.Q_i.K_i. \quad (2)$$

where p and Q are the respective prices (assumed for simplicity to be unaffected by the research induced shifts) and quantities, and the crucial K_i s depend, in turn, on direct and spill-in efficiency effects of research impacts, namely E_i and E_j , construed as ratios of research benefits to costs, e.g.,

$$K_i = k_{ii}.E_i + k_{ij}.E_j. \quad (3)$$

To parametrize this scheme for the two regions, consider now the following indicative numerical values for purposes of simple illustration (Table 1).

[Table 1 near here]

These pseudo-data indicate, in back-of-the-envelope form, some alternate pathways and some of the relative possibilities of research-based impacts on poverty. One can readily modify the numbers to see the varying impact of changed assumptions. In so doing it is clear that the overall impact of research on poverty in this setup does indeed depend on several key local parameters, implying that any “answers” to the general question of poverty targeting are, as we all knew, highly knowledge-intensive.

And so it is crudely illustrated that the impact of research in alleviating poverty is a starkly empirical issue, depending on much local context regarding research opportunity and poverty incidence. Adding on overlay of environmental quality to the analysis would surely further emphasize this general finding, although given the relative lack of knowledge about research opportunity in the resource-management field, overall uncertainty may be increased. Any sensible system of giving a strong poverty orientation to agricultural research prioritization is bound to be highly information demanding. As Cox, Farrington and Gilling (1998) set out, screening of research proposals will require careful crafting, and reaching priorities across diverse agroecological and country boundaries will demand much research-opportunity and poverty-incidence information that is presently scarce. Mike Nelson tells me he thinks this is a key issue for setting research priorities, namely determining the “information threshold” for coming to grips with site specificity—perhaps even more critical in dealing with environment issues than for poverty. One of the perhaps more readily answered specific cases may be in strategizing for soil-fertility research in Africa (Scoones and Toulmin 1999, especially part 5.3), although the answer may well be “do little” in many instances. And dealing appropriately with spill-in and spill-out issues across such boundaries (in the manner conceptualized by Cox, Farrington and Gilling 1998) will be especially difficult under the existing knowledge situation. All this may add to the challenge and interest of research resource allocators but will surely frustrate big-picture policy makers who would wish to shift CGIAR and other research resources in ways that overtly meet their imperative new agendas.

IFPRI organized an international conference in May 1998 that addressed these research policy issues in a highly consultative manner (Anon. 1998). A report on the meeting (Anon. 1998, p.1) opens with an outstanding example of a “development narrative” that Leach and Mearns (1996b) would decry as representative of conventional-science and donor-adopted perspective, but narrow and shallow in its generalization and insight. It may thus be worth repeating:

“Land degradation is advancing at an alarming rate in Sub-Saharan Africa, particularly desertification in dryland areas, soil erosion and deforestation in hillside areas, and loss of soil fertility in many cropped areas. The degradation of fragile lands is particularly worrisome because it is often irreversible or can only be reversed at high cost. ...[T]he most important factor ... is human activity. Most experts believe that the key driving force is at the nexus of poverty, rapid population growth, and inadequate progress in increasing crop yields.”

The poor are clearly implicated, as is their reproductive tendency. Agricultural science comes in for implicit criticism too. The report goes on to arrive at a set of recommendations that constitute rather generic development themes, closely analogous to the contemporary World Bank rural development strategy (World Bank 1997), with the exception that poverty reduction per se is, surprisingly, not mentioned. The CGIAR System can and must do better than this. Indeed, IFPRI for just one Center has done so, as suggested in the several mentions above. Targeting resource degradation and marginal/fragile lands per se does not seem too useful, so this observer is inclined to side with TACML. Going for the biggest research-harvest pie, as would Alston, Norton and

Pardey (1995), still seems like cogent advice providing that wise concern is simultaneously given to dividing the pie. Overlaying poverty considerations is understandable and reasonable, given the centrality of this System objective, and as Kerr and Kolavalli are showing, but the procedures and data ideals are yet to be refined.

Conclusion

In conclusion to these diverse reflections, I concur with Hazell and Fan (1998) that the needed successful agricultural intensification of less-favored lands used by many poor people will require new approaches, new partnerships, new information sets and definitely time and innovation with much on-line evaluation, in order to deal with the measurement challenges and the noise that comes with risky climates and markets. Clearly, agricultural research will be an important player in the innovatory developments that will lead to reduced poverty and improved natural resource management. Given the nature of the research challenge, however, there will be a need for particular patience on the part of investors in such research. High natural variability of many such environments also means that progress will be less than immediately clear and unambiguous in many instances. In an era of donor fatigue (Anderson 1998), such qualities may be difficult to find unless there is a carefully orchestrated educational program to sensitize investors about the nature of the new challenges they have set with their choice of agenda.

For cogent *research policy analysis*, research in a variety of settings over a reasonable length of time will be needed in order to achieve a deepened understanding of household decision-making processes relating to resource management and poverty situations, especially in terms of the links to land and its quality (e.g., Reardon and Vosti 1995). Pending such improved information, and it seems that the situation has not really changed much since the Graham-Tomasi (1991) analysis, it is thus concluded here that consideration of degradation and marginality of land per se does not lead to unambiguous and valuable insight to better prioritization of agricultural research that is oriented to reducing poverty and improving food security. There will be global implications of research oriented to poverty and the environment too, although these have not been brought out above (El-Swaify 1999 has a great synthesis). Other pertinent aspects dodged in this short review include the differential impacts on poverty of changing technologies within households (Lawrence, Sanders and Ramaswamy 1999), and within heterogeneous communities (Baland and Platteau 1996).

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Table 1: Indicative poverty impacts of agricultural research in two regions.

Parameter	Region 1	Region 2	Comment
E	10	5	Greater research opportunity in favored
k1	0.2	0.02	Similar cost reductions
k2	0.01	0.2	but greater cross effect from marginal
K	2.1	1.1	Unit impact larger in favored
Q	100	200	Larger marginal domain
p	10	8	Marginal price disadvantage
DA	2100	1760	Favored gets larger income change
B	2000	1000	Non-farm lower in marginal [assume DB zero]
a	0.002	0.003	These critical multipliers relate implicitly to
B	0.004	0.005	poverty incidence and circumstances
-DP%	4.10	5.28	So this example favors the marginal.

Endnotes: (as per *Food Policy* format)

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³ Derek Byerlee cautions me that such suggestions must recognize that the research impact on production costs and supply shifts (K) must be sufficiently large, or the “most appropriate” intensity will still be zero.

⁴ Derek Byerlee advises me that there is no evidence of underinvestment for the marginal lands of India, and he believes the reverse is the case for wheat.