

# World Bank Research Evaluation: Agriculture and Rural Development

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I will organize my comments along the lines of the questions provided in Annex 2. Detailed comments on the individual research projects follow.

### 1. In your area of expertise, has the Bank made a significant contribution?

My expertise is on the microeconomics of economic development in Africa. Bank research has undoubtedly made a significant contribution to this area, but at the same time has missed important opportunities.

One of the major contributions that the Bank has made has been through the collection and dissemination of data. The most successful of these efforts has been the set of LSMS household surveys. These data have been used within and outside the Bank to discover and analyze numerous features of the economic environment. Many of these discoveries have been closely linked to the original poverty-monitoring purposes of the surveys, but others range more broadly over a large terrain of questions of concern to the process of development. In addition, there has been important institution-building associated with the LSMS process. For example, the Ghana Statistical Service is now in the field with Ghana Living Standards Survey 5.

A number of substantive areas of research in Africa have seen significant contributions from the Bank. In particular, I am familiar with recent important research on: civil war and post-conflict transitions; health in general and HIV/AIDS in particular; gender dimensions of development and household organization; risk, insurance and household responses to crises; education and child labor; corruption; and land rights.

There are two missed opportunities that seem most apparent to me. The first is the lack of sustained attention to long-term, systematic data collection. The LSMS program, of course, is a partial and welcome exception. However, the Bank is in a unique position that could be leveraged to make extraordinary progress. It has the institutional stability and established global presence to support countries to achieve monitoring capabilities akin to those of the NSO in India. Data collected on a broad range of activities by households and firms over long periods of time (either in the form of panels or repeated cross-sections) that permit surprising and *a priori* unpredictable connections to be drawn are particularly needed. The process of economic development involves multiple transformations of people's economic status, health, political participation, property rights, and built and natural environments. Long-term, consistent, thoughtful and broadly conceived data collection efforts are fundamental to increasing our understanding of these phenomena. The Bank has a critical role to play in the creation of these global public goods.

The second is the apparent separation between "operations" and "research." I cannot comment on the use of Bank research in ongoing operations. However, it appears

that Bank programs and projects offer unexploited research opportunities. Most obviously, this could come in the form of insights to be gained from researchers participating in the design of certain projects. I am confident that several of the other reviewers will be suggesting substantial increases in the use of randomized design in Bank projects, and I am fully supportive. More generally, consultation and collaboration with researchers during project design, coupled with the early opportunity for appropriate data collection could open up broad new insights into development processes.

2. In your area of expertise, has Bank research focused on the most important policy issues for developing countries? Do you feel Bank researchers have appropriate incentives to allow identification of the most important issues?

Much of the research has focused on important policy issues for African countries. In fact, it is difficult to think of examples of Bank research on Africa that does not have a reasonably strong connection to important policy questions. It is easier to find examples of important policy questions with an insufficient quantity of high quality research from the Bank. Here are five examples, but they could be multiplied:

- a. Program evaluation and public service delivery. Many specific examples could be cited. For instance, it is still the case that I do not know of a single reliable large-scale evaluation of the impact of a microfinance program in Sub-Saharan Africa. Nor is there sufficient work on the effectiveness of rural health care delivery systems. The Bank has a responsibility to lead in this area.
- b. Institutional innovation. There is some evidence from cross-country studies of the importance of a set of particular institutions for patterns of growth and poverty reduction. However, for well-known reasons this evidence is only indicative and is not informative about specific institutions or the mechanisms through which they affect patterns of development. For example, we have only spotty knowledge about how systems of land tenure influence agricultural growth or income distribution.
- c. Infrastructure. This is a difficult area to research, partially because of the long time period that must be examined. The involvement of the Bank in planning and advising on infrastructure investments and its potential ability to make long-term commitments make it uniquely suited for research on the relationships between infrastructure improvements and changes in economic activity.
- d. Firm dynamics. I discuss below the inadequate contribution of Bank research to knowledge concerning manufacturing development in Africa.

- e. Nonfarm enterprises and rural diversification. There is evidence of an important and little-understood set of interactions between household organization, migration, specialization in activities by gender, investments in nonfarm enterprises and the growth of nonfarm employment in many parts of rural Africa. Diversification out of agriculture in low-potential areas is a key strategy for many countries, but there is little work on how this process takes place.
3. In your area of expertise, has Bank research reflected awareness of substantive knowledge from other research available and in progress and sufficient knowledge of subject countries?

Most of the research I am familiar with does reflect adequate knowledge of both current research and the subject countries. However, it remains the case that most of the best Bank research that I know is drawn from South and East Asia. I think that Bank research in those regions has benefited tremendously from the deep familiarity of many researchers with institutional arrangements and social conditions in those countries. Work in SSA has suffered from the relatively smaller number of researchers who have had a long-term focus on and familiarity with the continent.

4. Has the Bank made appropriate selection of topics for data collection and surveys? Have data and surveys been well-maintained and disseminated?

The short answer here is simply no. Some data and surveys are well-justified, superbly executed, well-maintained and disseminated. The LSMS program, of course, has had some challenges and problems, but nevertheless can serve as a model for the development of a scientific infrastructure of research on development.

Too often, however, data collection efforts are hurried, fitful, abandoned, hidden, too narrow, and casual. For example, despite the original promise of the RPED panel surveys of manufacturing firms, the Bank has not been able to maintain an open, well-defined data collection program for understanding firm dynamics in Africa (partner institutions outside the Bank have stepped up to fill some of these gaps). There has not been a systematic policy of making data collected in Bank research projects available to researchers outside the Bank. On numerous occasions (I can recall four specifically) I or my Ph.D. students have been unable to obtain data that had been used in Bank research. This is an area of research infrastructure in which the Bank should be the global leader. Instead, it lags.

5. In your area of expertise, how could future Bank research better serve developing country objectives?

The two suggestions I make in point (1) are the most important. These are areas in which the Bank can and should excel.

6. Particularly for ongoing projects that you have evaluated, please comment on the proposal review, revision and selection process.

I evaluated only one ongoing project (126 – Risk and Insurance). This project is relatively narrow, well-timed, and focused on the important policy objective of evaluating the potential of weather-index insurance for helping farmers deal with risk. It might have been possible for the project to have moved more quickly into the pilot marketing phase. In most cases, data limitations implied that it was not possible to reach firm conclusions about the extent of basis risk. A set of trials with appropriate evaluation with a number of different products was the only reliable guide to the usefulness of the products.

7. What is your overall assessment of Bank research?

The projects I reviewed were extremely heterogeneous. Only one of the projects made a substantial contribution to new knowledge in development economics (127 – Community Driven Development).

Another project is particularly illustrative of the tensions that must be commonplace for Bank researchers. In project 162 (tariffs and subsidies), compromises were made in order to obtain timely estimates of the relative importance of developed countries' agricultural subsidies and tariffs for developing country welfare. These estimates were necessary for ongoing trade negotiations, and good judgment was used in choosing appropriate compromises. Nevertheless, the compromises call into question the validity of the resulting estimates. This is good and necessary policy research, but it is not reliable scientific evidence.

Two other projects are flagship reports (on gender, and on agriculture). They are synthetic reviews, with no substantial attempt to do new research. One of these two succeeded well in its goal of providing a rich and useful overview of the state of knowledge of gender dimensions of economic policy in developing countries.

A final two projects (129 – Weather & Price Insurance and 126 – Risk and Insurance) are concerned with a relatively narrow and well-focused question regarding the usefulness of new forms of weather-based index insurance products for dealing with agrarian risk. Neither makes an important methodological contribution, nor is there a rigorous empirical investigation of the impacts of these products on farmer choices or welfare. Nevertheless, this is clearly a very useful sequence of projects for familiarizing operational staff with the principles of index insurance, developing proposals for the basic form of index insurance products, and organizing pilot marketing of proposed products. This is the set of projects that most closely approximates what one would expect to receive from an extremely high caliber consulting firm.

Looking beyond the specific projects that I assessed, it is clear that Bank research has made important contributions to our knowledge of developing economies. At the

highest scientific level, there are a number of specific advances (in understanding poverty dynamics, risk, certain aspects of health and education, firm organization in the transition from socialism) that can be attributed to Bank research. There is a substantial body of data collected under Bank auspices that forms an infrastructure for scientific research. And it is apparent that a great deal of Bank research has contributed directly to the programmatic needs of operational work.

However, there is also a great sense of missed opportunities. First, Bank research has not taken full advantage of the opportunities of being part of the Bank. That is, we have not seen enough research that uses the interventions sponsored by the Bank to gain insight into the economic organization in poor countries. Second, the Bank has not used its institutional strength to support long-term, systematic, broad based data collection efforts.

## **Evaluation of Individual Projects**

### **129 – Weather and Price Insurance**

#### **A. Objectives**

1. What specific questions or hypotheses did the study seek to answer?

This small project plays the important role of summarizing a host of earlier work on weather and price insurance, and describing the initial experience of pilot projects for dealing with agricultural risk in developing countries.

2. Are the topics and objectives critical for policy in the developing or post-socialist country?

This is research that is directly relevant to an important dimension of economic and social policy.

#### **B. Design and Implementation**

1. Was the methodology appropriate and well-executed? Was it innovative?

This is an extremely useful and accessible review of the theory of risk in agriculture and of innovations in index insurance, along with evidence from pilot projects. The reviews are thorough and well-considered, and this is a very timely presentation of this evidence as pilot projects for index insurance projects spread. The organizing framework is provided by the notion of the life cycle associated with innovation in institutions. Pilot projects for index insurance are just now entering the replications phase, so it is essential to learn from the difficulties (Nicaragua, Morocco) as well as the apparent successes (India).

The work places discussion of government and World Bank actions on weather insurance products into the appropriate context of market failures. What's wrong? Why might there be a need for policy? Two key issues are identified: "cognitive failure" and "expectations of government or donor disaster relief." The second is a straightforward (although obviously not easily dealt with) commitment problem. The first is not well-documented in the work of this project, so it remains a weak element in the chain of reasoning that justifies subsidies for index insurance projects.

2. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

Absolutely.

3. How reliable were the data? If appropriate, were surveys properly designed and executed? Were data compiled properly from appropriate sources and aggregation? Were data limitations considered in the analysis?

Data were not collected specifically for this project. The review appropriately emphasizes the importance of impact evaluation at this stage of the overall work.

4. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The conclusions are thoughtful and well-justified. The most interesting aspect of the work is the discussion of the successes and failures of specific pilot projects, in which careful attention is paid to anomalies and difficulties in implementation.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

Yes, for the main paper. The short progress report may have been too technical.

2. If applicable, are policy recommendations commensurate with findings?

Yes.

### **D. Results**

1. What are the key findings of the study? How do they advance country policy in the field?
  - a. Developed country approaches to agricultural risk are a poor guide for developing countries.
  - b. Index insurance provides a potentially valuable instrument for mitigating the consequences of agricultural risk at low administrative cost, and is less subject to problems associated with asymmetric information than many other forms of insurance. The most important challenges in developing index insurance markets are minimizing basis risk, improving insurers' and consumers' knowledge of the time-series properties of the index, and dealing with the moral hazard associated with donor or government disaster relief.

- c. The conceptual foundations of risk management in agriculture are reasonably well-understood. It is time to move toward extensive piloting of weather and price insurance products, designed for local conditions in specific countries. The project includes a very useful review of initial experience along these lines.

**Research Quality Indicator Form – 129 Hess**

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed		X			
Clarity of the project focus and stated objectives		X			
<b>Analysis</b>					
Theoretical/conceptual framework		X			
Empirical application			X		
Statistical and econometric methods			X		
Use of existing knowledge and resources			X		
<b>Data</b>					
Awareness of other data sources			X		
Data compilation, cleaning and cataloging				X	
Survey design and sampling					
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations		X			
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations	X				
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate					
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy		X			
<b>Actual or likely impact of research on:</b>					
Government policy		X			
Future analysis	X				
The development community in general				X	
<b>Overall Quality of Research</b>		X			

### A. Objectives

1. What specific questions or hypotheses did the study seek to answer?

What is the relative effect on average developing country welfare of a 50% cut in developed country agricultural subsidies (both domestic and export) versus a 50% cut in protection (tariffs and nontariff barriers)? Are these effects homogenous across developing countries?

2. Are the topics and objectives critical for policy in the developing or post-socialist country?

Absolutely. These results, if correct, provide important guidance for developing country negotiations on distortions in agricultural trade.

### B. Design and Implementation

1. Was the methodology appropriate and well-executed? Was it innovative?

The task of estimating the impact of trade distortions on welfare in developing countries is truly daunting. The authors have made good progress by making appropriate, if dramatic, simplifying assumptions. The most important of these is the decision to cast the work entirely in a partial equilibrium framework. As a consequence, none of the general equilibrium effects associated with the resource reallocations contingent upon trade reform are addressed in the work. The benefit is that the problem becomes potentially tractable. The partial equilibrium theoretical framework is precisely and sensibly specified.

The second important simplification is the decision to use cross-sectional data only. The consequence of this assumption is that the relevant elasticities are identified only from variation across countries in subsidies, tariffs and transportation costs. This makes it much more difficult to address the significant omitted variables problems that are likely to lurk in the background. The benefit is that by doing so, units can be chosen for each commodity so that the world price can be normalized to unity, and the very complicated problems associated with dealing with unit-values as a proxy for price can be avoided.

There are of course a host of other assumptions that are required to obtain estimates of the impact of changes in trade distortions on developing country welfare. These, however, are the two most notable.

2. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

The paper largely reflects adequate awareness of the existing literature, with an important exception noted below. The paper does not require deep understanding of specific countries.

3. How reliable were the data? If appropriate, were surveys properly designed and executed? Were data compiled properly from appropriate sources and aggregation? Were data limitations considered in the analysis?

A wide range of existing data was compiled to execute the paper. This seems to have been done carefully. Unfortunately, some serious data limitations do affect the paper. Data on trade distortions are notoriously difficult and potentially misleading; this paper seems to be among the best at handling these issues. Trade preferences are entirely ignored. Price data, of course, is not available. Nor are transportation costs (although other authors have made significant efforts to deal with this). Domestic production and consumption are not observed at the same level of commodity detail as the trade statistics permit. These limitations are properly noted.

4. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The conclusions accord well with the research findings. However, they are stated with somewhat more certainty than I think is justified. The most important difficulty is that the import/export elasticities are identified off of the cross-country correlations between the value of net imports and measures of trade distortions (equation 7). There is a rich literature in the political economy of trade that explores the determinants of some of these distortions. The conclusion is that we can expect the magnitude of the policy-induced distortions on trade to depend on characteristics of the country that will also affect its net imports of particular commodities. This renders the estimated elasticities suspect. For example, the key conclusion is that border protection is a far greater source of welfare loss to developing countries than are domestic subsidies. This conclusion is based in part on the finding that the elasticities associated with domestic subsidies are estimated to be very small. If domestic support tends to be larger in countries that (conditionally) import more, this estimated elasticity is biased down, leading to potentially misleading conclusions.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

This is a difficult paper. I don't think that it could have been simplified without obfuscating the procedure that was used. However, the economists who advise policymakers on trade negotiations should not have difficulty in reading the work.

2. If applicable, are policy recommendations commensurate with findings?

Yes.

#### **D. Results and Cost-effectiveness**

1. What are the key findings of the study? How do they advance country policy in the field?

The most important findings of the study are

- a. Some simple descriptive findings about distortions. For example, subsidies are highly concentrated among a relatively small number of products. They are most commonly used by developed countries. Tariffs are concentrated on commodities that are also subsidized.
- b. Reductions in domestic support and export subsidies have very little average impact on exports, imports, or welfare of developing countries. In contrast, tariff cuts have much larger effects.
- c. There is substantial heterogeneity across countries in these effects. The effects are much smaller for the least developed countries.

**Research Quality Indicator Form – 162 DEC Highlighted**

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed		X			
Clarity of the project focus and stated objectives	X				
<b>Analysis</b>					
Theoretical/conceptual framework		X			
Empirical application			X		
Statistical and econometric methods				X	
Use of existing knowledge and resources			X		
<b>Data</b>					
Awareness of other data sources		X			
Data compilation, cleaning and cataloging		X			
Survey design and sampling					
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations		X			
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate					
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy			X		
<b>Actual or likely impact of research on:</b>					
Government policy		X			
Future analysis				X	
The development community in general			X		
<b>Overall Quality of Research</b>			X		

## 126 - Risk and Insurance

### A. Objectives

1. What specific questions or hypotheses did the study seek to answer?

The goal of this project is to evaluate the extent to which weather-based index-insurance products can be used to help farmers in developing countries. This is an extremely well-defined and tightly focused research project.

2. Are the topics and objectives critical for policy in the developing or post-socialist country?

While narrow, the question has immediate policy relevance, and careful thought along with sensible theoretical and empirical analysis can contribute to in a straightforward way to the construction of appropriate policy.

### B. Design and Implementation

1. Was the methodology appropriate and well-executed? Was it innovative?

The methodology of the five country reports that I read was entirely appropriate for the first stages of this research project. The studies vary in quality and thoroughness. By a large measure, the Mexico study is the most serious and careful of the group. The policy is somewhat innovative. The idea has long been floated that weather-based index-insurance products could avoid the terrible moral hazard problems and extremely high transaction costs that have afflicted traditional crop insurance schemes. A number of questions arise, though, regarding the feasibility and value of such products.

- a. What is the feasibility of designing weather-based index-insurance products to minimize transaction costs and that will be close to actuarially fair? This requires data on historical patterns of weather and farm profits to construct and price possible products that will minimize basis risk. We also need to examine the time-series properties of these products, and the timing of revelation of information about the proposed products. The latter was not done at all. Administrative information on country-specific transaction costs of similar products is also required.
- b. How useful would such products be? The main methodology of the reports was to look for correlations between proposed products and farm yields. This is useful but very preliminary and tentative: yields are not equivalent to profits.

The research methodology is not innovative at all. There are no methodological advances, nor will there be substantial spillovers to other kinds of research. A partial exception is provided by the Mexico study, which contains interesting and surprising results on the success of FONDEN local crop insurance.

2. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

Yes. The project is largely concerned with the institutional details of designing and implementing index-based weather insurance in the subject countries. There is close attention to the country context.

3. How reliable were the data? If appropriate, were surveys properly designed and executed? Were data compiled properly from appropriate sources and aggregation? Were data limitations considered in the analysis?

No surveys were used. Data reliability varied widely, from very crude farm budgets, gross figures on agricultural yields, inadequate data on transaction costs, and some reasonable data on historical weather realizations.

4. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The key conclusion was that there is sufficient evidence regarding the potential usefulness of these kinds of insurance products that it is time to move toward experimentation and test marketing. I fully agree, and the India study provides initial steps toward this.

I was intrigued by the comment in the Ethiopia study that for index insurance products, “distribution costs, if similar to current arrangements at the lottery board, would be prohibitively expensive.” This is quite important and depressing – if the lottery board can’t get lottery cards to farmers at a cost that is not prohibitive, why should we even begin to think that another organization can get a more complex product distributed cheaply?

Only the Mexico study addressed an important concern, which is the timing of the revelation of information. In that case, “farmers are able to still buy insurance even when it is clear that rainfall will be poor.” Of course it is not trivial to determine pricing of weather derivatives, because that will depend on forecasting abilities. If prices are determined at some point in advance of the growing season, it will be necessary to stop sales before there are major changes in the information relevant for forecasting the index.

Two apparent problems/concerns were not addressed at all. These may be second order, but they strike me as worth considering when products are designed. First, if product and labor markets are imperfect, then there are general equilibrium effects on, for example, local food prices of the introduction of weather insurance to a subset of the local population. This could make the subset of the population that does not have access to the weather insurance worse off. Second, part of what makes informal insurance arrangements sustainable is the cost imposed on those who fail to meet their informal obligations. If this cost is mitigated by the availability of weather insurance, then spatially dispersed informal insurance may become less effective.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

Yes, there is very close attention to the mechanisms that can be used to develop these markets.

2. If applicable, are policy recommendations commensurate with findings?

Yes. The main policy recommendation is that there should be increased investment in trial marketing of weather index insurance. This is an entirely appropriate conclusion, for it is only by pilot marketing efforts that the impact of these products can be judged.

### **D. Results and Cost-effectiveness**

1. What are the key findings of the study? How do they advance country policy in the field?

The general finding is that there is a sufficiently high correlation between simple weather indices and yields to justify pilot tests of index insurance. This is a direct and appropriate policy recommendation. The studies provide good guidance toward the development of simple rainfall index products.

**Research Quality Indicator Form – 126 Risk & Insurance**

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed		X			
Clarity of the project focus and stated objectives		X			
<b>Analysis</b>					
Theoretical/conceptual framework			X		
Empirical application			X		
Statistical and econometric methods				X	
Use of existing knowledge and resources			X		
<b>Data</b>					
Awareness of other data sources			X		
Data compilation, cleaning and cataloging				X	
Survey design and sampling					
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)	<b>Widely varied across papers</b>				
Clarity of conclusions and recommendations		X			
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate					
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy		X			
<b>Actual or likely impact of research on:</b>					
Government policy		X			
Future analysis	X				
The development community in general				X	
<b>Overall Quality of Research</b>			X		

## **127: Community Driven Development: Nonfarm employment**

### **A. Objectives**

1. What specific questions or hypotheses did the study seek to answer?

This is a very diverse project. One component examined community driven development (CDD), the other nonfarm employment. Specific questions include: Have CDD programs been subject to rigorous evaluation? If so, how does their effectiveness compare with expert-driven projects? How can we understand the interactions between the CDD process and local conditions? How has the KDP project affected local conflict in Indonesia? What general lessons from the KDP experience can be applied to other CDD projects? On nonfarm employment, what is the relative contribution of rural industrialization and agricultural technological change to rural development in India?

2. Are the topics and objectives critical for policy in the developing or post-socialist country?

In both cases, these are essential questions.

### **B. Design and Implementation**

1. Was the methodology appropriate and well-executed? Was it innovative?

In each case, the answer is at least a qualified yes.

I read five outputs from the project. The first is a speculative paper by Rao that is extremely thought-provoking. He reaches across disciplines and across vocabularies more effectively than almost any other economist I know. Several crucial theoretical concepts are integrated into a common framework and then related to insightful case studies of CDD in India and Indonesia. The Mansuri/Rao paper is a very useful review of the literature on CDD. The Barron et al paper is an extensive and eclectic study of the relationship between the Kecamatan Development Project (KDP) and local level conflict in Indonesia. The project relied on a combination of quantitative and qualitative research in 41 villages in two provinces where the KDP was active. Villages were selected purposively for analysis, "based on the location of 'interesting' conflict cases identified in the qualitative research." Individual informants were selected to involve a wide cross-section of the population, with emphasis on people knowledgeable about the specific conflicts in each village. This is innovative and well-executed. The two papers by Foster and Rosenzweig rely on excellent panel data of Indian villages, and use a combination of descriptive statistics and appropriate econometrics to address the key question of the role of rural industrialization in India's rural development in a general equilibrium context.

2. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

In both cases the answer is yes. The papers are characterized by careful attention to local context, and thorough familiarity with existing work.

3. How reliable were the data? If appropriate, were surveys properly designed and executed? Were data compiled properly from appropriate sources and aggregation? Were data limitations considered in the analysis?

Data are used in the KDP project and in the Foster/Rosenzweig papers. In each case the data are rich and well-suited to the project. The KDP evaluation really takes place at the village level, so the non-random selection of individual respondents is not troubling. However, the non-random selection of treatment and control villages IS a problem. As a consequence, I find the qualitative analysis of that project much more compelling than the quantitative aspects.

4. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The overarching conclusion of the CDD component of the project is that the CDD process and the success of CDD projects are highly context specific. They depend on the existing social structure, on the implementation of the project, and on the local administrative, political and economic environment. This conclusion is quite consistent with the weak quantitative evidence presented in the project, and with the much stronger qualitative evidence from the KDP project. The main conclusion of the nonfarm employment component of the project is that rural industrialization in India has been much faster in areas less strongly affected by the green revolution technologies, and that this growth in nonfarm employment has had a very important (indeed, disproportionate) impact on rural poverty.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

Yes.

2. If applicable, are policy recommendations commensurate with findings?

Yes.

## D. Results and Cost-effectiveness

1. What are the key findings of the study? How do they advance country policy in the field?

The key findings of the CDD component include:

- a. Decentralized targeting is not always effective, especially of the poor within communities.
- b. There is some evidence that CDD improves project performance. The strongest evidence we have comes from the qualitative findings of the KDP study. KDP related conflicts almost never became violent (although other conflicts commonly did). The most serious conflicts directly related to KDP were based on perceptions of corruption in the project. The general lack of conflict is attributed by the authors to the community-based design of the individual projects, and to the mechanisms built into KDP for conflict resolution. Another possibility is that the community-based design means that KDP projects tend to reinforce rather than transform existing social relations.
- c. No studies look at elite capture with appropriate counterfactuals. We just don't know.
- d. There is little reliable evidence that CDD increases communities' capacity for collective action. The best evidence again comes from the KDP qualitative work, with the conclusion that it "creat[es] a positive precedent, in the process helping to stimulate demand for changes in the ways in which local decision-making and conflict resolution operate." However, they acknowledge that their quantitative evidence is not supportive, and there is certainly no appropriate control group on which to base this conclusion.
- e. CDD effectiveness and sustainability are much more likely in more cohesive communities and in an enabling institutional environment. Close monitoring and learning by doing are essential. As a consequence, scaling up is far from trivial.

The relevance of these findings for development policy is obvious. For the nonfarm employment component, the key conclusions are that

- a. Rural industrialization in India has been much faster in areas less strongly affected by the green revolution technologies.
- b. Growth in nonfarm employment has had a very important (indeed, disproportionate) impact on rural poverty.
- c. Growth in demand for output of nonfarm workers, and growth in agricultural productivity raises wages, but the former has been more important.

These findings suggest that increasing the mobility of nonfarm capital (and output of nonfarm enterprises) is a very important complement to improvements in agricultural productivity for reducing poverty.

**Research Quality Indicator Form – 127 CDD**

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed		X			
Clarity of the project focus and stated objectives				X	
<b>Analysis</b>					
Theoretical/conceptual framework		X			
Empirical application		X			
Statistical and econometric methods	X		X		
Use of existing knowledge and resources	X				
<b>Data</b>					
Awareness of other data sources		X			
Data compilation, cleaning and cataloging			X		
Survey design and sampling	X		X		
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations		X			
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate					
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues	X				
Provides a sound basis for policy			X		
<b>Actual or likely impact of research on:</b>					
Government policy		X			
Future analysis	X				
The development community in general		X			
<b>Overall Quality of Research</b>		X			

## **Flagship Report – Engendering Development**

### **A. Objectives**

1. Are the topics and objectives critical for policy in the developing or post-socialist country?

The topics and objectives are clearly essential for understanding processes of growth and development. The policy issues addressed in the report have been raised repeatedly and forcefully by many groups in civil society in a variety of countries. The only reason one might downgrade the importance of these issues for policy is that they are not strongly amenable to policy manipulation. Many of the crucial dimensions of gender play out within households, and it is very difficult for policy to reach inside the household. The report is appropriately sensitive to these difficulties.

### **B. Design and Implementation**

1. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

The project does reflect a superb awareness of the research that was current at the time it was written. The report covers the entire globe, so it is difficult to be sure that local conditions are adequately addressed in all specific instances.

2. Is the project design appropriately anchored in a sound and coherent analytical and empirical framework?

Overall, the project is well-anchored in a sound and coherent analytical framework. For example, much of sections 3 and 4 are really excellent, with careful and nuanced attention to the difficulties of understanding households, gender roles, social norms, and their interactions with various formal and informal institutions. There are, however, exceptions. I'll discuss two of the weaknesses.

First, "equality" can be a difficult term to define. On page 35, the report defines "gender equality in terms of equality under the law, equality of opportunity ... and equality of voice. We stop short of defining gender equality in terms of equality of outcomes...." The report goes on to give very convincing reasons why defining gender equality in terms of outcomes is problematic. However, the authors find it impossible to avoid judging the degree of gender equality in terms of outcomes. They don't get two paragraphs before they are referring to gender inequalities in earnings, and the report is rife with discussions of inequalities in consumption, life expectancies and health outcomes. It's hard to see how this can be avoided; but it requires a coherent treatment of the decision to examine outcomes.

Second, chapter 5 tackles the important and terribly difficult question "Is Economic Development Good for Gender Equality?" It is argued that increased incomes open up avenues for increasing gender equality. As a general principle, the reverse can be true as

well (in northern Nigeria, the practice of female seclusion became much more strict as household incomes rose). The strongest part of this argument is that based on case studies from East Asia, Africa, and countries in transition from socialism. In these sections, the mechanisms that relate economic change to changes in the status of women are discussed, and evidence from a wide range of sources is compiled into fairly convincing arguments. Within this chapter, cross-country evidence is largely handled well. Terms are typically chosen with care (“the cross-sectional association between gender equality and income is evident...”), although the casual reader is likely to make incorrect causal inferences. This care is abandoned in the conclusion, in which paths of gender disparities in a variety of outcomes as incomes rise are simulated from a simple misspecified regression model. Strong and bold interpretations are based on these correlations, in stark contrast to the care with which evidence is handled in the rest of the study. The substantive problems are apparent upon examination of the regression model in appendix 2. It is not plausible that income and the measure of gender rights used in the regression are uncorrelated with unobserved variables that influence the ratios of gender-specific outcomes that are the dependent variables in the regressions.

3. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The report draws appropriate sensible conclusions from the evidence it compiles. The key conclusions are largely uncontroversial: reform institutions to establish equal rights and opportunities; foster economic development; redress persistent disparities in command over resources and political voice. The third recommendation is clearly the most controversial, but the specific suggestions provided are made with care and appropriate sensitivity to varying conditions across societies.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

Yes.

2. If applicable, are policy recommendations commensurate with findings?

Yes, they are. The practical steps that are suggested are sensible, and they are presented with sufficient diffidence and with calls for close attention to local conditions. Numerous examples are provided of the importance of adjusting policy to differences in the social, political and economic environment.

### **D. Results and Cost-effectiveness**

1. What are the key findings of the study? How do they advance country policy in the field?

There are three:

- a. Reform institutions to establish equal rights and opportunities.
- b. Foster economic development.
- c. Redress persistent disparities in command over resources and political voice.

Only the third is at all controversial. They offer a wide range of proposals in a variety of areas (from education to political reservation to infrastructure). This is not the place for a detailed discussion. Some of the suggestions are less well-justified than others, but the discussion is largely sensible, guided by thoughtful economics, and appropriately qualified.

**Flagship Research Quality Indicator Form**

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed				X	
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)	X				
Clarity of conclusions and recommendations	X				
Extent to which conclusions are based on analytic evidence			X		
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues			X		
Provides a sound basis for policy		X			
<b>Actual or likely impact of research on:</b>					
Government policy		X			
Future analysis		X			
The development community in general				X	
<b>Overall Quality of Research</b>		X			

## **Flagship Report on Agriculture**

### **A. Objectives**

1. Are the topics and objectives critical for policy in the developing or post-socialist country?

The topics and objectives are absolutely essential for understanding processes of growth and development. The importance of agricultural growth for alleviating poverty is well-established, and the Bank and other international donors have a set of very important roles to play in the sector. The policy issues addressed in the report have first order importance

### **B. Design and Implementation**

1. Does the project reflect awareness of existing knowledge from other research available at the time and does it adequately reflect a good understanding of the country(s) in question?

The authors of the report have extraordinarily rich and detailed familiarity with agriculture and agricultural development policy. They are aware (and have produced a good share) of the relevant research in the area.

2. Is the project design appropriately anchored in a sound and coherent analytical and empirical framework?

Unfortunately, this superb knowledge is not adequately reflected in this report. I was very often left perplexed as to where to go for further information on many topics discussed in the report. The report relies too much on internal Bank documents, informal research reports from a variety of international research organizations, and on assertions that are unsupported by references. Given the stature and knowledge of the authors, I believe that virtually every statement in the report is likely to be correct, and it may be that the intended audience of policy makers in developing countries and donor agencies will rely on the report as is. If they do so, they are likely to move policy in the right direction. However, those who wish or need to go into specific areas in more detail get too little assistance from this report. I could give many examples of this inadequacy, spread throughout the report. Here, I will list only five:

- a. “Agricultural growth now depends far more on expanding the application of scientific knowledge to farming than on expanding the amount of land, water, and external inputs dedicated to agriculture.” No suggestions are provided on where to go to learn more about this assertion.
- b. The discussion of “Financial Institutions to Serve the Poor and Develop Private Enterprise” in chapter 3 discusses the “financial systems approach,” but provides no guidance to substantive research on the approach.

- c. The sources of an inverse relationship between farm size and productivity (p. 79) are highly controversial and likely to vary significantly across farming systems and market environments. This is passed over without comment.
  - d. “Numerous studies have empirically illustrated the high returns to investments in rural infrastructure” (p. 98), but the only references are a pair of unpublished research reports. There is a large set of difficult technical and substantive challenges to successfully estimating the rate of return to infrastructure investment. One would like to see peer-reviewed studies that make the case.
  - e. Table 6.2 provides lists of successful pilot projects in a variety of investment areas, without reference to follow-up materials.
3. Are the conclusions consistent with the research findings? Were problems/concerns noted and reviewed?

The conclusions of the report, I believe, are largely well-justified, although these justifications are not well-documented in the report. The problems and concerns that might be raised with respect to the underlying research are not addressed at all. This comment probably reflects my predilections as an academic – we qualify virtually everything we say with caveats and qualifications, and that surely would be a poor model for a report such as this. Nevertheless, there is a “cheerleading” quality to the report that makes the conclusions less than fully credible. Box 2.8 is a good example of this: “...agriculture continues to ‘pull beyond its weight,’ as measured by its contribution to GDP, because of its unique ‘externalities.’” All sectors have their unique externalities; it’s certainly not clear that in the aggregate they are more important for agriculture than for industry or for services.

### **C. Accessibility**

1. Were project reports and publications presented in a manner appropriate for and accessible to the intended audience(s)?

The report is written in a very accessible style, and the key findings are presented clearly and forcefully.

2. If applicable, are policy recommendations commensurate with findings?

See B.3.

### **D. Results and Cost-effectiveness**

1. What are the key findings of the study? How do they advance country policy in the field?

International and national government support for agriculture in developing countries has declined, but should be increased. The five key priorities should be:

1. Fostering the provision of global public goods (removing trade-distorting agricultural subsidies in developed countries, supporting international agricultural research, and dealing with global warming)
2. Accelerating policy reform in agriculture in developing countries (market reforms, trade liberalization)
3. Encouraging domestic institutions that support private sector growth (improved regulation, extension, property rights and finance)
4. Encouraging decentralization and empowering the poor
5. Providing infrastructure and national public goods.

These conclusions are largely uncontroversial, and the arguments for them are sensible. They are largely framed in the context of market failures and there is close attention paid to the specific steps that need to be taken to “get agriculture back onto the development agenda.”

## Flagship Research Quality Indicator Form

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
<b>Topics</b>					
Importance of the issues addressed	X				
<b>Output Quality</b>					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations		X			
Extent to which conclusions are based on analytic evidence					X
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences			X		
<b>Extent to which research:</b>					
Increases knowledge and understanding of the issues				X	
Provides a sound basis for policy				X	
<b>Actual or likely impact of research on:</b>					
Government policy			X		
Future analysis				X	
The development community in general				X	
<b>Overall Quality of Research</b>				X	

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