

## **Nancy Birdsall**

### ***Gender, Law and Development***

#### *A. Objectives*

I am not sure what the objective was (or objectives were) of the overall set of gender papers. There is reference to a “cluster” with three different research studies, one of which is gender, law and development. Since the “best” reading on the excel sheet was the chapter on social norms, laws and economic institutions of a policy research report, I assume that the objective of that cluster is the one I am supposed to be reviewing.

I interpret the objective of this specific topic to have been the use of economic concepts and tools to review and assess the overall situation of women across the developing world, and develop ideas and recommendations for improving the status of women and enhancing their contribution to development, especially via changing laws and improving economic institutions. This is certainly a central development topic, on which the Bank’s research staff has a comparative advantage in access to country-specific knowledge combined with cross-country perspective.

#### *B. Design and Implementation*

The design and implementation are excellent on all counts. The policy research report is very well done. The trade paper (competitiveness and discrimination in Taiwan and Korea) is a good example of new knowledge generated, and economists’ and others’ priors about largely benign effects of competition tested (and found at least partly wrong). The Rodgers and Zveglic papers obviously were inputs to the policy report.

The Basu and Ray papers are of high quality, although apparently they are part of a different cluster on intrahousehold bargaining models. I am not sure that the full implications of those findings have been thought through in terms of policy – this would be difficult. Perhaps this is an area where the Bank needed a review but did not need to commission new work – where academia has the comparative advantage.

#### *C. Accessibility*

The policy research report is readable. But it is too long for many potentially interested parties. Was there something shorter, crisper? So much good effort – but I see no information on number of sales or other shorter, more country or region-specific outlets for the ideas and recommendations.

It is interesting and surprising that the excellent technical paper by Basu (endogenously determined balance of power, published in the Economic Journal) was chosen as “best paper” (for the intrahousehold cluster). I gather the criteria for best paper in all the projects were technical sophistication, not accessibility or (obvious) policy relevance.

#### *D. Results*

This is a very broad set of work. The key finding is not a surprise, namely that this is a development challenge that can be better addressed by better public policy. I doubt, despite the excellent work, that the marginal addition to better policy has been large. This may be an area where, unfortunately, more information and better understanding are not the key determinants of progress. Country-specific studies on laws/economic institutions, done by local scholars, could have a larger impact. If the excellent work already done is not used to frame and define new country and region-specific work, much of its potential value will be left unexploited.

## *Perspectives on AIDS*

### *A. Objective*

This is a critical issue. Various questions are addressed. The paper on condom use asks whether there are market exchanges of unsafe sex for non-money payments in a high AIDS environment, and at what “price” in non-use of condoms, including among adolescent girls.

### *B. Design and Implementation*

The design and implementation are well done. There is a nice review of the literature on orphans in Africa. We are not asked to opine on costs, but it is difficult to eschew noting that if the cost of getting these two papers done was \$19,000, which seems to be the case, it was certainly cost-effective. It is a sensible review with clear implications (orphans), and a good paper with appropriate methods asking questions the answers to which ought to influence policy and programs in Africa.

### *C. Accessibility*

Some sort of “brief” on the results of the condom paper ought to be done for the African policy and program people. Is the implication of the result that until overall incomes increase there is bound to be a market in unsafe sex?

### *D. Results*

Good. See above.

## *Determinants of Health Status (Hammer and Wagstaff)*

### *A. Objectives*

The objective is to understand the determinants of health status, apparently with a major focus on causes of inequality of health status in developing countries, and based in part on country work on Vietnam. This is a key topic for policy.

### *B. Design and Implementation*

This is an impressive set of papers. The Lancet piece is a compelling summary backed up by an impressive breadth and depth of other work by Bank staff. Principal researchers are obviously key contributors to the field. It is a good combination of the country-specific work with issues-oriented work – the latter for example with respect to health insurance and implications for poor households of catastrophic health problems. The data and methods are of highest quality and the author's conclusions are consistent with research findings. The research staff was careful not to overdo what they can conclude but clear on the implications of what they have found. Very clear and practical policy ideas are set out in the Wagstaff and van Doorslaer chapter (on Vietnam) in Glewwe et al.

### *C. Accessibility*

These are excellent papers, accessible mostly to economists and others familiar with research methods but written clearly enough that others willing to get to conclusions can. However, much more could be done in conveying the implications and lessons to a larger community, particularly in public health. I am not sure this should be primarily the task of the research staff. This is a general problem in all the Bank-sponsored work by the Bank's research staff. For example, the excellent chapter in the *Handbook* on equity in health finance covers the issues for the OECD countries. What are the implications of this work for the absolutely central problem in developing countries of lack of progressivity in health spending due to the structural set-up of health systems and the way health services are financed.

I found it odd that the “must-read” readings emphasized methods and research excellence, not policy implications. This may be okay if there are complementary efforts to ensure that where there are policy implications they are made clear. This is essentially the same oddity as the choice of the Basu paper in the gender topic as the “best paper.”

An excellent critique (by Wagstaff) of the WHO approach to measuring equity is not (I think) getting the attention it deserves in the broader, non-technical public health community.

### *D. Results*

Key findings have to do with the relative importance of the many causes of health inequities, and the differences and changes over time in their relative importance for different income groups. That the poor are falling farther behind in relative terms in Vietnam (and in general that inequities by income are rising – per the Lancet essay) should be a wake-up call. That some of the increase appears to be a Kuznets-style outcome of faster overall growth may be a mitigating factor in Vietnam, to the extent income growth continues.

I hope the direction and questions addressed in this research are being sustained in Vietnam, with continuing support by the Bank, by local scholars there. But I wonder....

## *Nutrition*

### *A. Objectives*

This is one of the few overall projects where I got clear information on the objectives, in the form of what was apparently the original research proposal. It is very clear, very important, very straightforward. The objective is to assess the impact of community-based nutrition projects on malnutrition in Madagascar and Senegal and the determinants of any such impact.

### *B. Design and Implementation*

The design and implementation are all A+ -- appropriate methods, given the data available. The conclusions are consistent with the findings. In addition to the Madagascar work (The Senegal work was not completed? Not clear to me.), there is a very nice paper on child malnutrition – the limits to MDG success via income growth alone. The Madagascar study is a good example of careful and creative use of standard monitoring data to extract some information on effectiveness (in this case the marginal treatment effect), exploiting panel data and using matching.

It does raise the question whether the Bank overall is making the right tradeoff between having extremely clever and sophisticated research staff (as in this case) vs. financing more randomized controlled experiments, including with random assignment of communities (or at least sequenced entry of communities into programs, with timing of entrance randomized) as well as of individuals within communities. I do not know. The latter may too often be politically impossible. If so, then bravo to the approach taken in Madagascar, insofar as it demonstrates the potential to use more standard data.

Bravo overall to any major research effort put into serious evaluation of the impact of an intervention. The Bank ought to sponsor these more often. I am well aware of the incentive problems, and of past and current heroic efforts to overcome them. I hope the current effort can be sustained – despite the experience of the past, which is not encouraging, and despite the fact that the incentive structure inside the Bank and in borrowing countries has not changed.

### *C. Accessibility*

The accessibility is excellent for other researchers. The findings may need to be presented in different form in Madagascar and, once completed, in Senegal. The policy implications set out are fully commensurate with the findings.

### *D. Results*

A key finding in Madagascar is that the program inputs are reducing malnutrition more where the program has existed longer, and that the differential return is greater in poorer communities. Since income growth “is not enough” to guarantee gains in nutritional status to use the language in the review paper noted above, any finding that a program is making a difference, especially in the poorest communities, is a good basis for greater confidence in persisting with the kind of spending the program entails.

## *Issues in Health Care Delivery and Health Systems*

### *A. Objectives*

The project focuses on health system issues, especially in East Asia, with much of the work in the “must read” papers concentrated in Vietnam and China. It makes sense for Wagstaff to be using his knowledge of Vietnam issues and data to address not only the equity issues of the health project above, but also health system issues (although I do not know which came first or whether they were simultaneous). Among the other papers are an excellent review in the *Journal of Economic Perspectives* on absenteeism in health systems, and a nice piece (Hammer) on doctors’ incentives to use fully their knowledge in public vs. private services. It is very sensible for Bank economists to be looking at health system issues systematically – and the studies on health insurance, absenteeism, and implications of health shocks for household expenditures are excellent in exploiting well micro data to address system issues.

### *B. Design and Implementation*

Design and implementation are all excellent. Gansu is another example of the use of differences-in-differences plus matching.

### *C. Accessibility*

The JEP piece on absenteeism is a good example of combining rigor and accessibility. It is an excellent contribution, and well-placed in the JEP since it may encourage other economists to consider work on service delivery issues. For much of the other work, I make the same comment as on other projects: accessible to other researchers and to economists, but not to those without technical and methodological interests or knowledge. Again I am not sure this should be the responsibility of the research staff.

For the country-specific work, I assume that the research staff has extensive informal contact with operations staff and with researchers and policymakers in the countries – in this case Vietnam and China – so that the implications at the country level are communicated. That would be a minimum: the implications for the public health community at the international level are substantial, as the issues are similar in many other low-income countries, and more effort ought to go into communicating them, if not by the research staff, then by some others in the Bank.

The excellent Gansu study is another example of very clever Bank research staff exploiting the serendipitous availability of data from other sources in order to evaluate a Bank-financed project. The research staff properly signal the unfortunate fact that for the other five or six provinces of China, where the project has operated, they cannot do the same because the other source of data exists only in Gansu. Research staff and management have a responsibility to complement their increasing sophistication in the use of second-best data with internal lobbying for the Bank to develop, with its

borrowers, much more first-best data. I know that DEC is doing this already – current efforts should be applauded, reinforced and sustained.

As in other projects, there is not emphasis in the research papers on policy recommendations – which may be entirely appropriate. I am curious what the Chinese and Bank operations staff thought of the interesting result in Gansu (of the possible decline in immunizations associated with the project, for example, in turn associated with the demise of village services, as the “better” clinical services were used more). Did they ask Wagstaff and Yu to say exactly what the implications are for the design and nature of the program? (I gather the answer is not clear.) Did the results of the China health shocks study lead to some new thinking about the inadequacies of the current minimal level of health insurance?

#### *D. Results*

This is a large project with many results – not useful to repeat here. Of all the potential research that could be done on health, I would put the highest priority for Bank research staff in this kind of work on health *systems*. The Bank is probably better placed, because of its depth in economics, than other international institutions to address health financing, organization and other system issues. And it is probably better placed than most economists in academe because its operations by their nature tend to provide support to and through the health systems of member countries. Good research on health systems has as much or more chance to lead to policy changes in the way systems are organized and financed as other kinds of health research, even on inequalities. Research on health systems can be useful for program design within the constraints of health systems, but less useful on the non-marginal issues that health system problems pose.

## *Engendering ICT*

### *A. Objectives*

The purpose of the project was to assess the extent to which World Bank ICT projects and components attend to the gender issue, apparently in order to develop recommendations for enhancing the extent to which Bank-financed ICT investments are used by and for women. This is a worthy objective, although I am not sure the topic is a high priority for the Bank from a research point of view, given that the use of ICT by women may be at most only marginally sensitive to specific policy and program interventions relative, for example, to undoing the larger social, economic, and other barriers women face in many developing societies. In addition, research findings alone are not likely to be the key to Bank staff paying attention to gender issues in ICT (and other) projects. Leadership, incentives, and the bully pulpit probably matter more.

### *B. Design and Implementation*

Most of the findings are essentially descriptive of the world as it is – with women less likely to be employed in ICT sectors as one example, but without analysis of whether the problem is one of demand or supply. In general, there is not enough information in the report itself to assess whether some of the findings reported were based on studies that were conceptually more sophisticated in terms of causation and methodologically sound in terms of the counterfactual.

### *C. Accessibility*

The report is well written and the web tool is outstanding. This project is a near-model of accessibility. It would be nice to know for this (and for other studies) something about “hits,” unique visitors, or whatever the right measure is, of the use of the website and downloading of the study (studies).

There is no way to assess whether the recommendations, which tend to be to do more to make ICT available to and used by women, were always commensurate with the findings of country-specific and micro studies.

### *D. Results*

The report outlines how the Bank could do better in the case of “missed opportunities.” It does not address any changes in the structure of incentives Bank staff face.

## *Social Funds in Peru*

### *A. Objectives*

Apparently this is an assessment of the impact of the Social Fund – a Bank-supported project – in Peru on education progress, particularly for the poor who were meant to benefit disproportionately. For this research project, I received just one published article. Since social funds have been a popular kind of project for the Bank (and other donors and creditors) to support, knowing whether they are making a difference for targeted groups is a high priority.

### *B. Design and Implementation*

The design and implementation are excellent. The project deals well with selection and endogeneity issues, and exploits well a combination of social fund data and household data. The conclusions are consistent with the findings and clear in terms of caveats.

(This is another case of a missed opportunity for the Bank to have encouraged, prior to the start of the project, an approach that would have allowed obtaining first-best data for a more straightforward and convincing evaluation of impact.)

### *C. Accessibility*

As in the cases above, this is very clear for the research community and economists. Presumably other Bank staff have made clear the (happy) results to the Peruvians.

### *D. Results*

The Social Fund's support for school infrastructure has reached poor districts and is associated with increased attendance of poor children. The project should have enhanced political and financial support for the fund.

## *Primary Health Care*

### *A. Objectives*

Apparently the objective is economic analysis of primary health care -- policy and services. This is a central issue for health policy in developing countries. As noted above, the project addresses issues of how health systems work – beyond specific “vertical” functions (immunizations, maternal and child health services) to the tradeoffs among different approaches to system-wide organization and financing.

### *B. Design and Implementation*

It is essentially a first-rate synthetic analysis drawing on many micro and other studies, and applying economic tools to bring fresh perspective. The project shows a willingness to question longstanding assumptions in the public health community about the effectiveness of certain approaches. It is innovative. The conclusions will remain controversial, but that is the nature of the beast – the conclusions are clearly linked to the analysis. (I disagree on one point: Health spending/GDP is not a good measure of health spending in absolute terms; and it may be that absolute spending matters, not just spending relative to GDP.)

### *C. Accessibility*

Two papers are fully accessible to the policy community. *The World Bank Research Observer* is a good outlet. A third paper (in *Social Science and Medicine*) is accessible only to those with technical background.

### *D. Results*

The international community may be putting too much emphasis on primary health care services that are publicly provided, from the point of view of improving the health of the poor. Administration of public primary health care services is difficult to do well and unlikely to be effective in countries where institutions in general are weak. Much primary health care that is provided consists of simple, curative services that can be provided privately. The evidence that more spending on primary care yields more health is not there.

More generally, the “result” of the project should be to encourage debate on a key issue, which in the global health community has not been under scrutiny.

## *MENA Trade Investment and Development*

### *A. Objectives*

The objective is to increase growth and job creation in MENA. This is obviously a key topic. The argument is that a key to those objectives is to liberalize trade, with “right” sequencing.

### *B. Design and Implementation*

The argument that the full potential is untapped is persuasive (e.g., Box 1.5) and based on sensible analysis. The report is not clear or convincing on “managing the transition,” perhaps reflecting a lack of much empirical research at the country level that quantifies elasticities (of losses to which groups under which amounts and sequencing of reforms in which types of economic settings). There is no political economy analysis, which is unfortunate. If trade and investment opening were obviously beneficial and easy to do, why are they not happening? Political economy analysis might suggest creative approaches beyond the tried and true.

### *C. Accessibility*

The presentation is very nice. It is accessible to students in the MENA region for example. It has to be as long and detailed as it is to build credibility for its ideas, but something else is needed for the busy policymaker and the heads of state, such as PPT style, briefs, etc.

### *D. Results*

A key finding is that countries in the MENA region are missing the boat (not “globalizers”) and that they can get on board and speed ahead if they change their trade and investment policies and also do a lot of other things right. I am not sure that policy will be advanced at the country level for the reasons noted above: political leadership may need help not on the right thing to do, but how to do it at low cost in terms of losers, whether losers are the poor, interest groups, etc. If they look at events in Mexico, Bolivia, etc., they will not be encouraged to follow World Bank near-orthodoxy without more than this nice book says.

## *MENA Unlocking Employment Potential*

### *A. Objectives*

The objective is to persuade the MENA political leadership that they need to make their labor markets more flexible, and in general let go of the “redistributive-interventionist” vision of social policy – despite the success of that approach in keeping poverty and inequality low. This is probably critical for policy – but I am not sure. Is this the most economically fruitful place to start reform, sufficient to offset its political risks for the current leadership? (Since poverty is low, maybe the head of state of country x should live with 1 percent per capita growth and stability, rather than risk instability for another 1 or 2 percentage points of growth. That growth itself would depend on doing all kinds of other complementary reforms right at the same time.)

### *B. Design and Implementation*

The report is full of good information and analysis. The conclusions are to “develop a new vision for a social contract” and more concretely, reduce government employment and wages. I am not sure these are convincingly backed by compelling evidence of the current costs of the high government employment and current rigidities, such as high severance pay affecting private sector employment – at least relative to other problems in economic policy and governance. But that may reflect my skimming the report and not reading it in depth. It is a very difficult case to make: So far things are not too bad. But the trend looks bad. So if you do not introduce highly disruptive reforms now (reduce government jobs and pay.), your successors might be in big trouble. As with the trade report, the typical Bank style of good analysis combined with lots of “shoulds” on the policy side puts a heavy burden on the analysis. It is not always clear that the analytic work supports the many shoulds set out with so much certainty.

### *C. Accessibility*

See above re trade flagship.

### *D. Results*

See above for “results.”

## ***MENA Better Governance***

### ***A. Objectives***

The objective is to “enhance the dialogue in MENA on governance.” Since MENA as a region is well below average on governance, particularly given its income, and since institutions are key to development, the issue is a development priority.

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

A+. Nice appendix on the indices. (Are the three indices developed available electronically, so MENA and other scholars can use them, including for the non-MENA countries?) Nice appendix re estimating the effects of governance on growth. The issues set out in the report – inclusiveness and accountability – are anchored in the data developed and the analyses.

### ***C. Accessibility***

Excellent. This report is for a broad audience. It backs up the famous UNDP Arab HDRs and provides the data for others to extend and elaborate the analyses. In this case, making available on the website some of the technical background work that must have been done would make sense. Is there a website that helps MENA scholars find papers and develop their own analyses?

As with the MENA trade flagship report, something much shorter would complement this report and reach a much wider audience.

### ***D. Results***

See above. Governance matters for growth. Governance is below average in MENA compared with other regions, given its income. Here is a menu of ideas for how to get improvements – ideas for civil society, policy leaders, scholars, etc.

## *MENA Gender and Development – Women in the Public Sphere*

### *A. Objectives*

The objective is to advance the discussion of gender equality issues in MENA by framing the issue in terms of economic necessity, that is, to analyze the potential for women's greater economic contribution to the "region's new development model" (the latter explained and proposed in the accompanying three flagship reports – see above).

### *B. Design and Implementation*

Good. Mostly descriptive. In Chapter 3 the estimates and discussion of forgone returns (to economy, to households) are clear and reasonable.

### *C. Accessibility*

The report should have a long half-life as a reference for students in MENA. A short piece to complement this report would reach many more people in the MENA region. The first Arab HDR got much more press attention for singling out women's status. That reflects the broader issue of what issues the Bank can most credibly and effectively address, independent of the merits of the research itself.

### *D. Results*

MENA's achievements in advancing women's health and education are not being realized by gains in women's labor force participation – implying forgone economic gains for economies and societies. Longstanding social norms, buttressed by "protective" regulation and family law, are a key constraint. The policy conclusions are that change has to be led from the top as well as supported at the grassroots.

## **Nancy Birdsall**

### ***Annex 2: Strengths and Weaknesses of Bank Research (Human Resources)***

Answers to questions posed are followed by a brief note on several specific concerns.

1. The Bank has made a serious and substantial contribution in health research – especially the economics of health, where its staffing, and access to micro data covering health and economic characteristics of households, for example, have been ably exploited. The Bank’s research on health systems seems especially important. Whether Bank work is “significant” relative to academic and other sources of research, in terms of quantity, quality, and notable findings or perspectives, is difficult for me to judge. I am not sufficiently expert in health economics.

Outside of health, I am not sure the Bank’s contribution has been as substantial and serious. The sample of projects I reviewed included one project on nutrition, which was excellent; again the Bank has emphasized economic questions and tools with good results. I am surprised to see so little work on education – but that may reflect the sample of projects. The policy report on gender did a good job of exploiting the large existing literature, while at the same time commissioning new research in areas where there were clearly gaps and a dearth of serious empirical work.

Three issues arise here. One: I don’t know the universe of research projects. (I didn’t not review the abstracts sent, which were only for 2004. It seems to me there ought to be some descriptive summary for the panel on issues/topics covered, geographic coverage, etc. in each major area.). Two: I am not sufficiently familiar with the universe of social science research in nutrition, gender issues, education, and health.... Who is?! Three: What is meant by “significant” in the question?

2. Does Bank research focus on the most important policy issues? Yes: Systems, /program evaluation, /financing, /implications for the poor, and /equity.

There could be more emphasis on the political economy of public service delivery (although apparently this is ably covered in a recent WDR – Devarajan); on the financing and organization of education systems, in particular potential for more local financing; on historical analysis of successful education and health systems in now-advanced economies (a la Peter Lindert); and on keys to success with education in East Asia – financing, incentives, etc.

3. Bank researchers do a good job surveying the field and reflecting the latest information and methods. Country-specific studies seem to reflect country knowledge.

4. The Bank’s staff have made appropriate selection of topics for data collections and surveys. Most useful have been household surveys, LSMS-type, which provide a base for addressing a variety of policy questions. (Full disclosure: I was involved in the development of the LSMS approach and the fight to implement it at the Bank.)

There are threeTwo problems, however;

(i) Survey data are a public good. There are no incentives for Bank staff to collate, codify, and make available survey data. There are few incentives for Bank staff to ensure ex ante that countries will not restrict access to the data. Often to obtain access to data, non-Bank researchers have to resort to informal and personal contacts. That is the case with few exceptions. There has been progress ....but from 0 to 3 on a 10ten-point scale, and mostly as a function of individuals, not as a function of structural changes.

(ii) There are not enough panel data on households. ....The Bank ought to put the kind of investment into developing panel data now as it did now two decades ago to get the LSMS surveys going. It Ought to have started that a decade ago . . .

(iii) I am not sure that the skills and experience of Bank research staff are fully exploited in country-specific work. Research staff have incentives to maintain their standing in their fields – and there are fewer risks in doing so with existing data, using sophisticated tools, as opposed to digging in and understanding countries and their political and institutional characteristics.

5. Future research would better serve country needs if there were more emphasis on the public good of panel data. (pPerhaps there could be one major effort in each region. – This would be a huge investment, and should be done with other sponsors....); There could be more emphasis on the political economy of health, education and other public service systems; and much more attention to serious impact evaluation of development interventions, including but not restricted to those financed through Bank projects.

6. I don't not know enough about the proposal review process to comment.

7. Bank research meets a high standard. It need not be at the frontier – that is for academics, yet it is often close. However, I do have concerns, noted below.

My concerns

1. Education. I am puzzled by the death of good education research that I saw. Indeed, was there any at all beyond what was reflected in the gender report. The only Bank research person I can think of who has worked on education in the period under study is Lant Pritchett, who did an important piece (where has all the education gone?) raising the key question of how to reconcile high private returns in micro studies with what he showed is the absence of any good evidence that education contributes to growth in aggregate (cross-country, admittedly) studies using production functions or Barro-type growth regressions.

Is this another example of the Bank ignoring tough questions and continuing endlessly to emphasize the work on returns to education of Psachropoulos – who himself would surely agree that the “social” returns he presents do not include unmeasured possible

externalities at all levels? I am not sure the Bank has ever acknowledge in its policy work that the world has little if any information on the “social” returns to different levels of education. Thus its insistence on the primacy of primary education is NOT in any serious way defensible on the grounds of good evidence.

And now Pritchett has a new paper compiling existing evidence suggesting that for all practical purposes most children in primary school in developing countries are learning almost nothing at all...but this work was not actually sponsored by the World Bank but by us (at the Center for Global Development).

So unless I am missing something, I now ask more directly: Why were there no education papers in the Human Resource sample I received? Where is the critical mass of serious Bank work on education? Is education research stock of the Bank solely the excellent work on absenteeism and the other controlled trial studies? Is there any work on education analogous to the systematic efforts to learn about health systems (including through randomized controlled experiments and use of “matching”)?

2. Cost. I was not asked to comment on cost. But my impression is that the Bank spends a huge amount on research, making it doubtless the biggest development research center in the world. Whether there is sufficient value for money is not clear.

3. There are not sufficient incentives for dissemination and communication of research results. I doubt there ever will be. Research staff should have some responsibility but may not be the best at dissemination and in any event will never have sufficient incentive to do more. The job cannot be done outside the Research Department. Perhaps it should be part of the job of research management (the Chief Economist and department directors) to manage communications and dissemination staff. They should be producing short briefs, radio and video summaries, training materials, website tools, etc. Research managers should be judged on the basis of website visits, citations, testimonials, etc.

It's amazing to me that I received no information on citations, web hits or impact/influence, as part of the review.

4. More of the Bank's research budget should be spent on support for institution-based research in developing countries. Copy the IDB model. More research staff should be in the country offices. It is not just or mostly a matter of working with individuals in developing countries, although even that is far too little and limited. It is a matter of sustained twinships, etc., with university departments, research institutes and so on, in borrowing countries. This could be done without additional administrative budget and without more begging from donor Trust Funds. It should be done by reallocating DEC and other research budgets, including, if necessary, through reductions in full-time Bank research staff..

5. After 60 years, it is time for the Bank to finance more impact evaluation. Francois Bourguignon has taken excellent leadership on this issue, but without structural changes in incentives and a clear, even quantitative, mandate from the Board, I doubt the program

he has championed will be sustained and institutionally consolidated. In the human resource area, the Bank should take leadership in pushing all the donors and developing countries to learn and adjust to new knowledge through transparent, credible impact evaluation. Mexico is leading the way, ....where the Bank should have been three decades and more ago.

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