



## COMMENTARY

# THE WORLD DEVELOPMENT REPORT AT THIRTY: A BIRTHDAY TRIBUTE OR A FUNERAL ELEGY?

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Shahid Yusuf's review of the *World Development Reports* (WDRs) is elegant and insightful, but also wistful and nostalgic. He clearly believes that the WDRs have known better days, and I agree with him. He is positive about the future, but I am not sure I agree; I think the problems that afflict the WDRs have deep causes that will not soon go away.

In my comments, I shall follow the same general outline as does Yusuf. I will begin with my understanding of the function of the reports, and I will review some of the most influential reports—and their possible influence on development thinking—as well as the general tone and content of recent reports. Like Yusuf, I shall not be afraid to use the exercise as an excuse to think about economic development more generally and about the role of the World Bank in particular.

In what follows, I shall draw freely on the review of Bank research—including the WDRs—that was carried out by an outside panel consisting of Abhijit Banerjee, Nora Lustig, and Ken Rogoff, with myself as chair. Our report, *An Evaluation of World Bank Research, 1998–2005*, has been available on the Bank's Web site since September 2006 (Banerjee and others 2006). I note, however, that although the report is a joint document, the review panel is in no way responsible for the views expressed here, which are entirely my own.

## The (Multiple) Roles of the *World Development Report*

The *World Development Report* is the flagship publication not of the World Bank as a whole, but of its Research Department, headed by the Bank's chief economist. The chief economist always has overall responsibility for the report and on occasion uses it as a vehicle for publicizing his or her own views about development policy or the importance of particular topics. Joe Stiglitz on information, Nick Stern on the investment climate, and François Bourguignon on equity are recent examples. In all cases, the *WDR* provides a summary of Bank thinking and research on a particular topic—or on an interrelated set of topics—and tries to position its own views in the forefront of current development thinking and debate. Yusuf writes that “The *WDR* can again become a vehicle for mobilizing global opinion and for guiding strategy,” summarizing both its aim and the view that it is currently failing, although it has succeeded in the past. *World Development Reports* summarize not only the Bank's own research, but also outside academic research, not only from economics, but increasingly from other subjects, including political science, sociology, psychology, and epidemiology. These summaries reputedly put the *WDRs* on many college reading lists, though I am unaware of any evidence. Because the *WDR* is perceived as very important within the Bank, intense internal competition surrounds the choice of topic, with different groups jockeying for prominence for their own pet issue or research topic. This role does much to ensure the continuation of the reports and may be as important in doing so as any success in mobilizing global opinion and guiding strategy.

The evidence that the *WDRs* have—or ever had—such an influence is notably thin. Citation counts are presented, which are unimpressive to my eyes, but are scarcely relevant. The reaction of the intended audience—policy makers and their advisers around the world, newspaper editorialists, or even teachers of economic development—is not well measured by citations in the ISI Web of Knowledge database or on Google Scholar.

But even in a time when economic development and foreign aid are very much in the public and academic minds, and when the *New York Times* has a world poverty correspondent, neither the *Times* nor the *Journal of Economic Literature* anxiously awaits the appearance of a new *WDR*. (Compare this situation, for example, with the extensive reaction to the new poverty counts in late August 2008.) Newspapers in Delhi, Kampala, or Cape Town may evince more excitement, and reactions there

could usefully have been documented. Other commentators in this volume are better placed to assess this international reaction and to comment on whether the policy makers and advisers routinely use the *WDRs*. On the publicity side, my impression is that the most heavily publicized of the recent *WDRs* was *World Development Report 2000/2001: Attacking Poverty*, which became a news item, not for its content, but for the internal disarray that it revealed within the Bank, particularly on the role of growth in poverty reduction.

The production of the *WDRs* is expensive, something that is not discussed in Yusuf's essay. At any given time, approximately eight full-time researchers are at work on the current, previous, or next report. Measured in numbers of people, this team constitutes about 10 percent of the Bank's research effort, which takes no account of the fact that the *WDR* team is typically drawn from among the Bank's best and most senior researchers. Nor does it count the financial costs of the world tour that follows the publication of each report. If the reports have not been successful, it is not for want of commitment by the Bank. Yet research in the Bank, like the Bank as a whole, is under increasing budgetary pressure. Now is surely a good time to think about whether the value of this one item is worth what it costs, for which we would need a much fuller accounting of costs and benefits than is currently provided.

## The Quality and Intellectual Legacy of the *World Development Reports*

The research review panel summarized its views of the *WDRs* as follows:

The *World Development Reports* have sometimes been instrumental in changing the way that the world thinks about some aspect of development, such as poverty, health, or population. In recent years, they have, to an extent, become the victims of their own success. Because they are seen as so important, they must incorporate the views of large numbers of people, inside and outside the Bank. In consequence, they often seek to minimize conflict and to emphasize "win-win" situations instead of trade-offs. They often lack sharpness and focus, and are sometimes incoherent, especially when it proves impossible to reconcile the views of the various commentators and authors. . . . [T]heir regular appearance contributes to the Bank's standing in the development community even if, to some extent, they are trading on their past reputation (Banerjee and others 2006: 8).

If this view differs from Yusuf's, it is only in emphasis.

The three best remembered *WDRs* are those on fertility, on poverty, and on health. The 1984 fertility report took the previously standard, though even then rapidly fading, view that population growth was indeed a problem for economic development, that more mouths meant less for each (the lump fallacy), and that the “tragedy of the commons” meant that the individual decisions of parents about their fertility were unlikely to lead to good outcomes. Perhaps the most important intellectual legacy of this report was the establishment of a National Academy of Sciences panel under the chairmanship of Sam Preston, which produced an authoritative modern account of the issue and which takes a very different view from the *WDR* (Preston, Lee, and Greene 1986).<sup>1</sup> Yusuf comments that the Bank dropped the issue after the report, and indeed the tide was turning against the international population control movement from the mid-1980s on. Yet much harm had already been done, as documented in Matthew Connelly’s (2008) *Fatal Misconceptions*, which while not painting the World Bank as the principal villain in this shameful history, does not absolve it either. In any case, the population report was clearly an example not of Bank intellectual leadership, but of the Bank being well behind then-current best thinking.

The 1990 poverty report is famous for introducing the international dollar-a-day poverty standard and the associated counts. These counts have continued to date, regularly updated by Martin Ravallion and his team, who were also the original authors. They have had an immense effect on development practice and on development debate, not least through the use of the dollar-a-day standard to define the first of the Millennium Development Goals (MDGs) and the appointment of the Bank as the subsequent scorekeeper. It is worth noting that this intellectual contribution, one of the Bank’s most prominent, was not in the area of policy making or of theory, but in the area of measurement. The dollar-a-day standard illustrates how important measurement and scorekeeping have been in development and in the assessment of the Bank itself. Yet measurement plays little role in Yusuf’s paper, an issue to which I will return.

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1. Yet the idea of a population threat is a hydra that will never die, and it is showing signs of life again in the wake of the current boom in world food and commodity prices, as well as in Jeffrey Sachs’s most recent book *Common Wealth* (Sachs 2008).

The dollar-a-day standard is not without its problems and detractors. It provides the measurement underpinnings not only for the first MDG, but also for at least part of the Bank's current, almost exclusive focus on poverty reduction. One of the problems comes from the fact that the measures are tied to the purchasing power parities from the International Comparison Program (ICP), so that the global poverty line and the associated counts change with every revision of the ICP, whose own measurements are sometimes on shaky ground. The latest (2005) version of the ICP, which had greater cooperation from China and India than ever before, brings hundreds of millions of Chinese and tens of millions of Indians into the international poverty counts who were previously thought to have escaped (see Chen and Ravallion 2007, 2008). Although Chen and Ravallion take the view that the 2005 ICP is simply better—because it is more comprehensive and because it better controls for the quality of goods and services across countries—this argument is by no means obviously or unqualifiedly correct, and a real risk exists that the constantly shifting standard will eventually bring the counts into disrepute. That previous estimates are discarded with every new round of the ICP certainly undermines public understanding of what is happening to global poverty and causes a great deal of confusion—as demonstrated, for example, by the immediate reaction in the Indian press to the latest counts. For example, Surjit Bhalla, a longtime critic of the Bank's poverty work, noted that if the latest ICP estimates are correct, and if India's growth rates are correct, Indian living standards in 1950 could not easily have supported life.

More fundamentally, the success of the dollar-a-day measure carries with it the risk that the objective of the Bank becomes not just the elimination of poverty, but the elimination of dollar-a-day poverty. Given the uncertainties of just who is poor by this criterion—with hundreds of millions of people being reclassified with each new set of measures—directing all attention to people below the line and ignoring those just above it makes no sense. Of course, the problem is more general than the international lines. Many local domestic lines that are used by the Bank for country policy advice have a substantial arbitrary component, and many have little local political legitimacy. Governments are—or at least should be—responsible to all of their citizens, not just to those below an arbitrary and uncertain poverty line.

The 1993 *WDR* on health is also famous, mostly for its introduction of the disability-adjusted life year (DALY), although this concept and its subsequent sweeping of the world is attributable less to Bank researchers than to Chris Murray, who was a consultant to the *WDR*. The DALY, like the dollar-a-day standard, has become a central tool of health measurement around the world for computing the burden of disease associated with different conditions, for permitting a combination of mortality and morbidity, and for assigning priorities. Again, many may criticize the DALY measures, particularly the arbitrariness of the weights that they attribute to different diseases—adding together migraines, quadriplegia, or schizophrenia—as well as of the dangers of using DALYs as a guide to policy and taking seriously the discounting of the lives of people with disabilities and diseases. The success of the concept may owe as much or more to the vacuum that it filled than to its own conceptual soundness. But the 1993 *WDR*, more than any other Bank report, put the Bank on the map as a major player in global health. It is also famous for reputedly persuading Bill Gates that international health was important, certainly an excellent example of the *WDR* mobilizing global opinion and shaping strategy.

It is noteworthy that both the 1990 poverty report and the 1993 health report are best known for their introductions of new tools for *measurement*. Although it is too early to know which recent *WDRs* will be as influential, my guess would be the report on service delivery, which also introduced new measurements, from the Bank's important surveys on absenteeism among health and education workers around the world. New measures changed and shaped the debate more than new analysis. This fact is perhaps not surprising. More than other international agencies, the Bank is well equipped with data and with high-quality researchers and consultants who are able to present these data in new ways that have long-lasting influence on the way that people think about development successes and failures.

### **The *World Development Indicators***

In the early days of the *World Development Reports*, many of us would wait anxiously for a new one, and when it arrived, we would ignore the words up front and turn quickly to the tables at the back. These tables

became the *World Development Indicators (WDI)*, later spun off into an immensely successful stand-alone product. In the early days, the production of the *WDI* was essentially a retail operation, with the Bank assembling information that others had collected. Over time, the Bank has become a major data provider in its own right—for example, collecting household surveys, conducting the Doing Business and Investment Climate surveys, and—most recently—managing the latest round of the International Comparison Program. In consequence, an increasing fraction of the data in the *WDI* is generated in-house. The *WDI* database is accessed by tens of millions of subscribers around the world and is used not only by academic researchers, but also by economic commentators, policy makers, and policy advisers around the world. Of the 18.8 million registered online users, 10 million are in low- and middle-income countries. The provision of these data is exhibit A in the Bank's case to be a knowledge bank, and their development is an achievement for which the *WDRs* should take much of the credit.

### **Declining Fortunes: From a Star Is Born to a Red Dwarf or Even a Black Hole?**

Yusuf's paper leaves the strong impression that the *World Development Reports* are not what they once were, and some of these concerns are also reflected in the summary statement from the panel review quoted previously. The *WDRs* certainly suffer from being the consensus reports of a large bureaucracy among whose members serious differences of opinion exist that cannot be resolved without confusion, banality, and contradiction. Despite the Bank's increasing importance in measurement and data provision, the *WDRs* have not had a distinguished history of handling empirical evidence; too often bad—or simply incredible—evidence is presented along with useful and interesting new findings. Some of this history reflects unresolved differences being papered over by any evidence that can be brought to hand. Some of it is the enthusiasm of young researchers, whose fascination with new techniques has not always been tempered or restrained by the more seasoned judgment of their managers, among whom statistical and econometric expertise has not always been a priority. In fairness, economics as a whole has moved from a subject dominated by

prior theorizing to one dominated by empirical evidence, and the transition from one to the other has been far from smooth. As a result, there has been little help from the outside.

More fundamentally, we also need to ask whether the decline in the *WDRs* reflects a decline in the quality of thinking in the Bank, or at least in its Research Department. I do not think that a decline is the major source of difficulty, but there are causes for concern. In the earliest days of the *WDRs*, the Bank's Research Department could and did attract leading scholars in international development. Very high salaries and generous pension arrangements were certainly part of the attraction, but so was the sense of moral purpose—that working for the World Bank, thinking about economic development and the alleviation of poverty, and passing on expertise were a good way to spend a working life. Not only did the Bank attract good new PhDs, but it also attracted a substantial number of assistant professors who decided that policy advice plus research was more fulfilling than teaching and research. A good deal of this thinking still goes on, and some of the young researchers in the Bank are clearly very good indeed. But the salaries (and pension benefits) are now much less, and very much so relative to academic salaries, which have risen rapidly in the meantime. The original pension arrangements have also made it possible for some of the Bank's best thinkers to quit the Bank for academia and think-tanks—Harvard, Princeton, and particularly the Center for Global Development—while they still have many years of useful contributions ahead of them. I suspect that more than any of these factors, however, the decline in the attractiveness of being a Bank researcher results from a growing skepticism that the Bank is doing much for international development and about whether aid, particularly as dispensed by the Bank, does much for economic growth and the reduction of poverty.

One version of the history of development economics within the Bank runs in terms of a steady broadening of focus, with each step a response to failure at a previous narrower focus. In the earliest days of the institution, much of its expertise was in engineering, with specialists who could help countries construct roads, dams, ports, or even whole industries. Economic policies were a matter of planning, of coordinating the engineers and their projects. By the 1950s and 1960s, it became clear that many of these projects were not contributing to the social good. One distinguished set of

intellectual responses explored the idea that projects that were profitable at distorted market prices might not do much to help development—or might even hurt it—because the prices were so misleading. In response, and within the general framework of optimal growth theory, researchers, including Bank researchers, developed systems of cost-benefit analysis based on shadow prices that were supposed to be used by the Bank and client countries to evaluate projects. When, in turn, these procedures foundered on their simplistic treatment of policy making—few governments of developing countries could accurately be described as social planners optimizing an infinite stream of consumption—the Bank moved toward more systematic and comprehensive policy reform, in which market prices—and macroeconomic policy—were to be “got right” first.

In the ruins of the structural adjustment programs, the Bank moved out into an even broader agenda of political and institutional reform, which brings us more or less up-to-date. One notable feature of the broadening is the diminution of expertise. The engineers knew what they were doing, even if their expertise did not extend to ensuring that their dams or steelworks were socially beneficial. The growth and welfare economists of the 1950s and 1960s had a sophisticated understanding of their models, though not of the motives of policy makers. A broader spectrum of economists understands the consequences of price distortions or of unsustainable macroeconomic policies. And although we are not entirely without expertise, reforming governance and institutions is a much taller order than building a water delivery system or even a petrochemical plant.

One interpretation of this much simplified narrative is that the problem was not well conceived from the start, that the very idea of outside expertise helping countries to develop is misconceived—and possibly even harmful. As we move from posing questions to engineers to posing questions to political scientists, the answers may move from telling us “how to” to telling us “not to.” In their recent summary of thinking in political science, Moss, Pettersson, and van de Walle (2008, p. 269) note that large aid flows “may undercut the very principles the aid industry intends to promote: ownership, accountability, and participation,” essentially because the presence of the large donors inhibits the development of the democratic contract that would allow development to proceed. If this argument is correct—and I think it plausible, but I do not know for sure, or

what kinds of aid (international public goods, some health interventions?) are exempt—then the development expertise that is the center of the World Bank’s mission may not exist in useful form or, at the least, needs to be fundamentally rethought and restricted. And if the *World Development Reports* are the handbooks of development expertise as contained by the Bank, they too may have a limited future.

In the end, I suspect that the nostalgia in Yusuf’s history is not for a *World Development Report* but for the World Bank itself.



## COMMENTARY

# THE WORLD BANK AND THE EVOLVING POLITICAL ECONOMY OF DEVELOPMENT

KEMAL DERVIŞ

Shahid Yusuf's essay on 30 years of *World Development Reports (WDRs)* is a masterful overview of what has at the same time been 30 years of development economics at the World Bank. I will first focus on one key aspect of the overview: the evolution of the political economy of development economics at the World Bank, influenced, of course, by my own perceptions of the 1980s and 1990s, two decades I spent at the World Bank. I will then turn to the future and to one key dimension that I think has been missing in the *WDRs*.

There is no doubt that development economics at the World Bank, and with it the *WDRs*, have been and will continue to be influenced by the political and intellectual environment of the times. The Executive Board does influence the management and the staff, not only because it has some "decision powers" over policies and strategies but also, and perhaps even more, because positive recognition by the board is a sought-after prize, and criticism is perceived as a big setback. Positive recognition by the president of the institution and by the chief economist is also something very valuable, influencing careers and promotions. The ideological and intellectual orientations of the president and of the chief economist clearly influence the work of economists at the World Bank.

Shahid Yusuf stresses these influences in his overview, showing how development economics at the World Bank and the content of the *WDRs* moved from strong faith in planning and in the role of the state, along with the quantitative models championed by Hollis Chenery and his colleagues in the late 1970s, to the structural adjustment approach of the 1980s and early 1990s. This period coincides broadly with what are often called the Reagan-Thatcher years, marked by much greater emphasis on the market, on “getting prices right,” and on both liberalization (particularly trade liberalization) and privatization. The second half of the 1990s saw renewed emphasis on poverty reduction and on the need for proactive poverty-reducing social policies, particularly after James Wolfensohn took over as president in 1996, with Bill Clinton in the White House and Tony Blair soon after at 10 Downing Street. I agree with much of Yusuf’s analysis, but I do believe it somewhat exaggerates the influence politics and ideology have had in the two-plus decades reviewed.

Several factors make it difficult for any particular political ideology to “take over” the World Bank—and I believe that is a very fortunate state of affairs. Moreover, although the influence of the U.S. and U.K. treasuries is, of course, very important, particularly on big programs, it is less so regarding the economic work done and the many and very decentralized interactions that take place with member countries. Although its headquarters are in Washington, D.C., and English is clearly the language in which World Bankers work, the World Bank is, both by the composition of its staff and by the very nature of the business it conducts, a truly international institution. In decades past, no small group of governments has easily been able to direct the work of the thousands of economists and other professionals who make up the staff. Over many decades, the institution has—as have many other institutions—developed its own “DNA,” which is deeply rooted in the experience staff members gain around the world and the interactions staff members have with professionals and citizens in places as diverse as Brazil, China, the Arab Republic of Egypt, India, Malawi, Nigeria, and Vietnam, to name just a few.

The Executive Board also is—and has been—a very diverse body. Much of the world is represented and expresses itself. It is true that the voting weights are outdated and do not today reflect the realities of the 21st century. A significant change in the “weights” countries have at the Bank

is overdue and is essential for the overall legitimacy of the institution. Nonetheless, diversity of voice exists, and different coalitions form and then dissolve over time, depending on the topic at hand or the particular period concerned. A good and articulate executive director from a smaller country can have substantial intellectual influence. Finally, presidents and senior managers have visions and have shown leadership, but to be successful, they must also convince the staff, listen to the accumulated experience, and be open to feedback.

In my experience, the driving force of the changes in emphasis so well described by Shahid Yusuf in the 30 *WDRs* reviewed, as well as in the content of development economics at the World Bank, has been more the evolution of academic thinking than of politics as such. That aspect, too, is emphasized in the essay, but I would stress it even more. Since Robert McNamara and Hollis Chenery, the institution's strongest links have always been to the academic work on development, and the *WDRs* themselves are expressions of that link. For example, the work done at Princeton introducing relative prices and price-sensitive demand and supply functions into the older, rigid Leontief input-output models was adopted by the Development Research Center of the World Bank and facilitated a more market-oriented approach to development policy. That academic work imported into and championed by the World Bank in the late 1970s turned planning models into policy and market simulation models, which were later widely used to analyze the structural adjustment policies of the 1980s. Both continuity and strong interaction with academia existed throughout that process, with political ideology playing a lesser role. As another example, one can mention the very wide use of domestic resource cost estimates and effective protection rates to measure the social costs of price distortions and trade policies, which owed more to the academic work ongoing at the time than to ideology. Two Bank chief economists were, with Béla Balassa (also a senior presence at the World Bank throughout the late 1970s and 1980s), intellectual originators of these concepts; however, Anne Krueger was politically right of center, whereas the late Michael Bruno was close to the Israeli Labor Party.

More recently, the academic work on the role of institutions in development and labor markets, as well as on the microeconomics of information

and market structure, has strongly affected the economic work at the World Bank after the mid-1980s and the *WDRs* in the 1990s. Chief economists such as Stanley Fischer, Larry Summers, Joseph Stiglitz, Nicholas Stern, and François Bourguignon have clearly been impressive academics, and their academic and policy analysis achievements brought them to their positions more than any political or ideological bent they may have had. This is not to say that all economic work at the Bank has been of the highest quality. Too much of it has allowed simplistic and, yes, sometimes “politically correct” cookie-cutter prescriptions to pass as analysis. But it has been the creative and academically grounded work that in the end earned recognition and respect.

I am not sure how strong a difference exists between Shahid Yusuf and myself in assessing the weight of the different influences on the *WDRs*. But I do want to stress the power of the link between academia and economics at the World Bank, the strong institutional DNA built over decades with a value system emphasizing analytical skills and academic recognition, and the difficulty of linking the choice and role of chief economists in a simple way to primarily political or ideological factors.

It is interesting to note that the United Nations Development Programme’s *Human Development Reports (HDRs)* provide another example of how an institution’s DNA and intellectual tradition cut across the tenures of chief executives with very different political homes. The *HDRs*, which have succeeded in providing tough competition to the *WDRs* in terms of influence and attention, have from the start emphasized poverty reduction, income distribution, and the role of public policy. And yet the *HDRs* were launched under William H. Draper, appointed with the then determining influence of the U.S. Republican administration of the late 1980s.

The second point I would like to make, looking at 30 years of *WDRs*, relates to the almost exclusive focus on the “country” or “nation-state” as the unit of analysis. It is true that the first *WDRs* contained global projections that later were spun off and became the *Global Economic Prospects* series, but only a very weak link exists between the projections and the development policy analysis contained in the *WDRs*. The latter is country focused, and the international economy, as such, is not in the forefront of analysis. This country focus does, in fact, faithfully reflect what is practiced by most academic economists when they run growth

regressions or when they do case study work trying to distill the lessons of development policy experience. The unit of observation is almost always the country, without much attention to the international system within which country policies have to operate.

The importance of export orientation, openness to trade, human capital policies, investment rates, or financial sector policies is most often analyzed giving individual countries equal weights as units of observation. China and Lesotho each constitute one observation point per year of available data when regressions are run. This equal-weight and nation-state-focused nature of much of comparative development economics has at least two weaknesses. The first weakness relates to the relevance of the findings. Suppose, for example, that one finds that total factor productivity growth is more important than capital accumulation in explaining differences in growth performance—except for India and China, which are, in Yusuf’s words, “accumulating physical capital and pouring it into industry at a feverish pace.” Should one then turn the statement around and say that capital accumulation is the dominant factor for half of the developing world because these two giants account for about half of the population of developing countries? To what extent should size matter when drawing conclusions? This question has no easy answer; it presents theoretical and empirical challenges. Nevertheless, I do think that the fact that much data come by country units should not make us forget the extreme size differences involved.

Another dimension of this problem relates to policy space. The degree of freedom of the policy maker and the effects policies can have are clearly affected by the world economic environment, but more so for smaller countries and very open economies. Take an example that is currently particularly relevant. It is well known in theory that international capital mobility constrains monetary policy. The nature and effects of policy responses by the Brazilian, South African, or Turkish central banks to the crisis level challenges that emerged in 2008 greatly depend on the interest rate policies of the Federal Reserve and of the European Central Bank. Analyzing macroeconomic or structural policies of particular countries without putting them explicitly in a global context is increasingly difficult. Systemic international developments affect most elements of development policy, including labor market, agricultural, tax, energy, trade,

and financial sector policies. Given the degree of interdependence that characterizes the 21st-century world, country-focused analysis increasingly must be complemented by analysis of the world economy as a system. What may be needed is a kind of hierarchical analysis, where local development; national development; regional development (Africa, Latin America, the Middle East, and so on); and global development are parts of a systemic approach that tries to capture what matters at what level and what freedom of action policy makers have at these various levels.

Some *WDRs* have gone beyond the country as the basic unit of analysis, including the 2009 *WDR* on spatial issues. An explicitly multilevel approach could mark a new start for the *WDRs* and respond to the realities of the new global world of the 21st century. The *WDR* planned for 2010 on the topic of development and climate change, chosen by Bob Zoellick, could become a path-breaker in that respect. Clearly, climate is a global issue and a global public good. The importance of climate-related policies for development can be analyzed only in an explicitly multilevel framework, where global, regional, and country-level policies interact to determine outcomes that cut across national boundaries.



## COMMENTARY

# THE INDOMITABLE IN PURSUIT OF THE INEXPLICABLE: THE WORLD DEVELOPMENT REPORTS' FAILURE TO COMPREHEND ECONOMIC GROWTH DESPITE DETERMINED ATTEMPTS, 1978–2008

WILLIAM EASTERLY

The intellectual tragedy of 30 years of *World Development Reports* (WDRs) is that they never accepted the reality of the great unpredictability and uncertainty of economic growth in the short to medium run. The WDRs keep trying to find ways to raise growth in the short to medium run when the economics profession does not have this knowledge. They seek to explain short-term fluctuations in growth when there is no evidence base for such explanations. As a result, they fall prey to many of the classic heuristic biases about randomness (à la Kahneman and Tversky), including frequent use of circular reasoning, and they lose the opportunity to carry on a fruitful debate about the best way to handle this uncertainty and to make development more likely in the long run (Gilovich, Griffin, and Kahneman 2002; Kahneman, Slovic, and Tversky 1982).

What is the state of our knowledge about growth? First of all, country growth rates are not persistent over time, which was documented as long ago as Easterly, Kremer, Pritchett, and Summers (1993). High growth is

mostly transitory, reverting to the global mean in the following period. This finding was bad news when most of the candidate explanations of growth were very persistent country characteristics. Of course, there could have been time-varying variables that explained the time-varying element of growth. Unfortunately, the second characteristic of our growth knowledge is that we have failed to identify any such robust time-varying variables (or for that matter any robust persistent variables). Levine and Renelt (1992) established this failure convincingly early in the growth literature. It further showed itself in the 145 different variables found to be “significant” in growth regressions with fewer than 100 observations (Durlauf, Johnson, and Temple 2005). The last hope was Bayesian model averaging to identify the small number of variables that were robust in most regressions (Doppelhofer, Miller, and Sala-i-Martin 2004). Even this hope vanished recently when Ciccone and Jarociński (2008) showed that Bayesian model averaging gave completely different “robust” variables for different equally plausible samples (World Bank versus Penn World Tables or successive revisions of the Penn World Tables).

In defense of the *WDRs*, the economics profession was also slow to admit the inexplicability of growth fluctuations. However, a wide spectrum of economists has by now conceded we don’t know how to raise growth in the short to medium run (Easterly 2001; Lindauer and Pritchett 2002; Harberger 2003; “Barcelona Development Agenda” 2004;<sup>1</sup> Rodrik 2006; Solow 2007; Spence Commission 2008).

A random effects regression on the panel of per capita growth rates from 1960 to 2005 reveals that only 8 percent of the cross-time, cross-country variation in growth is due to permanent country effects; the other 92 percent is transitory (which is equivalent to stating the lack of persistence of growth rates identified in Easterly, Kremer, Pritchett, and Summers 1993). The transitory does not have to be mechanically “random” in the sense of coin-flipping; it could well be one-off movements caused by human action. It could be an entrepreneur finding a “big hit” in exports, like cut flowers in Kenya or garments in Bangladesh; it could be a smart policy move that

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1. The “Barcelona Development Agenda” is a consensus document resulting from a meeting of economists in Barcelona, Spain, in 2004. Signatories of the document include Olivier Blanchard, Guillermo Calvo, Stanley Fischer, Jeffrey Frankel, Paul Krugman, Dani Rodrik, Jeffrey Sachs, and Joseph Stiglitz.

was in the right place at the right time; or it could be a bubble caused by an information cascade or other kinds of herding. On the negative side, it could be a dramatic mistake by a policy maker or a private entrepreneur. Still the transitory might as well be random in the sense that we cannot usually explain or replicate what just happened.

Hence, many of the classic Kahneman-Tversky heuristic biases about randomness have played themselves out in *WDRs*.<sup>2</sup> Take, for example, the fallacy of the “hot hand,” when a basketball player makes a string of baskets in a row. The hot hand bias is to falsely conclude that the player’s skill has temporarily moved to a higher level, whereas actual calculation shows that a player is no more likely to make the next basket after a hot streak than at any other time. The problem is that we expect randomness to show up as alternating hits and misses when in fact it often displays streaks of hits. Another way of stating this fallacy is Kahneman and Tversky’s sarcastically named “law of small numbers” (Kahneman, Slovic, and Tversky 1982). In the case of the *WDRs*, we falsely draw conclusions about how to achieve superior long-run performance from too small a number of observations, without allowing for the large role of transitory factors in a small sample. The small numbers refer both to a small number of “successes” and a small number of annual observations (even 25 years may not be long enough, as will be discussed).

*WDRs* abound with statements reflecting this fallacious viewpoint, as summarized by Yusuf:

If [China and India] can rack up rates of investment and growth that are the envy of the world under the most makeshift of institutional conditions, need other countries more attuned to the market strive after greater perfection? China was growing when it had few if any market institutions; as its institutional structure has strengthened, it has continued growing with investment serving as the principal driver without a clear relationship running from the specifics of institution building to growth.

China and India definitely reflect some genuine success, but their sudden shift upward in growth is also bound to reflect some inexplicable, transitory factors that do not help us understand success (and it is even worse to break up their performance into subperiods, as with China in the last sentence).

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2. A wonderfully entertaining summary of this and other related research is a recent book for non-technical audiences by Mlodinow (2008).

One systematic way of showing the hot hand fallacy at work is by simulating a mechanical procedure to identify “success.” The example I use is not from *WDRs* but from the Spence Commission (2008); however, the *WDRs* (as shown by the quotes above) definitely do informally what the Spence Commission did more formally, so this example is just a way to formalize a comment on the *WDRs*’ worldview.

The Spence Commission identified “success” as (essentially) any 25-year period of gross domestic product (GDP) per capita growth above 5 percent.<sup>3</sup> This procedure sounds like a pretty good bet, but in fact it was very likely to pick up a large element of transitory performance for two reasons:

1. Selecting on high values of the growth outcome will very likely include large positive realizations of the transitory component. This problem is all the more likely because the permanent component of growth outcomes exceeds 5 percent in only 1.8 percent of realizations (whereas the temporary component will exceed 5 percent by itself in 26 percent of realizations).
2. Selecting on the time period (*any* 25-year period out of a 45-year sample from 1960 to 2005) further biases the episodes toward those that had large positive transitory outcomes. The time period is selectively biased to be one that started and ended so as to include a large number of large positive transitory outcomes.

A Monte Carlo simulation based on the parameters from the random effects regression shows that the Spence Commission’s definition of “success” will occur in about 9 percent of countries, which is far more than the 1.8 percent of countries that have a genuine permanent country growth above 5 percent (granted the assumptions about the permanent and transitory components being normally distributed). In the event, the Spence Commission found 13 “success stories.”<sup>4</sup> Interestingly, India did not make

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3. I say “essentially” because the commission inexplicably used total GDP growth rather than per capita growth. Its criterion was GDP growth above 7 percent, so with population growth usually about 2 percent, I convert this criterion to a per capita growth criterion of above 5 percent.

4. I did 25,000 runs of per capita growth in countries for 45 years, in which growth is the sum of two orthogonal components: a normally distributed permanent component  $N(0.0176438, 0.0155495)$  and a normally distributed transitory component  $N(0, 0.0506495)$ . The means and standard deviations are taken from the random effects regression over 1960 to 2005 of all countries

it on the Spence exercise, suggesting that informal discussions of success stories are even looser than the excessively loose Spence criterion.

The Spence Commission spent a lot of time analyzing these high-growth countries as if they completely reflected fundamentals. However, the other bad news about the bias toward including a large transitory element is that this procedure will likely not even pick the right countries. The same Monte Carlo simulation reveals that about 37 percent of the countries that are in the top 9 percent according to the Spence criteria are *not* in the top 9 percent of permanent country growth rates. The Spence Commission successes (just like the *WDR* success story analyses)—even as they are carefully being picked apart to discern their innermost secrets—are bound to include some ringers that just got lucky.

Why is such flawed analysis pursued by such talented and well-trained economists? Yusuf notes with frustration that “even with good policies, the growth of the typical developing country rarely climbs much above 3 to 5 percent per year [1 to 3 percent per capita].” Yusuf notes that this figure “is impressive by historical standards, but countries in a hurry to catch up aspire to faster rates of growth.” The Spence Commission and the *WDRs* just cannot accept that 5 percent per capita growth is rare (expected to occur in 1.8 percent of the sample). It is easy to see the appeal of a definition that makes this yearned-for outcome 4.8 times more likely, and so economists are often willing to overlook that this increased likelihood is likely spurious.

So we see “growth booms” as attainable because we think they reflect an intentional shift in the country’s fundamentals upward, which could be replicated elsewhere. Again, this assumption could possibly be right, and we could have confirmed it if we had achieved any success in explaining cross-time variations with some variables capturing fundamentals—but we have not done so. Or the *WDRs* could successfully be doing qualitative analysis that would help identify ways to trigger a growth boom. However,

with complete data so as to have a balanced panel (95 countries). The Spence Commission found 13 “success stories,” but the commission does not say how large its sample of countries with the necessary data was. Thirteen would be 9 percent if the sample was 144 countries, which sounds a little too high for countries having complete data. Of course, one run of 100 or so countries is not large enough to give a precise estimate of the percent likelihood of “success”; such a small sample estimate could vary considerably around the expected value computed from a large value of Monte Carlo simulations.

Yusuf's review shows instead the frequent changes in messages, the sloppy vagueness of explanatory factors, and a complete lack of success stories in replicating growth booms through expert advice in the *WDRs*. It seems like the hot hand fallacy may instead explain our unproductive fascination with growth booms.

This heuristic bias is so hardwired into us as humans that we actually do worse than rats on the hot hand fallacy. In a classic laboratory experiment, subjects were shown a light that flashed either red or green. They were allowed to watch for a while and then were asked in successive rounds to predict the next flash. The experiment was rigged so that red was randomly flashed twice as often as green, although the subjects were not told so. The rats pursued the optimal strategy of always guessing red. The humans did not. The humans thought they perceived occasional "hot streaks" of green and would then guess green. As Mlodinow (2008) says "humans usually try to guess the pattern, and in the process we allow ourselves to be outperformed by a rat."

Another heuristic bias is called the "halo effect." This effect is the well-documented tendency (verified in many psychology experiments in the laboratory) to assume that an individual who excels on one dimension will also have superior talents on other dimensions (as subjectively evaluated by the observers in the experiments, for which there is no factual basis whatsoever by the design of the experiment).<sup>5</sup> So, for example, we expect our successful male politicians to also be good husbands (despite abundant evidence to the contrary). And *Fortune* magazine's annual ranking of the World's Most Admired Companies ranks companies on eight very different dimensions, which are all suspiciously correlated with the company's latest financial performance and with each other. So Cisco Systems was highly rated on quality of management, quality of people, innovativeness, and so forth in 2000, when its stock value was high. When the stock collapsed after 2001, observers suddenly detected that every dimension got worse at the same time: the same management and people had overnight become low quality and not innovative (Rosenzweig 2007: 61–62).

One particularly remarkable laboratory finding came from an experiment in which subjects observed two people executing a task. The

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5. This effect is also the subject of an excellent book for nontechnical audiences (Rosenzweig 2007).

experiment had been carefully rigged so that the two people's performance was equal. The subjects were told that one of the two people would receive a large payment and that this assignment would be *random*. The subjects were then asked to describe the performance of the two agents. Despite the subjects' knowledge that the payment was random, they gave superior marks on multiple performance attributes to the agent who received the payment.

In the *WDRs*, a country that excels in achieving high growth is assumed to also excel in having wise leaders, good institutions, entrepreneurial citizens, and so on. The latter characteristics are hard to measure objectively, so these subjective assumptions are hard to prove or disprove. Then, to go from the halo effect to pure circular reasoning, we conclude that these wise leaders, good institutions, and entrepreneurial citizens explain the high growth.

Perhaps the worst single offender with respect to the halo effect and circular reasoning in the *WDRs* was the introduction of the concept of the "investment climate." This concept absorbed one entire *WDR* and yet lacked any theoretical definition or any agreed-upon measurement. Something so vague is bound to be seen wherever good outcomes are happening and then flexibly deployed to "explain" success. Yusuf diplomatically acknowledges these problems: "Nick Stern, the Bank's chief economist from 2000 to 2003, was instrumental in making the assessment of the investment climate in member countries an integral part of the Bank's economic analysis of countries. His conception of the determinants of this climate was sweeping . . ." It was so sweeping as to use what Yusuf politely calls an "eclectic selection of evidentiary material." Yet the appeal of circular reasoning through the halo effect still holds: "Did Botswana, Chile, China, India, and Mauritius as well as the East Asian economies achieve growth mainly by mending the investment climate . . .?"

The halo effect contaminates the endless and increasingly useless analysis of the East Asian success stories. Hong Kong, China; Taiwan, China; the Republic of Korea; and Singapore are very unlikely to be ringers; they almost certainly represent genuine long-run success on growth rates. Yet the halo effect falsely anoints every single aspect of these countries as also being ultra-exceptional and then jumps to the unwarranted conclusion that every such factor contributed to the remarkable success. The successful

East Asian characteristics are subjectively chosen, and it is even worse that they seem to keep changing with whatever is the latest fad in development thinking. Yusuf states:

East Asian economies, by virtue of their successful growth performance, became the ones to emulate. The message distilled from their experience was that market-guided industrialization within the milieu of a relatively open economy could result in rapid growth if industries were able to compete in export markets.

...

[T]he success of a China or a Korea or a Singapore rested on the state's readiness to trim the public sector, encourage private enterprise, and build market institutions, but in each case, the state has remained large, powerful, and interventionist. Directly and indirectly, the public sector encompasses a major share of GDP.

...

Everyone can see that market institutions in successful East Asian industrializing countries are at best functional and at worst weak and minimally supportive. The interesting issue is how an assortment of institutions of varying capabilities and degrees of maturity can, with the help of a strong developmental state, produce good results using the local knowledge that policy makers surely have.

Then, to make things yet worse, we jump to conclusions from an even smaller number of recent observations in which the Gang of Four slowed down:

Other high-performing countries in East Asia have seen their growth performance flag while their institutions have matured, albeit slowly. However, all these economies have also witnessed a decline in investment and a partial withdrawal of the state from the forefront of economic decision making.

As if this were *still* not bad enough, the analysis of the few top performers is contaminated even further by yet another selection bias: the survivor bias. Suppose that a set of drivers was going from New York to Washington, D.C., driving Lamborghinis at 150 miles per hour down I-95. We are in Washington and interview the Lamborghini drivers who arrive. We wax ecstatic at the drivers' trip to Washington in under two hours (compared with the usual minimum of four hours), their willingness to take bold risks, and the overall superiority of the speeding Lamborghini drivers to the other plodding drivers on I-95. Because we observe only the ones who arrive in Washington, we are unaware that many (plausibly a large majority) of the Lamborghini speedsters were pulled over and arrested for

reckless driving and never made it to Washington, not to mention a few who were killed or maimed in traffic accidents because of their insanely risky driving. So on average, the hockey moms driving minivans, who arrive in Washington in five hours or so, outperformed the Lamborghini drivers. Our conclusion that going 150 miles per hour in a Lamborghini is a formula for success in getting to Washington is false; we were led astray by survivor bias.

We induce a survivor bias when we analyze only the top “success stories.” I doubt very much that the success of the Gang of Four is entirely explained by survivor bias. But this example does show the risks of praising every aspect of the experience of the Gang of Four. Some strategies may have been very risky, and by concentrating only on the success stories, we miss the experience of other countries that may have followed the same strategy and crashed and burned. Survivor bias makes the whole methodology of obsessively dissecting every aspect of the success stories very suspect. The remedy is simple: to assess the growth payoff from factor *X*, we should study *all* countries—both those that had factor *X* and those that did not—and ask, “What was the average payoff?” So take, for example, the conclusion sometimes reached that the Gang of Four’s success is due to authoritarian leaders pursuing industrial policies. But the track record worldwide of dictators picking winners is very poor, so why are we so sure that this factor contributed to the success of the Gang of Four? And even if it did, which is basically nonfalsifiable, why do we think it is replicable elsewhere—that finding which is most relevant and *is* falsifiable?

Of course, the general enterprise of assessing all possible factor *X*s to find the secrets to growth success has not been helpful either (see the previous discussion of growth literature), but at least this exercise was not contaminated by survivor bias. We have still learned something from the failure of growth regressions: that there is no universal factor *X* that works everywhere to reliably raise growth—because if there had been, it surely would have shown up as a robust determinant of growth in our extensive effort at cross-country regressions.

On a more positive note, how *should* we deal with a world where there is so much uncertainty about growth determinants? Despite this uncertainty, a substantial number of countries (Australia, Japan, the Gang of Four, and countries in Europe and North America) have already achieved a high level of per capita income, which must reflect good average growth

performance over some suitably long period. The problems with randomness get progressively alleviated the longer we make the period of analysis. Studying the *level* of per capita income rather than growth rates as a measure of success or failure is one way to focus on the long run. The *WDRs* have been forced by the peculiar conventions of development economics to exclude most of the countries that actually succeeded the most at development, and so they rarely invoke any lessons from the long histories of countries that are now rich (except for the Gang of Four), as compared with those that are still poor. In contrast, a slew of papers that were published in top journals in economics studied levels for the whole sample and attributed development success to long-run factors such as property rights, democracy, trade openness, and technological creativity. These papers have their own problems resolving correlation and causation, but they are still clearly superior to the methodology of the *WDRs*; the latter have been led fatally astray by glaring biases in the treatment of transitory components of volatile short- to medium-run growth rates.

Perhaps one way to unify the findings of the levels regressions—a theoretically appealing way to understand how systems can handle vast short-run uncertainty—is to hypothesize that systems that respect individual rights do the best in the long run on economic development. Such individual rights include property rights, rights to dissent from prevailing conventional wisdom, rights to trade whatever with whomever you want, rights to enter new industries and start up new firms, rights to advocate new political directions, and so on. The theoretical appeal of this hypothesis is that individual rights can handle systemic uncertainty by exploiting individuals' superior localized knowledge and powerful incentives to solve their own local problems, which will lead to superior performance even if no policy maker at the top knows how to raise growth rates.

This possibility is obviously just the beginning of such a discussion, and this brief discussion is a long way from confirming this or any other hypothesis. The sad thing about the *WDRs* is that they missed out on such fruitful and deeper long-run discussions about the best systems for achieving development under uncertainty by diverting all their energies to a futile attempt to find patterns in this uncertainty. Are our heuristic biases, like those described here, so strong that future *WDRs* will continue this tragic intellectual failure? As usual, it is hard to predict.



COMMENTARY

# THE EVOLUTION OF DEVELOPMENT ECONOMICS AND EAST ASIA'S CONTRIBUTION

TAKATOSHI ITO

Shahid Yusuf has summarized the 30-year history of the *World Development Report* (*WDR*) in light of the intellectual evolution of economic development philosophy. The review is quite extensive, and it goes beyond a summary of the history of the *WDR*. The reader benefits from Yusuf's insights about how development economics has changed and how political priorities in development have changed over more than the 30 years (the history starts well before the *WDR* was born). Yusuf's writing is filled with the pride that the *WDR* was the first major publication of this kind by an international financial organization.

In the essay, chapter 2 reviews the historical development of the *WDR* from volume 1 to volume 30. Chapter 3 covers crucial issues that have been debated, and chapter 4 explores the direction for the future.

## Comments on the Essay

In explaining the history of the *WDRs*, Shahid Yusuf has successfully identified three different threads: changes in the president and chief economist of the World Bank, changes in *WDR* emphasis, and changes in development economics literature. Those who were remote from politics

in Washington, D.C., and the World Bank would learn with interest how changes in the presidency have altered both the Bank's and the *WDRs'* emphasis.

In 1978, President Robert McNamara and Chief Economist Hollis Chenery created the first *WDR*. According to Yusuf, Chenery “encouraged McNamara to pursue the idea of an annual publication,” and “McNamara entrusted Chenery with the task of preparing a flagship report.” The first report was only 68 pages long. Increasing length has both benefits and costs. Yusuf admits that the report has become so large that few now read beyond the executive summary.

Transition from McNamara and Chenery to President A. W. Clausen and Chief Economist Anne Krueger shifted the Bank's emphasis from a dual objective of growth and poverty alleviation with macroeconomic emphasis on the availability of external finance, to microeconomic advice on getting the prices right. Krueger, “a staunch advocate of market solutions, ... hitched the Bank's approach to development firmly to market forces.” In the 1980s, the political environment of Ronald Reagan and Margaret Thatcher also influenced thinking in development economics. Yusuf notes that the pendulum swung from state help to the market because of the failure of the state in many regions, but the pendulum swung too far because of ideology.

A big change occurred when James Wolfensohn became president in 1995. It is interesting to know that Wolfensohn “desire[d] to contain the influence of economists in the Bank.” Was this economics in the narrow sense? I ask this question because both Amartya K. Sen and Douglass North, who were supporters of Wolfensohn, are economists—Nobel laureates—after all. Joseph Stiglitz, chief economist from 1997 to 2000, is also a Nobel laureate. It must have been a shift of emphasis within economics broadly defined.

In the 2000/2001 *WDR*, Yusuf describes the following new consensus. “Growth was necessary but not sufficient,” which he observes completes “almost [a] full circle ... to the views expressed in the earliest *WDRs*.... It had to be supported by infrastructure and other services so as to build human capital, especially among the poor, and to lessen the inequity of assets and incomes.” Is this observation encouraging or discouraging? The Bank's views shift as Bank executives—president and chief economist—change, as

explained well in the earlier pages. The new consensus is more a matter of course than a big surprise or new insight for Asians and continental Europeans. In those economies, the government has played an important role in education, from primary to advanced, as well as in social and economic infrastructures. Deregulation and liberalization were conducted in a gradual manner. Is going full circle over some 20 years a reflection of the changing ideology and political environment of American economics and politics? Maybe the history suggests that the World Bank should be modifying its tradition so that presidents, vice presidents, and high-ranking economists from France, Germany, Japan, and other non-Anglo-Saxon economies are represented in addition to mainstream fashion in American economics. Appointing a chief economist from China may be a good start.

Yusuf concludes the summary of his 30-year history by noting three shifts over the years:

1. From state directed to market guided
2. From structural issues to sectoral issues
3. From macroeconomic concerns to microeconomic concerns

This summary succinctly captures the changes of emphasis over three decades quite well. They all seem reasonable, but again the balance is important. In this connection, it is commonly believed that a division of labor exists between the International Monetary Fund (IMF) and the World Bank. The IMF is in charge of macroeconomics and sectoral issues rather than microeconomics and structural issues. From this point of view, the shift in emphasis from macro to micro in the World Bank makes sense. This shift may be viewed as a welcome retreat from “mission creep.” But in terms of the second shift, shouldn’t the World Bank continue to address structural issues as well as sectoral issues?

In chapter 3, Yusuf takes up important topics where debates continue. In the section on “Growth through Perspiration,” the debate over the source of growth, whether capital accumulation or total factor productivity (TFP), is reviewed. Certainly, increasing investment is important, but it is difficult for some countries to achieve. TFP is also difficult to promote by policy, although education and knowledge would possibly increase TFP. In the section “From Machines to Institutions,” Yusuf reviews the debate over whether growth comes first and institutions follow or whether good

institutions are a prerequisite for growth. The idea of “Inspired Growth” became popular in the literature of new growth theory, but in reality the bulk of growth comes from capital accumulation. In the section “Resource Balances and Capital Flows,” various issues on use of foreign capital are reviewed. The so-called Washington Consensus is discussed. Then the discussion on the “Role of the State” is a recap of the changes in thinking over time. As Europe has implemented denationalization since the mid-1980s, the role of the state has been reconsidered downward. The *WDR*, however, took a position that privatization and denationalization should be done in a gradual manner. That idea seems to be a departure from the more radical thinking of Big Bang. However, Yusuf seems to disagree with the *WDR* interpretation of the East Asian miracle as an unqualified endorsement of market economy; the government did not withdraw from failing industries. East Asia remains a paradox in the mainstream view of the role of state. The section “Reducing Poverty” describes changing thinking about poverty reduction, from meeting the basic needs in the late 1970s and early 1980s to promoting “pro-poor” development strategy. The pro-poor policy is to promote human capital development that would contribute to decreasing poverty and encourage less unequal distribution of income. The section on “Aid and Growth” gives an important recap on the use and effectiveness of aid—a first step to rid the world of poverty. A consensus hardly exists in the academic literature about how big aid should be.

In a section called “A *WDR* Policy Scorecard,” Yusuf gives a high mark to the *WDR* for having been “powerfully instrumental in raising awareness on the extent of poverty and in exhaustively cataloguing the many ways of erasing it.” It identified the importance of capital investment and, later, human and knowledge capital for growth. But Yusuf admits that the “*WDRs* are silent on what it takes to reach 35 percent rates of capital investment.”

Chapter 4 is about the future of the *WDR*, “Where To Now?” Yusuf lists the future challenges. First, he shows the long-term data of per capita GDP growth of the Republic of Korea and the United States. Both show the steady growth of income with some fluctuations around the trend, with the U.S. growth rate lower than Korea’s (figures 4.1 and 4.2). The point of the figures is whether economic policy made any change over the long-term

natural force (autonomous growth). It seems a bit unfair to show the two more or less successful cases and a good period for Korea. In addition, the long-term data mask the occasional deceleration and acceleration.

Yusuf then explains the importance of institutions. His understanding seems to be much more reasonable than what is commonly seen as the Washington Consensus, however. The following sentences struck me most as a promising starting point for future direction:

The interest of policy makers lies not in whether the state should be large or small or more or less interventionist; the interest is in what specific forms of intervention over a period of time yield the best results under similar external circumstances. The same is true regarding institutions. Everyone can see that market institutions in successful East Asian industrializing countries are at best functional and at worst weak and minimally supportive.

Yusuf raises five specific topics that he considers key for the future of the *WDRs*: “Putting Knowledge to Work,” “Warming Climate, Scarce Water,” “The Geography of Human Habitation,” “Resilient Complex Societies,” and “An Equal Marriage of Politics and Economics.” Each of these topics has a large literature behind it and controversial, ongoing debate in front. This comment is not the place for lengthy arguments; however, let me point out some important missing pieces. As mentioned in the beginning of this section, a puzzle remains: When the “technology of development” is so widely shared—not the least through the *WDRs*—why are there so many laggards? Why is there a great and widening divergence? Why aren’t the ranks of “tiger economies” growing by the year? These questions should be highlighted. The World Bank may put more focus on the least developed countries, defying the logic of development and growth that predicts a takeoff. *WDRs* may have been putting too much emphasis on analyzing successful middle-income developing countries, and the World Bank has been busy lending to those good-credit borrowers. Memory of poor performance of the “laggards” may have been erased with debt reduction. The World Bank may be well advised to shift its resources from China and India—where the private sector as well as the World Bank can do a lot—to Africa and to the poorer countries of Latin America and Central Asia. The future research plan should include a serious analysis of the laggards, however painful and politically difficult it may be.

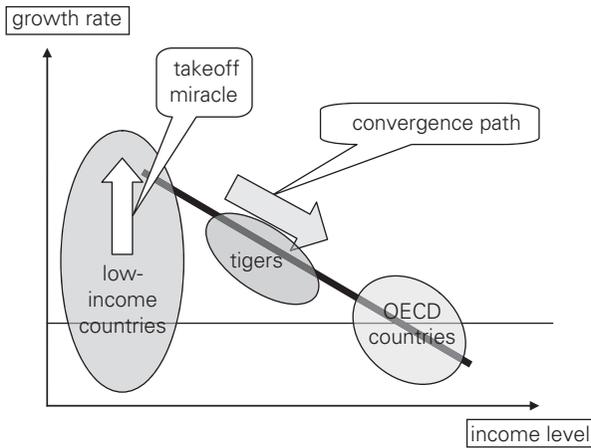
## Big Push, Development, and Growth: A Synthesis

In the past, economic development and economic growth were two different subjects. On the one hand, development deals with long history, institution building, big government policy, structural changes, and transition of industrial structures, for example, from an agrarian economy to a manufacturing economy, and to a service-oriented, advanced economy. Quantifying development success or failure is often very difficult, but case studies are needed. On the other hand, (old) growth economics stresses the commonalities across countries. When a country is equipped with capital, labor, and technology, then growth occurs. With the initial state of income level, the production function, and the saving rate being given, the rest is automatic. No policy is needed. No institution is needed. Convergence to the steady state is autonomous and guaranteed.

With the emergence of new growth theory, the line between development and growth theories has been blurred. Emphasis on institutions—repeatedly mentioned by Yusuf—is a hallmark of new growth theory. Factors that influence growth (convergence) are now on the right-hand side of growth regressions. However, new growth theory emphasizes standardization and quantification so that cross-country regressions can be implemented. Also, regressions need a long enough data series with a fixed starting year, often taken as 1960. Policy change and reforms and structural breaks cannot be treated at the same level of detail as in standard development economics.

The most difficult part of development and growth is the miracle of lifting a low-income country from a low-growth trap to a reasonably high-growth path. The four tigers—Hong Kong, China; Taiwan, China; Korea; and Singapore—made that transition in the 1970s. East Asian economies made the transition in the 1980s, and China and India accomplished it in the early 1990s. Once the country moves from a low-income, low-growth state to low-income, high-growth state, then the “convergence” of growth theory works, unless political meddling hinders the process. The initial miracle—Big Push or takeoff in the old development theory—is the key and not known even in the series of WDRs. The takeoff part desperately needs a building up of institutions, reforms, policy interventions, and so on. Once a country is on the

**Figure C.1: The Transition to a High-Growth Path**



Source: Author.

convergence path, a gradual withdrawal of policy interventions may be desirable, and old and new growth theories apply. This view is shown in figure C.1 (see also Ito 1995, 1998). A similar pattern is empirically established in Ito (2000).

It is obvious from figure 1 that linear growth regressions that mix pre-takeoff countries and tiger-OECD countries would not yield clear-cut results. The importance of institutions matters most for the takeoff.

### Underappreciation of East Asian Experiences

A delicate relationship has existed between East Asia and the World Bank over what is the right development strategy. Policy makers in East Asian economies felt that government interventions in identifying sunrise industries and allocating scarce resources, including foreign exchanges, were helpful in industrialization. However, these government interventions were regarded as a source of distortion and corruption in the rest of the world and in mainstream World Bank thinking. Yusuf mentions the East Asian tigers as a case for openness:

These economies were portrayed as single-mindedly pursuing growth through the export of manufactures, relying mainly on market forces to guide the allocation of resources and exploiting the advantages of greater openness to gain access to

overseas markets and to ensure the competitiveness of their industries. Although the degree to which market forces were responsible for directing resource flows to areas of comparative advantage was far less than was assumed, and although most tiger economies nurtured industries behind trade barriers, the East Asian economies, by virtue of their successful growth performance, became the ones to emulate.

This quotation is a very diplomatic description of the political-economy controversy that took place between East Asia and Washington, D.C., in the 1980s. In this respect, it was not the *WDR*; rather, a special World Bank study that resulted in *The East Asian Miracle* (World Bank 1993) was comprehensive in taking up both views and striking a good intellectual balance.

The high economic growth of the four tigers was followed by the growth of several southeast Asian countries, including Indonesia, Malaysia, and Thailand. As a region, Asia seemed to be a successful case. The Asian crisis of 1997 and 1998 dented Asian confidence. However, since 2001, the Asian region, with China and India, has become the center of world growth again. Asia presents a difficult case for both those who advocate market solutions and those who are more sympathetic to government interventions. The *WDR* could have taken East Asian experiences more carefully with respect to the true reasons for success and transferability of the lessons to other regions. The crucial differences between the Asian developing countries and developing countries in other regions, especially the laggard countries, should be identified and analyzed.

In summary, the East Asian miracle seems to be a miracle still—a miracle of takeoff, a transition from a low-income, low-growth state to low-income, high-growth state. That magic should be the focus of the *WDR* in the future, and the experiences of East Asia, including China and India, will be worth taking seriously.



## COMMENTARY

# THE WORLD DEVELOPMENT REPORT: DEVELOPMENT THEORY AND POLICY

JOSEPH E. STIGLITZ

I had responsibility, to varying degrees, for five different *World Development Reports* (WDRs). The first, on the role of the state, was begun by my predecessor; the next two, *Knowledge for Development* and *Entering the 21st Century*, I saw through from beginning to end; and the final two, *Attacking Poverty* and *Building Institutions for Markets*, were initiated while I was chief economist but completed after I left.

Many of the WDRs that had gone before focused on a particular aspect of development, a particular sector—education, health, agriculture. I saw the WDR as an opportunity to redefine broader views about development.

One of the hardest struggles—and I was only partially successful—was to change the concept of the WDR. Traditionally, it has summarized “received wisdom.” The goal was to summarize the received wisdom in a few, easily understood “messages.” The messages, in turn, were intended to set the policy agenda: they were messages that World Bank staff could bring to developing countries around the world. I was worried about this approach for several reasons. It smacked too much of a “one-size-fits-all” cookie-cutter approach—unless the messages were so anodyne as to be almost meaningless. And I was very much of the view that the role of outside advisers was to share experiences and general principles. Democratic development required that each country make its own decisions—in the

simple way we put it, “the country was in the driver’s seat.” Our role was to help the country think through these decisions.

In this perspective, the objective of the *WDR* was to begin a global dialogue, a democratic conversation about some of the most contentious issues in development. It did not bother me that we might not know the right answer. Indeed, it bothered me more that we sometimes pretended to know more than we did. To me, the role of an outside adviser was more to ask the right questions—or to help those in the developing countries ask those questions—than to give the right answer.

### **The Role of the *WDR* in Thinking about Development More Broadly**

To me, then, the *WDR* was an instrument to begin the change in thinking about development. Even before I came to the Bank, I was convinced that the Washington Consensus doctrines represented the wrong approach, at least for many countries. The economic theories on which the Washington Consensus rested had long been discredited. My own work on imperfect and asymmetric information and incomplete markets had contributed to undermining the theoretical foundations. And the World Bank’s own report on the East Asian Miracle—on which I had worked—had shown that the most successful countries had not followed these recipes (World Bank 1993). But a gap remained between these insights from modern economic research and the perspectives of many policy makers.<sup>1</sup> I knew that many people in the Bank still believed in those ideas, and I saw the *WDR* as a way of beginning a global conversation—inside and outside the Bank. Not surprisingly, as each *WDR* went through the process of development within the Bank, difficulties were encountered. Many were uncomfortable with the ideas; many with the underlying economic analysis, which often exposed the limitations of models that had traditionally been relied on by those within the Bank; and many more were uncomfortable with the policy conclusions that emanated from the analyses.

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1. I used the keynote addresses to the Annual Bank Conference on Development Economics in the first two years that I served as chief economist to focus attention on that gap—and to work to reduce it (see Stiglitz 1998a, 1999).

But the controversies were, perhaps, even more tense at the level of the Board. We touched on raw nerves. For the first time, we began to question the positions taken by the United States or other Group-of-Seven countries. To me, it was clear: we were international civil servants representing the interests of the developing countries. Inevitably, sometimes that would go against the position of the United States, whose policies were often driven by special interests. I was perhaps more aware of this than previous chief economists, who had come from academia. I had come directly from serving as chairman of the Council of Economic Advisers under President Bill Clinton. I had seen these special interests at work. The council had argued, on a number of occasions, against the positions taken by the U.S. Treasury and the U.S. trade representative. At the time, I was lucky, because the U.S. executive director, Jan Percy, was also focused on the concerns of the developing countries, and she was sufficiently influential within the administration that she could push back against Treasury, when necessary.

### *Knowledge for Development*

Some examples illustrate. In the 1998 *WDR* on knowledge, we had to discuss, if ever so briefly, the role of intellectual property. I had opposed Trade-Related Aspects of Intellectual Property Rights (TRIPS), the intellectual property provision of the Uruguay Round, when I was on the Council of Economic Advisers.<sup>2</sup> So, too, had the U.S. Office of Science and Technology Policy. We thought it was bad for U.S. science, for global science, and for developing countries. I had seen firsthand how TRIPS was shaped, not by the concerns of U.S. science, but by our pharmaceutical and entertainment industries. I had no illusions: it was special-interest legislation. But my concerns about the adverse effects on developing countries were strengthened after I came to the World Bank. It was increasingly clear that what separated developing countries from developed countries was not just a gap in resources but a gap in knowledge, and it was imperative that this gap be closed. I had come to that view when I participated some years earlier in the World Bank's study on East Asia.<sup>3</sup> The unprecedented success of these

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2. Part of my opposition was in fact based on my own research on the determinants of technological progress. It was simply not true (as the advocates of stronger intellectual property rights seem to claim) that stronger intellectual property rights lead to faster innovation and growth.

3. A version of our report was published as *The East Asian Miracle* (World Bank 1993).

countries was based on their closing the knowledge gap through heavy investments in education and technology. I reformulated that idea in my Prebisch lecture (Stiglitz 1998b) on development as transformation, and it is a view I have continued to develop with my colleague Bruce Greenwald.<sup>4</sup> It was a view that was strongly shared by the president, Jim Wolfensohn, who saw the Bank as a knowledge bank. But TRIPS made access to knowledge more difficult, adding new impediments in the struggle to close the knowledge gap.

In the *WDR*, we called for a more balanced view of intellectual property rights, recognizing that the “optimal” system for developing countries would be different from that for more developed countries. That call has now been taken up in the World Intellectual Property Organization, where the developing countries have called for a development-oriented intellectual property regime. In the decade since the *WDR* on knowledge, the limitations of America’s intellectual property regime have come to be recognized even in the United States, and there are increasing calls for reform (see, for instance, Stiglitz 2006: chapter 4; see also Stiglitz 2004b, 2007).

I anticipated, though, that we would encounter trouble from the United States even with our carefully phrased call for a balanced intellectual property regime. But I also knew that we should be criticized by developing countries for not being more critical of TRIPS. We did our homework, consulting extensively with various executive directors. So when the United States launched its expected attack, saying that the *WDR* should take a stronger stance in favor of “stronger” intellectual property rights, several developing countries were prepared to launch a counterattack, urging us to take a more critical stance. So effective was their attack that the United States staged a hasty retreat.

### *Corruption*

My first *WDR* also engendered a political controversy, because it raised, for the first time, the issue of corruption. This *WDR*, like most of the other *WDRs*, reflected both my interests and concerns and those of the Bank president. The year before coming to the Bank, I had given a keynote

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4. In particular, we have asked, how can one design an economy (society) to best enhance its learning capacities? See Greenwald and Stiglitz (2006).

address (Stiglitz 1997a) at the Annual Bank Conference on Development Economics on the issue of the balance between market and government (see also Stiglitz 1997b).

In my work on the economics of the public sector, I had helped develop the market failures approach to the role of government (see Atkinson and Stiglitz 1980; Stiglitz 1986): markets often failed to yield efficient (let alone socially just) outcomes; well-structured government interventions could make everyone better off. I had attempted to identify what the government should do and how it should do it. While I was at the Council of Economic Advisers, I became involved in another project: Vice President Al Gore's initiative on "Reinventing Government," which tried to make government more efficient, more effective, and more responsive to citizens' wants and needs. If one believed (as I did) that the government had an important role, it was important for the government to perform its role well.

My own research (and that of others) had ended the theoretical debate about Adam Smith's invisible hand: markets were not, in general, efficient (see Greenwald and Stiglitz 1986; for a more general interpretation, see Stiglitz 1991). But many conservatives responded that, while government *might* effect a Pareto improvement, in practice, governments typically made things worse. Clearly, *sometimes* they did so, but also, in the most successful countries, the government had played an important role. However, if the government was to play the role it should in helping to create a fair and efficient society, one had to do what one could to improve the efficiency and effectiveness of the public sector.

In developing countries, one of the factors impeding the effectiveness of the public sector was corruption. The Bank's charter precluded the Bank from getting engaged in *political* matters, and some on the Board viewed corruption as a matter of politics—not economics. I had thought that the boundary was less clear than it seemed. To me, the issue of privatization of social security was an intensely political matter; so too was the issue of whether central banks should focus exclusively on inflation. There had been intense political fights on these issues in the United States—in which the Clinton administration seemed to take the opposite view from that taken by the World Bank and the International Monetary Fund (IMF). The administration's research on corruption showed that corruption

affected economic growth, and economic policies (such as wages paid to civil servants) affected the level of corruption. This research demonstrated that corruption was well within the remit of the Bank. The *WDR*, and the research that went into it, thus had a profound effect on the direction of Bank and IMF policy: after Paul Wolfowitz became president, the Bank seemed to behave as if corruption was *the* most important development issue. Although the Bank clearly went overboard, and although there was undoubtedly some corruption in the corruption agenda, that *WDR*'s effects on the Bank and on the broader developmental dialogue were deep and long lasting.

### *Poverty*

Every 10 years, the Bank has been doing a *WDR* on poverty. The Bank had helped focus attention on the large number of people in poverty—a focus that increased with the Millennium Development Goals enunciated just as I was leaving the Bank. The *WDR* was a natural follow-up to work we had been doing, called *Voices of the Poor* (Narayan and others 1999, 2000; Narayan and Petesch 2002). We had asked, “What aspects of their lives contributed most to the suffering of the poor?” We discovered—not surprisingly—that the poor were concerned not only about their lack of income but also about their lack of security and lack of voice. We had concluded that the exclusive focus on income (as in the 1990 *WDR*) was wrong, and under the direction of Ravi Kanbur, we decided to take a broader perspective. Not surprisingly, again, the approach drew political criticism—including from the U.S. secretary of treasury (and former World Bank chief economist), Lawrence Summers.

The debate was part of a broader development controversy. Some argued for trickle-down economics: countries should maximize growth, and that would be the most effective way of reducing poverty. Most of those within the Bank had moved away from that view. The evidence was overwhelming that growth did not *necessarily* reduce poverty. Trickle-down economics did not *necessarily* work. If growth was accompanied by increasing inequality, poverty could actually increase. The problem was that many of the Washington Consensus policies that the Bank and the IMF had argued for in the past had contributed to—or had at least been associated with—increasing inequality. And that was especially true of policies like capital

market liberalization, which the U.S. Treasury had advocated. Such policies had not led to any or much increased growth<sup>5</sup> but had led to more instability, and the greater instability had led to more inequality—which was particularly pronounced in the context of the East Asian crisis.

To be sure, one could not have sustained poverty reduction without growth, which was why we had begun to focus on poverty-reducing growth strategies. The Comprehensive Development Strategies on which the Bank was then focusing<sup>6</sup> called attention to important complementarities that had often been missed in the past: trade liberalization might, for instance, by itself lead to more poverty, because jobs were destroyed faster than they were created. Only if accompanied by policies to enhance access to credit and technology might trade liberalization lead to reduced poverty.

Thus, the 2000/2001 *WDR* suggested not only that the policies being pushed by the U.S. Treasury might be bad for poverty in the narrow income sense, but also that they were even worse if poverty was more broadly conceived. For if capital market liberalization or trade liberalization was associated with greater economic instability, then the insecurity to which it gave rise might contribute even more to the worsening plight of those at the bottom.

Other policy controversies were also directly implicated. The Washington Consensus policies had argued for privatization of social security, but private social security accounts left individuals exposed to the vagaries of the market (all too evident in the 2008 market crash) and did not even insulate against the risks of inflation. Unionization and collective bargaining, part of the core labor standards around which broad global consensus existed, had attempted to increase worker security. Yet Washington Consensus policies had often argued for greater labor market flexibility, code words for eliminating or reducing hard-fought-for social protections. Although the evidence and the theory of the effects of such policies on growth or stability were ambiguous (Stiglitz, Easterly, and Islam 2001; see also Stiglitz and

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5. This view has now become accepted even by the IMF (see Prasad and others 2003; Stiglitz and Ocampo 2008; Stiglitz and others 2006). My theoretical work had explained why that might be so (see, for example, Stiglitz 2004a).

6. The intellectual foundations of these strategies were, in part at least, provided by my Prebisch lecture (Stiglitz 1998b).

Rey 1993), such policies clearly may contribute to greater poverty in the broader sense.

Similarly, one of the criticisms of IMF (and, to a lesser extent, World Bank) loans, with their extensive conditionalities, is that they undermined democratic processes—reducing the scope for the voice of those affected. But such policies could be criticized as contributing to “poverty” in the broader sense, which recognizes the role of voice.

Our commitment to giving more voice to those in the developing countries—and making the *WDR* a vehicle through which democratic dialogue on development issues would be engendered—was reflected in the process of writing the *WDR*. We had organized extensive consultations throughout the world, posting each draft of the *WDR* on the Internet. This approach served us well in the ensuing controversy.

The U.S. Treasury demanded that the income aspect of poverty be given primacy. This demand went against the global consensus that had been generated in the process of our global discussions. Ravi Kanbur’s resignation created a global furor. The Bank had to give weight to the process by which the *WDR* had been written, including the large number of consultations with scholars, government officials, and nongovernmental organizations in developing countries and in the development community. In the end, the Bank was forced to accept as the final draft a version that was close to that *before* the U.S. Treasury had unilaterally demanded its invasive changes.

### *Institutions*

The 2000/2001 report was undoubtedly the most controversial *WDR*. But almost every *WDR* involved internal debate and discussion—precisely because the *WDRs* involved issues of importance where important differences of opinion existed.

Under Wolfensohn, the Bank had moved beyond projects to policies—and beyond policies to institutions. As the 1997 *WDR* emphasized, the public sector made a difference. But some governments—and some governmental institutions—were more successful than others. Some were less corrupt or corruptible than others. Within economics, the awarding of the Nobel prize to Doug North highlighted the importance of institutions.

But what makes for good institutions? And how can we create them? During the East Asian crisis, there was much discussion of the weaknesses of the East Asian institutions—financial institutions, financial regulatory bodies, corporate governance. Many were told to imitate U.S. institutions. Since then, confidence in what makes for good institutions has weakened. The Enron and WorldCom scandals highlighted weaknesses in accounting, financial institutions, and corporate governance in the United States. But the subsequent passage of the Sarbanes-Oxley Act gave renewed confidence in the institutions of the United States: its public institutions had faced up to the underlying weaknesses in corporate governance and had taken action. I was more skeptical. I had argued that perhaps the most fundamental flaw had to do with stock options, which provided incentives for bad accounting and short-sighted behavior (Stiglitz 2003b). But nothing was done. I and others had worried too about the bonus system that had encouraged excessive risk taking and the lack of regulation. I had worried that securitization was increasing problems of information asymmetries and decreasing the quality of lending (Stiglitz 2003a). Few would say today that the institutions of the U.S. financial sector—its rating agencies, its regulatory authorities, or its commercial or investment banks—are exemplary.

Although these ambiguities formed a backdrop to the heated debates in the formulation of the *WDR*, the real controversy concerned the role of institutions: did they “fill in” for market failures, or did they often help to preserve existing inequalities, frequently giving rise to inefficiencies in the attempt to do so? My own research had shown that the naive view that nonmarket institutions helped to remedy market failures (for example, by providing insurance when markets failed to do so) was wrong or at least needed to be more nuanced. Nonmarket institutions could actually be dysfunctional, enlarging market inefficiencies (Arnott and Stiglitz 1991).

But the distributional critique of institutions was, in a sense, even more fundamental.

### *Urbanization*

Not all the *WDRs*—even those that raised big issues—were controversial. As we ended the 20th century and looked toward the next, we decided to use the 1999/2000 *WDR* to focus on some of the big megatrends and, in particular, on urbanization. Historically, most people living in developing

countries have lived in the rural sector. And even today, the vast majority of those in poverty live there. Yet there have been large migrations from the rural to the urban sector, and in some places (such as China), such rapid development of some parts of the rural sector has occurred that it has become urbanized.

Urbanization—and development urbanization—bring their own advantages (ideas can spread more rapidly) and problems (especially with respect to housing, the environment, and transportation). This *WDR* helped push forward the thinking that will be needed if these problems are to be addressed.

## The *WDR* and Specific Policy Issues

Although to me the most exciting aspect of the *WDR* has been the role it has played in rethinking basic issues of development, in doing so, it has helped the rethinking of numerous specific issues. I mention four that were highlighted in the *WDRs* with which I was involved. Sometimes a case for a particular policy was built up over several years—and over several *WDRs*.

### *Primary versus Secondary Education*

The Bank had long emphasized the role that education (including female education) played in development. It had—rightly, I think—emphasized primary education. It had done so because many developing countries spent large fractions of their education budgets on tertiary education, of benefit only to the elites. But the Bank had, we concluded, gone too far. The countries that succeeded best in development (those in East Asia) had also invested heavily in higher education. They had realized that one had to close the knowledge gap, which required individuals with high levels of education.

### *Health*

The 1993 *WDR* focused on health. Health is an important determinant of productivity. Access to health care is an important determinant of health, but knowledge about health is as, or even more, important, one of the points emphasized in the 1998/99 *WDR*. For instance, many people in

developing countries suffer from inadequate nutrition, but even within their existing budgets, such countries could do better. Knowledge about how to avoid dehydration was critical in preventing a large fraction of children's deaths from diarrhea. Knowledge about where to place latrines in relation to sources of drinking water could prevent many gastrointestinal diseases.

### *Social Insurance*

The 1997 *WDR* argued that developing countries suffered as often from too little government action, from a failed state, as they did from too much government. The 1998/99 *WDR* helped to explain one pervasive source of market failure of particular importance in developing countries: imperfect information. This is of especial importance in helping to explain the absence of insurance markets. Finally, the 2000/2001 *WDR* emphasized the importance of security—including health and economic security—as an aspect of poverty.

Together, these three *WDRs* provided a compelling case for government action in the area of social insurance—an area to which the Bank was paying increasing attention, especially in the context of the problems arising from the East Asian crisis and the transition from communism to a market economy.

The debate on this issue within the Bank has not been easy, with some arguing for a more limited role than others do.<sup>7</sup> Although the Bank had pushed many countries to privatize their social security systems, the outcomes of some of the privatizations were less than fully satisfactory. The problems in transition were not trivial. Because the government was deprived of essential cash flows, severe fiscal problems were artificially created, in some cases contributing to severe economic crises. Argentina is an admittedly controversial case in point. Many blamed its crisis on its fiscal problems, but had it not privatized its social insurance system, its budget would have been nearly in balance. Transaction costs turned out to be large. And the imposition of burdens of risk on individuals was far from trivial. When the United States had a national debate on privatization of its social security system, support was overwhelming for keeping it public; in

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7. A sense of the debate is given by *New Ideas About Old Age Security* (Holzmann and Stiglitz 2001), and especially chapter 1 (Orszag and Stiglitz 2001).

the 2008 crash, there was a national sigh of relief that social security had not been privatized.

### *Access to Finance*

The standard economic model that ignores information imperfections may work well in some countries in some sectors; it does not work well in most sectors in most developing countries. That was one of the important messages of the 1998/99 *WDR*.<sup>8</sup>

The 2000/2001 *WDR* emphasized that growth might be necessary to reduce poverty, but it was not sufficient. One had to look for growth policies that alleviated poverty and enhanced equality. In the case of some policies, a trade-off between growth and equality may not even exist. One example is providing universal education. Making sure that every child can live up to his or her potential reduces poverty, enhances equality, and promotes growth.

So, too, does access to finance. Standard economic models denied the possibility of credit rationing. Yet modern economic theories, based on the economics of information, highlighted in the 1998/99 *WDR*, explain why it is likely to occur and why alternative ways of providing finance, such as the peer-monitoring microcredit schemes pioneered by the 2006 winner of the Nobel prize, Muhammad Yunus and the Grameen Bank and the Bangladesh Rural Advancement Committee, are likely to be far more effective (see also Stiglitz 1990).

The Bank has taken an increasingly active role in promoting micro-credit and access to finance, an agenda to which the 1998/99 *WDR* may have contributed.

## **Concluding Remarks**

Throughout its history, the *WDR* has played an active role in shaping thought and policy, both within the World Bank and in the wider development community. It was sometimes overly ambitious, hoping to be able to summarize in a few clear messages the received wisdom on a key aspect

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8. Like most *WDRs*, this one was built on extensive work done within the World Bank in earlier years (see, for instance, Hoff, Braverman, and Stiglitz 1993).

of development. The world is too often too complicated for that to be done. When the *WDR* did so, it risked reemphasizing the obvious or what was well accepted, or conducting the discussion at such a high level of abstraction as to be of limited use. Occasionally, it became the publication vehicle for official Bank doctrines—a summary of beliefs of the moment. Even here, it served a helpful role, at least for students of the evolution of economic thought, for they could see how thinking about development evolved over the years.

But to me, at least, its greatest contributions occurred when it helped to frame controversial issues, when it pushed the boundaries of thinking, when it opened up new frontiers—thinking about issues that had previously received too little attention—when it sparked a global debate. In those cases, the *WDR*'s effect was not only immediate, but also likely to be long lasting.

