

Evaluation of World Bank Research Daron Acemoglu, MIT

This document provides the evaluation of all the projects that were assigned to me (and were either sent to me in the mail or were included in the CD I received; papers on two of the projects were not included, and thus I am not evaluating these). In evaluating these projects, as indicated in the terms of agreement, I looked at the main piece of work highlighted in the instructions sent to me, and in some cases I looked at one or two supporting pieces of research as well. Since these are supposed to be the flagship research output from parts of the World Bank Research Team, throughout I have applied very high standards. To give a benchmark, for papers, I have applied similar standards as I would for papers submitted to the *Review of Economics and Statistics* (of which I am one of the editors). For books and other output, I have adjusted the standards somewhat, taking into account that they are not purely academic and they are trying to reach a broader audience.

Summary

In all of the research I have evaluated, with one exception, I have not found any that would be of high enough standard to be published in a top field journal or in a general interest journal like the *Review of Economics and Statistics*. Many of the papers are well written and are aware of the relevant literature, but are not necessarily at the frontier of research. Having said that, some of those are quite competent surveys or minor contributions to well-defined literatures. In another group of papers, I have found serious deficiencies in terms of empirical work or conceptual framework.

The books and the longer pieces of work that have emerged from some of the projects that I was asked to evaluate are somewhat more mixed. Many of these books contain useful details and would be of interest to the policy-making (semi-academic) public. The works on civil war are especially notable because this is an area that has not received enough attention from academic or policy circles. Thus it is laudable that a team of researchers at the World Bank has worked on this topic, bringing detailed information from individual country studies. Given this mixed situation, the book projects are somewhat more difficult to judge. Overall, they do not make major contributions to the research frontier, but they are useful in disseminating information and drawing attention to a very important topic.

On the basis of my reading, I would think that it would be more productive for the World Bank to focus its research in a very few areas where high quality research can be produced. In other areas, the research should explicitly apply already developed frameworks to data that are available to World Bank researchers. My own experience and interaction with World Bank researchers suggests that there is plenty of talent to carry out both types of research. Thus the problem is one of finding the right type of research for World Bank researchers to focus on productively. The current style of research in the World Bank, a mixture of applied research (in line with the World Bank's objectives and interests) combined with an attempt to contribute to the research frontier, has not been extremely successful.

Evaluation of Individual Projects

Sample 13 East Asia Prospects by Shahid Yusuf

Two books: Innovative East Asia: The Future of Growth, and Under New Ownership: Privatizing China's State-owned Enterprises

A. Objectives

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

The study tries to synthesize sources of innovation and rapid growth in East Asia. In light of a range of evidence from industry-level studies, measurement of human capital and evaluation of the structure of East Asian economies, it reevaluates the economic prospects and policy choices available to a number of East Asian economies in the aftermath of the 1997 financial crisis. It has an explicit (macro) policy focus, but it also touches on issues related to firm governance, trade systems, education, and dynamics of innovation and technology adoption.

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

Broadly, yes. The questions might be viewed as central to one conception of World Bank research, where the role of the World Bank would be to give policy advice to already functioning economies about how to support certain sectors and make transitions across different sectors and support innovation, etc. This view of World Bank policy may be disputed, however. An alternative would maintain that it should not be the World Bank's job to give advice about which industries a country should invest in and how they should run their innovation policy. The World Bank's technical assistance might be more usefully limited to transition economies where there is need for transfer of know-how of market systems and micro help for the most disadvantaged parts of the world. If this is the perspective, then the emphasis in the current book about demand management and industrial policy as well as innovation policy may not be as central to the focus of World Bank policy.

B. Design and Implementation

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

The book is mostly a survey; thus there is no innovative methodology. It surveys a large literature, and clearly covers parts of the literature. How useful these types of surveys written at a level not quite accessible to the lay public and at too low a level for academics really is open to question.

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

There is good institutional detail on the countries in question, although the frontier research on innovation, political economy, financial problems and corporate governance is not necessarily reflected in the book.

3. How reliable were the data?

The data used in the book are standard national accounts type data combined with various different measures of human capital indicators, international test scores, wage data, international trade data, etc. Most of these data are standard; even if there is some question about the reliability of some, this is not something the authors of this book could have avoided.

4. Are the conclusions consistent with the research findings?

Yes.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Broadly, yes. The book is written for an audience that is well versed in economics but not researchers. For this audience, it could be deemed to be at the appropriate level. How large this audience really is in practice is a question mark, however.

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

I don't believe so. I think as with much of the other World Bank research I have seen, there is too much of a willingness to jump to policy conclusions from relatively little evidence that would be open to multiple alternative interpretations.

D. New Results

1. What are the key findings of the study?

The key findings of the study involve the policy measures that are necessary for a rapid and efficient transformation of East Asian economies, especially in the sphere of innovation and human capital. These are not really findings, rather interpretations based on a range of interesting correlations.

Under New Ownership: Privatizing China's State-owned Enterprises

A. Objectives

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

The book focuses on the implications and the process of the recent waves of privatizations in China, and is informative about the process of reform toward a capitalist economy in China and how such reforms can be made most beneficial for economic growth.

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

Absolutely yes. This is a good case of World Bank expertise being potentially useful. Although I believe political economy issues are ultimately much more important in determining the direction that the form of privatization will take in China, clearly the process of economic transformation in China does require technocratic help from outsiders. The World Bank is in a good position to

provide such help, and this book is a good survey of what is known and a good introduction to what can be done.

B. Design and Implementation

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

The methodology was not meant to be innovative, but it is appropriate and well executed.

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

By and large, yes. The book seems to build on existing work on transition economics and major works on China and transition economics are cited and well interpreted. Some of the issues touched in the book, however, are much more connected to research in political economy, corporate governance, and law and economics. There is very little in the book on these topics. So in some sense, while the book covers the “old style” research relatively well, it is quite a bit inside the frontier in terms of new findings and approaches.

3. How reliable were the data?

The data presented in the book are quite fascinating. To the extent that I understand, this is new and very useful data. I am not in a position to judge whether the data are reliable.

4. Are the conclusions consistent with the research findings?

By and large, yes. The conclusions about the benefits from market-oriented reform and privatization seem to be well supported by the evidence. The conclusions are a bit shakier when it comes to obstacles that the process will face. The importance of politics is not necessarily emphasized enough, although according to many this could be a main factor in determining the pace of economic reform in China.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Yes, absolutely. The book is well written, and it is acceptable for the audience for which it is intended, which is policy makers and the potential general public interested in China (and to some degree, already informed about some of the processes going on in the Chinese economy).

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

I don't believe so. Again, there is too much of a tendency to jump to policy conclusions.

D. New Results

1. What are the key findings of the study?

The main finding is the importance of market-oriented reform and privatization in transforming the Chinese economy. Although this process is fraught with dangers and creates a number of

imbalances, the study shows the beneficial effects, and also documents the necessary supplemental changes that would be useful.

Sample 23

Evaluating Public Policies and Projects under Uncertainty to the Application of Real Auctions, Odin Knudsen, Pasquale Scandizzo Main Project: Biotechnology Risk and Project Interdependence

A. Objectives

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

Not clear exactly, this seems to be a hodge-podge. It emphasizes the importance of risks and opportunities in biotechnology and potential externalities and irreversibilities. It's not clear why agricultural biotechnology is any different, nor is it clear what the contribution here is supposed to be.

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

No. I could not see any obvious relationship.

B. Design and Implementation

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

It certainly is not innovative. This is very standard material. It is not particularly well explained, and at least in this paper, relevant references are missing. Somebody reading this paper would not realize that this is one of the most standard methods used in investment analysis (none of the key papers or books is referenced). Some of the other papers in this project do reference crucial sources. The methodology is appropriate for answering questions related to irreversibility of investment, but again, whether this is something that is important in the context that it is being discussed is not clear, and even if it is, all of these methods are well understood in the literature.

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

Again, this project does not quite reflect this. Other projects in this research program do.

3. How reliable were the data?

There are no data here.

4. Are the conclusions consistent with the research findings?

There aren't really any important conclusions here.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

It is not clear who the intended audiences are. Only academic economists or those with a Ph.D. would follow or be interested in the continuous time modeling of irreversible investments. For those, the models are trivial and uninteresting.

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

No. From the standard models some quite strong policy recommendations are drawn about over and under-estimation of project net benefits. It seems totally misplaced.

D. New Results

1. What are the key findings of the study?

Not clear.

Sample 23

Odin Knudsen, Pasquale Scandizzo

Uncertainty and Economics of Concessions for Natural Resources

Previous comments about the project on biotechnology risks and project interdependence apply to this paper as well. The purpose of the paper is unclear. Uncertainty and externalities are emphasized, but all of the results are fairly well known. This paper is better in that it at least acknowledges some of the existing literature. All of the other criticisms apply.

Sample 23

Maria Santoro

Main project: Investment, Irreversibility and Economic Fluctuations

A. Objectives

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

Not clear. This is just a straightforward survey of an existing literature. To my best understanding, there is nothing new here, and the material is already surveyed in other places.

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

I don't believe so.

B. Design and Implementation

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

There is no new methodology and no innovation. It is simply a survey.

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

Yes. The project shows a very good command of existing literature, and many different branches of the existing literature are well surveyed.

3. How reliable were the data?

There are no data here.

4. Are the conclusions consistent with the research findings?

No major conclusions, just a survey of existing research and approaches.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

It is not clear who the intended audience is. It is certainly not academics, since this material is well known and better surveyed in other places. I cannot believe it is for policy makers or non-World Bank personnel. So it is unclear who the audience is.

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

There are no policy recommendations.

D. New Results

1. What are the key findings of the study?

No major key findings. Just a survey and summary of the literature, with a conclusion that more on the aggregating of microeconomic dynamics of investment is necessary.

Sample 23

Odin Knudsen, Pasquale Scandizzo

Main project: Trade Secrets, Patents and Options

A. Objectives

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

The paper looks at the well-established question of protecting intellectual property rights with different instruments, with the new twist being that there is dynamic uncertainty. The problem is set in the context of a real options model (similar to the literature on investment under uncertainty and optimal stopping problems). It compares trade secrecy and legal action against imitators as potential methods of protecting intellectual property.

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

By and large, no.

B. Design and Implementation

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

The methodology is well executed, but not very innovative. The only innovative part of the project is the explicit comparison of trade secrets and legal action. Trade secrecy is discussed in a number of papers in the IO literature, and these papers do not use the same type of comparisons as here.

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

No. An entire industrial organization literature on protection of property rights is entirely ignored.

3. How reliable were the data?

There are no data here.

4. Are the conclusions consistent with the research findings?

There is too much jumping to policy conclusions from an incomplete analysis of a simple model.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Again, it is unclear who the intended audience is. The level of analysis is not sufficient for publication in any respectable scientific journal. Some of the material is certainly too heavy for policy makers and non-Ph.D. Bank staff.

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

There should not be any policy implications from this type of model. The policy implications are too much stretched in my view.

D. New Results

1. What are the key findings of the study?

The study characterizes a number of parameter conditions under which trade secrecy may be useful, and looks at the value of legal action as a function of the prevalent intellectual property rule and legal costs to recover damages.

Sample 23

Odin Knudsen, Pasquale Scandizzo

Social Standards, Poverty and Redistribution

Same comments as the above project apply here as well.

Sample 53
Conflict and Development
Paul Collier, Nicholas Sambanis
Understanding Civil Wars, Volumes 1 and 2

These two books contain interesting case studies of civil war, which is a very important and under-studied topic. They are thus welcome. Both volumes build on a number of papers by Collier and Hoeffler, for example, "Greed and Grievance in Civil War," and "Military Expenditure: Threats, Aid and Arms Races." Given this, I will answer the questions specifically for these two papers, which also provide the framework for all of the study's applied specific countries, and then I will comment on the books separately in more general terms.

A. Objectives

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

The "Greed and Grievance" paper tries to empirically distinguish between various determinants of civil war. It brings a conceptual framework based on the "Crime and Punishment" literature, where potential factors are distinguished into opportunity and grievance factors (pull and push factors). Using this framework, it tries to judge both whether opportunity or grievance factors are more important, and also which characteristics of countries make them more likely to have a civil war.

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

Absolutely yes. This may be the most important question for development in Sub-Saharan Africa and perhaps in other parts of the world.

B. Design and Implementation

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

No. The theoretical framework is rather simple and does not build on anything that has been done in the past 20-25 years. The econometric framework is very deficient. It has a number of serious conceptual and methodological problems. First of all, at the end the regression is one of endogenous variables on endogenous variables. But all of the results are interpreted as causal effects. The main regression models pool data across different periods, but run simple OLS regressions without including time dummies, country dummies or adjusting the standard errors for the fact that there are multiple observations from the same country without any type of fixed or random country effects. (Some robustness checks include fixed and random effects, but still fail to include time effects---except somehow for the logit analysis---and the fixed-effects models are difficult to interpret, since they do not include time dummies, thus they are being largely identified from world-level time-series changes.)

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

The papers written in the early 2000s are not at the frontier in the econometrics or applied research of the time. Much of the research in the 1990s and 2000s has applied much more sophisticated methods and has paid much more attention to endogeneity issues.

3. How reliable were the data?

I think the data set on civil war is as reliable as exists, and it is a very useful resource.

4. Are the conclusions consistent with the research findings?

I believe generally not. Contrary to the claims in the paper, the regression evidence does not test any well-specified hypothesis, and the correlations that are interpreted as causal effects are really no more than correlations.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

The papers are written in a style that would be appropriate for international relations journals. These papers would not be published in any second or third-tier economics journals.

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

It is too early to jump to policy conclusions.

D. New Results

1. What are the key findings of the study?

The key finding of the study is that the main determinants of civil war are existence of natural resources, GDP per capita, and population size. GDP growth and democracy have some effects, but those are limited. Ethnic variables, geography variables and income inequality do not seem to have as much of an effect, but some of these may be because of measurement error or quality of data.

Comments on the Book

Now returning to the two volumes on understanding civil war, the bottom line is that these are very important books because of the topics they tackle. At the end, I feel that they failed to achieve the great potential they have, since the two books are written as if there exists an already well established and appropriate econometric and conceptual framework based on the Collier-Hoeffler model. All of the country studies use exactly the same kind of regressions with exactly the same interpretation. However, many of the chapters have very interesting details. For example, in volume 1, I learned a lot from reading the chapters on Burundi, Congo and Mozambique. And in volume 2, the chapters on Lebanese civil war and Colombia were quite informative, as were the chapters on the less well-known civil wars in the Caucasus (and the avoidance of civil war in Macedonia).

These books could have been much more influential and could have reached a much broader audience if they were written without the Collier-Hoeffler econometric framework. And the books would have been better if they had emphasized the details of civil war and acted as a signal to the policy and research community to pay more attention to issues of civil war, both for their human suffering consequences and their economic implications.

I think the books are a partial success, since they will not be appropriate for a general audience, and their econometric and conceptual methodology makes them inappropriate for an academic audience.

Sample 53

Main Book: *Natural Resources and Violent Conflict: Options and Actions*

The same comments about Understanding Civil War, Volumes 1 and 2 apply to this book as well, but in fact in this case the rich details about specific countries is missing. Therefore, I find this book somewhat less satisfactory than the other two. However, I found Chapter 3, "Who Gets the Money? Reporting Resources Revenues," a very interesting analysis with lots of useful information.

Sample 53

Pablo Fajnzylber, Daniel Lederman, Norman Loayza Inequality and Violent Crime

A. Objectives

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

This paper looks at the interesting link between income inequality and violent crime across countries. The staff undertakes one of the few studies of the hypothesis that violent crime is partially caused by inequality.

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

Yes and no. I think understanding violent crime is very important, but of course inequality is not something that can easily be dealt with by policy choices of the World Bank. Nevertheless, this is an important research topic for World Bank researchers to be investigating.

B. Design and Implementation

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

The methodology is quite well executed. The authors use various different empirical methods and they show good awareness of what their methods do and do not accomplish. They look at both cross-country correlations and panel data information using generalized methods of moments estimation. Although there is nothing innovative in terms of econometrics or economic theory, this is well done applied economics.

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

Yes, the authors show very good awareness of both econometric research related to this area and also of the conceptual issues.

3. How reliable were the data?

I don't know about the violent crime data. This falls outside my expertise. I find it hard to believe that it is extremely reliable, but perhaps it gives an overall picture of differences in violent crime across countries. The inequality data, which are one of the main independent variables, are of very low quality. It would be a great thing for the World Bank to invest more in collecting better inequality data based on household surveys. Much of the information on the gini coefficients is very unreliable; they are computed using different methodologies across countries. This of course has implications for the findings.

4. Are the conclusions consistent with the research findings?

Largely, yes. The paper does summarize its findings fairly well, and the authors are hands on table when it comes to the regressions and what they mean.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Yes. The paper is very well written as an applied economics piece and it is in fact published in a second-tier field journal. Among all of the research I have evaluated, this is probably the highest quality research, and this is reflected in its publication in the most respectable place among all of the research I have seen.

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

The authors are wise enough not to jump to too many policy conclusions.

D. New Results

1. What are the key findings of the study?

There is a strong cross-country correlation between inequality and violent crime. Within-country variations are also consistent with this, although the relationship is weaker. There is also a high degree of persistence in violent crime rates, for example homicide rates and robbery rates.

Sample 57

Paul Collier

Main work: Breaking the Conflict Trap: Civil War and Development Policy

This book also uses the Collier-Hoeffler framework as an organizing framework for thinking about civil war. Thus, it shares the shortcomings of the previous three volumes I have commented on. Nevertheless, I have found this volume to be better in many respects.

The point of “Conflict Trap” is a nice one, and is used quite effectively in the book. The implications of civil war for the spread of poverty and disease are well illustrated. In some places, regressions are more carefully interpreted as predictive rather than causal frameworks. These are all important improvements over the previous three volumes.

Nevertheless, again there are too many policy conclusions based on relatively weak evidence.

Sample 72

Ibrahim Elbadawi, Nazih Richani

Systems of Violence and Their Political Economy in Post Conflict Situations

A. Objectives

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

The main question is about post-civil war violence and its implications on development, and how such violence can be prevented.

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

Yes. Civil war issues are extremely important, and technical and other types of assistance from international organizations, the World Bank included, could be quite important in dealing with post-conflict, post-civil war situations. These situations are not fully understood currently (in fact, they are not even studied much), thus this is an important piece of direction.

B. Design and Implementation

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

The paper is a series of case studies, which have some useful detail but are not extremely systematic.

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

There is no theoretical framework here, and almost no attempt to link this to any hypothesis, theory, or micro-mechanisms. This makes this rather unsatisfactory according to my own perception of social science.

3. How reliable were the data?

There are very little data used in the paper. There are some numbers about homicide and gangs, but no serious empirical work or data.

4. Are the conclusions consistent with the research findings?

Given that the paper involves just a number of case studies, it is difficult to reach conclusions that are reliable with any degree of certainty. Nevertheless, the study does reach a number of conclusions, and I think this may be jumping to conclusions rather than reaching them appropriately.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

No. The paper is poorly written as a piece of social science.

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

No. There are no firm findings, but ample policy conclusions.

D. New Results

1. What are the key findings of the study?

The study is successful in documenting the formation of gangs and prevalent violent crimes in post-conflict situations. It is suggestive in linking this to low state capacity following post-conflict situations and also to low opportunity cost of crime and gang-related activities.

Sample 150

Explaining African Economic Performance

Paul Collier and Jan Gunning

Main paper: Explaining African Economic Performance

A. Objectives

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

The main question is what are the main factors explaining the poor performance of African nations over the past 35 years.

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

Absolutely yes.

B. Design and Implementation

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

The paper is mostly a survey. It does provide a relatively good summary of the main pieces of research up to the mid and late-1990s on African economic prospects, and it is a critical survey in providing some perspective on what type of explanations may or may not be appropriate.

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

Yes. It is, by its nature, building on existing research and it shows good awareness of both economics and political science research on African economic prospects.

3. How reliable were the data?

There are no original data in this study.

4. Are the conclusions consistent with the research findings?

No. The paper jumps to conclusions about social capital, while there is nothing in previous research or even in this paper that suggests that social capital is a major factor.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Yes. The paper is generally well written and accessible to academic and non-academic economists.

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

No. The evidence does not support social capital and related policy recommendations.

D. New Results

1. What are the key findings of the study?

The paper surveys a large literature; as such there is no single key finding. Aggregate performance has been very poor in Africa, especially in Sub-Saharan Africa. Explanations blaming this on geography or the tropics are just a way of re-labeling the problem, and the paper nicely points this out. Its survey of credit, labor and product markets is useful, but nothing jumps out as a key finding.

Sample 151

William Easterly

Main work: *The Elusive Quest for Economic Growth*

A. Objectives

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

This is a book written for non-economists as well as economists about attempts by the World Bank and other international organizations (or according to the author, economists) to help the process of economic growth in the poorest parts of the world. The main hypothesis or argument of the book is that development policy based on old-style development economics or Solow-type growth models has been a disaster. It is most successful as a critique of the World Bank and its traditional policies.

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

Absolutely. This is an evaluation of the World Bank in its entirety.

B. Design and Implementation

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

The methodology here is in the rhetoric. The book is very effectively written. It manages to communicate important ideas to laymen, and also provides food for thought for economists.

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

Yes. The author is certainly well aware of various different research in the area. Since it is a book for a general audience, it does not always give credit in the way that academic works would, but generally it shows a good understanding of the area.

3. How reliable were the data?

There are no new data in the book.

4. Are the conclusions consistent with the research findings?

Yes and no. The critique of the World Bank is very well placed. But from there the author jumps to his own favorite interpretations (or perhaps even policy interventions). This is not quite well justified on these grounds.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Absolutely yes. The book is beautifully written for the general public.

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

Not quite, as commented above.

D. New Results

1. What are the key findings of the study?

The key finding would be that the World Bank got it wrong for a long time, and big investments, aid and outside interventions do not generally work.

Sample 152

David Dollar, Aart Kraay

Main paper: Growth Is Good for the Poor

A. Objectives

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

The main question is whether growth reduces poverty.

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

Absolutely yes. Since a large part of the policy circles and media care about poverty as much as growth, the implications of economic growth on income distribution are of central importance. This is a first-order question.

B. Design and Implementation

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

Yes and no. The paper falls within the “cross-country regression” framework, and within this literature it uses standard methods and it is well executed. However, one might question whether this methodology is inappropriate for answering the questions that the paper poses. There is a debate on this issue.

The key question is whether poverty as measured by the income of the 20th percentile changes with growth. Income of the 20th percentile is defined (and in fact calculated) as the share of the 20th percentile in aggregate income times the level of income. Thus, there is a mechanical relationship between this share and the level of income. For example, if income inequality numbers are just made up and are therefore highly persistent over time, this regression will lead to a coefficient of unity, since the income of the bottom 20th percentile will co-vary mechanically with mean income. The current paper does the best that can be done with the available data, but does not engage the sort of questions raised in this paragraph, which are central to the interpretation.

A number of economists would argue that the central question of the paper, on the relationship between economic growth and poverty, would be much better answered using pooled household surveys across countries, which are now available.

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

Yes and no. The paper is very well aware of the cross-country growth literature and some of the problems and prospects of this literature. But there is almost no mention of the literature that uses micro data to answer similar questions.

3. How reliable were the data?

In my view, the main problem of the paper is that the data on inequality are not very reliable. Cross-country inequality data are computed in a very suspect manner, and I find them generally unreliable.

4. Are the conclusions consistent with the research findings?

The conclusions are consistent with the regression. However, the regression methodology, as explained above, may bias the results toward finding the conclusions that the authors do find.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Absolutely yes. The paper is very well written and very well argued. It was consequently published in a leading field journal, *Journal of Economic Growth*. Together with the inequality and violent crime paper above, it is the highest quality output in terms of publication record.

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

Generally yes. The policy recommendations are consistent with the regression evidence, but as pointed out above, there might be some question about how reliable the cross-country regression evidence is in this context.

D. New Results

1. What are the key findings of the study?

The key finding is that growth is good for the poor. In fact it is the main determinant of the income of the lowest quintile of any country.

Sample 158

Beck, Clark, Keefer, Walsh

Main work: Database on Political Institutions

This is a database rather than an individual project. I will therefore not answer the questions specifically.

I think for this project it suffices to say that this is exactly the kind of project that the World Bank should be involved in. Building data from various sources that are important for World Bank relevant questions and that can be used by other researchers is a first-order public good that is provided by the World Bank and should continue to be provided. To the extent that I can judge, this is a well-done and careful data construction.

Sample 12:

Pjilip Keefer and David Stasavage

Main paper: The Limits of Delegation: Veto Players, Central Bank Independence, and the Credibility of Monetary Policy

A. Objectives

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

The main question is how governments achieve credibility, and how the credibility of policy interacts with political structures, as measured by the number of veto players. These questions are illustrated in the context of central bank independence and the credibility of monetary policy.

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

These are important policy issues, but it is not clear whether they are first-order for World Bank policy. Central bank independence may be quite second-order for most developing countries, which suffer from much more structural problems.

B. Design and Implementation

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

The paper has both a theoretical and an empirical component. The theory seems to be correct but a little on the simplistic side. The empirical evidence is interesting. However, the authors interpret it too much as a causal relationship, which is really not necessary. The correlations are interesting even when they are properly interpreted as correlations. Nevertheless, one would have to worry about what “veto players” means. The concept of veto players, for example, as introduced by Tsebelis, is rather a slippery concept and difficult to map to reality. Both Tsebelis himself and follow-up research by people like Henisz provide indices for veto players, but it is not clear how reliable these are and exactly what they measure.

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

Yes, generally this is the case. There is more awareness of political science research than economics research, however. There is a lot of theoretical work in economics that touches on the same set of issues, and especially the more theoretical research in this area is not cited or internalized in the current paper.

3. How reliable were the data?

I do not find the data on veto players to be particularly reliable.

4. Are the conclusions consistent with the research findings?

Generally, yes.

C. Accessibility

1. Were the project reports and publications presented in a manner appropriate for and accessible to the intended audiences?

Generally yes, although the paper is written more in a political science style, and both the theory and empirical work can do with quite a bit of clarifying and formalizing. This would be useful both to show what the essence of the model and implications are and to make it possible for others to reproduce the empirical work in the papers.

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

The paper is generally cautious in reaching policy conclusions and recommendations.

D. New Results

1. What are the key findings of the study?

The main result of the paper is that delegation to an outside authority, such as the central bank governor, is more likely to enhance the much-needed credibility when there are multiple veto players. This has an intuition similar to that of the separation of powers results in the political economy literature.

Questionnaire on strengths and weaknesses of the Bank's research

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

Generally, no. The best three papers in the areas that I have reviewed are "Inequality and Violent Crime," "Is Growth Good for the Poor?" and "The Limits of Delegation." The first one is empirically the most careful and makes an important contribution, even if it does not contain theoretical or methodological contributions. The second one is very important in terms of the question it poses, and comes up with provocative answers. Data quality and methodology limit the validity of the conclusions, although the paper is well recognized and important in the literature. The third paper is still a work in progress, and something interesting might come out.

2. In your area of expertise, has Bank research focused on the most important issues for developing countries?

Yes and no. The focus on civil war and post-conflict problems including violent crime are extremely important. This is a major contribution of the Bank's research. Asking questions about the relationship between growth and poverty is also very important. Contributions to political institutions, good data on political institutions as in the work by Beck and Keefer, is also very valuable.

On the other hand, of those I was assigned, there were projects that are difficult to interpret as well, for example those on the use of real options for evaluating biotechnology risks, etc.

3. In your area of expertise, has Bank research reflected awareness of substantive knowledge from other research available?

Generally yes, but there seems to be at least a couple of years of lag between frontier research in academia for it to have any kind of effect on Bank writings in general.

4. Has the Bank made appropriate selection of topics for data collection and surveys?

Yes. Data collection on political institutions, and also on privatization in China, are excellent examples of this. The project on civil war is also quite valuable in this respect.

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

I don't quite know what is meant by this question. I suppose country objectives are very diverse, and it would be naïve to expect Bank research to cater to country objectives.

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

The only ongoing projects were those on uncertainly, biotechnology and natural resources, which I have commented on above. These do not seem appropriate for the Bank's purposes, nor do they contribute to significant research in the area.

7. What is your overall assessment of Bank research?

It is very important for the Bank to be involved in academic research, and it is very important that this research has a macro (equilibrium) dimension, as in the work on civil war and institutions or on the effect of economic growth on poverty. It would be a mistake for all of the research of the Bank to be about micro issues, since many of the questions of interest for the mandate of the Bank are macro in nature. Such questions require a combination of micro and macro tools to be answered.

In this light it is commendable that the Bank has put a lot of resources in areas related to economic growth, institutions, civil wars, political instability, etc. Nevertheless, I believe that the quality of this work can be improved substantially, especially given the large number of high-quality researchers in the Bank.

It would be much better for the Bank to focus its high talent resources in a few areas, and in the remaining (majority) of areas to use its resources for data collection and application of already-established frameworks rather than trying to develop new theoretical and empirical frameworks.