Selecting Development Projects for the World Bank

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Foreword

Now more than ever, the World Bank is examining how it does its business and how it can achieve its goals in a more effective way. Especially since the Task Force Report on Portfolio Management and the earlier review by the ECON Task Force, there has been increasing attention to the quality of the economic analysis of the projects financed by the Bank. These reviews highlighted the problems encountered in undertaking risk and sensitivity analysis, examining alternative project designs, and measuring or quantifying project costs and benefits. They also noted the difficulties encountered in the large number of projects for which no systematic evaluation or quantification of costs and benefits was done, and if that is at all even possible. A lively debate has ensued on how we should do better.

This paper jumps into that debate. Among other things, it challenges conventional thinking on economic analysis, particularly in the use of shadow prices. It argues for not attempting to quantify the *unquantifiable* and for lowering the standard discount rate used by the Bank. It also argues that the uncertainties faced by projects are not easily amenable to analysis and that we should appreciate that the upside and downside risks are likely not symmetrical. The paper points out that the appropriate response to such risks and uncertainties lies in a market economy with appropriate incentive structure, competent management with the flexibility and autonomy to respond rapidly to changes. There ought to be robust measures of long−term and sustainable project contributions to development objectives. Finally, the paper recommends that the selection of projects for Bank financing be made on the basis of the Bank's comparative advantage. Hence, the selection process ought not be divorced from the Bank's own institutional capacities and ability to influence policies. That takes *learning* .

We are grateful for the author for sharing his ideas and for provoking thinking on these important issues.break

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Preface

The core of the World Bank's business has always been the financing of projects with a view to promoting development and thus alleviating poverty. In order to make the greatest contribution to those aims with its scarce resources of investible financing, staff time and administrative budgets, the Bank needs methods that allow it to make good choices between potential projects.

This paper contends that the Bank has in fact made quite reasonable project choices in the past, perhaps even better ones than would appear from some of its own retrospective examination of project performance. However, says the paper, this has not happened through the careful application of the methods for the economic analysis of projects that the Bank has formally adopted and to which it is still, in principle, committed. It has happened because staff have tended to by−pass and disregard those methods, and use their common sense.

This paper aims to "clear away the rubble" of old discredited theories, and to make a start towards the construction of new project selection methods that fully reflect the World Bank's own characteristics, aims and comparative advantages.break

Abstract

This paper examines the methodologies for project evaluation that, in principle, have been adopted and are stylized by the World Bank. These mandate the evaluation of project benefits and costs at so−called *"shadow"* prices, that correspond to an economy free from distortions (including, in some variants, the maldistribution of wealth). The paper establishes that this recipe is wrong, and is likely to lower the overall quality of investment, essentially because projects so selected would still have to be implement ted and managed in the light of actual market prices and rules. If market imperfections are excessive, they cannot be by−passed: they should be reduced. This is a major responsibility of governments. If markets, while imperfect, are reasonably operational, there is overwhelming evidence that investments that respond to them lead to growth and development.

The paper rejects attempts to quantify the benefits of every type of project. Improvements to health and the environment and many other public goods cannot be given dollar values or equivalents in terms of privately consumed goods and services. Nevertheless, a discount rate is needed to help decide the design of projects and

make choices between projects. The 10 percent real rate the World Bank has traditionally used is excessive, according to the Paper, which, on the basis of international interest historical experience, recommends a rate of about 4 percent as a first approximation. The use of a higher rate by the World Bank may partly explain its failure to see the coming of the deb t crisis (which is indeed inexplicable if investments in developing countries yield at least 10 percent), and has also led to bias against environmental and other projects with very long−term benefits.

The Paper stresses that uncertainty is a dominant feature of all projects. It should lead to reduced emphasis on precise calculations of costs and benefits, and greater attention to adaptability, through comparative advantages, well−in formed and well−motivated managements, educated workers, and a market−oriented environment. The Paper also notes that theoretical models present project choices as the comparison of several fully detailed blueprints. This is wholly unrealistic, because such blueprints take time and cost money. Potential projects selected for detailed study thereby acquire the overwhelming benefit of doubt. Conversely, potential projects not reviewed are thereby condemned. Recognition of the nature of such early decisions should be translated into management and personnel policies likely to improve the results, notably by placing greater emphasis on the identification of truly good development opportunities.

Traditional methods aim at identifying projects that best contribute to development. According to the Paper, what the World Bank needs is to identify activities that would maximize its own contribution to development. That can involve projects too difficult or too unattractive for others, but well suited to its own comparative advantages. The Paper recommends that these comparative advantages should be carefully defined and form the focus of project selection.

The Paper stresses that none of these ideas are sharply contrary to actual World Bank practice. However, they go counter the Bank's theoretical models of reference, and even the prescriptions of still valid operational memoranda. Theories should now be clarified in order for new research to take off vigorously in new directions.break

. . . Pour moi, je n'ose être d'aucun avis; je ne vois qu'incompréhensibilité dans l'un et l'autre système; et après y avoir rêvé toute ma vie, je suis aussi avancé qu'au premier jour.

Il est bien triste d'avoir tant d'idées, et de ne pas savoir au juste la nature des idées.

Je l'avoue; mais il est bien plus triste et beaucoup plus sot de croire savoir ce qu'on ne sait pas . . . ".

VOLTAIRE − dictionnaire philosophique − *Idée* .

As for me, I do not dare to be of any opinion; I see nothing but obstacles to understanding in either system; and after having thought about it all my life, I am no more advanced than on the first day.

It is sad to have so many ideas and not to know the nature of ideas.

I confess it; but it is much sadder still, and much more foolish, to believe that one knows what one does not know.break

Summary

Much of the World Bank's development contribution comes from its financial transfers on terms more favorable than can be obtained from financial markets. The Bank also has general advisory functions in macroeconomic

policies and in more specialized fields. The development contributions related to these functions are independent of the objects of its financing. By choosing particular projects to finance, and thereby also helping with their preparation and implementation, the Bank wants to achieve the highest development impact. The aim of this paper is to examine the project selection methods that would allow the Bank to approach this aim.

"Shadow" Prices

Traditional methodologies and World Bank operational policies demand that project costs and benefits be evaluated using *"shadow"* prices, corresponding to an economy free from distortions (including, in some variants, those relative to the distribution of wealth and other initial advantages). This recipe gives no thought to what happens to projects so selected when they are implemented and, upon completion, managed, when they must necessarily respond to actual market prices and to regulations prevailing in the real world, not to *"shadow"* prices and rules. Setting up some projects in observance of *"shadow"* prices and rules, while others are set up, and all are managed, in observance of market prices and actual rules, is bound to result chaos, not in enhanced efficiency. Fortunately, in World Bank practice the use of *"shadow"* prices is perfunctory and seems to have little direct influence on actual project decisions.

This is also true of the recent reinterpretation of the method. This maintains the validity of methods using *"shadow"* prices, but rejects prescriptions for calculating them. It contends that *"shadow"* prices cannot be calculated in thoroughly distorted economies, but they should be used in mildly distorted economies as a check on the social desirability of projects designed in the light of market signals. In this variant, the World Bank would use *"shadow"* prices like some governments use licensing functions, letting market−related considerations determine project design and operations, but vetoing some projects in the light of its own views of the social optimum. There cannot be much merit in a method that stresses that efficiency prices must be used in project evaluation to correct price distortions, but can calculate them only when distortions are mild. More to the point, the scope for mischief is much reduced in this more modest variant of the method, but it is not eliminated; the preceding paragraph's objections are still valid.

A Bank project that eschews a profitable and fully legal process deemed socially unattractive may cause overall recourse to such a process actually to increase. If (e.g.) a project avoids an environmentally harmful but authorized process, thus raising the price of its products or causing its own size to be reduced, this may ultimately increase the profitability of competing projects, perhaps particularly lavish users of the same process. Thus the Bank's *"shadow"* environmental preference may actually increase environmental damage.break

Market prices and actual regulations are best used to evaluate projects. The applicable prices and rules are naturally those, present and projected, expected to prevail at the relevant times; these expectations should incorporate, among other things, future policy changes. Benefits and costs so calculated indeed diverge from what they are truly worth for society, but there is much empirical evidence that in economies where prices and the distribution of wealth and other initial advantages are not thoroughly distorted, projects designed and operated in the light of market signals bring about development and contribute substantially to social welfare.

None of this means that markets are perfect, or that they reach a reasonably undistorted state without government intervention. On the contrary, governments must deliberately intervene to reduce distortions, including those affecting the distribution of wealth and other initial advantages. There is some advantage to interventions that bear on prices (e.g. by internalizing external costs and benefits through taxes and subsidies), but non−price rules are also needed. When distortions are high, they should be corrected through policy changes, not attempted to be by−passed.

Nor should trust in markets lead to systematic mistrust of public investments. Levying a price is always costly, and may entail a higher inefficiency than the waste related to the absence of a direct price: rural roads are a good

example. Public incentives are also needed for many public goods, when benefits are largely external, and also when the provision of a good or service is intended as a correction to the maldistribution of wealth and other initial advantages (e.g. education). While public intervention is needed in such cases, the actual investment and production may be either publicly or privately managed.

Non−Quantifiable Benefits

Many project benefits and costs cannot be quantified. This impossibility is intrinsic to many social preferences, and not something to be surmounted through more diligent work. Societies are essentially unable to put a price on many human and environmental costs and benefits. Some values may seem indirectly revealed by social behavior, like prenatal care budgets and their relationship to infant mortality may seem to indicate the value placed on infant lives. In reality such choices are specific, and their implications cannot be extended to other circumstances. Many choices must therefore be made largely in non−arithmetic terms, without sharply defined cost−benefit ratios and rates of return. Public choices concern programs and projects in their totality. Health, police and education programs and their project components can be decided without calculating the dollar value of the benefits brought to society by marginal spending on schoolbooks, police badges and vaccines; and without attempting to equate them to the marginal value of spending on marshmallows. In World Bank practice, only about a third of recently evaluated projects were attributed quantified benefits.

All the more reason to be as precise and quantitative as possible. The overall expected benefits from projects and from individual project components must be clearly described. Specific reasons must be given for all elements of expenditure, and quantified as much as possible: it may well be impossible to attach a monetary value to the social benefits of greater school attendance, but it is often possible to determine how many more pupils are expected to attend as a consequence of a project, how much they will learn (and how is this tocontinue

be determined), at what cost per pupil. Whenever this is possible, it is desirable to do so. A single answer may not be derived from the simple comparison of costs and benefits, but multiple comparisons should shed light on the final decision.

The Opportunity Cost of Capital, and the Discount Rate

In traditional project evaluation methodologies the composition of the investment program and the "*opportunity cost of capital* " are jointly determined. This process breaks down if the benefits of many projects are unquantifiable and (as is the case) much investment is decided without reference to rates of return. In practice, the World Bank has generally referred to an *"opportunity cost of capital estimated at 10 percent"* in real terms.

This value is much too high. In many countries with poor development performances, country conditions have depressed the marginal efficiency of investment towards zero. When country conditions are better, it should normally be possible to finance investment projects through borrowing, and the marginal efficiency of investment (or opportunity cost of capital, or the discount rate) should not be higher than the marginal cost of international capital.

Exceptions include countries where the marginal efficiency of investment is patently lower, some with over−abundant capital, and many with dismal inherited or policy−induced overall conditions that reduce investment performance. Inversely, in rare circumstances, international markets may not yet recognize the high potential of countries that are reconstructing or restructuring following natural or man−made catastrophes, including those induced by economic policies. Borrowing and investment are then rationed, and the marginal efficiency of investment and the applicable discount rate remain high.

However, international markets have long demonstrated their preference for over rather than under−estimating creditworthiness. The proper general approach to the discount rate is to equate it to the market rate of international borrowing. Historically, this has rarely exceeded 4 percent in real terms. Accordingly, 4 percent should provide the first cut approximation to the Bank's discount rate. Higher rates should be used only when they demonstrably apply.

Lower rates demonstrably apply to the marginal efficiency of investment in many badly performing countries, where the realistic alternative to World Bank projects may often be complete waste or anti−social private benefits. Nevertheless, for these countries the appropriate Bank discount rate, essentially on moral grounds, may well be the expected real returns to safe international investments, say about 3 percent (returns to investors are somewhat lower than the borrowing rate). The strict application of such a rule in the past (let alone that of the notional 10 percent discount rate) would have caused World Bank lending to be re−allocated away from many of the poorest countries, with the most unfavorable country conditions.

Past use of a grossly excessive discount rate of reference induced some bias against projects with remote returns, including environmental benefits and human capital enhancement. This bias was probably modest, mostly because in practice the Bank only made modest use of its own official project selection theories and guidelines. Much more seriously, the implicit reference to such a high marginal efficiency of investment, and its stark contrastcontinue

with the actual cost of foreign borrowing (itself long under−estimated by the World Bank), blurred the Bank's vision of the dangers of excessive borrowing and of over−indebtedness. It contributed to its condoning excessive borrowing in the 1970s, and to delaying recognition of the full weight of the debt crisis in the 1980s. Use of more realistic discount rates should correct these biases. However, it should not be allowed to lower criteria for project efficiency; many projects are supra−marginal, and among projects with fully quantifiable benefits the better ones will have higher discounted present values than those less desirable.

Uncertainty and Risks

Projects face an inherently uncertain future. Even the nature of possible future events cannot be described with confidence, and few indeed are the uncertainties that can be assigned rational probability values. Political risks, like those of civil or foreign war, or of governments repudiating past agreements are legitimate deterrents to private investment, but the World Bank's mission requires it to disregard such dangers.

Some other risks and uncertainties may be expected to have symmetrical impacts, and these can be disregarded. However, project beneficiaries are rarely indifferent to symmetrical risks, and this asymmetry of welfare impacts not only may reduce their welfare, it also affects their attitudes and may therefore contribute to project failure. Redistribution mechanisms and *"safety nets"* are costly and difficult. Financial risk coverage techniques exist and multiply, but they are rarely cheap and often transform rather than reduce risk.

Depending on moods, fashions and the state of animal spirits, decision−makers sometimes unduly concentrate on projects' apparent advantages, allowing all that can go wrong to be concealed by Hirschmann's "*hiding hand* ". More recently, environmentalists and similar pressure groups have concentrated on the visible drawbacks and potential dangers of specific actions (particularly those of large infrastructure projects), while dismissing or disregarding those of inaction. Deliberate efforts must be made to examine strategic alternatives, and to bear in mind that inaction is also a form of action.

Little can be done about reducing uncertainty, but much to improve ability to confront it. At the macroeconomic levels, this requires the rapid transmission of multiple information, best performed by decentralized market economies; incentives provided by markets shaped by public intervention so as to bring private interests

reasonably well into line with social welfare; and reasonably good distribution of initial advantages, including widespread general education. At the project level, this translates into well motivated, well informed competent management and well−educated responsive labor.

A Learning Strategy

The Bank must know the projects it decides to implement. Knowledge demands resources and time, which are limited. Budget pressures preclude much time or resources being spent on studying investments that are ultimately not made. Most decisions to study a project must therefore be expected to lead to its being ultimately selected and implemented. Such implied preliminary decisions may, of course, be later revised; but budgetary constraints are opposed to such revisions being frequent, and to their affecting projects whose study has alreadycontinue

cost much time or resources. Managers and staff come under implicit pressure not to reject such projects, and therefore to proceed with them despite possibly emerging doubts against their desirability. The general uncertainty of the future can be marshalled to help overcome such doubts (nothing is certain to fail, nor to succeed). Rejecting a project after having sanctioned its study highlights that mistake; if a doubtful project is approved the mistake, if it is one, will only be proven much later, and is by then likely to be harmless to personal careers. Implicit decisions not to pursue certain ideas are most difficult to review and to reverse, yet they are also those least easily sanctioned through personnel policies. Conversely, it is most difficult for personnel policies to identify and promote the qualities of quasi−instinct needed for identifying the best routes to accelerated development early, and for focussing on them.

The World Bank should explicitly recognize how strongly final project selection and similar decisions are influenced by the early definition of studies and research. Such full recognition should cause decision−making at these stages to become more formal. It may also lead to resources being shifted to the process's earlier stages, strategic thinking and project identification. Finally, management and personnel policies should shift the balance of incentives against proceeding with wrong choices and in favor of early identification of original desirable strategies.

Criteria for Project Selection

In the words of the major project selection manual published by the World Bank, *projects should be selected in the light of their contribution to the maximization of total [. . .] national income";* and the aim of traditional project selection methods is to help ensure this. But this is an unsuitable objective for all those who do not have a monopoly of investment decisions. Neither the World Bank nor a Government would maximize their development contribution by selecting the "best" development projects; other investors (e.g. in the private sector) might undertake those projects, finance them through other means and implement them on their own just as well, or perhaps even better. The Bank must aim at selecting the projects whose own development impact it most improves by its intervention; those projects which, when selected by the Bank, cause overall development to be better than if any other set of projects had been selected by it. Mutatis mutandis , the same consideration applies to public sector investment decisions.

Project selection should be determined by the Bank's capacity to contribute to overall development benefits more if it intervenes through some projects and sectors than if it intervenes through others. In practice, financial considerations, like speed of disbursement and ability to attract co−financing, have sometimes played a role in project selection, but they should rarely do so. It is partially a truism to say that the Bank's comparative advantages lie in areas where others cannot easily tread. This condition is most likely satisfied in areas where policy requirements are not fully known, where resistance to new policies is considerable, where projects and policy change are intimately related.

In World Bank practice, the criterion of comparative advantage plays an important role, but one that is reduced and confused by continued reference to the projects' own development contribution. The conflict is most acute when the Bank's comparative advantage arises out of its "*clout,* " its ability to impose on the country certain policies not otherwise pursued orcontinue

applied to the particular sector. The more harmful the policies pursued without Bank involvement and the greater the opposition to change, the greater the merits of Bank involvement; but the greater, too, the risk that policies will not improve and the project will fail. Even in case of success, it takes time for policies to improve, and the achievements of the project itself may meanwhile be quite modest. Ex ante, reference to project success may deter World Bank interventions in sectors where they would be most helpful, and orient them towards projects which themselves are expected to do well; but would do just as well if the World Bank were to leave them alone. Ex post, project audits may attach excessive importance to the projects' own modest contribution to development, and unduly disregard how much the overall development path may have been lifted by the Bank's intervention in this project rather than others.

Project selection is subject to the constraint of the lending program, and to a separate one on administrative expenditures for project implementation. With a lending program of given size, projects with higher development contributions may be selected if the administrative budget for project implementation is raised (and vice versa). Conversely, project quality may be depressed if increases in the lending program are not accompanied by adequately rising project implementation expenses: an impact of pressures to lend which is also well known to Bank practitioners, and which, in more recent years, has been partly relayed and complemented by downwards pressures on administrative expenses.

In the short run, the Bank's comparative advantages are heavily influenced by the skills of its staff and by its institutional store of knowledge. Potential structural comparative advantages are related to the Bank's international character and financial weight, which give it a unique combination of capacity to define desirable policy changes, and *"clout"* to impose them. By deliberately shaping its staff skills and project involvement to exploit this long−term potential comparative advantage, the Bank should be able to sharpen and develop it.

Conclusion

Most conclusions are implicit in the preceding summary, including the finding that there can be no simple arithmetic rule for identifying and selecting the best development projects. Not surprisingly, therefore, this review has not come up with a simple new rule. Nor did it find enormous faults in the Bank's present practices. These can be further improved, however, if some misleading theories are finally fully discarded. Better project selection practice can also be helped by personnel and management policies that stress the Bank's comparative advantages, favor its specialization, and give primacy to original strategic thinking. Project selection is largely determined by the areas picked for early study, perforce on the basis of general knowledge and an overall sense of needs (best, though quite imperfectly, called *"development intuition"*). There is little room for comparisons of major alternatives, whose precision, in any case, would be largely spurious in view of the uncertainty that dominates the future.

The Bank should concentrate on helping remove the grosser distortions that stifle the volume or reduce the efficiency of private investment in fields best served by it, notably a large part of market−oriented production. It should help complement public investment in fields it serves best, notably the production of public goods, and of goods and services on which it is difficult or costly to impose prices (e.g. roads). It should pay particular attention to the policycontinue

conditions that ensure efficient use of these services. Profit maximization, or even maximization of the Bank's development contribution are not realistic aims, because alternatives cannot be precisely compared; but the Bank should aim at eschewing the least useful interventions (through *"good"* projects and through bad ones) and at favoring those where it does useful things others are unlikely to do, or to do as well.

Within the Bank, this aim is most likely to be served if personnel and management policies are deliberately geared to defining and refining comparative advantages, and also towards identifying and promoting those with the *"development instinct"* , and penalizing those who fail the test. This will be difficult. The Bank is alone in its field, so, unlike most private firms and even governments, it cannot compare its decisions (and those of its staff) to the decisions of other organizations. Also, the time lag from development decisions (including the crucial implicit decisions not to pursue an idea) to their demonstrable impact is long. Even if personnel policies attach great care to relating careers to the development impact of early decisions, the linkage will only be established with great difficulty, and will remain tenuous; but in the absence of such deliberate efforts, there will be no linkage.

At the country and project level too, the presence of responsive managers operating in an environment that feeds them information and gives them incentives is the best promise of successful projects. Those efforts that aim at establishing and nurturing such managements and their operating frameworks are the most likely to contribute to successful development.break

Introduction

The World Bank's Articles of Agreement stipulate that *"the loans made or guaranteed by the Bank shall, except in special circumstances, be for the purpose of specific projects of reconstruction or development"* .1 The term *"project"* itself was left undefined, and we can leave it so, relying on the simplest possible definition: *"something planned", "a large or major undertaking"* .2 From its inception, therefore, and for almost half a century now, the Bank has had to decide what "*things planned"* , what undertakings, which projects to finance. These decisions are taken gradually, in the course of a partly formal process which includes general economic studies, Bank strategy formulation, project identification and preappraisals, and finally leads to a formal *"project appraisal"* . Subsequent to project implementation, the validity of both the implementation process and of the original project selection decision are re−examined ex post in project completion and operations evaluation reports.

Private firms make investment decisions and develop or select corresponding investment projects mostly in order to enhance their own profits. Governments and international financial institutions like the World Bank do it, in principle, in order to enhance their favorable influence on the welfare and development of the target countries. A considerable literature deals with project evaluation for those concerned with economic development (as distinct from those concerned with profit−oriented businesses). Much of its original aims were well−meaning: to impose discipline on non−profit making projects, to design objective project selection criteria and thus constrain the freedom to make arbitrary decisions and engage in ill−justified ventures for the sake of ill−conceived and ill−defined "strategic" impacts or supposed social desirability. Most of this literature falls under the broad heading of "*economic (and social) evaluation of projects"* a .

Profits are the residual of transactions in which products are sold, inputs are bought and factors of production are paid incomes at market prices. Private project evaluation methods aim at helping profit−motivated entrepreneurs choose profitable projects. They ask the question: "*what project is good for this entrepreneur, concerned with his own profits?* By contrast, economic or "social" analysis of projects asks "*What projects are good for a country?"* or, more narrowly, *"Is this specific project good for the country? does this project increase collective welfare* ?").

The profit generated by the contemplated investment projects does not always adequately represent collective welfare. One problem is that various distortions and externalities may cause social costs and benefits to diverge

from those signalled by market prices. Also, many developmental projects do not principally aim at marketing their products, and the notions of *price* and *profit* may not be easily applied to them. Most of the traditional project evaluation literature, including the World Bank's own guidelines, has concentrated on resolving the problem caused by the divergence between private and social costs and profits through the use of "*shadow* " orcontinue

1 INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT, Articles of Agreement, Article III, Section 4 −*Conditions on which the Bank may Guarantee or Make Loans* − vii.

2 Webster's Desk Dictionary of the English Language, Random House , 1983.

"*efficiency* " prices, in lieu of market prices.

The World Bank variant of the methodology of project selection was derived from this literature by Herman van der Tak and Lynn Squire (VDTS) more than twenty years ago. It drew heavily on the industrial project evaluation method devised by Ian Little and Arthur Mirrlees (LM) for the OECD somewhat earlier. VDTS also refer to a similar evaluation manual prepared for UNIDO (UNIDO) in 1972, which differs from LM in relatively minor respects. The "Méthode des effets" (CHERVEL) developed in France by M. Chervel is quite different. Instead of calculating economic rates of return, CHERVEL helps build a framework for setting out the planners' criteria, and describing the implications of a project; but it does little to prescribe desirable criteria.3

LM and UNIDO strictly confined their methods to projects in the industrial sector. VDTS set no sectoral limitations; they claimed, at least implicitly, universal applicability, and their method was deemed to have it when it was first introduced and used in the World Bank. They also did not limit the applicability of their method to ensuring the selection of Pareto−efficient projects, i.e. of projects that were efficient in production provided one disregards the income levels and other moral claims of the income recipients, as well as the uses to which they put project−generated incomes. Though they did not devote much space to the explicit consideration of environmental impacts, they would no doubt have argued that these too should be incorporated in the calculation of project costs and benefits. LM and UNIDO may also have wanted externalities to be valued and taken into account.

Present World Bank practice limits the use of the VDTS method to a minority of projects, though not confined to the industrial sector. Attempts to give income distributional weights to project costs and benefits were soon abandoned. Environmental impacts are now generally examined, but they too are rarely attributed specific values. Moreover, in World Bank practice of recent years, the benefits of a vast range of projects have not been quantified at all. How are the merits of such projects to be compared among themselves and with those of projects with quantified benefits? Considering market distortions, are these quantified benefits truly quantifiable? Can reference to a set of *"shadow"* prices allow the selection of a set of projects truly reflecting social priorities? How do project selection methods based on the comparison of the projections of the remote future cope with the inherent uncertainties of the future? How relevant are project contributions to social priorities to the selection of projects by the World Bank? These are some of the issues to be examined in this paper.break

3 See Bibliography at the end of this paper.

I—
"Shadow" Prices: Bridge between Social and Private Returns, or Blind Alley?

Market Prices, Private Profitability and Social Utility

In an economy with perfect competition, private marginal costs and benefits correspond to those of society, provided wealth and other initial advantages (e.g. education) are also distributed in conformity with social preferences. In such an economy, the maximization of private profits also maximizes social benefits. With somewhat less confidence, one may also assert that even if wealth and other initial advantages are imperfectly distributed, but the other conditions of perfect competition are nevertheless fulfilled, so−called *"Pareto* " efficiency may still prevail: production would be most efficient under these conditions, and if taxation of incomes involved zero costs, incomes could be re−distributed in ways that would make every member of society better off than under any other alternative.

In practice these conditions are rarely even approached. Differences between *social* and *private* returns are caused by various *"distortions"* . Some are related to misguided policies. For instance, it has been highly profitable in many developing countries to produce passenger cars. However, if a major contribution to such profits comes from a high duty levied on comparable imports (while automobile parts for assembly are imported duty free), such privately profitable activities do not contribute equally to social welfare. The additional price paid by consumers, above the world price, may bring no corresponding benefit to producers; part of it is likely to be absorbed by higher costs. The additional price may not even contribute to tax revenues, if in fact little or no import takes place with that high rate of duty. There may be an intermediate position − say, 50 percent duty − which would allow consumers to buy cheaper cars, and government to receive higher customs duty receipts, with which to build roads, or low−income housing. More generally, when an unintended effect of government policies is to cause market prices to diverge from what would be economically efficient, the private and social benefits of investment projects and other actions also diverge.

Another type of divergence between private and social cost and returns derives from classical externalities. These take many forms; environmental impacts provide the best example. A factory that pollutes without hindrance creates a nuisance, thus imposing on the public costs for which it does not pay; the costs it has to bear privately are the only ones reflected in the prices it charges; but the social costs of its production are higher. A fishing boat that reduces the catch available to others, or irrigation works that either reduce the water others can get, or increase their drainage costs, all create negative externalities. Other external effects may be less clearly defined, but nevertheless real. Health expenses, particularly the prevention of contagious diseases, also benefit the public, not just the individual who is their main object; there are also external benefits attached to education.

The distribution of income and wealth may be another cause of market prices diverging from social costs and benefits. Society, or the authority responsible for project selection, or outsiders like the World Bank, relying on their own value systems, may consider the rich to be too rich, or to be rich for the wrong reasons such as plunder, corruption and drug sales. Consequently, a benefit accruing to them may not be as valuable to society as a benefit accruingcontinue

to very poor people or to those enriched through productive activities. The exercise of the privileges of wealth may also distort prices: thus the willingness of the rich to pay very highly for proximity and privacy may be the cause of high urban land prices and consequent overcrowding in slums. Society, the Government or the World Bank may consider that land allotted to increasing the size of a wealthy man's private plot has a lower social worth than land allotted to housing a hundred poor families, though market prices indicate the reverse relationship.

Many divergences between private and social benefits have what one might call *natural* causes. The conditions of *"perfect competition"* are rigorous and many. They do not only relate to public policies and legal systems. They also include things like the shape of production functions (costs must be rising over the relevant range of production, and marginal costs must exceed average costs), information (it must be widespread and evenly

shared), transaction costs (they must be nil) . . . Very few of these conditions are exactly fulfilled in real life. For instance, many services and large−scale industrial operations normally operate with short−run marginal costs much lower than their average costs or long−run marginal costs: the short−run marginal cost is actually close to zero for uncongested highways, cinemas that are not fully sold out, airplanes with empty seats . . . Yet the equality of price to short−run marginal costs is one of the key intermediate conditions on which the full efficiency of perfect competition rests. This pricing inefficiency is sometimes compounded by the high ratio of the cost of actually levying a price to the cost of the product (for instance, for toll−roads or newspapers.

Public policies, including the broad customary and legislative framework, can increase the divergence between social and private costs and benefits, and different policies and changed customs can reduce such disharmony. If tariffs are too high, they can be lowered; if water charges are too low, they can be raised. Pollution can be regulated or taxed, so as to "*internalize* " part of its costs, that is, cause it to be borne or abated by the polluter. Pasture lands can be enclosed and fishing rights taxed or regulated, to preserve natural resources. Even if excessive inequalities cannot be reduced through confiscation or land reform (for fear of worse), they may be reduced through inheritance and other wealth taxes; or it may be possible to mitigate some of their impacts. For instance, the government might subsidize education (for all or for certain groups) so as to cover the costs corresponding to its external benefits, leaving only the equivalent of private benefits to be covered by school fees; government subsidies, loans or guarantees may help access to higher education by those who otherwise could not afford it for lack of adequate incomes and creditworthiness. Zoning laws and similar regulations may restrict the ability of the rich to use land in ways which society, represented by the government, considers wasteful; public housing may be provided; governments can preempt the use of land for projects they consider to have high priority.

New laws and policies are particularly important when the distortions themselves are mainly due to laws and policies. If tariffs and other protection are considered unbalanced and excessive, they should be restructured and reduced. If taxes are insufficient and biassed, they should be raised and redirected. But the frontiers between departures from perfect competition due to the nature of things, and those related to policies, are blurred. It may sometimes be feasible and efficient to cover the cost of roads through special tolls, but (at least until the advent of flexible electronic pricing) marginal cost pricing of road users has been generally impractical. If piped water is underpriced, that is clearly a simple policy failure; if groundwater is over−soft

exploited, that is at least in part due to the nature of things, and the difficulties of controlling such activities.

Removing distortions is always the most desirable solution. It is not always feasible. Tensions between competing aims and component elements of society cannot always be fully reconciled; even more frequently, they cannot be reconciled merely through market−based interventions. The notion of "acquired rights" − to the free use of a resource, to engaging in an occupation, to retaining old wealth − will often be opposed to efficient pricing. Actions to limit access to a previously free resource, or to charge payments for it, change the distribution of wealth and incomes. Enclosures of British lands abolished earlier rights to pasture; highway tolls and parking fees levy costs which are not matched, user for user, by the corresponding benefits of reduced congestion: hence frequent resistance to such measures. Pricing itself is rarely costfree. A price can only be levied if a barrier exists, a notional tollgate. That is so taken for granted as to barely constitute a problem for apples and oranges and most other goods, even though protecting the properties and charging for the consumption of the product is a significant component of their final costs. But pricing runs into technical and economic problems, and even legal and moral ones, for services like the use of roads, and for the sale of natural resources like water, the use of sewer discharge facilities or the use of the atmosphere as a sink for discharged gases.

This last example also highlights the need to be organized so as to be able actively to enforce the rights of individuals and of the community. This need for enforcement somewhat blurs the distinction between supposedly distortive quantitative controls and market−based interventions. The real problem (e.g.) for many polluting

discharges is not whether to regulate quantities or to set prices, but how to obtain knowledge of the quantities discharged, how to know whether anything is discharged at all, and by whom, things equally necessary for pricing and quantitative controls, and even for simple interdictions.

"Shadow" Prices for Project Selection

As long as distortions persist, social aims are not perfectly served by the projects that are most attractive in the light of market prices. The use of *"shadow"* prices in such circumstances has been widely sanctioned. It forms the basis of the traditional project evaluation methods. It continues to be endorsed by present World Bank instructions on the "Economic Evaluation of Investment Operations,"4 which are derived from them. UNIDO, LM and VDTS all propose to bridge the gap between private and social costs and returns through the use of "*shadow prices* " that reflect real scarcities and opportunity costs.5 In the automobile case mentioned above, for instance, the cif price of imported cars (or various approximations to it) would be the *"shadow price"* of domestic cars,6 thus indicating that domestic production would be lesscontinue

4 The World Bank Operational Manual, OPERATIONS POLICIES OP 10.04, April 1994.

5 Originally, "shadow" prices referred to the dual of a linear programming allocation problem. The shadow is then cast by the defined quantitative constraints.

6 There are various variants to this, notably the "domestic resource cost" of automobiles, also calculated at the border equivalent, as import or export substitutes, of the materials and factors of production used. All these variants ultimately refer to international prices.

profitable or, most likely, actually loss−making for the economy as a whole. Factories polluting the waters or the atmosphere would be assigned a *"shadow"* cost corresponding to the burden imposed on society by the pollution. Broadly speaking, for *"tradeable"* or actually *traded* products, export revenues and import costs provide the essential anchor point to shadow prices, the various methodologies differing mostly in the choice of *numéraire* , foreign exchange or domestic currency. Other products' *"shadow"* prices are derived from their opportunity costs in terms of traded goods. Surprisingly little is said of externalities, but they are to be either valued directly (by attributing a value to the corresponding nuisance or pleasure) or in terms of the monetary costs actually imposed on society (if, say, water pollution is directly and wholly reflected in added water purification costs for others).

The traditional methodologies do not mention *"shadow"* rules; but it stands to reason that these would have to complement *"shadow"* prices, just like, in real life, actual rules complement and determine the use of the price system. Actual rules may govern emissions of lead and of various carcinogens, and now govern cigarette smoking. If these rules do not correspond to the social optimum determined by the project evaluator, he may presumably choose to look at projects in the light of *"shadow"* rules, more, or less, stringent. In the rest of the discussion, rules will often be mentioned, and always present at least implicitly. Just as market prices are complemented by legally binding rules and regulations, it should be assumed that *"shadow"* prices are complemented by *"shadow"* rules.

VDTS have proposed that similar adjustments should also be made to market prices in order to reflect society's distributional and end−use preferences. They assume that the government is the interpreter of social priorities, and they chose income *"freely spendable"* by the government as their numéraire, because the government can, by definition, allocate such income to whatever purposes it considers optimal. Incomes accruing to all other beneficiaries and costs born by all other economic agents must be multiplied by a coefficient <u>at most</u> equal to one. In practice, the recommended adjustment related to the ultimate use of incomes (savings or investment) often goes in the direction opposite to that of the adjustment for income distribution (savings are preferred, and people with higher incomes save more), but it can nevertheless be substantial. This is certainly consistent with the common

sense observation that, from the point of view of national development and social welfare, incomes that merely increase the consumption of those who are already very rich are less valuable than those that alleviate the misery of the very poor.

Such proposals presume that the project evaluator has freedom to impose his ideas of what is efficient and fair through his choice and design of investment projects, but lacks the possibility to do so through his influence on common laws, policies and regulations. Public investment is presumed to be more amenable than fiscal policy to the needs of the poor; the government is presumed to be dedicated to investing only in projects that lead to internationally competitive production facilities, while it uses excessive tariffs and quotas to provide protection an incentives to inefficient producers; public rules, regulations, fees and taxes are presumed to be not conducive to environmental preservation, but public investment pursues that aim. Postulating major contradictions in public policies is not, by itself, unrealistic; yet such systematic pervasiveness appears, to say the least, quite exceptional. One should also ask, if such deep contradictions remain between general policy and investment policy, who is then to determine "shadow" prices, in the light of which views of efficiency, what social preferences, what ethicalcontinue

rules? One may seriously doubt that such systematically contradictory government views and policies can be reconciled in a single unequivocal set of *"shadow"* prices.

It is perhaps more realistic to make similar assumptions for the World Bank: that it cannot persuade a country to change its policies but it can always impose its will on projects itself finances. The *"shadow"* prices are then the World Bank's; they reflect World Bank views of efficiency, World Bank societal preferences. That may all be fine; but if that is what is meant, that is what should be stated, and clearly; and there should be no surprised discovery that the project has been imposed upon a government and society that do not feel *"ownership"* of it.

Assume away these serious difficulties. Assume that conviction, politics, or World Bank pressure cause a government that pursues bad policies in general to look for good investment projects. Even under that assumption, it is very much to be doubted that *"shadow"* prices actually improve project selection procedures or, ultimately, the country's welfare. One reason for such doubt relates to the general theoretical *"second best"* question of welfare economics. It has been rigorously demonstrated that an economy meeting all the criteria of perfect competition functions efficiently, thanks to the prevalence of *efficiency prices* that guide all economic decisions. It has similarly been demonstrated that an economy that does not meet these conditions functions inefficiently (i.e with less than full efficiency). It has not been demonstrated that in such a sub−optimal economy overall efficiency is improved by the use of *efficiency prices* to guide some project selection and related decisions. To quote one of the authors of the UNIDO method:

There is strictly speaking no theoretical guarantee that decisions based on 'approximately correct' shadow prices will lead the economy in the right direction.[. . .] in my opinion, 'an act of faith' has to be made if such cosmic doubts are raised about the whole basis of applied welfare economics.7 (my underlining).

Reflecting similar fundamental doubts LM note that

it is very difficult indeed to evaluate projects if [. . .] irrational situations are very prevalent. . . . We are well aware that this situation has arisen in a number of developing countries. To the extent that we have to assume it away, we are expressing a faith in the development of better policies.8

(my underlining). In other words, the use of shadow prices does not help much in highly distorted situations; yet if distortions are mild, they are believed to be helpful.

How demanding is such an *"act of faith"* ? Imagine a universe where central project selectors busy themselves evaluating an immense, steady flow of fully prepared and precisely described projects. Each detailed description covers the investment project itself, its purposes, composition, technical characteristics, timing etc., as well as the future activities of thecontinue

7 Vijay Joshi, in the Bulletin of the Oxford Institute of Economics and Statistics, 34:1 (February 1972), p. 4; quoted in JH .

8 LM, op. cit. p. 9293 para.74.

completed project once its starts operations, its staffing, input−uses and outputs . . . This coverage is not only qualitative but also quantitative: it specifies what will be used and produced, and how many and how much of each. Out of this flow, the project evaluators pick out the best few projects, and launch them on their henceforth pre−determined paths, setting the precise composition of the investment, bricks and mortar and machinery, and also that of the future product mixes and of the factors of production and inputs to be used. No other project exists; only those so selected ever come into being. In such a universe, it may not be too difficult to believe that the use of appropriate *"shadow"* prices leads to a better set of projects being selected. If pollution entails a negative externality, or if export taxes unduly depress the price of an input, appropriate shadow prices would correct for these distortions; they lead to the selection of projects less profitable at market prices, but eschewing pollution (thanks to the use of a costlier, cleaner fuel) and using an input whose market price is higher, but one that cannot be profitably exported. The *"act of faith"* required to believe in the merits of "*shadow* " prices in such a universe is not too demanding.

LM explicitly postulate a model close to such a universe:

Imagine a Central Office of Project Selection (COPS) . The Plan has laid down a sum−total investment target for the current year, and for several succeeding years. A flow of project appraisals is coming from those responsible for making them. Put in its simplest terms, the problem is to fill up this year's investment budget by selecting the best from the list of projects coming forward.9

This passage is crucial to understanding the theoretical bases of LM's recommendations, and of all proposals to use *"shadow"* prices in project evaluation. It does not mention the implementation of projects or their later operation; this very silence must imply that there is no divergence between the blueprints attached to the *"lists of projects coming forward"* and their implementation. VDTS do not make a similarly explicit statement, mostly because they took the use of *"shadow"* prices and their implications for granted. There are indications a−plenty that the authors have in mind a similar mechanism. Neither one of these traditional project appraisal handbooks ever ask how the implementation and operation of such facilities would be ensured. Nor has the World Bank done so, despite its long−standing formal commitment to using *"shadow"* prices in project evaluations. In its case the explanation may well be that despite its formal commitment to the method, the World Bank has put it to little practical use.

The *"act of faith"* required to believe that such a universe usefully models the real world is itself immensely demanding, and excessive. Not even the late unregretted GOSPLAN could so pre−determine the actual implementation of investment plans and the subsequent operation of newly created facilities. In the real world, there exist many project selectors, each making independent investment decisions, and most without any conceivable possibility of referring to *"shadow"* prices: one selector's rejection of a project therefore does not prevent a similar project from being realized by others. In the real world, provision must also be made for managing project implementation and the subsequent operation of completed projects. This requirescontinue

9 LM p. 62.

varying, but always large, amounts of decentralization. Lower−level managers during the implementation phase, and all managers later involved in operating the project (including the general manager of the completed facility) must largely be guided by market signals: actual, not *"shadow"* prices, and legally enforced regulations.

"Shadow" Selection and Real World Management

There are many objections to such use of *"shadow"* prices in project selection. To begin with (a point which has many implications,, and to which we will have to return again and again), the government is rarely the only investor or project selector in a country, and the World Bank never. The Bank (or the government) may well reject, e.g. an automobile production project on the basis of economic analysis using *"shadow"* prices; but if in the light of market prices automobile production appears profitable, then other investors will decide to produce automobiles, with or without government help, other than the help they get in any case from the high tariff. They would then either implement a similar project or a different one for the same purpose; and they may do so in a way which is socially even less attractive than the project turned down by the government (or the World Bank) on the basis of the analysis based on *"shadow"* prices.

Suppose now that the World Bank does select a project because it appears attractive in the light of *"shadow"* prices (throughout most of this discussion one may also think in terms of the national government instead of the World Bank). This opens up a new dimension of problems at the implementation stage of investment projects. In real life, no central agency can hold sway over every detail of project implementation; nor can implementation fully follow a prepared blueprint. Implementation details must be decided by managers responsible for the project, in the light of evolving circumstances in the real world. These circumstances are signalled to managers by the prices they must actually pay for inputs and factors of production, and by the regulations they must actually obey. Only markets can fill the minimal information needs of such decentralized management. The alternative would be to instruct the general manager in charge of the project and all his subordinates to ignore market prices and legally binding regulations, and instead take all decisions concerning the project, at all levels, in the light of *"shadow"* prices and internal rules. This would be clearly impractical. Such an alternative cannot have been seriously envisaged even in the more planning−oriented environment of the 1960s and 1970s. It was certainly not put into effect by anyone, least of all the World Bank.

A fortiori, markets must guide the managers responsible for the operations of completed investment projects. Of course, this does not mean reliance on prices only. Markets and market−reliant operations are also guided by rules and regulations that govern many aspects of economic life; but these too are real rules, actually operative and legally enforceable (and normally actually enforced). They cannot be guided by *"shadow"* rules.10 They must maximize profits in the light of actual prices and wages they receive and pay, rules and regulations that actually allow them to do some things and forbid others, actually compel them to pay fines or go to prison.break

10 Once again, one must stress the close relationship between prices and rules. The main choice is to regulate the dumping of garbage in the street; the exact combination in which certain times, places, containers and types of garbage are forbidden and others are subjected to payment of fees is secondary. A market rule, like a market price, is one generally applicable; a *"shadow"* price or rule is one that is only applied by the selector and designer of a project.

A project designed for broad input and product mixes determined in the light of "*shadow* " prices would have to be implemented by managers who aim for efficiency in the light of market prices and legally binding regulations. Technological, product and factor−mix choices made in the light of *"shadow"* prices would appear sub−optimal to such managers, who would tend to depart from them, when appropriate in the light of market signals. The construction component of the investment may have been intended to make use (e. g.) of low *"shadow"* −priced bricks laid by low "shadow"−waged unskilled labor. During actual construction, however, works managers would want to substitute low market−priced cement poured by specialists earning market wages. The project may be

designed to avoid using a polluting construction process; but if it is cheap (in terms of market prices) and legal, managers would use it. If the project incorporates pollution control equipment, economic in the light of the *shadow* cost of pollution, or necessary in the light of *"shadow"* rules forbidding it, managers would not use the equipment if such use is costly, and if pollution is not really penalized in real life. If the project incorporates socially desirable practices forbidden by legally valid regulations − say flexible work schedules − managers would not actually follow such practices if doing so could land them in jail.

The *"shadow"* prices advocated by most project evaluation methodologies under such circumstances would reflect scarcities and opportunity costs as they affect society as a whole. In the earlier automobile example, the *"shadow"* price of domestic cars would be derived from the cif price of imports; analyzing the project in the light of this price would show that domestic production would be less profitable or, most likely, actually loss−making for the national economy. In the light of *"shadow"* prices, the project would be rejected. One difficulty (already mentioned) is that even if the Bank (or the government) rejected the automobile project, others may implement it, perhaps even less efficiently.

Conversely, the production of machine−tools may be unprofitable only because of an overvalued currency and, say, artificial scarcities of some inputs; it would appear profitable in the light of proper *"shadow"* prices. Let us follow the fate of this machine−tool factory. Assume the World Bank decides to finance it. Its managers must operate a plant (and units of a plant) that, in the light of market prices and valid regulations, would appear to have been designed purposely to produce unprofitable product mixes, using unprofitable factor proportions, in compliance with non−existent regulations and perhaps in conflict with existing ones. If the gap between *"shadow"* prices and market signals is high enough, a complete ex−post rearrangement (in today's jargon, *retrofitting*) might be irresistible. Managers of factories designed to produce machine tools (profitable at shadow prices) would wish them to be retrofitted at great economic cost to produce automobiles, profitable at market prices. Managers of generating plants set up to use natural gas (cheaper at *"shadow"* prices, taking into account externalities) might retrofit them to use petroleum fuel, if that is cheaper at market prices; they might even switch to coal if the market price difference warrants it, at economic and financial costs (and pollution−generation) greater than if they had designed the project in the light of market prices in the first place. Even without such a complete overhaul, there would be strong incentives to change all things changeable in conformity with actual market incentives and legally binding regulations. This would generally be more costly than conforming to market signals in the first place; it may also be more inefficient even relative to the objectives reflected in the shadow prices used by the project evaluators.

Suppose domestic steel is subsidized. Because of this, at market prices it is cheaper than plastics, though the latter actually have a lower social cost. A new project is to produce a goodcontinue

either with steel or plastic. Using *"shadow"* prices in project analysis, it is designed to use plastic, and it somehow manages actually to do that when it begins current operations. It reflects the higher market price of plastic in its own sale price, thus perhaps inducing other consumers to make greater use of other steel products, cheaper at market prices but even more wasteful, in social terms, than the project in question would have been if it had been designed as a steel user. Or again, suppose that a new production facility is designed to use more stringent pollution controls or other environmental measures than are imposed on the rest of the economy: *"shadow"* rules more stringent than actual or market rules. This raises its costs, possibly shifting demand to other, environmentally much more burdensome, producers and processes.

Or again, assume that legal standards in a country provide a certain compensation to those forcibly evicted from land needed for investment projects deemed to be in the public interest; and that the World Bank considers that true social costs are better reflected in more favorable *"shadow"* rules. Projects may then be rejected because the costs imposed by these *"shadow"* rules are excessive. The same projects might then be carried out by others, perhaps less well; or other projects may cause even larger numbers of people to be displaced, but indirectly, so

that it is impossible to apply the *"shadow"* rules. Such inefficiencies and confusion are not confined to the operation of the facilities directly created by the project; more generally, overall efficiency may be lowered if a part of the national economy is subject to separate prices and rules − even if these prices and rules would be optimal if applied to the economy as a whole.

Completed investment projects cannot be managed efficiently by a central planning organization (not even by the World Bank), nor by reference to such an organization's value systems and the *"shadow"* prices and regulations based upon it. Even approximations to such centralized management would be grossly inefficient. Yet even a modicum of decentralization must rely on actual market prices and on binding regulations, not on *"shadow"* prices and rules. Gearing major investment decisions to one set of price and regulatory signals, ignored by most investors and producers, while gearing implementation and operation decisions to another set of signals, is not a recipe for greater efficiency. It is a recipe for chaos.

LM have conceded that

not all the distortions (i.e. lack of correspondence between prices and real costs or benefits) in the price mechanism (. . .) can be adequately dealt with by using accounting prices in project selection. Many of the distortions can be satisfactorily dealt with only by removing them − i.e. by adopting policies which lead to a proper correspondence of prices, and costs and benefits.11

This warning is repeated in an aptly named *"envoy,"* 12 which notes that LM's "*methods are likely to give the best results where most extensive use is made of the price mechanism."* 13 However, they do not indicate how bad are the results of these methods where irrationalcontinue

11 LM p. 37.

12 Envoy or envoi, from the French, is the name given to the final stanza of a ballad. In French ballads, the envoi always starts with the word "Prince", and usually is in the form or a request or warning.

13 LM p. 100.

situations do prevail, and where the use made of the price mechanism is not *"extensive"* . Nor do they say how good (or how bad) is the answer given by the methods when distortions cannot be dealt with *"satisfactorily* [. . .] *by removing them"* .

VDTS seem to disregard this problem altogether. They seem to hold that the practicability of using "shadow prices" for broadly Pareto−efficient allocation decisions is self−evident, and call this *"traditional practice"* . Their list of *"possible objections"* 14 deals entirely with those deemed to be addressed to the inclusion of end−use and income distributional considerations. The difficulty − indeed, the utter confusion and gross inefficiency − likely to be produced by allowing project selection to be guided by one set of prices, and project implementation and the subsequent management of completed projects by another, is not recognized.

World Bank Practice

Discussions of an earlier draft of this paper have bought out a different, more modest, but ultimately not a better view of the appropriate use of *"shadow"* prices. It has been suggested that project design should be optimized in view of the applicable market prices and regulations. The project so designed should, however, be also evaluated in light of *"shadow"* prices. This, it was said, should help establish whether the project satisfies minimal cost−benefit criteria from the social point of view; if not, it should not be selected by the Bank.

Such a use of *"shadow"* prices would greatly restrict their scope relative to the traditional recommendations. They would not serve to optimize project selection or the composition of projects; they would merely serve as an ultimate safeguard against being lured into socially undesirable projects by misleading market information. This limited scope is consistent with the intention, expressed by the Bank in recent years, to abstain in principle from lending to economies (or to sectors) found to be *"thoroughly"* distorted − though how thorough is thorough enough has not been thoroughly defined, and a glance at some highly distorted economies that have in fact received substantial World Bank lending leaves one thoroughly puzzled. One should nevertheless note that if *"shadow"* prices differ from market signals (and if not, the discussion is irrelevant), projects well conceived and implemented from the point of view of market signals, as they would be by competent managers, would be sub−optimal from the point of view of *"shadow"* prices. Satisfying one optimality criterion necessarily precludes also satisfying the other. Consequently, if project selection criteria are tough enough, no project might satisfy both eligibility criteria.

Yet even such limited use of *"shadow"* prices is more likely to worsen than to improve World Bank project selection. To realize this, one must clearly define what *"improve"* means. The World Bank's objective is to further development. Therefore, its project selection is *"improved"* if overall development proceeds better with the projects it selects than without them. it. Even if *"shadow"* prices are used only for limited tests, to eliminate projects not meeting *"shadow"* price criteria, this may well make World Bank projects look nice and neat, and keep the World Bank away from difficult and messy areas; but it is likely to cause the overall composition of the investment program to worsen, and the World Bank's overall contribution to development to decline.break

14 *VDTS p. 7.*

The problem is that of alternative investment decisions. These may concern the same or similar projects, implemented without World Bank assistance, perhaps by the government, but also by the private sector. The alternative may also consist of very different activities responding to the same market incentives, but not responding to the *"shadow"* deterrents to World Bank investment. Assume, for instance, that domestic fuel prices are taxed too heavily − they are excessive; their *"shadow"* prices are much lower. The World Bank then considers a highway project, whose economic justification rests on road user savings. At market prices, the project satisfies all cost−benefit requirements; with lower *"shadow"* fuel prices, it does not. At first blush, it seems that the project is not good, and that it is eminently reasonable that the World Bank should not finance it. After all, a share of road user savings (the share on which the project justification hinges) consists of taxes; the amounts saved by the road users would be lost by the government.

Yet the World Bank's decision only affects its own program. The Bank abstains, but the national government, or the provincial authorities, may still implement the project, perhaps less well. Even if they do not, the *"without project"* alternative is not simply that trucks proceed on the old road, paying their high fuel taxes to the government. They may make short−cuts through the desert, trading excessive wear of tires and chassis for fuel savings; or truckers may make other fuel−saving investments, like diesel engines or larger trucks, more costly in the aggregate and less effective than the highway. If none of this happens, if indeed they continue taking the old road and pay the high fuel prices and taxes, the resulting high transport costs will influence locational investment decisions. Instead of saving transport costs through reduced fuel usage, consumers and producers would then save fuel indirectly, by relocating their plants, houses and activities. Things and people would be packed together more closely than would be desirable, and the economy would bear correspondingly higher costs. The World Bank's own set of investment projects may look better, particularly if it also avoids financing the projects whose locational decisions have been heavily distorted − but the country's overall investment program may have become less efficient.

In the 1950s, Oskar Lange advocated decentralized management in the then centrally planned economies through the use of computed efficiency prices. However, these were not to be applied just for project selection. They were

to be imposed upon all managers in everyday operations. Unlike for LM, VDTS etc., they were true market prices, not just *shadows*, though derived from computation rather than competition. Nevertheless, when serious consideration was given actually to using the system, in Yugoslavia and in Poland after 1956, its drawbacks rapidly appeared. Many people had once underestimated the importance of a rational price system, and over−estimated the possibility of by−passing it through appropriate analytical techniques. The irreplaceable merits of market guidance have now been broadly recognized. The World Bank has long shifted to single−minded confidence in the virtues and efficiency of market−based solutions and market−related management techniques, yet it has continued to reaffirm its confidence in economic analysis methods based on the use of shadow prices.15 Ex post evaluations of projects also refer to *"shadow"* prices in calculations of project costs and benefits.

An extenuating circumstance for the World Bank is that it has not much practiced what it preaches. In recent years it has made very limited use of "shadow" prices, improving their usecontinue

15 *ECONOMIC EVALUATION OF INVESTMENT OPERATIONS, Op. cit.*

mostly by reducing it. The VDTS method was never formally abandoned, but it is actually applied only to a minority of projects, and in a perfunctory manner. The assumptions and data underlying the calculations are rarely presented in full in project appraisal reports, and they are given even shorter shrift in ex post evaluations. This evolution was the result of practice, not of methodological debate, which has been largely absent since the time when the method was imposed, in the 1970s. One might say that the analysis of economic rates of return based on *"shadow"* prices still constitutes the World Bank's sole formal organized project evaluation religion, but religious fervor is notably absent, and even formal church attendance has drastically declined.

VDTS had explicitly proposed to extend the use of *"shadow"* price techniques to weigh income distributional and similar considerations. After a few very tentative attempts to give different weights to project benefits according to the beneficiaries' income levels, World Bank practice soon came to limit the use of *"shadow"* prices to evaluating the purely economic benefits of projects. Later the World Bank has laid increased emphasis on "*poverty alleviation* " as its major objective. Practically every project's relationship to that objective is now discussed, either (in the majority of cases) as part of the project's justification, or occasionally to explain why the project is justified despite the lack of much direct impact on poverty alleviation; but this impact is normally not incorporated into calculations of project benefits.

Another aspect of World Bank practice in this respect is more disturbing. Strict computation of true *"shadow"* prices is extraordinarily demanding in terms of data. These demands can never be met fully nor, in practice, even approximately. This is so even if the calculation is restricted to Pareto−optimal *"efficiency"* prices, and if income distributional aspects, final uses and externalities are ignored. LM, VDTS et al . recognize this difficulty, albeit somewhat grudgingly; but the various simplified estimation procedures they suggest are neither truly simple, nor are their information needs easily met. A methodology that is difficult to understand, relies on uncertain assumptions and arbitrary projections based on vague data, and is subject to very limited scrutiny: all the ingredients are present for bending calculation results in the desired direction, perhaps even involuntarily. How often such bending occurs is difficult to judge, not the least because appraisal and follow−up reports (supervision, project completion, operations evaluations . . .) give few details on ERR calculations and the assumptions going into them. However, the temptation to bend the results may have been stronger in earlier days, when these numbers exerted a greater influence on project selection decisions. The temptation may well have been weakened by the drastic decline in the importance attached by the World Bank to ERR calculations. There is probably less incentive nowadays, and few obvious occurrences, of *"shadow"* prices tilted in ways needed to justify projects.

Needed: Faith in Markets

A world where prices and regulations do not fully reflect social benefits and costs presents deep and real problems to the analyst and project selector. When distortions are severe, when market prices and legally binding regulations differ markedly from the socially desirable optimum, allowing market conditions alone to guide investment decisions is clearly not going to lead to a pretty good approximation of a social optimum. The social optimum itself is confused in such conditions: what one point of view considers severe distortions would not be allowed to remain if others, with at least equal powers, did not consider them desirable. However, allowing some investment decisions to be guided by *"shadow"* prices and rules,continue

conform to one set of optimality, while most other investment decisions and all operating decisions are guided by market signals, would lead to even worse results, compounding social inefficiency with firm and project−level chaos. The traditional "solution" to market distortions is wrong. So is the reduced scope version, in which *"shadow"* prices are only used for an ultimate check on projects; this offers no indication that it is likely to improve a country's overall investment program. The World Bank and other project analysts should return to the *"act of faith"* needed to rely on *"shadow"* prices, and (translating it into Portuguese) make an *auto da fe* of the LM−VDTS project selection methodologies.

Bad policies should not be by−passed; they should be changed. Every effort must be made to bring about policies that help narrow the gap between market signals and social desirability. However, such gaps will surely remain, and even if distortions are small, if market prices form a "pretty good" approximation of social costs and benefits, it still cannot be demonstrated that selecting investment projects in the light of market prices, with the aim of profit−maximization (whenever profit is relevant) will actually lead to a "pretty good" approximation of a social optimum. But historical experience shows that in such circumstances, when moderately distorted markets are allowed to guide most investment decisions, country after country made pretty good progress in development and welfare.

When dealing with economies that are only moderately distorted (and where wealth and income distribution and other social conditions are only moderately distasteful), whenever prices and profits are relevant to a project benefits and costs should be calculated in relation to market prices only, taking into account actual rules and regulations. The costs and benefits so computed will not be equivalent to social costs and benefits; they may indeed greatly diverge from them in any specific project. However, there is ample empirical evidence that when market signals are followed in reasonably undistorted economies, the overall result is investments and other economic activities that tend to cause development to proceed, and to enhance social welfare.

Such an acceptably undistorted state is not reached automatically. This is not a *"natural"* state that would always come about if only governments abstained from interference. None of the above arguments in favor of reliance on market signals should be used in favor of a *"hands off"* attitude of government, or against government interventions. Such interventions are often needed to render markets more efficient or to bring about socially more desirable distributions of initial advantages and incomes. Neither the original nor the continuing conditions of market efficiency may exist without government interventions. Rules concerning property, the definition of the commons, competition and collusion, information disclosure, and much more, need to be defined; their respect must be imposed; social programs must be provided and their financing assured; the distribution of income and wealth must be shaped, through taxation or otherwise; externalities must be defined and internalized as much as is practicable, through appropriate rules, taxes and subsidies. Many desirable public interventions modify prices; others impose actions not directly induced by prices and profits (and first of all, the respect of property rights). Every economy needs many such actions, though it is a good rule to undertake them sparingly and knowingly. However, modifying prices and imposing new rules are very different from leaving prices and rules as they are, yet selecting investment projects in the light of prices and rules as we would have wished them to be.

When an economy is thoroughly distorted, and expected to remain in that state, no investment offers good hope of a substantial positive development impact; nor is it possible tocontinue

estimate what would constitute good investment. In such situations, no technique allows market distortions to be bypassed and a good investment program to be developed. The problem is not the one alleged by present World Bank practitioners, that *"shadow"* prices cannot be calculated; it is that imposing ideal prices (and rules) on a few decisions does not lead to overall improvement of an economy where most other decisions are taken in the light of very different information.

When distortions are mild, however, empirical evidence amply shows that investments following the profit motive collectively achieve a good development impact, even if individually their impacts somewhat diverge from the social optimum. For public sector investments too, and for World Bank projects, when a project justification refers to quantified economic costs and benefits, these should be calculated at market prices (as they are expected to affect project investments and operations). Any attempt at systematically overriding those signals, and using *"shadow"* prices to select projects, or even, more modestly, just to vet those already selected, is more likely to worsen than to improve the country's overall investments.

The market information to be used is, of course, not limited to present prices and regulations. Just like any investor, the World Bank (and, mutatis mutandis , the government) should try to forecast the prices and other market conditions likely to influence different stages of the project: to return to the previous example, the highway project would of course not be justified if fuel prices were known to be on the verge of tumbling down. Even more obviously, if policies are expected to change, this change should be taken into account, and would be, by any investor in possession of a modicum of common sense.break

II— Non−Quantifiable Benefits

Who Knows the Price of Everything?

Early treatises on the *"economic analysis of projects"* had limited themselves to industrial projects. This is explicit in LM's title, and for UNIDO it is implied by the sponsoring organization. This was not merely to limit discussion to a more manageable field, nor simply because industry in those days had particular attractions for development economists. LM candidly recognized that their proposed project selection methodology was fundamentally inapplicable to many development projects, because for many *" . . . projects, (. . .) plausible quantitative assessment of benefits cannot be made. This is true of health, education, defence, police . . . "* 16 Industry probably presented the least difficulty in quantifying benefits, especially as LM paid little explicit attention to social and environmental externalities, where problems with quantification were most likely to be encountered.

One may recall that LM's (and VDTS'S) project selection method rests on the ability of the COPS to allocate investible funds among the innumerable prepared projects brought to it. At the end of the process, COPS will have approved all projects that yield a present value of at least zero, calculated at the social discount rate, and rejected all other potential projects. This process is vital to establishing the social discount rate itself (also called Accounting Rate of Interest and Opportunity Cost of Capital). It is the rate which, through this project approval process, exhausts all funds available for investment . The same is true, with some variants, for VDTS's accounting rate of interest, which is "simply the internal social rate of return on the marginal public sector project, this being the discount rate that ensures a balance between the supply and demand of public investible resources."17

Such a process is inoperable if substantial investible funds must be allocated to potential projects whose ERR cannot be calculated, and which must be approved or rejected according to quite different criteria. A discount rate cannot then ensure a balance between the supply and demand of investible resources. Even conceptually, it is not clear whether the *"social discount rate"* (under its many names) has any meaning at all if it cannot constitute the cut−off line between desirable and undesirable projects. LM do not actually flag this difficulty explicitly, but they obviously recognize it, and provide an elegant device for palliating it. They group separately all projects with quantifiable benefits, and "*assume that the investment budget for this area has been decided. When we speak of the investment budget, we henceforth mean the budget for quantifiable investment* (sic)"*18* (meaning presumably investment with quantifiable costs and benefits).

Obviously, this assumption is in general quite unrealistic, and its practice would be highly undesirable. It is difficult to conceive of circumstance under which it could be efficient or desirable for national planning and financial authorities to divide projects into two large groups,continue

16 *LM p. 62.*

17 *VDTS p. 142.*

18 *Id.*

according to whether or not their benefits are quantifiable, and then to decide independently how much is to be invested in each group of projects, without regard to investment costs, benefits, needs and opportunities in the other group. On the other hand, if needs and opportunities in both sectors were weighed before investible funds were allocated between them, then in effect some way would need to be found to compare the benefits between groups; we would then be back to the dilemma which LM had sought to assume away.

VDTS do not limit their method to industrial projects only, and they seem to advise that it should have universal application. Tellingly, the title of their book, the "Economic Analysis of Projects ", itself contains no sectoral restriction. Nor does the body of the work. The illustrative list of sectors in Chapter 1 ("*infrastructure, industry, agriculture, education ..."* 19) (my underlining) seems to express an intention to apply the method to all types of projects. This is also implicit in the option for

rates of return that take explicit account of the impact of the project on the distribution of income both between investment and consumption and between rich and poor . . . 20 that involves attaching suitable weights, to be determined by the appropriate decision−maker, to the benefits of the project that accrue in different forms (consumption or investment) and to different beneficiaries (rich or poor) . . . 21

If the preference (positive or negative) for a low−saving poor over a high−saving rich can be precisely quantified, and reflected in "*suitable weights* " attached to various project benefits, than it should not be too difficult also to quantify judgments relating to the benefits of education (or environmental preservation or the enhancement of the status of women, though these objectives did not loom large in the VDTS discussion).

VDTS give some recognition to the difficulty of quantifying the benefits of some projects, but it is brief and ambiguous. The section on *"Cost minimization"* refers to "*(. . .) cases in which valuation of the benefits is difficult − for instance, improvement in health services . . .* ", and notes that in such cases an "*assessment of the least cost per unit of physical output (. . .) may be helpful . . .* ". However, they then go on to remark that "*the differences in costs (. . .) are not, and should not be used as a substitute for a proper measure of the benefits of such projects . . . "* 22 Quantification of certain benefits may be difficult, but it is obviously not deemed to be impossible; and it is deemed necessary for correct project−related decisions.

The logic of VDTS is, then, to quantify all social costs and benefits of all types of investments. The universe of complex considerations governing the desirability of a project is thus ultimately reduced to a single figure, the ERR. The Bank's then President was also known to be confident in man's ability ultimately to reduce all complex decisions to a single quantifiedcontinue

19 *VDTS p. 15.*

20 *VDTS p. 3.*

21 *VDTS p. 6.*

22 *VDTS p. 42.*

"*bottom line* ". Not surprisingly, the project selection method reflecting such a belief was also endorsed by the Bank.

For a time, quantified benefits were indeed attributed even to some projects in "*social sectors* ". The benefits of, e.g. "*births averted* " through population and family planning projects were purportedly measured, assigned a value in "*constant* " national currency units, discounted back to the present, and related to the dissemination of contraceptive means. They thus served to derive an ERR for such projects (usually deemed to be of the order of 3080 percent). The favored method for valuing the benefits of a "*birth averted* " usually referred to discounted lifetime earnings minus the discounted costs of consumption, including those of child−raising. There is also some precedent for income−distribution considerations to be reflected in the "*shadow* " prices assigned to the income flows generated by project benefits.

Such early attempts at calculating ERRs for all projects never quite came into general use and were soon abandoned. For a long time now, quantitative benefits have been attributed to a minority of World Bank projects only. The projects for which ERR calculations are deemed not to be feasible include most *"social"* and similar projects whose main object is not to raise the output (or reduce the costs) of market−oriented products. Projects in human resource development (including education, health and family planning), pollution control and most other environmental projects fall into this second category.23

The Consumer Surplus

Difficulties arise for market−oriented projects which change the Bank's estimate of the *"shadow"* price of the project's product. This rarely arises for tradeable goods (whose *"shadow"* prices are derived from international prices), but more likely for power and similar infrastructure services. *"Consumers gain an increase in the consumer surplus whenever a project lowers the price from what it otherwise would have been. This increase should be treated as part of the benefits of the project"* . It may sometimes be difficult, but it is *"desirable to quantify them in all cases when they are significant."* 24 However, the OM warns, project benefits should also be adjusted for the income distributional and similar implications of the consumer surplus.

This is questionable. The OM's prescription should, presumably, be interpreted as excluding the part of the consumer "*surplus* " related to reduced earnings by producers, as this surely does not constitute a surplus for the economy as a whole (actually, the term "*consumers* " refers to the users of the project's output, and they may themselves be producers). Let us consider the true net surplus only. To simplify, suppose an entirely new project, say to produce electricity in a country (or region) where none is produced. The plant being set up will produce and sell 100 units at a unit price of 100; but we know (say, through consumer surveys) that it would be possible to sell 50 units at a price of 200 each (and fewer units at still higher prices). The consumers of those 50 units will indeed enjoy an additional consumer surplus of at least 100 each. Not counting its costs, the benefits brought by the

project to the national (or regional)continue

23 *Annual review of Evaluation Results 1992, Report No 12403, the World Bank, Operations Evaluation Department, October 13 1992. Annex Table 1.1.*

24 *World Bank, Operational Manual Statement 2.21 of May 1980, # 24 and 25.*

economy appear to be worth at least 15,000 − the 10,000 actually paid, plus the 5,000 that is the minimal estimate of the additional amount some consumers would have been willing to pay for electricity. The market value of the output, i.e. the price multiplied by the volume of projected sales understates the benefits of the project. If annual costs (including interest) exceed 10,000, the project would not proceed, and net social benefits which could amount to 5,000 or more would be lost. The operational rule appears to be justified; only by treating the increase in the *consumer surplus* as part of project benefits can the project selector proceed with this apparently desirable project.

Yet there are strong objections. Most projects are decided under profit−maximizing rules only. Most investors only look at prices and revenues; they disregard consumers' surpluses, except for discriminating monopolists who can confiscate them to their own profit. It may be desirable to enact rules or make arrangements which, in certain sectors, would transfer consumer surpluses to producers; it may even be desirable to have such an arrangement for a specific project, for instance, when the investment is very lumpy, and the short run marginal cost of production much lower than the average cost − within certain ranges of production a large electricity plant may be a good example. However, it is not at all clear that the general allocation of investments and of other resources would be improved if some projects − those financed by the World Bank (or those of the public sector in general) were systematically decided on the basis of benefits including the consumer surplus, while others, including all private sector projects (except those with discriminating monopoly powers) perforce excluded them. Nor is it even clear that the overall functioning of an economy would necessarily be improved if all producers were to act as discriminating monopolists.

Suppose the power project just discussed goes ahead, because its benefits exceed its costs if the consumer surplus is counted. At market prices, it runs a deficit. If that is met through subsidies, can one be assured that social welfare is improved if society at large subsidizes a project, part of whose impact is to enlarge the consumer surplus of some consumers? Suppose the project can, somehow, act as a discriminating monopolist and charge higher prices to those consumers who are willing to pay them. In this stacked example, everyone seems to benefit, as consumers get a product they want, and for which they pay as much as they are willing to pay. But suppose that the project is beneficial even if it has to charge a single price, and comes into being: should it nevertheless be enabled to discriminate, and make a corresponding super−profit? More generally, the issues are whether general welfare is improved when the investment decisions of a category of firms are determined as if they were discriminating monopolists; and, if so, should they effectively follow the pricing rules of discriminating monopolists, and, if not, what pricing rules should they follow.

Though absolute rules are unlikely to be absolutely valid in this difficult field, one might perhaps reformulate in the following way the instructions regarding consumer surplus: as a rule, only market signals, including the market price, should be taken into account in a project's benefits − the consumer surplus should be disregarded. However, project authorities themselves may find it desirable and feasible to act as discriminating monopolists: in the power example, they may, for instance, levy large subscription fees or similar charges. The government may also find it desirable to subsidize the project. The practices of discriminating monopoly may be allowed or even encouraged in some cases; or specific subsidy and tax measures may be applied to encourage investment in certain circumstances, when this improves efficiency and equity. From the point of the project, however, such policies are part of the market environment;continue

project benefits should reflect their impacts, but not that of consumer surpluses kept by consumers.

As with other rules, actual World Bank practice has been inconsistent, and it therefore probably produced results less bad than would have the consistent application of bad rules. Quantified consumer surplus is sometimes a frill in project justification (added on to show that the project is even more beneficial than had been shown earlier); sometimes heroic efforts are made to calculate the consumer surplus illustratively for projects otherwise justified on non−quantified grounds. However, variants of consumer surplus are also used as the principal justification of some projects.

The consumer surplus should not be confused with minimal estimates of the price consumers (users) of a product would all be willing to pay for it. For instance, road user savings are, in some sense, a *consumer surplus* if no toll is levied; but, properly restricted to those who would have made equivalent trips even if the project under consideration had not been constructed they constitute an estimate of the price all should be willing to pay, which is also a minimal estimate of project benefits.

Disregarding the theoretical issue whether the consumer surplus should be included in project benefits, the Bank has nevertheless found it difficult actually to calculate consumer surpluses even for market−oriented productive projects. Quantifying project benefits for many other projects has been found immensely more difficult, and in fact it has rarely been done in recent years. All in all, in a recent group of 277 loans and credits, the ERR was computed for only about 30 percent of all projects.25 Moreover, even when it is computed, it often leaves out some benefits and costs that are not quantified; for instance, institutional, environmental, health or educational aspects.

Yet the ERR continues to figure in the project selection process for this minority of projects. If anything, in its ex post evaluations, the OED attributes an even more important role to the re−estimated ERR's relationship to the original estimate and to the "opportunity cost of capital, normally set at or above 10 percent."26 Substantial shortfalls from either of those figures are taken as prima facie indications of project failures. Yet when the major part of investible funds is allocated to projects without quantifiable benefits (and when even projects with quantifiable benefits are often said also to possess non−quantifiable benefits, and also non−quantifiable costs), it is impossible to give a meaningful definition to the *"opportunity cost of capital"* ; it is a label without content.

The Monetary Benefits of Education (& of the Early Death of Retirees?)

The concept could be salvaged through the LM fiction that the investment budget for projects with quantifiable benefits is somehow pre−determined. But while this device serves to palliate a difficulty, as an assumption it is quite unrealistic. The shares of investible funds to be allocated to different types of projects at a given time and place depend on the store of potentialcontinue

25 Annual Review of Evaluation Results 1992, Op. cit., p. vii and p. 3.

26 Annual Review of Evaluation Results 1992, Op. cit., p. vii.

investments in all sectors, and on their partially interdependent potential development contributions, quantified and unquantified. There is presumably both complementarity and competition between the two types of investment. Opportunities in (say) agriculture both give reasons for postponing investments in education (agricultural investments will give high returns fast) and for advancing them (because agricultural progress raises the need for educated manpower, engineers, agronomists, literate peasants . . .). Similarly, directly productive investments with quantifiable benefits may fulfill certain environmental needs, yet create others. Determining the size of either program without looking at the size and composition of the other would be senseless. Anyway, if the World Bank (or a government) could predetermine the amounts to be invested in projects with quantifiable

benefits (and therefore also in the other types of projects), why not just push the fiction further and predetermine the amount to be invested in each individual project, thus forgoing the need for a project selection methodology?

This is obvious and needs little elaboration. However, it has implications for the *social discount rate*, which has been defined as the rate which, when used for selecting investment projects, ensures that investible funds are fully used. When investment projects cannot be selected with reference to such a rate, the concept of the social discount rate itself, under all its names (including *Social Discount Rate* and *Opportunity Cost of Capital*) loses its meaning and its usefulness even for selecting projects with quantifiable benefits.

It may therefore be tempting to make another gallant effort to attribute quantified values to all project benefits and costs, a necessary and sufficient condition for the precise determination of the desirable distribution of investments between those sectors and, say, manufacturing and railways. The temptation to make such an effort seems also to be felt in another context. It is reflected, notably, in proposals to "correct" GDP figures to reflect all sorts of other influences on welfare.

A priori , it seems tempting to attribute monetary values to project impacts on human capacity and health, the environment and other non−market oriented costs and benefits, as a precondition for exactly equalizing marginal returns from investment on health, education and, say, highways. Economists and others have, in fact, frequently calculated the benefits of investment in education, and the rates of return attached to it. Attempts have also been made to extend similar calculations to other social and human expenditures, and to environmental costs and benefits. Ex post statistical calculations that show the relationship between education and private earnings are probably no more unreliable than other cross−section studies. It is possible, with similar reliability, to show correlations between education and other favorable and unfavorable behavioral patterns with impacts, say, on health and family planning. Economists and others sometimes use these as indicators of the benefits brought by education. However, while correlations are certainly shown up by such studies, the direction of causality is not. Rather than a unidirectional causal impulse from education to other desirable behavioral patterns, causation may originate from other, unspecified cultural phenomena which predispose both to education and to other behavior (and which may facilitate access both to education and to high incomes). Such a more complex line of causation would certainly fit observed phenomena, like the higher education levels for girls, lower mortality and higher family planning practice found in South than in North India.

In other words, education is clearly highly beneficial. The higher earnings closely correlated with higher education are, however, not all due to the benefits of education itself; andcontinue

it may be impossible to cancel out all such other influences through statistical techniques. Furthermore, social benefits may not be fully measured by earnings: the contribution to social welfare made by drug dealers may be no higher than that made by monks, and even that of lawyers does not necessarily exceed that of school−teachers.27 It is somewhat paradoxical to deduce the social value of education from market prices (education−related wage differences), while recognizing or assuming that markets do not directly induce the provision of education services commensurate to the benefits of education.

Yet even if the benefits of education itself were precisely known, those of education projects may not be. To return to the Indian example, Indian sociologists have suggested that the difference in the school attendance rates of girls between North and South India is due not so much to the supply of schooling as to family and societal attitudes, in particular the fear, by no means unfounded, that girls in North India incur a significant danger of being molested on their way to and from school. If so, providing more schools and teachers may not bring the expected benefits. Thus extremely high economic rates of return attributed to early World Bank population projects failed to materialize, in part because the projects acted on contraceptive supply when demand was the greater constraint. One may also recall, not facetiously, that in some traditional societies only men with "modern", Western−oriented attitudes shaved or trimmed their beards. Equating beards with conservatism, several

modernizing governments imposed smooth faces or other cosmetic changes.28 Beards disappeared, but reactionary thinking did not always go with them.

Even if education's relationship to private earnings could be reliably deduced, other benefits (including those most important to society as a whole) are external to the future workers and could prove more resistant to quantification. This applies even more strongly to health and similar benefits and costs. Attempts to relate those to wage earning expectations have been rightly ridiculed; but no obvious substitute comes to mind. Perhaps this is partly because in different circumstances human societies place different values on health and life. Thus most societies are willing to spend vast resources on saving a specific individual from mortal danger (a drowning man, a child lost in the woods), yet may constrain other forms of spending much more cost−effective in terms of lives saved anonymously at unspecified times (preventive medicine, prenatal and neonatal care). Or, to illustrate another seeming inconsistency, the World Bank itself has expressed great concern with the plight of specific groups and individuals displaced by development projects it finances, yet it continues to advocate adjustment policies that displace much larger number of people in often more difficult conditions. Some such revealed choices might be inconsistent, though they need not be. They show that different values are placed on the same things according to circumstances.

Such choices also illustrate what should be obvious, but is sometimes forgotten, that human welfare is valuable by itself; actions that affect it cannot be measured by their sole impact oncontinue

27 *The faith earlier expressed in markets was pragmatic and related to their overall impact. It was definitely* <u>*not*</u> *faith in their ability to value each individual in proportion to his true contribution to social welfare ("true* " contribution being, of course, determined by me).

28 *Early in this Century, Reza Shah Pahlavi in Iran and Kemal Atatürk in Turkey banned beards; so had, much earlier, Peter the Great in Russia. Symbols of traditionalism in those societies, beards were considered subversive by the government of Napoleon III in the 1860s and the World Bank in the 1960s. Reza Shah and Atatürk also forbade women to wear veils in public. Before Atatürk, the Young Turks had ordered the turban to be replaced by the fez. Both Peter the Great and, a little later, the Spanish government, banned some traditional clothing (in Spain, this lead to riots, the "motin de Esquileche").*

productivity and consumption requirements. Many societies are willing to incur a cost for birth prevention and abortion, yet also to save an infant life, even if the infant is unlikely ever to work. Similarly, most societies also incur costs to prolong the life of retirees. These actions can therefore be presumed to contribute to social welfare. But welfare is also subjective, and cannot be fully reduced to a common denominator, a measurable single numéraire. What contributes to welfare and by how much is often a matter of taste, not subject to methodical debate: <u>de gustibus non est disputandum</u> . All this renders illusory the pursuit of quantification, in terms of a single monetary value, for the costs and benefits of many projects. Some things have a value, but do not have a price.

The Value of Environmental Externalities

Efforts at quantifying environmental benefits and costs run into similar obstacles. There is considerable ambiguity even on the cost to be attached to the depletion of a marketable resource, like minerals including petroleum. The present price is known; but neither future prices nor the extent of reserves are known with any certainty, and (like all projects) the cost of getting the resource to market may have to be estimated. Competitive bids for concessions to exploit the resources may provide an indication of their market value; but environmentalists would argue that these are generally under−estimates, and indeed political risks, investor short−sightedness or restraints on competition may inhibit bidding. There is also considerable historical evidence that, at least well into the 1960s, monopsonistic practices among major oil companies reduced the bid price of concessions below true competitive

levels. Yet deviations in other directions also exist; technical change has been known to reduce or eliminate the scarcity of certain products, or otherwise reduce their value: thus pollution concerns have reduced the value of sulfur and of many varieties of coal.

Mineral reserves have always been known to be non−renewable resources, they have long been appropriated, clearly the property of someone (possibly the government); and their end products, the minerals themselves, have precise market prices. Attributing a price to other natural resources runs into much greater difficulty, particularly when the realization that something constitutes a *"resource"* is itself recent, and perhaps partial. Forests (both tropical and non−tropical) are good examples, within national boundaries; sea fish internationally. In both cases, there has been a time when human exploitation was well within the limits of natural reproduction: the original stock was not affected, and needed no price. That situation is now long past, but rationing mechanisms have not fully evolved. The difficulty is perhaps greatest for *"sink"* functions. Discharges of garbage and pollutants on and under the ground, into water and the air have long constituted local nuisances; they must now obviously be limited, but the exact value of the nuisance is difficult to fix and the tollgate where the price is levied is difficult to erect.

The first problem often lies in defining property rights and imposing their respect. Who has a right to using a lake as sink: previous users, the country or countries around it, the World? what about ground and air? whose interests determine the nuisance value of pollution, or more broadly the social cost imposed by the depletion of any non−renewable resource? Equally difficult, how to ensure that polluters do not pollute or pay for it? how to erect the equivalent of the safeguards which compel householders to pay for land, and shoppers to pay for the shoes they buy? These questions are not unrelated. Prices are normally determined through the interplay of demand and supply, which are influenced by the preferences and financial resourcescontinue

of individual owners, producers, and consumers. It does not seem possible to determine a strict equivalent to such prices without establishing the ownership of natural resources and payment mechanisms for using them.

When governments oblige users to pay a resource fee, deemed to represent the true value of the resource (or of the cost of compensating for it by treating effluents), project evaluators may treat the fee itself as the true price of the resource (as evaluators of sectoral policies, they may meanwhile question its adequacy). The same is true if total use is limited by regulatory means, so as to preserve the self−renewal of the resource, and if permits are tradeable. In effect, in such cases externalities have been internalized. Market prices exist, and future prices can be projected, and should be used. Similarly, project evaluators should accept existing and likely future regulations as given, even when they are not price−related. If the discharge of certain effluents is allowed on payment of a fee, that is the cost the project evaluator should include in his calculations; and obviously, the project should include the cost of preventing the emission of forbidden effluents. Even if the project selector thinks these prices and rules do not represent social costs, they are the ones to be used, for the reasons given in the preceding section. If other industrial enterprises and householders are allowed to discharge raw sewage, there is no guarantee that overall welfare is improved by imposing rigid constraints on the discharge of effluents by World−Bank projects only.

It may often be impractical to provide tradeable permits; it is best simply to forbid certain actions. It clearly makes little sense to institute street−cleaning projects if everyone is free to throw garbage everywhere; but it may also be unwise to establish permits or set variable fees for depositing garbage in the streets; it is best simply to forbid it. Some garbage nevertheless does get thrown out; picking it up, and cleaning the streets produce benefits. However, it is impossible to determine the price (i.e. the unit value) of these benefits. Nor is it very useful: the government must allocate an overall amount to a cleaning budget; it need not know exactly the precise value of each additional dose of cleanliness.

It is very difficult to put a price on the benefits corresponding to the clean−up a polluted lake, if polluting activities are now forbidden; and it is not necessary to do so. The resource "*lake cleanliness* " cannot be

appropriated, made private. People along the shore benefit more, but not all of them may, and others also do: casual visitors, those who like the smell, those who like to know that the lake is clean (think of the far−away interest in Lake Baïkal and Lac Léman). Though some of these can be made to bear the costs of clean−up, it is impossible to derive a price, a value of the benefit, from willingness to pay for what is largely a public good. Ultimately, a political choice must be made; or rather, a set of political choices: if total clean−up costs so much, is it worthwhile (in preference to other public projects or lower taxes)? Is the marginal cost of somewhat cleaner water obtained at higher cost worthwhile?

Many people would agree that preserving tigers in the wild brings a benefit; and most people would say that so does preserving the lives of village women from tigers. The Government of India must then devise projects that further these partially conflicting aims. These will involve expenditures (on fences, on relocation of tigers and villages, on guards, on compensation . . .), and rules (on approaches to forest land, on grazing, on the treatment of errant tigers, on tourist access). Rational decisions must be made, and these will result in a finite total expenditure on preventive measures and finite foregone benefits of grazing; fewer tigers willcontinue

survive than the biological maximum; and some village women will nevertheless be killed by tigers. All these variables should be weighed; but the weighing would not be facilitated, or the rationality level raised, by attributing specific prices to tiger and human lives, in order to equate marginal benefits and costs all round. One cannot determine generally applicable values; postponing death by 80 man−years may not be the same if it means saving a baby girl or postponing by a week the deaths of 4000 old men. Conditions are so specific that, at the project level, the evaluator would have to examine and determine public "demand" for saving women and tigers in a great variety of circumstances; it is just as easy, and more rational, to look at projects in their totality. Approving the project implies that saving 1000 tigers (at the cost of no more than x women mauled; and vice versa) is worth the combination of resources and rules and interdictions needed; it better not be translated into dollars per woman and per tiger.

When the benefits of a project are clearly collective, it is often easier to decide whether the benefit package is worth the cost than to try to estimate the unit value of benefits − all the more so as many of the projects whose benefits are now deemed non−quantifiable relate to " *"public goods"* , which cannot be divided into units or supplied in marginally larger numbers, in many dimensions: one can clean up a lake more or less completely, but not clean up completely 90 percent of the lake. Weighing the overall worthwhileness of benefits and costs (including the nuisance value of restrictions) should not be less rational than weighing their unit values; and it should be a great deal easier.

In summary, the project evaluator seeking social benefits should first examine the policy context. If that is clearly antagonistic to the aims sought by the project under consideration, the project should be rejected or the policies changed. Investing to clean up a lake into which everyone still freely dumps industrial pollutants and raw sewage is a waste of money. If the policy context is acceptable (it need not be optimal), fees and decreed prices may give an acceptable indication of the unit price to be attributed to project benefits. Unit prices are, however, not indispensable to rational decisions. The price of most public goods need not be quantified; benefits of education, health, public order and security, those of many environmental actions are collective and one can quite well weigh the total benefits of the project against total costs, and even decide on trade−offs between incremental benefits and marginal costs, without having to define a unit price. This is how The World Bank has been deciding for the majority of all its projects; this is how governments determine their education and health budgets, within overall budgets that also have to cater to the needs of highways and other "hard"investments; and this is how many other major economic decisions are made. There is no evidence that thorough quantification of benefits is required for rational decision−making. It did not take complex value calculations to determine that if Nauru was to survive its mineral deposits, a high proportion of export proceeds had to be saved. Kuwait's *"Fund for Future Generations"* or Indonesia's prudent use of oil revenues were not prompted by specific depletion adjustments to GDP, nor Nigeria's profligacy by the lack of them.

Quantifying What Can Be

The bulk of the benefits of many projects cannot be quantified, generally for intrinsic reasons. Other projects have mostly quantifiable benefits and costs, but also some environmental and human implications that cannot be translated into precise dollar amounts: environmental impacts, income distribution effects, consumer surpluses fall into this category. Even for purely market−oriented elements, prices diverge from social values, and this divergence cannot becontinue

bridged by the use of "*shadow* " prices. Yet ultimately some projects must be chosen in preference to others, without quantitative comparison of their economic rates of return.

Quantitative measurement of costs and benefits, though rarely conclusive, should nevertheless be pursued. Some costs, some benefits are likely to have monetary values: they should be known. When an objective can be reached through several separate means, these should be compared, and the least cost alternative selected. This is quite obvious when the final achievements of alternative projects are truly identical, and when costs are easily and unequivocally comparable. Between two ways of spanning a river at a given place with bridges of given strength and durability, the cheaper is unequivocally better. However, rarely is the choice so simple. More likely, even in this example the two bridges do not have identical designs, capacities and strengths. Even such differences may not always be amenable to fully quantified comparison. If differences in design lead to different transit times, the cost of vehicle delays is partly quantifiable, but even for commercial traffic, consumer (*producer*) surpluses may be involved; as for passenger traffic, the value of time saved or wasted is particularly difficult to determine; and any actual pricing scheme would leave consumer surpluses. These are often difficult to estimate, and raise additional issues as to how they can be included in project benefits without unreasonably and inefficiently tilting the balance of decisions in favor of such projects, and against those that must rely on private profit motives exclusively.

Thus even in such a simple case it may be impossible to quantify the difference between the benefits of two very similar projects in a fully convincing manner. Yet one should calculate all that can be computed. Doing so does not fully resolve project choices, but helps illuminate them. The same goes for projects much more remote from markets and from quantifiable products. The monetary (internalized) costs of investment and functioning must, of course, always be quantified as fully as possible. Estimating them gives some handle on partial cost−benefit analysis; it is essential for budgeting investment and operating expenses. Other aspects, like the impact of market distortions, environmental and health impacts and other externalities, relationship to income and wealth distribution patterns and other social goals, can often be partially quantified without being necessarily reduced to a single monetary number.

How many tigers, how many individuals and species are *"saved"* or otherwise affected by a project, what impacts it is likely to have on health (positive and negative), on education, on the free time available to consumers: these bits of quantitative information cannot be reduced to a single monetary numéraire, because even physical units are not strictly equivalent: waiting time in crowded buses is not equivalent to that in air−conditioned limousines; nor is Joe's equivalent to Mary's. But the accumulation of such information improves the bases of decision, and should be pursued energetically.

Assuming it can be established that traffic death rates are inverse functions of speed limits and of investment in traffic regulation, no one will demand a speed limit of zero, and few will clamor for unlimited speed. There is no rational formula for determining precisely how speed limitations and accident−preventing investment should be traded for mortal accidents − nor even how accidents should be *"traded"* among one another, how many broken legs should be considered equivalent to one fatality. Estimates based on costs and earning power would clearly be unacceptable − fatal accidents to retired people will presumably not be considered boons to society. Yet one also knows with intuitive certainty that better decisions will be made if information about the number and nature of

accidents, and about their relationship to speed andcontinue

investment, is known and considered. It is impossible to decide only on the basis of quantitative cost−benefit analysis; but far from being a reason to neglect the quantitative description of whatever cost and benefit element that can be so described, this should actually enhance efforts to find and present systematically all the quantitative information that is meaningful.

The Opportunity Cost of Capital

Whether or not all the economic costs and benefits of all projects could be quantified, whether or not one could arbitrarily allocate a certain amount of investible funds to projects with quantifiable benefits, now the benefits of many projects are not actually quantified; now investible funds are not separately allocated to hard projects. The *opportunity cost of capital* (or *accounting rate of interest, social discount rate, shadow interest rate* . . .) cannot be derived in the manner implied by its definition, by ranking potential development projects. Nor has it been, in practice, much calculated in the various ways recommended by LM, VDTS and their followers. The World Bank has generally affirmed it to be 10 percent, in real terms; and that is the rate most frequently used by its project evaluators.

Interest rates are an indispensable tool of project selection; even Soviet planners repeatedly had to re−invent them, despite Leninism's supposed anathema. Without interest rates, one cannot meaningfully choose between projects and project components whose outputs or inputs differ mainly by their time profiles. Such choices must be made for mostly market−oriented projects, and for others: one needs to distinguish between education or health projects that differ mostly by the time profiles of their outputs or costs; one needs a method for comparing, for placing, as it were, side by side, projects that differ both in outputs and time profiles.

The discount rate used can radically change the technical−economic choices within projects; it also affects the classification of projects and their apparent acceptability. Too low a rate, like underpricing any resource, may lead to wasteful use of capital. But this does not mean that the higher the rate, the more rigorous the analysis in which it is used. Over−pricing can be just as inefficient as under−pricing a resource. An excessive discount rate may cause the long−lasting benefits of long−maturing projects to be excessively discounted, while giving exaggerated value to rapidly accruing but non−lasting benefits. An excessive discount rate would inefficiently tilt the balance against current uses of capital for future benefits and resource savings; against long−gestating projects, a category which encloses many environmental and human resource development projects; against commitment to lengthy efforts in the hope of remote benefits.

Short of going through the calculation of shadow rates, a work so complex and so reliant on unavailable information or arbitrary assumptions that even those World Bank economists committed to the method have rarely undertaken it, another method is needed. The World Bank generally assumes that the applicable discount rate is 10 percent in real terms. This paper has stressed that markets provide the best first−cut approximation to prices; so why not use them to derive the appropriate discount rate?

The price of capital differs from other prices in many crucial respects. The most obvious is that most prices are cash prices; the price of capital cannot be. This entails several consequences. Even where financial markets function well, the true value of the prevailing real interest rate is at best an estimate; except for the very shortest−term loans, the true real interest rate will be known only after the fact. At any one time loans of different maturities may carrycontinue

different real rates of interest, none of them actually known. This makes it difficult to estimate the real rate of interest. Moreover, in many developing countries even these indications are lacking, as financial markets are not

well developed.

Yet one can estimate the real interest rates that have prevailed historically. For a century, from 1815 to 1914, British government paper was the epitome of the risk−free loan, and its yield varied around 3 percent, with upper and lower boundaries of about 4% and 2%. Since 1914, when the US took over preeminence, its government bonds have tended to yield less than that, ex post . In the 1970s, long−term World Bank projections assumed a base capital cost of 1% for developing countries. In November 1994, long−term bond yields in the US and Germany are respectively about 5 and 4 percent above current inflation rates, and are widely assumed to contain an additional inflation premium. Rates on short−term but renewable loans are well below 3 percent. In Japan, depending how inflation is defined, rates are somewhat or much lower. Four percent is surely not an excessively low estimate of such rates in the past or now; five percent is quite a high one. There is no empirical support for ten percent.

True, average returns to equity were sometimes higher than real market interest rates, even over long periods. In the past few decades the returns to equity were artificially boosted by the unanticipated low ex post levels of real interest rates: equity got more than it deserved because unanticipated inflation caused bondholders and bankers to get less than they had bargained for. Yet even so, since 1963, while US consumer prices increased by a factor of 4.65,29 the IMF's index of industrial share prices rose by a factor of 6.9, for an annual average increase, in real terms, of 1.3 percent. Stocks corresponding to the Dow Jones industrial' index, bought in 1963, would have yielded about 9.5 percent in nominal and just over 4 percent in real terms, including reinvested dividends.30 These rates apply to the average of all investments. For most discounting purposes, one should use the marginal rate: that corresponding to the least profitable project that is still worthwhile. That is the proper discount rate to be applied to technicaleconomic decisions (like deciding whether or not to install a piece of fuel−saving equipment). It is also the proper rate to use when considering the desirability of foreign borrowing, and similar macro−economic decisions. Clearly, if the average of Dow Jones companies yielded 4 percent, the marginal company must have yielded much less.

Another way to look at this is that creditworthy investors (including those who had assets to pledge) could borrow for rates not substantially higher than those paid by governments. Whoever in industrial countries confidently undertook an investment could usually finance it at a rate of 4 percent or less, in real terms.

How relevant is this to developing countries? If developing countries in general had an abundance of potential projects yielding higher returns, and yet capital did not flow to them, this would mean a global market failure of immense dimensions, which historical experience does not bear out in normal times. Capital has generally been available to creditworthy developing countries at rates only marginally higher than the base rates discussed above. It is certainly generally available to them today. There is no reason to believe that projects with quantifiablecontinue

29 *Source: IFS.*

30 *Source: Danforth Associates.*

rates of return of 10% in real terms are amply available in developing countries, yet do not find financing in international capital markets, at interest rates of about 4 percent in real terms.

Exceptions and market failures have most frequently taken the form of bubbles, when markets overestimated the availability of high−yielding projects in developing countries and the resulting creditworthiness, and provided too much capital rather than too little. But the reverse market failure also happens, usually because markets delay in recognizing radical changes in policies or other circumstances. The World Bank itself was created because the founders had good reason to believe that post−war financial markets would be reluctant to lend for good projects.

The rationale for IDA was partly different; even if projects are good and yield high rates of return, it may not be feasible to squeeze out the savings required for debt service, nor desirable from the point of view of an international community keen to help the very poorest countries. However, lack of creditworthiness for loans on market−related terms was also seen to be caused, at least in part, by low marginal returns to investment.

More recently, following the bubble psychology of the seventies, financial markets may not have reacted fast enough nor discriminately enough to structural adjustment. There may still be a few countries with marginal investment opportunities yielding substantially more than 3%. For instance, in China labor is abundant, wages are kept down by the pressure of the State; general organization, education levels, infrastructure are reasonably high; yet political fears and other market imperfections still limit access to world capital markets, though decreasingly.

Regrettably, there has been much more observed historical experience with countries and regions where the marginal efficiency of investment − the yield of the marginal investments effectively undertaken − has been much lower than the real interest rate prevailing in international and well−developed national capital markets. That has certainly been the historical experience of the centrally planned economies, and only in a few of them is the present position already markedly different in this respect. Even in sub−Saharan African countries, where investment ratios were much lower, their relationship to low growth rates (negative per capita) indicates very low marginal returns to investment. Low rates of return to investment may also have prevailed in Latin America in the 1970s.

How does this affect project selection? In the course of discussions on an earlier draft of this paper, a different definition of the *"opportunity cost of capital"* was advanced, to defend the use of the World Bank's customary ten percent rate. *"The marginal efficiency of investment was indeed much lower than ten percent(real) in most countries"* , so went the argument, *"and indeed close to zero in quite a few countries; but the marginal opportunity for the World Bank should be set high; it should be defined as a desirable standard that forces the Bank to look for good projects"* . This is a superficially attractive but profoundly false argument.

The discount rate is not only a project selection device; it is above all a price and, for investors, a cost. Setting any cost too high forces investors to look for projects that would, at regular costs, be highly profitable: only out of such notional profits can the excess cost be paid. Only the most highly efficient steel producers would contemplate investments in countries where the price of iron ore or energy is set too high; only the most efficient farming projects would be worthwhile in a place that charged excessive rents, wages or fertilizer prices. All such excess prices would help weed out inefficient projects; but they would also impose their own element of inefficiency, by forcing investors to be excessively sparing of iron ore or energy, of land,continue

labor and fertilizer. Setting an excessive price to capital forces similar distortions on projects, wrongly exchanging savings on original investments against later material and energy expenses and human or environmental damage, reducing the appeal of capital formation with long−term benefits.

It would make no sense to use one discount rate for selecting projects, and another one to make internal choices within projects: within project choices can themselves be treated as separate projects, and clearly, the same criteria must apply. Yet if international loans can be obtained for 4 percent (in real terms), more cost−saving machinery should be installed than if they cost 10 percent; more energy−saving investment is warranted; different mixes of investment and labor are to be used. Why should the World Bank turn down, say, a project or project element that consists in installing insulating equipment to save heating costs, if this can be fully financed by a private international loan at a real rate of 4 percent, and if the savings yield a surplus over debt service costs? And surely, whatever is valid for a project element like this is equally applicable to the selection of complete projects.

As is well known, projects should not be ranked by their internal rates of return; they should be ranked by their net present values, computed with the appropriate discount rate. When viewed in the light of a 4 percent discount

rate, some projects that have negative net present values when discounted at ten percent may be more desirable than other projects, that happen to pass the 10 percent discount test. The present value of very large but remote benefits dwindles when viewed in the light of an excessive discount rate: planting teak that matures in a hundred years, sending to kindergarten a baby who will only work in twenty years, safeguarding a resource that will, in any case, last a couple of generations.

None of this is novel; most treatises on project selection recognize that projects should not be ranked by rates of return, which surely implies that it is undesirable to use an excessive rate of return as criterion for project selection. The treatises also make a bow in the direction of the net present value (NPV) of projects, discounted with the appropriate discount rate, as the only appropriate criterion for ranking projects; in ordinary terminology, for the ordinary entrepreneur that contemplates developing a project with money borrowed and money whose alternative use is safe government bonds, the equivalent term is *"profit"* . The World Bank should aim for high standards; but using an excessive discount rate is not a good way to achieve them. However. it is perfectly appropriate, using the applicable discount rate, for the World Bank to look for projects that yield a positive NPV or profit. It may set minimal NPV standards, and select only projects that generate a minimum of so many millions NPV, or so much discounted gross present value relative to the discounted present value of the investment. That is very different from imposing a minimal discount rate.

In not so well run developing countries and those over−burdened by history, from Algeria to Zambia even the average (let alone the marginal) efficiency of investment has been much lower than the conventional World Bank 10%, much lower than market rates of interest, and often lower than zero. These are also the countries where the most unsatisfactory World Bank projects are concentrated. It is unrealistic to pretend that if, say, a piece of fuel−saving equipment had not been applied, or if any other World Bank project had not been implemented, the capital thus "saved" would have automatically earned (e.g.) Zaïre a real income stream of 10 percent forever; or even of 3 percent. OED repeatedly writes that this or that of the World Bank's Central African projects has a negative present value when its returns are discounted bycontinue

the *"opportunity cost of capital estimated at 10 percent":* if this means anything at all, it is that if not channelled to the World Bank project under consideration, the worst alternative use for the resources would have yielded returns of 10 percent. Central Africa should be so lucky!

If the Bank were concerned with the world−wide opportunity cost of capital, and allocated its lending accordingly, it should then re−allocate its lending from regions where numerous projects "fail" to regions where they succeed. But that would be contrary to the logic of the Bank's operations and to the mechanics of its country−specific lending programs. It would also be contrary to the Bank's original mission, which was to allocate funds to countries facing special difficulties of reconstruction and development, not to countries where the funds could bring the highest and safest returns. This is not to say that lending programs should in no way reflect differences in expected rates of return (though obviously not by trying simplistically to equalize them internationally). IBRD lending, in particular, less constrained than IDA by aid budgets and the like, responds to lending opportunities that can be stylized as the volume of investments yielding ERRs (and other development impacts) higher than a notional opportunity cost of capital. For IDA credits, there is much less flexibility, but lending still adapts to situations correctly diagnosed as highly unsatisfactory (no potential project with significant positive development impact), and to some extent even to very promising situations. Nevertheless, for the purposes of this paper it is not highly unrealistic to assume that country lending programs are set before the project selection process. It may (or may not) be desirable to change country allocation mechanisms: that is another story. But it is certainly undesirable wrongly to blame project selection or implementation deficiencies for deep−seated country problems.

Many domestic investments have had zero or negative rates of return in a wide range of developing countries. It is reasonable to assume that marginal resources not drawn to World Bank projects would have had similar fates;

having avoided such waste is at least one of the yardsticks by which to measure the success of past projects. As a single yardstick, however, it is certainly not adequate. It should normally be possible to finance good projects at an interest cost no higher than the international real interest rate of about 4 percent; it should always be easily possible to get such a rate of return from capital, by investing it internationally. The international return on safely invested capital should provide a floor to national discount rates − at least under the assumption that such would indeed be the alternative use of capital not absorbed in World Bank projects, to the exclusion of socially conceivably less warranted uses, like private foreign bank accounts or imports of champagne. This assumption may not be fully realistic everywhere.

Implications for Macroeconomic and Project Choices

The World Bank has used 10 percent in real terms as its first−cut approximation to the discount rate to be used for project selection. It can be assumed that the same rate has also been used to make technical choices, because the difference between project selection decisions and technical decisions within a project is not intrinsic, but just one of packaging; a given project with alternative technical choices can always be also defined as different projects. Nevertheless, in practice, the reference to an excessive discount rate may have only marginally distorted actual World Bank project choices, because of the limited role actually played by cost−benefit computations in project decisions. Though also marginal, the impact of their use in ex post evaluations may well have contributed to an excessive focus on project−related problems rathercontinue

than country level issues: the implication of an *"opportunity cost of capital"* of 10 percent in Ruritania is that, given country conditions, with careful search and preparations 10 percent was the minimum rate of return that an investor should have obtained in that country: there should be nothing much wrong with such a country, even if bungling investors (and the World Bank) fail to find the opportunities that are there.

The impact on macroeconomic analysis and advice has been much worse. The assumption that the marginal efficiency of investment is everywhere at least 10 percent, while real interest rates on international financial markets are usually much lower, implies that additional international borrowing on market terms can help implement additional investment at a cost which is usually a small fraction of project benefits. This belief underpinned the World Bank's ambitious expansion of IBRD lending in the late 1960s and 1970s. During the years immediately preceding the international debt crisis, the World Bank also held such views. One obvious implication was that high borrowing on market−related terms was generally deemed good, provided the loan proceeds were actually invested. The assumption that all investment yield at least 10 percent (in real terms) made it appear desirable to borrow as much as possible at market interest rates, that were normally quite low. This tendency may have been exacerbated by the World Bank's underestimation of international interest rates during the crucial years. In the late 1970s and until 1982 World Development Reports and most related projections assumed that a 1 percent real international interest rate would prevail. With investment projects yielding a minimum of ten percent return and capital deemed to cost only 1 percent over the long term, it is not surprising that the World Bank saw foreign borrowing on market terms as the solution to most development problems, and failed to foresee the debt crisis until it occurred. The returns assumed for marginal projects were much higher than the costs of additional capital on market terms. Consequently, the Bank was less preoccupied with the likelihood of excessive borrowing leading to over−indebtedness than with the threat of market failures that would suspend the flow of new loans. When real dollar short−term interest rates rose above 10 percent, this was rightly seen as a very temporary phenomenon. As past borrowings were deemed to have been invested in projects that yielded at least 10 percent in real terms, many Bank analysts wrongly concluded that over−indebted countries only faced a liquidity crisis. This analysis seemed to call for a tiding over strategy, pending the unavoidable decline of market interest rates. There could be no real long−term solvency problem if funds borrowed at real interest rates of 3 percent and less were invested in projects that yielded at least 10 percent. The re−emergence of major private capital inflows makes it all the more urgent to put the Bank's macro−economic advice on a sound footing, and to cease systematically over−estimating the marginal efficiency of investment.31 break

31 *This Report was written and submitted for publication well before the new Mexican crisis.*

III—
Uncertainties and Risks

Known Probabilities and Unknowable Possibilities

A General who ignored the *"fog of war"* would win few battles. Uncertainties and risks loom large for anyone concerned with plans for the future. Strangely, they are given short shrift in the traditional project evaluation methodologies. Only when they get to the 191st of their 219 pages do LM refer to them, and then somewhat dismissively:

It is not unnatural to discuss investment projects, as we have done in earlier chapters, on the assumption that their costs and benefit are known in advance. . . . Usually it will do little harm if uncertainties are ignored."32 " . . . on almost any plausible weighting, it is unlikely that a large allowance for uncertainty ought to be made.33

VDTS devote just over two of their 140 pages to the concept of risk. They recommend that

the best estimates of the variables and parameters . . . should be the expected value obtained, in principle, by weighting each possible value by the probability of its occurrence.34

How one is to get to know this *"probability of occurrence"* is not revealed. In any case, its calculation may be reserved to *"special cases"* . Simpler *"sensitivity analysis"* may do in standard cases. An early Operational Manual Statement on "*Project Appraisal* "35 (codifying World Bank practice) discusses various aspects of appraisal (economic, technical, social . . .) but does not refer to uncertainties. The more detailed note on the Economic Analysis of Projects36 devotes approximately 1 of its 15 pages to *"Sensitivity and risk analysis"*, proportionately rather more than in the earlier theoretical work; but with little impact on the recommended methodology, which is based on VDTS without the frills of social implications. Even the latest statement of Bank policies,37 though comparatively prolix on risk, mentions it in only one of its eight paragraphs. Only Hirschmann,38 in the traditional project literature, gives uncertainty a central role.

World Bank projects, borrowers and staff actually face enormous risks and uncertainties. Their depth and amplitude are well illustrated by two examples. The major effort at including risk analysis in project evaluation was incorporated into a report prepared by Louis Pouliquencontinue

32 *LM p. 191.*

33 *LM p. 200.*

34 *VDTS p. 44.*

35 *Operational Manual Statement No 2.20 of January 1984.*

36 *OMS 2.21 of May 1980.*

37 *OP 10.04 of April 1994.*

38 *Development Projects Observed, Op. Cit.*

in 1969.39 Its first case study was a port project in Mogadishu. The FY 92 Operations Evaluations Department report noted the paucity of successful pollution control projects, with the shining exception of the outstandingly successful project in Sarajevo. The notorious man−made catastrophes at those two sites, which have obviously defeated the purposes of both these projects, fell well outside the scope of any of the dangers and risks considered. They illustrate the thoughtful comment in the Asian Development Bank's (ADB) discussion40 of risk analysis, that economic and political events have a habit of falling well outside previously envisaged probability ranges. Is this not also part of what Albert Hirschman discovered with his *hiding hard?* 41

Risk and Uncertainties

All human undertakings are subject to risk and uncertainty. Risk, in the mathematical and statistical meaning of the term, is a defined set of chance events, whose occurrence is governed by a probability distribution that, strictly speaking, should be known. Somewhat more loosely, one can still speak of risk when the probability distribution is not known, but can be estimated through statistical techniques from historical experience. The most strictly defined type of risk is that of a number coming up 2, 3, n times successively at roulette. Less strictly, the risk accepted by an insurance company is inferred from the past frequency of occurrence of unrelated events, each of which is different (no two fires are alike), but whose differences cease being relevant when they are pooled with a large number of events of the same type. The probability of occurrence of the insured events can thus be calculated, and the premium determined. To protect themselves against the possibility that an entirely different experience may be relevant, insurance companies re−insure themselves against major catastrophes and casinos limit the amounts that can be gambled on a single stroke of luck.

The probability of a given rainfall in a year can be estimated on the basis of past experience. Records do not cover the whole past, and such calculations are based on recent weather records which, on certain assumptions, allow the universe to be inferred. Similarly, the future prices of commodities that form a project's main outputs and inputs are unknown, but their probability distributions can often be inferred from their past behavior. Deviations from the projected central values constitute risks that are not actually known, but can be estimated with some precision under certain assumptions.

As opposed to such defined risks, with uncertainties the very nature of possible events is itself partly unknown, let alone their frequency or probability of occurrence. Anything can happen in future − and, so it is said, generally does. There can be no insurance, no coverage against this sort of broad uncertainty. Analysis, research and other forms of learning can, by spending resources and time, push back the horizon somewhat, point at areas where new developments may be lurking beyond the horizon, sketch in some shapes and cross out others; but in thecontinue

39 *The use of risk analysis in project appraisal,* Louis POULIQUEN. World Bank Economic Department Working Paper 39, 13 March 1969.

40 *Risk analysis and project selection: A review of practical issues. Keith Johnson. The Asian Development Bank , Staff Paper N* o 28. August 1985.

41 *Albert Hirschmann, Development projects observed.*

ultimate analysis much of the future remains inherently uncertain and un−knowable. Some of this uncertainty is, in some sense, absolute: it is not only impossible to know the probability of occurrence of certain events, it is also impossible to know even the nature of all the events that might occur, and impossible to relate the impact of events known to be possible to a project under consideration.

The dividing line between foreseeable risk and uncertainty is blurred, not neat and sharp. For instance, in his model work on risk analysis42 Pouliquen also includes the risk (or subjective probability) that production of the

main export crop will fall short of forecast levels; and the risk that the completion of physical work will take longer (and cost more) than forecast. Unlike rainfall and commodity prices, the universe of these events has not been subjected to detailed statistical examination; it is not even fully identified and probably cannot be firmly delimited. What is the prior experience relevant to determining that Somalian banana production will be substantially less than projected, or that harbor improvement works will take longer? Even when one excludes from consideration the truly great uncertainties, the looming catastrophe that actually affected Somalia, one can only draw on information that may at best be relevant, but nevertheless bears on different events. The history of banana production in the same country is relevant, but it had been played out under different conditions; so is also experience with the speed and efficiency of implementing other projects in the same country, and similar projects in other countries. The more similar the projects and countries, the more relevant the experience; but the degree of similarity cannot be specified or measured. Indeed, the main issue will often be what the similarity is: is the Bangla Desh experience more relevant than the Korean? is the Ministry in charge of ports like the shrewd fellows in charge of highways, or like the bunglers in charge of housing? Ultimate estimates of risk may benefit from studying "comparable" events, but what is comparable, what is the scale to be used for the comparison, and therefore all ultimate judgments concerning risk, are bound to be subjective.

Few actual likelihoods can be computed with the same accuracy as the risk of loosing at roulette. Complete information on rainfall may go back fifteen years or hundred fifty. In the latter case it provides a very good basis for estimating the expected average amount of rainfall and the risk of deviations from it − but only within the framework of the past 150 years' climate experience. We also know that there have been major climatic changes in the past. Climates may change again, but we do not know when, nor how the change will affect rainfall in our project area. Similarly, no matter how well documented a price history, abrupt technical change, geo−political revolution or a revulsion in tastes may move demand or supply outside the bounds of experience.43 Even when there seem to be data from which to draw statistical inference, a judgment call must also be made to determine the relevance of those data, and the likelihood that they will remain relevant in future.break

42 *Op. Cit.*

43 *For over two thousand years the value of silver had been about 10 percent of the value of gold. Then, in just a few centuries, it fell to about 6 percent. It took almost another century to bring it to 4.5 percent around 1970, and only a further 20 years to reduce it to less than 2 percent.*

Political Risks

The brunt of any uncertainty falls on the borrowing country and the more direct project beneficiaries; but the World Bank itself shares in it to some extent, through the impact on its reputation and the support it enjoys, and through the impact of risk on possible default. Yet its mandate requires it to ignore some risks. Fifty years ago private capital markets truly functioned only in the United States, and refused the political and macro−economic risks entailed in lending to the reconstructing countries of Europe and to developing countries. The World Bank was created to cover that risk (in the everyday rather than the mathematical sense of the word). For private lenders, lending to countries notoriously close to civil war or similar catastrophes would be a sign of economic myopia; for the Bank, it may simply be fulfilling its mission, and helping to restore creditworthiness.

Even if the political risks of the implosion of Yugoslavia and of the breakdown of Somalia had been correctly identified, they should not necessarily have induced the Bank to modify the nature, nor perhaps even the amounts, of its involvement in those countries. Accepting such risks is part of the World Bank's mandate. The Bank is a high profile international organization with universal membership; were it to withhold its lending because of broad political risk, it would contribute to the actual advent of the political developments whose threat had deterred it. With hindsight, one sees that World Bank lending did not prevent the breakdown of Yugoslavia; yet if it had held back from assisting that country while there still was a chance of a peaceful solution, it would be

blamed, and rightly, for having contributed to the final breakdown.

If it actually allowed political risks to affect its lending, the Bank would be shown up sometimes as naive (when political crises nevertheless affected countries were it had lent), sometimes as unduly timid (when countries where it had allowed political risk to deter its lending nevertheless preserved their stability). In other cases, when it had correctly foreseen political crises, it could be accused of having actually provoked them by suspending its help. In such cases, it would be impossible to prove that a bolder attitude would not have prevented a crisis.

The argument does not only apply to extreme decisions, like lending opposed to abstention; nor only to variations in the volume of lending. It also applies to variations in the composition of lending and the selection of projects. By deliberately selecting projects that would fully make sense only if a country were to break up, the World Bank might contribute to the break−up. As long as political risks are truly political and truly still risks, not certainties, the Bank should ignore them.

The Yugoslavia and Somalia examples are quite extreme. A fortiori, many lesser political risks should be ignored, provided they are carefully and correctly identified. The World Bank would violate its mandate, and might seal the fate of enlightened governments, if it refrained from lending in support of suitable projects or desirable programs merely because of the possibility that the present government might fall and the new one might be hostile. The same consideration applies when the visible risk is not that the government may be removed, but that it may not fully implement an agreement. That possibility always exists, yet if it were allowed systematically to preclude projects or other assistance whose success is dependent on future actions, such prudence would ensure that the actions never take place.break

There is always some possibility, however unlikely, that even the most difficult and heroic policy measures will be implemented; and it is also always possible that obviously necessary and easy policy steps will not be taken. Such risks present difficult problems that require fine judgment. By assuming them, the Bank also imposes them on the borrower, that must ultimately service the corresponding loan, and must immediately find complementary funds. If a loan is refused because there is a risk that agreed conditions will not be respected, this would greatly reduce the chances of policy reform ever being undertaken. Yet if the loan is granted and the country reneges on the conditions, it is thereby condemned to carrying the burden of debt service without the assistance from new policies that would help alleviate it.

These judgment calls are all the more difficult as the World Bank must also guard against transforming the acceptance of political risks into the pursuit of political objectives − even if it may not always be able to guard itself against such objectives being imposed upon it. The deliberate assumption that political stability will be maintained should not justify wilful support to a tottering government, or even simply to an undesirable one; trust in oft−repeated, oft−broken promises of reform should not justify the dole that keeps in power the dispensers of bad policies, sometimes with the idea that the alternative would be even worse. Obviously, development lending cannot proceed once certain risks, however political, are actually realized: say, in the middle of a civil war; nor should it proceed just when the war is all but sure to start. Similarly, distrusting promises all but certain to be broken does not constitute undue avoidance of political risk; judgments must be exercised. Political risks must be accepted, they should not be overlooked.

Programs can often be designed and loan disbursements structured so as to minimize the loss consequent to a realization of the risks. World Bank structural adjustment loans, for instance, are often sliced up, each slice or *"tranche"* being supposedly released only after the implementation of agreed prior conditions. Similar arrangements may also be made for project loans, relating each step to certain agreed policies having been put into practice prior to presentation of the loan to the World Bank's Board, to its signature, effectiveness, and even certain disbursement stages. In principle, the disbursement can be halted at any time, the loan can even be prematured if prior agreements are not respected. These precautions have been shown not to be always effective

because the Bank has often been reluctant to exercise its remedies. The temptation to trust the new promise as much as the old one may be enhanced by the realization that suspending the process ensures and highlights its failure, while continuing may possibly preserve the chances of success. It also postpones the need to admit failure, and may contribute to the diffusion of responsibility for it. Also, withdrawal of support for an incomplete project − at worst, for half a bridge − is a much heavier penalty than failure to support the project in the first place.

While it should accept political risks, the Bank should also recognize and act upon project failures due to political developments, including a government's failure to respect agreed conditions; it must carefully distinguish between these circumstances, whose real frontiers are blurred. Political risks should always be clearly identified. This is administratively difficult, and is becoming more so with the spread of the notion (and its acceptance by the Bank) that no deliberation, no document should remain confidential. Publicly voicing political doubts is often tantamount to acting on them, and can create the same negative impacts; yet such doubts cannot be correctly weighed, rationally allayed or substantiated, if they are hidden even from internalcontinue

debate. Correct identification of political risks is also important for ensuring enlightened ex post debate and evaluation of World Bank projects and programs. The realization of deliberately accepted risks does not constitute failure, and the deliberate acceptance of even high risks does not necessarily constitute naiveté It is important to recognize this; but it is equally important to ensure that risks taken because they were not seen, and foregone opportunities for mitigating them, should not all be covered up as deliberate policies.

The Equal Risk Assumption, Sensitivity Analysis and Switching Values

The earlier quotations from LM and VDTS are fair summaries of the brunt of traditional advice concerning risk. Analysts are frequently advised to assume that deviations from projections will be symmetrically distributed, and that it is just as likely that a project would turn out better than expected as it is that it would turn out worse. The project evaluator is then advised to disregard uncertainty. If he does not do so, alternatively, he is advised to treat the uncertainties affecting project outcomes as if they were known and quantified − to treat them as true *risks* .

Risk analysis consists essentially in attaching explicit numerical values to known risks, through the application of historical experience or through subjective processes (also, presumably, built on experience). The combination of these risks applied to the central (i. e. most likely) value of the projected benefit allows the *"expected value"* of project benefits to be derived − rather like the *"expected value"* of a casino's gains at roulette is derived from the known probability distribution of individual numbers. The great advantage of the risk analysis process is that it forces those dealing with the project to think intensely about possible causes of failure, and to assign probabilities to them.

As shown above, few risks are actually known; they must be estimated, and the objectivity of risk estimates is limited. It is relatively easy to estimate the probability of minor deviations from a central forecast, but major deviations are much more rarely envisaged − hence the remark of the ADB, already quoted. Risk estimates are derived from experience that bears on a given statistical universe; estimation techniques usually exclude the possibility of a different statistical universe being relevant, and only rarely is this envisaged by project proponents. Furthermore, even when individual risks are well measured or appreciated, the validity of the technique also depends on the correct determination of the interdependence of the events that constitute the risk. This covers a full spectrum from fully independent to necessarily joint. The nature of the relationship at either end of the spectrum is generally quite clear, but partial interdependencies may be hidden, or exist only for extreme values of some variables: for instance, the pace of project implementation may be generally quite independent from output prices, but be influenced by extreme values which, through famine or plenty, affect labor supply. Project proponents and evaluators have had a tendency to underestimate the strength of correlations between extreme risks. To return to the earlier example, the risk of shortfalls in banana production and delays in harbor construction may be quite independent under normal circumstances, but closely correlated when caused by civil

war.

Sensitivity analysis consists simply of indicating the deviations from central estimated values that would bring about some threshold event − for instance, a shortfall of crop yields or crop prices (from project estimates) that would cause a project's estimated ERR to fall from 16%continue

to 14%. Rather than such arbitrary deviations from the expected values, it is better practice to look at *"switching values"* , those values of key variables that would cause the project to loose its justification or otherwise cause a key indicator to *switch* from good to bad. In strict quantitative analysis, the project's net present value would *"switch"* to negative; but the technique can also be used when the benefits are not quantified, say to define the delay, or cost overrun, that would cause the number of children sent to school to fail to reach a specified threshold.

Though sensitivity analysis is presented as a substitute to risk analysis, its use implies at least approximate knowledge of relevant probabilities. When it is stated, as is common practice, that *"a project is expected to yield an ERR of (e.g.) 14 percent on central assumptions, and will yield at least 10 percent even if* (say) *the product price falls short of its central estimate by one third"* , such a statement would quite lack information content if a one third shortfall from estimates were considered a normal everyday occurrence. At least by implication such a statement suggests that a one third shortfall is quite huge, most unlikely to occur. The ranges used for sensitivity analysis have often proven to be too narrow − actual events have had a habit of falling well beyond them. When projections are based on partial historical experience, it may be possible to estimate the mean corresponding to the experience of the historical universe, and also to derive the standard deviation. On those bases, one can project the expected value of future events, and the probability of deviations around it, as long as the historical rules remain valid. Small deviations can therefore be predicted with reasonable accuracy. However, deviations sometimes occur that are much larger than can be explained in the light of such rules: deviations of the magnitude of many historical standard deviations occur not as a chance occurrence within that universe (whose probability would be infinitely small), but because the laws governing this range of events have undergone a revolutionary change. On the basis of historical experience, an analyst could have foreseen the risk that the price of silver might fall to less than five percent of the price of gold; but it was impossible to foresee that it would fall to less than two percent; the price actually reached lies quite outside any probability range based on multi−secular historical experience.

The Project Focus That Blurs the Risks of Action (Including Inaction)

The dangers of one particular course of action, including inaction, are sometimes blurred by the focus on the action and on the project. Depending on fashion, mood, and the state of *"animal spirits"* , the project focus can bring out most sharply either the advantages or disadvantages of the action envisaged. When fashionable, financial engineering appears to have only advantages. Government intervention in many fields has often been justified by accurate perception of the risks and dangers of an economy left entirely to the private sector, combined with a tendency to ignore the potential errors and failures associated with government intervention. Fashion now seems to have moved to a reverse tendency, to the assumption that all the management and agency problems associated with old public sectors suddenly disappear through privatization − and other problems may now be disregarded. When *"animal spirits"* are high, or certain forms of action have strong political backing, the project focus is mostly positive. Advantages seem clear, and moves to forestall problems and cure defects are in sharp focus. Disadvantages tend to be minimized. The infinite possibilities for things to go wrong are dismissed; stress is laid on the avoidable aspects of past experience, and on examples of the great difficulties surmounted in similar circumstances. The uncertainties of human responses to project needs or to government policies have often been viewed in such optimistic ways. Suchcontinue

optimism may have actually biassed project selection; or it may merely have led to overestimating the overall returns from investments. It is easy enough, ex post , to show all that went wrong; it does not follow that alternatives would have been preferable.

When animal spirits are low, or because other concerns become important or fashionable, or when the public interest is less well represented than narrow−issue pressure groups, the same sharp project focus can turn negative, bring out the troughs rather than the peaks, the risks and dangers (which may be real enough) rather than the possibly much greater but blurred disadvantages of inaction. This has tended to happen with the rise of environmental and related concerns in recent years. Though the World Bank itself has repeatedly spoken out about the environmental costs of poverty, and the environmental and human benefits of development, many small but vocal pressure groups seem to show much greater concern for, say, those directly displaced by specific development projects than for those displaced by unbearable poverty and inability to obtain enough water and meet other basic needs. Thus (e.g) criticism of the Narmada *(Sardar Sarovar*) project in India has focussed on its harmful environmental impact, and on the forced displacement of people inhabiting the reservoir area. It gave short shrift to the negative environmental impact of continued encroachment on degraded hill forests by a growing population of semi−settled cultivators; or to the regional impact of water and power shortages the project is intended to relieve; or, much more broadly, to the costs and implications of alternative ways of ensuring development. Excessive focus on the risks of planned projects and other contemplated actions may blur the drawbacks of alternatives that are necessarily more diffuse; desirable actions may thus be unduly delayed or rendered excessively costly.

Safety Nets and Risk Mitigation

From the assumption that risk is symmetrical, or that it is measurable and known, it is easy to proceed to dismissing its importance. Project evaluators are then told to focus on the mathematical expected value of project benefits. The World Bank's own internal guidelines (Central Projects Note 2.0244) (CPN 2.02) recommend that usually expected values be calculated and treated no differently from risk−free (certain) estimates, on the grounds that for a country as a whole, risks from a large number of projects cancel out. The guidelines, and project selection literature in general, reflect a certain opposition to risk−mitigation measures and to supposedly lower−risk alternatives, in favor of maximizing the expected value of rates of return. This is implicit in the cursory treatment of risk in LM and VDTS. Old but apparently still applicable CPN 2.02 recommends that *"if a country has many projects in its "portfolio" whose outcomes are mutually independent, the country need not be concerned with the variability of the NEV* (net present value) *of a project around its expected value."* 45 [. . .] However, one should guard against excessive country−level risk (in very large projects); and even projects *"small when compared to the country's national income [. . .] are large with respect to a particular region . . .* "46 break

44 *Central Projects Note 2.02 − Risk and Sensitivity Analysis in the Economic Analysis of Projects.*

45 *OPS 2.02, op. cit., para. 7.*

46 *Ibid. para 11.*

Identically likely upside and downside deviations from expected values, i.e . identical upside and downside risks, need not cancel out. If the biblical seven lean years had preceded the seven abundant ones, much of the population would have starved to death; much of the population would have starved anyway if it had not received divine advice to build buffer stocks. More broadly, symmetrical upward and downward deviations of benefits have asymmetrical welfare impacts; in the worst case, those who have starved in lean years cannot benefit from subsequent abundance, nor even bring it about. The quotations in the previous paragraph reflect the Bank's recognition of these issues.

Project selectors should distinguish bearable from extreme risks, and risks likely to affect adversely the behavior of producers on whom the project is dependent from those risks unlikely to have such an impact. Well−to−do farmers may accept the risks of a new crop or of innovative cultural practices; poor farmers may need more reassurance. Similarly, the dangers of a new market or a new process may be acceptable to prosperous industrialists, or even those of job loss to educated workers in a well−functioning labor market. Other risks with asymmetrical welfare impacts may, however, fall on people ill able to bear them: small farmers, workers with limited alternative employment prospects, the majority of the members of any given solidarity group. In such cases, if the risk materializes, the ensuing sharp decline in welfare, perhaps even a more acute catastrophe, will dwarf the benefits of similar and successful projects. The prospect of such events may also influence behavior, render farmers reluctant to innovate, labor uncooperative, local authorities obstructive, etc.

The asymmetry of the welfare impacts of positive and negative deviations of project benefits from their expected values tends to cause project beneficiaries to abandon profit−maximizing actions. This tendency may become particularly marked once they have experienced negative deviations. If so, lean years will not be offset by very favorable years: following the experience of lean years, the highest priority of those project beneficiaries who survive and have not migrated may have become the avoidance of further major losses. Their risk−avoiding actions may then also preclude major gains.

If the negative deviations are purely local, they may be offset by national action, the operations of a *"safety net"* . If other parts of the economy are sure to help out temporary losers, the highest expected value should indeed be chosen, and gainers compensate temporary losers. However, *"safety nets"* that provide income support require preparation and administration, and are themselves neither cost−free nor risk−free. Taxes or other income−sharing arrangements impose efficiency costs, which may be quite high. Moreover, it may be difficult to distinguish project−related safety nets from those designed generally to alleviate abject poverty. Developing countries cannot afford to allocate much for poverty alleviation through redistribution, and this little may not be enough to reassure project beneficiaries. These often have some property and other starting assets. Their fear that these might be endangered by innovative projects may cause them to withhold the full cooperation without which the project cannot quite succeed. Safety nets must also be credible: if project beneficiaries do not believe in them, they may still adopt risk−mitigating behavior themselves, and thus work partly to cross−purposes with the project. One may then end up with lower yields than if risk mitigation had been directly incorporated into the project design.

Risk−mitigating actions do not simply trade lower expected returns against reducedcontinue

variability; unavoidably, some risks are traded against other risks and uncertainties. In the crop example, the drought−resistant variety may turn out, say, to be more disease−prone, or to have more volatile prices. Project designers and selectors must be aware of the relationship between project level and macro−economic (and macro−geographic) risk: for instance, major irrigation works reduce local weather risk but increase its correlation with other local risks of the same type, and may also increase its correlation with risks to manufacturing (e.g. through hydro−electricity shortages). The composition of exports, and even their relationship to that of imports, influence the reliability of a central safety net; and, obviously, a project apparently highly attractive when considered by itself may turn out to be unattractive if it adds to a country's dependence on a single commodity − or, more broadly, to its sensitivity to a given risk or a group of correlated risks.

It is impossible to avoid risks and uncertainties; risk−mitigating actions that give up certain benefits against uncertain gains, (like multi−fuel power stations) may run into quite unexpected problems, their supposed flexibility of no help to resolve them. Nevertheless, great attention should be paid to the identification of the risks affecting a project, and to their interrelationship with other risks and other activities −greater attention than is indicated by the traditional methodologies and perhaps even by World Bank operational memoranda. In particular, it should be realized that if returns fluctuate, sharp negative deviations from the expected value can

disrupt the whole process, lead to reactions on the ground and to changes in policies that will prevent the realization of the positive deviations.

Financial Techniques for Risk Management

An increasing array of financial techniques ostensibly aim at offsetting certain risks. Ordinary insurance is the simplest. It allows offsetting a large potential loss by paying a certain but small regular premium. There are many related financial techniques, and new ones are constantly devised. Like insurance, risk mitigation through other financial instruments also has costs. It may also involve switching one set of risks for another. These are not always fully evident, and through new schemes, bubbles and panics, financial technicians have established a solid record in devising instruments whose merits become apparent earlier than their drawbacks.

The cost of insurance is usually clear, and the only specific risk relates to the reliability of the insurer. Most other financial instruments incorporate an exchange of promises. These, which include swaps, forward transactions and options, impose high visible costs on those with limited creditworthiness. As in all credit−related transactions, those that respect their obligations pay for those expected to default. Often, there are additional, invisible, or at least clouded, costs, in the form of new risks. Some financial techniques used to mitigate risks may actually change the nature of the risk without necessarily lowering its value; they may even increase the magnitude of the possible catastrophe. For instance, forward sales cover against price risks but amplify the risk related to volume fluctuations. A decline in prices would hurt in any case, particularly if it is accompanied by a decline in the volume of production; but it is trulycontinue

catastrophic if the expected but unrealized crop has been sold forward at a higher price.47

If there is a general rule at all in this complex field, it is the one prudent consumers and firms apply to insurance. They only insure themselves against the most burdensome risks, and try to avoid contracts which compel them to pay for the *"moral hazards"* in the behavior of others. Deductibles should be as high as the loss that can be afforded without severe inconvenience; financial reserves should be constituted against small losses, in preference to insuring against them; repetitive independent losses (e.g. claims of damages against a railway or large trucker) should be treated as an expense, rather than insured; the ideal medical insurance does not cover a fraction of all expenses, but only catastrophic costs. Financial coverage against unstable commodity prices and similar long−term risks is usually likely to be prohibitively costly or risky.

Strategic Aims, Adaptable Tactics, Flexible Management Structures

Traditional methods like LM and VDTS demand precise cost−benefit computations based on prices and costs projected ten to thirty years ahead; and the World Bank makes them, more or less, for the third of its projects whose benefits it considers to be at all quantifiable. Such projections are subject to extreme uncertainty, and the precision of the computations based upon them is more spurious than real. Quantified sensitivity and risk analyses are more likely to increase the spuriousness of the precision than its reliability, because there is generally little basis for defining either the margins between which the variables are most likely to fluctuate, or the magnitude of possible movement beyond them. What matters in the end is whether the investment works pretty well, yielding a significant long−lasting positive contribution to national income in return for the resources it first absorbed; the exact ratio of benefits to costs, the precise prices at which costs are incurred and benefits counted, matters little. Project selectors should seek to boost the reliability of the ultimate result, not the already spurious precision of the calculus, nor the proportion of projects covered by it.

The search for reasonable tests can benefit from private sector practice and experience. Long term investors examine the present and likely near−term future price of the product and of its inputs, and usually these exert some influence over their investment decision. They also look at longer term cost and price projections, and

sketch out projected profit and loss statements and cash flows for periods that may stretch out from five or ten years to twenty−five or fifty years beyond the time when the computation itself is made. Such statements are, however, considered illustrative at most. No sensible investor believes that conditions during operations of any but the most exceedingly short−term investments will actually conform to the original calculations. Much greater weight is usually placed on the conditions likely to ensure that the project is well implemented and operates smoothly. Some of these conditions can themselves be described in quantitative terms, but they do not form a direct part of cost−benefit calculations. Good investors lay great store on the project's likely ability to meet competition, and more generally to adapt to unforeseeable circumstances: on its comparative advantages more than oncontinue

47 *To illustrate: 100 000 tons were sold forward at $100/ton. Only 50 000 tons are harvested; and the spot price rises to $200/ton. The country (project, whatever) gets $ 5 millions for its harvest, but has to cover the difference between contract and spot price for the additional 50 000 tons it has contracted but cannot deliver, a payment of $ 5 million, that reduces the gross revenue from the harvest exactly to zero.*

the absolute margin between projected costs and prices; on the quality of its management and its ability to attract good managers in future; on the quality of its present labor and that of the labor pool from which future workers will be drawn, on their training and on arrangements for further training; on the adaptability of workers and their willingness to learn and undertake new tasks and accept new methods; on laws, regulations and customs governing work and labor relations; on political stability and economic management in the region where the project is to operate, etc.

Comparative advantages usually rest on more solid and more reliable foundations than absolute price and cost projections. Long−term cacao prices may well diverge drastically from even the best forecast; high comparative advantages of soil, climate, labor would make it likely that project beneficiaries could meet the competition even under unfavorable market conditions, and of course profit greatly if market conditions become favorable. High comparative advantage may even help deter competition. Comparative advantage enhances the margin of confidence with which a project can be deemed likely to be profitable. When in 1980 petroleum prices were projected to reach over $70 per barrel by 1994, many energy projects looked very attractive, even after the usual sensitivity analysis. Low cost producers should have proceeded confidently ex ante , and ended up with profitable projects ex post. It was not unavoidable that relatively high−cost projects should end up unprofitably, but it was foreseeable that they might do so.

More generally, the project selector should assure himself that the project is aimed in a generally good direction, that it takes a good departure; comparative advantages are a first sign of this. Beyond that, the main requirement is that the project should be able and likely to adapt to circumstances as they emerge. Some elements of adaptability relate to the nature of the project: projects that depreciate over a long period are necessarily less adaptable than fast−maturing, high cash flow projects which rapidly repay their investment costs. Short duration projects always provide more flexibility, because the capital generated by depreciation can, if desirable in the light of newly emerging information, be moved to different projects. Tree−crop projects have an unavoidable element of inflexibility; annual crops can be replaced by another crop each year. Highly specialized projects are less adaptable than those whose outputs have numerous and unrelated potential uses. Multi−purpose investment has greater adaptability; one type of traffic may be replaced by another for railroads, but not for pipelines. A dam that depreciates over thirty years is inherently less adaptable than a truck that depreciates over three years, and can meanwhile be shifted from one route to another; a dam whose main justification is providing electricity to a single aluminum project is less adaptable than one geared to water and power demand from many potential users. However, investors should also recall the uncertainty principle; multi−purpose uses may appear to ensure the persistence of demand for a product, only to be destroyed by a better product or a radically cheaper process for producing it: slide rules where needed everywhere, and are now used nowhere.

Project selectors should be careful when applying the World Bank's old (but apparently still valid) Operational Manual statement:48 *"Risk may also be reduced by a flexible design of the project that leaves future options open so that unexpected changes in circumstances can be better* continue

48 *OMS 2.21 op. cit. para. 74.*

coped with. Such flexible design is likely to impose additional costs that [. . .] may or may not be justified ." . . . While it is always true that *"additional costs [. . .] may or may not be justified"* , the statement seems to understate the essential advantages of flexibility, particularly as it comes in the context of a somewhat superficial treatment of uncertainty. Flexibility seems to be seen in a narrow sense, as a combination of two or three specialized uses (like dual fuel boilers). As an essential component of projects and other plans for the future, flexibility may involve little or no additional cost: but it may require fundamentally different arrangements, like *"just−in−time inventories"* , and even more broadly, flexible management systems. This sort of flexibility can bring vital benefits at no cost.

A side effect of the excessive discount rate used by the Bank is to place a high premium on early returns, arbitrary in amount but sometimes justified in principle in light of their lesser uncertainty. If the discount rate is lowered to a realistic level, a replacement mechanism is needed for penalizing projects that lock in resources for long periods in inflexible forms and for giving a liquidity premium to fast−liquidating projects. The discounted Net Present Value (NPV) or the profitability ratio are quite up to this challenge, provided they are not used mechanically: it is quite reasonable to accept an investment expected to return its principal plus capitalize interest plus a very small profit margin after one year, yet demand a much higher profit margin over the principal plus capitalized interest for projects expected (with much greater inherent uncertainty) to pay these returns only after ten years. Projects which depreciate fast, which return the original investment over a short period, have an inherent advantage, in that they have greater flexibility and greater freedom in reacting to future events.

Even when the cash flow is very high, it needs to be reinvested. Opportunity to adapt is nothing without the capacity to see it and use it. The essential element of flexibility, the only factor that offers a substantial measure of true protection against the uncertainty of the future, is adaptability: competent management, motivated to work in the project's interest; adaptable workers; efficient environment; friendly institutions.

Flexibility should be seen not just as a component of projects but as a main strategic thrust, a desirable systemic characteristic. Central planning systems may well have devoted great and capable resources to forecasting future developments and planning for all eventualities; but inflexible planning targets and rigid implementation tools reduced their ability to react to unforeseeable deviations of reality from the plans. This macroeconomic rigidity was compounded by rigidity at lower levels; no sector, firm or unit could adapt to a new circumstance until information had laboriously ascended to the center, and the consequent decision equally laboriously descended to firms and ultimately to the workers. The counter−example of central planning should hold lessons for the World Bank.

Flexibility is desirable in project conception and implementation, and in the management of completed projects and of economic systems. This is an organizational matter, which should involve few trade−offs; flexibility should not be obtained in exchange for efficiency: it is a component of efficiency. It should be pursued separately from the material objectives of the project, not through multi−purpose capital goods and large reserves, but through well−informed and adaptable management and labor, and through appropriate external political, policy and macroeconomic conditions.break

In sum, the *"fog of development"* is a dominant reality; risk and uncertainty must be at the center of development planning and project formulation and selection. Some risks must be accepted, because the core mission of the

World Bank is to accept political risks and related risks to development performance and creditworthiness − it can no more reject them than a national health insurance scheme can refuse to cover hereditary diseases.

The impact of other risks can be minimized. Carefully projecting costs, revenues and other benefits over the long run is a useful exercise, and can often be usefully complemented by additional risk analysis, alternative projections of costs and benefits under different assumptions, and the study of the probabilities assigned to those alternatives. However, success should not be dependent on the realization of these probabilities; nor should enterprise await the careful spelling out of each possible reaction to each possible development.

The key to development success is capacity and willingness to react well to new developments as they arise: to find out about them as soon as possible, reflect about them, define and decide correctly the new actions they require, and implement these decisions efficiently and obtain that subordinate units and agents also do so. Efficient armies do not advance into the fog of war without caring about risk, on the grounds that things may just as well turn out better than worse; nor do they pierce the fog of war by preparing war plans for every possible eventuality. They enable themselves to find out what is happening as soon as possible; and they train commanders at all levels, and troops, to react independently but in the light of broader plans.

Development projects must also recognize the uncertainty of the future; dealing with risk and uncertainty must be at the center of the design of project management, of the sectors that contain them, and of the macroeconomy. Planning for all eventualities, good foresight are of course necessary and useful; but they cannot go very far. Success is dependent on institutional arrangements, training and incentives that allow each economic agent to receive and understand information, and induce every agent to react to it well, and in the general interest.break

IV—
Learning Strategies

Arduous Discoveries

In traditional literature, project selectors have full information about an infinity of projects before choosing one of them. This knowledge comes to them all ready−made, through a *"flow of project appraisals"* 49 coming from *"those responsible for making them . . . "* The project selector invests no time and no resource cost into appraisal; all he has to do is compare internal rates of return or present values at various discount rates. His knowledge is complete: he still has some uncertainties (to which, we are told, he should pay little heed) because some uncertainty is inherent in the future, but he knows all that can ever be known about all potential projects among which he chooses the few projects to be implemented.

Reality is fundamentally different. Project selectors are rarely separate entities from *"those responsible for making . . .* " project appraisals. They are usually the same institution and pretty much the same people carrying out closely integrated functions. Their knowledge of projects is not served up ready−made. It is the fruit of deliberate costly effort, of a sort of investment whose final object is, by definition, largely unknown.

Great initial uncertainties surround any newly examined aspect of reality, any newly contemplated action or project. They can be reduced through a set of focussed efforts we may call research, study, or *learning* . Learning consumes resources and takes time. Both of these are constrained and must be spent sparingly. Yet only when they have been spent has enough been learnt about a project to decide knowingly whether or not it should be implemented. Spending the resources and time fully to appraise a project, rejecting it and then again spending resources and time to learn about another project (which might conceivably again be rejected); or simultaneously studying a large number of possibilities and bringing up a *"flow of project appraisals"* out of which a few projects would be picked out: neither of these practices nor any combination of them would be acceptable. Too much time

would be needed, too much would be spent on studies, to produce an unacceptably small flow of actual investments. The game cannot be played that way.

Because the future is inherently uncertain, a point much belabored in the preceding section, investment decisions must always be made without full knowledge of their costs and returns, and of the circumstances they will face during their operation. Because learning even the little that can be learned is time−consuming and costly, many decisions must be made without even such knowledge as could be obtained, in time, at a cost. Much is yet to be learnt about projects when they are selected. A fortiori , even more is unknown about alternative projects that are not selected.

In the World Bank, formal project selection decisions are embodied in resolutions of the Board of Executive Directors, following a project preparation and study process culminating in a final appraisal. In practice, the overwhelming majority of projects presented to the Board end up by being approved. The proportion is only slightly lower at earlier stages, like appraisal, pre−hard

49 *LM p. 62.*

appraisal and project preparation. Few potential projects end up by being explicitly rejected. Most rejection decisions are implicit, and generally taken at very early stages, when certain lines of study are simply not pursued.

Precisely because they are implicit, it is difficult to reverse negative decisions not to pursue a line of study leading to a certain investment, all the more so as such decisions are implicit, and therefore need not be motivated. In principle, early positive decisions are subject to being reversed later. As long as a loan has not been finally approved, a project can be modified or rejected. However, rejections are best decided in favor of alternatives, and well−known alternatives are few; learning, being costly, is concentrated on a few projects.

Learning resources would be intolerably strained if there were many rejections at a late stage, after much study. There must be little back−tracking, little acquisition of knowledge that is not directly useful in formulating and implementing projects. Once it is decided to learn about a project, this renders it highly probable that the project will ultimately be selected and undertaken. Yet, by definition, the decision to learn is taken when little is as yet known about the project. Early decisions to move towards a certain type of project must be adopted on the basis of very little initial information either on the path to that project itself or on other possible project paths; yet one cannot afford to have more than a very few cases in which this early decision is later reversed, and progress towards the project abandoned before implementation. The real project selection problem is far from being a choice between well defined and well known projects. It is the choice of one learning path and the exclusion of others, on the basis of limited information that can only be improved by drawing on a limited store of time and resources, and with the hope that the path chosen will ultimately lead to a reasonably good project, and that the paths excluded did not involve wonderful chances of gains, now forever foregone.

Within this overall context, the project selector must develop a learning strategy for reasonable research and project selection decisions. Time and resources must be devoted to research sparingly, so as to respect target dates and budgets. This implies early and seldom reversed decisions regarding main investment parameters; it excludes broad fishing expeditions, broad multi−directional attempts at defining many potential projects from which to choose the best few. The final result of the process must be good projects: projects which some omniscient *Observing Economist* would find reasonably satisfactory.

This is a general problem, an unavoidable consequence of the fact that learning takes time and resources. Children and youths decide what to study, and thus they commit themselves to professional careers about which they will actually start learning something only later. By the time they know what they are committed to, they have little opportunity to modify their commitment. Generals must choose their broad strategy before it is painstakingly

translated into detailed tactics and logistics; they rarely have the opportunity to decide later that their chosen strategy really demands too many wagon−loads of shells, and that an altogether different strategic plan should be adopted. Similarly in the World Bank, formal project appraisals rarely put an end to a project being considered, and can never bring to life a project that has not already received careful attention.break

In the World Bank, the stages preceding final project selection, each with its own decisions, broadly correspond to

A. the definition of a country strategy;

B. project identification;

C. project preparation (to acquire knowledge and understandings with country counterparts);

D. pre−appraisal; and

E. appraisal.

With limited budget resources, the World Bank must decide how much to spend on each of these stages. Because of budget and time constraints, choices narrow down quite early. Even at the beginning of the country *strategy formulation* process, every conceivable project is no longer in the running; reflection and informal discussions have already narrowed down choices enormously, even then eliminating most projects that might perhaps have been possible. The choice is then further narrowed down during the early formal stages of country strategy formulation. *Project identification* already has a fairly narrow sectoral focus. After the start of the project preparation phase or pre−appraisal, projects are only rarely and reluctantly abandoned; yet these are the first stages that actually give detailed technical and economic information.

The main decisions relative to the nature of the projects to be implemented, those concerning what to do , are taken before their implications are known in any detail. The learning process then continues through appraisal and beyond, to the detailed engineering and technical assistance plans that are component parts of the project itself. This later stage of learning essentially aims at refining the details of how to do the project; by the time of appraisal, there remains but a residual possibility that the project might after all be rejected. This possibility in turn dwindles almost into insignificance by the time concrete project determinants like construction costs can finally be calculated on the basis of detailed studies.

Learning Strategies and Administrative Processes

A good learning strategy uses resources sparingly. It proceeds without stumbling nor backtracking. It makes use of all the increasingly detailed knowledge acquired at each stage to further narrow down choices, until projects are finally selected and implemented. It ensures that projects that reach appraisal are not rejected, that tentative formulations that have reached pre−appraisal proceed further, and so on, backwards. It procures all the obtainable information effectively needed for the project, and reduces the remaining uncertainty to the inherent minimum. A good learning strategy also leads to the selection of good projects, of projects better than those that were implicitly rejected. It only excludes from investigation project paths that would effectively lead to undesirable projects.

LM's theoretical COPS is independent of those who are responsible for creating a "*flow* " of project appraisals. When real world project selectors and their acolytes are presented with appraised projects, this is far from being the first time they are taking project−related decisions. They are generally the same administrative units and often the same people who had to decide, much earlier, what parts of an infinitely large real world should be studied, with a view to formulating investment projects. Those decisions also have a fateful obverse; they

determinecontinue

what infinitely large set of projects should never even be considered further. The project selectors are responsible for the whole set of project−related decisions and for their consequences. Their professional prosperity depends on their perceived effectiveness in fulfilling these responsibilities.

The presence or absence of the first set of desirable characteristics is readily apparent. When a project is rejected at appraisal, this characteristic is clearly absent; resources and time have obviously been wasted, spent to no ultimate avail; the learning strategy visibly led to a bad project. It failed. Though less costly, failure is equally visible if a learning road leads into a dead alley at an earlier stage, when less time and fewer resources have yet been spent on it: if project identification focussed on a sector fails to identify projects; or if country strategy orients efforts towards a sector which is later abandoned.

The later, the closer to the final project selection decision, and the more radical the abandonment of such efforts, the greater the related failure. Rejection, say, of a coconut plantation project in favor of a palm−oil plantation in the same location is a marginal modification only; most of the acquired knowledge is actually put to use. Rejection of a dam project with irrigation and power components is much more radical; little of the technical, economic, even the sociological research performed in support of the project is likely to be re−used; but if the rejection is decided after a preliminary feasibility study, it is less costly than if it is decided after the completion of the final detailed engineering and social studies.

By contrast, the second set of characteristics is hidden. No one knows how development would have benefited from potential projects not evaluated, in sectors not studied, from strategies left unexplored. The costliest error may well be the one that leaves unexplored (and therefore un−selected) a potentially excellent development project; but this is an error which, by its very nature, necessarily remains unproven, and has a good chance to remain undetected.

Formal project appraisal is a delicate time for those responsible for devising the project learning and selection strategy. VDTS's project selector and LM's COPS are only to be commended if out of a hundred *"project appraisals [. . .] coming from those responsible for making them [, . . .]* 50 they select only one, and reject ninety−nine. For them, project selection is a new decision, which determines the future but implies no judgment on the past. In real life, a rejection implies the condemnation of the expens earlier incurred to acquire knowledge which is not put to final use. When a strategy is formulated and gradually leads, through staged studies, to the appraisal of a project which is then rejected, the mistake is obvious, the error is glaring and proven, the strategy is self−condemned. The same administrative structures and often the same individuals are responsible for devising and implementing the learning strategy, and for validating or condemning it. Managers who had decided what sorts of projects were to be identified, to be prepared, to be pre−appraised and then appraised are later largely responsible for deciding whether to select the project for final support. They face substantial institutional pressure not to reject any project that has come this far, and overwhelming institutional pressure not to reject many. They have corresponding self−interests.break

50 *LM p. 62.*

All this should be pretty evident, and is not due to any character defect in the concerned managers, nor to any institutional defect in the World Bank. It is inherent in the nature of decision−making when learning is time−consuming and costly. Similar constraints face Generals, who cannot be allowed to make detailed tactical studies of a multitude of strategies; private businesses that cannot make a multitude of detailed market studies before launching one new product, scientists who must choose a good hypothesis before subjecting it to lengthy and costly tests. The general whose strategic decisions turn out to involve excessive tactical and logistic

requirements, the product manager who develops many new products which detailed market studies later show to be unwanted, the scientist who time after time has to reject his hypotheses following appropriate tests, and the World Bank project officer or Division Chief who prepare projects which then turn out, on appraisal, to be uneconomic, are unlikely to face brilliant careers. This is as it should be in the interest of the institution. Institutional interests, and indeed the interests of development, are best served when little time and resources are spent on studying activities that are in the end not pursued, and projects which ultimately are not selected. Institutional interests are best served if strategic, project identification and preparation work are all brilliant, and cause full early concentration on projects that ultimately warrant selection and implementation.

Institutional interests and the interests of development also require that bad potential projects should be weeded out. The preceding section stressed that much uncertainty remains even when all possible studies (including appraisal) have been carried out. One can never be quite sure that a project should indeed be rejected; conditions for its turning out to be an excellent project (or adjustment program), however unlikely, can rarely be proven to be impossible. Project approval does not require presenting false data, hiding known facts, nor even making wholly implausible assumptions. By the time of appraisal an exceedingly heavy burden of proof is placed on opposition to any project that has come this far. Staff evaluation and promotion processes easily sanction the waste of learning time and resources. It is more difficult to sanction the approval of bad projects, as only those involved in the decision master the research that led up to it; others may learn whether or not the decision was justified only ex post, and even then their judgment will be surrounded with uncertainty.

The interests of development, and therefore the World Bank's institutional interests exigently demand strategies likely to lead to the best feasible development programs and their component projects. But if potentially excellent projects are by−passed, if they are not included in the early strategy and not investigated, the consequent lack of information prevents this error from being proven; it can, at most, be suspected. Little is known about projects not studied nor implemented; and most of that little, by those responsible for the negative decisions. Strategies that by−pass good development possibilities are bad; but this can be revealed only in the long run, by new investments in learning, and rarely with full certainty; earlier mistakes can be surmised, never proven. Thus the characteristic most likely to enhance the development impact of World Bank actions is difficult to promote; and breaches to it are exceedingly difficult to sanction.

In fields where results come soon, and where many organizations pursue similar aims, performance is revealed by comparison. The firm which does not bypass too many good ideas will tend to be most successful; the scientist with the imagination to formulate good hypotheses will advance knowledge in his field and obtain recognition; in war−time, the officers who do notcontinue

confine themselves to routine ideas will win battles and promotion. Not so in peace−time armies. Like they, the World Bank is alone in its field. Success or failure of development processes is revealed only in the long run, even then their relationship with project decisions remains uncertain; the potential results of alternative strategies remain hypothetical.

Improving Learning Processes

World Bank Staff are broadly conscious of the implications of decisions to study particular problems and issues; they are certainly highly conscious of budget constraints and related aims. Yet the learning strategy issue has not been formally identified as such, and formal decision−making is still largely geared to the assumption that project selection actually occurs only when the Board formally approves a fully appraised project. By formally acknowledging the nature of the process and the points where decisions actually take place and become all but binding, it should be possible to improve on the strategy in all three of its dimensions: increase the likelihood that truly bad projects are not selected; reduce the likelihood that potential strategies that would lead up to truly good projects are ignored; yet maintain tight constraints on the fruitless study of dead−end strategies.

Translating such principles into management processes and personnel policies requires detailed studies and careful implementation. This report can only make a few suggestions. As decisions to engage in studies de facto almost commit the Bank to the resulting projects and programs, these decisions should be taken explicitly, subject to formal authorization mechanisms. Such formal authorization should be based on reports that clearly present the objectives of studies and the projects to which they are likely to lead. At the same stage, much greater attention should be paid to the abandoned alternatives. Studies should not be undertaken merely because their ultimate object is attractive; there must be reason to believe that it is more attractive than alternatives that could be developed by alternative studies.

Development projects and programs demand great flexibility, competent, motivated and well−informed managements, and adaptable work forces. Similar qualities are needed in the World Bank itself. Much rests on the earliest decisions, taken when solid information is scarce. The quality most needed at that stage can be called, for lack of a better word, *"intuition"* . It should rest on deep and broad understanding of development processes and of country conditions, including the specific cultural characteristics prevailing at the particular time and place.51 Personnel policies should try to identify and foster those possessing such traits. Management practices should deliberately counterbalance the greater ease with which type 1 mistakes are identified and systematically search for type 3 mistakes. Those involved with specific countries and sectors normally have the best chance to know enough to avoid these; they are normally in the same management structure as the chain of responsible decision−makers. It is essential to preserve and foster their intellectual freedom to bring up new ideas and to expresscontinue

51 *Much recent World Bank literature has denied the importance of cultural influences. Such statements seem to be based more on neglect of national and regional differences and ignorance of cultural diversity than on solid studies. Of course, culture is based on history, and it evolves − sometimes quite fast. However, the patent falsity of theories, often tainted with racism, that rest on the permanence of cultural traits, should not lead to the denial of cultural differences, nor to the neglect of their study in the interest of development.*

dissent. As Type 3 mistakes are revealed over the long term only, in decades rather than years,52 deliberate periodic reviews should play a more important role in shaping management decisions; and they should be linked to personnel policy decisions whenever present managers can still be linked to past decisions.

In sum , three types of potential mistakes can be made. Too much can be spent on studies and learning that ultimately do not lead to any project. This type of mistake is self−signalled, easy to monitor and to repress; and in reality it is rarely made: few projects are conducted to the appraisal stage only to be abandoned then. The second type of mistake is the obverse of the first: it consists in approving projects that should have been rejected. The likelihood of committing such mistakes is obviously heightened by the desire to avoid the reality and the stigma of highly visible mistakes of the first type. Mistakes of this second type may be very costly, but they are rarely obvious, rarely become fully proven (because other explanations may be given for project failures), and when they are revealed, their links to specific decisions and to specific individuals are often already blurred. The third type of mistake, not formulating and implementing learning strategies that would have led to good development programs and projects is perhaps the most inimical to development and to the Bank's institutional interests. This is the least visible mistake, the most likely to remain forever hidden or unproven. The great challenge to the Bank's personnel policies is similar: identify and promote the people who will devise strategies and implement programs whose success will only be fully revealed in the very long run.break

52 *The implications of the borrowing strategies of the early 1970s were only revealed in the mid 1980s; it often takes much longer for strategic decisions to be proven to have been mistaken.*

V—
Whose Project Selection Criteria?

Picking the Best Projects?

The World Bank brings a general benefit to borrowing countries, in the form of loans on favorable financial terms. This advantage is independent from the object of financing. The Bank's economic analysis and its work with aid groups also bring benefits independent from project choices. But Bank intervention also has other benefits (or drawbacks), determined by the particular projects selected for involvement. It is these additional benefits, those attached to selecting some projects in preference to others, that form the object of this paper and of traditional project selection methodologies.

Imagine a public sector investment program composed exclusively of very high priority, well designed and well implemented projects; and that these constitute the best known, most easily identified and most surely profitable investment opportunities. Suppose further that the only alternatives this leaves to the private sector are privately less profitable and socially less beneficial projects, or capital exports and higher consumption; and that investments in social infrastructure and in similar fields, obviously unprofitable to the private sector, go begging. All the public sector projects may have been selected in accordance with the best traditional project selection methods; all may be socially highly beneficial. The World Bank itself still pretends to be selecting projects in the light of such methods. Yet, composed though it is of the best projects, this would not be a good public investment program.

What does project *"selection"* mean? A private, profit−motivated project selector will want to know whether a project is good for himself, for his profits. His decision whether or not to select a project does not determine whether the project will come into being; it determines whether the particular investor will undertake it. The national public sector (Government, COPS, Plan Organization . . .) and the World Bank have different aims: in principle, they want to contribute to the country's development; if they want to select this project rather than that one, their choice is determined in the light of the development contribution they can thus make.

In the traditional literature on the economic analysis of projects, COPS, the Government, the Central Planning Agency, the World Bank view and analyze projects in order to decide which shall come into being and which shall not. Project *"selection"* determines the choice between the project's being and nothingness, between virtuality and realization. It aims at answering the questions: *"is this project good for the country? should this project be implemented?*

Thus, according to VDTS, their method ensures that *"projects should be selected in the light of their contribution to the maximization of total [. . .]* national income"53 (*"undifferentiated"* or differentially weighted according to end−use and income distribution impacts). But selecting the projects that maximize total national income constitutes a proper preoccupation for a project authority only if it is in charge of all investments; and such indeed seem to be the functions of COPS and of VDTS's planning authority. Their selection or rejection of a projectcontinue

53 *VDTS p. 4 5.*

seems indeed to determine whether or not the project comes into being; there seems to be no other investor in their stylized country.

In real life, such a situation is found nowhere, and is even approximated only in North Korea. Governments face a continuum of possible decisions, from full public sector financing and implementation through joint ventures and

incentives, benign neglect, disincentives and ultimately full and formal interdiction. These decisions are sometimes combined: decisions to implement a project in the public sector may be combined with interdictions to proceed with similar private projects. Short of such interdictions, if the Government decides not to *"select"* a project, this does not normally preclude its being implemented by others. Conversely, if a government decides to pursue a project, this does not necessarily prevent the private sector from also selecting and implementing similar projects. A government decision that a project is *"good"* and *"should"* be pursued is by itself meaningless: it neither compels nor impels the private sector (or other government agencies) to invest in the project. To bring that about requires different decisions. Similarly, if the government decides that a project is bad, and does not itself implement it, this may not prevent the project from being brought about. To orient action and shape decisions, the only relevant question for any economic agent is *"what should I do?"* . This is different from "*what should be done,"* 54 except if there is only a single agent.

While it never has a complete monopoly of all investments, the public sector often has the monopoly of certain types or groups of projects. A single authority may then be in charge, say, of major highways and bridges; a highway project it selects will come into being, and one it does not select shall not. Such an authority may well ask which highway project makes the greatest contribution to the country's development or welfare; and if it receives a predetermined allocation of investible funds, it should allocate them to those best projects, very much like COPS allocates its *"investible funds"* . The highway example is realistic; in many countries the same will be the case for railways, for power projects, for education and other social sectors. Within such sectors, investible public funds should indeed go to the projects best for the country, and the questions asked by LM, VDTS and the World Bank's guidelines are relevant at such disaggregated levels, for the allocation of a pre−determined sectoral investment budget.

However, before arriving at such simple decisions, much more complex decisions need to be taken. The distribution of the government's capital budget, the allocation of funds between the Highway Department and others should not be arbitrary. At least partially and from time to time, it should be considered in conjunction with other budget allocations. As long as public sector investment serves, even indirectly, the same purposes as private investment in at least some sectors (or competes with other forms of public investment), its allocation should also be determined by the relative usefulness of public investment, not by the benefits of the projects it finances. If new busses are urgently needed, while there is also a need, though less urgent, for better equipped police stations, government funds should not necessarily be allocated to bus lines, which can be expanded through other means. Even in the sectors where traditionally the government was long allowed a monopoly, its justification is increasingly questioned. Once suchcontinue

54 *Unlike LM and similar traditional treatises, CHERVEL allows this issue to be addressed, but in practice barely addresses it. It too mostly deals with selecting the projects to be undertaken for the country's good, without explicitly addressing the selection of projects to be undertaken by the project selector himself.*

questions are raised, the issue immediately stops being "*what are the best projects* ?", and becomes "*which are the projects in which the Government's limited resources can do most good* ?

As for the World Bank, it never finances more than a fraction of all investments in any country. It has never had a monopoly over any sector. Its decision whether or not to select a project does not directly determine whether that project comes into being and is implemented, but only whether the World Bank itself is involved in it. Whether or not a project is *"good* " gives no indications of the advantages of World Bank involvement with it. The World Bank should clearly not look at all investment opportunities in a country and select those that are best, those that contribute most to development. These may be projects the private sector (domestic or foreign) may be most able and willing to implement perfectly; or they may require public sector involvement, but of a sort that the country's own public sector is well able to provide on its own. There may well be projects less attractive in several respects, but capable of benefitting greatly from the World Bank's contribution. The criterion for choosing World Bank

projects should not be the projects' contribution to the national economy, but the World Bank's own comparative advantages in dealing with these projects. Mutatis mutandis, the same applies to public sector projects in general.

Some have objected to an earlier draft of this paper that the World Bank (or the public sector) should never finance projects which the private sector might implement on its own. This is surely pushing to excess the comparative advantage argument. There are bound to be areas where private investment is possible, but public investment is to be preferred. The private sector may be so inept or so corrupt that efficiency is much enhanced by World Bank (or national government) intervention. Perhaps it is the size or time−scale of a project that are so overwhelming as to benefit greatly from public intervention in some form: e.g. through land grants for railway constructors in North America, through concessions or in more direct forms.

The objection also implies an untenably restricted definition of private−public competition. A public bus line would compete with a private bus line, and one might perhaps decree that it should never do so, or never be assisted by the World Bank. But a railway may also compete with a bus line, and railway passenger services remain heavily dependent on public sector help all over the world, including the US. Even investment in a highway, a bridge, a port, an airport or air traffic control may compete with busses, indirectly, by reducing the demand for bus services. One cannot firmly delimit many areas where private and public projects do not compete at all. Moreover, mostly private projects may also benefit from World Bank involvement, and one should not necessarily prevent them from doing so.

Firm exclusion rules do not hold up; choices cannot be avoided. Once this is accepted, it should become clear that these choices must aim at maximizing the contribution of the particular investment (the government's or the World Bank's), not the contribution of the particular project. In making that choice, the traditional methods for the *"economic analysis of projects"* provide very little help, because they ask the wrong question.

The World Bank should intervene where the benefits of World Bank intervention are greatest, in projects whose benefits the Bank is able to enhance more than it can enhance the benefits of other projects. It should select the set of projects which, without exceeding its lending program, allows it to make a greater contribution to the country's development than through anycontinue

other set of projects similarly constrained. That set will exclude some excellent projects, best let alone because others may implement them faultlessly; it should include projects perhaps only moderately beneficial by themselves, but whose beneficial effects are greatly boosted by World Bank intervention.

Mutatis mutandis all the discussion applies to the public sector too. It should not be concerned with determining which projects are good. Its main objective should be the establishment of a policy framework that ensures a proper distribution of starting advantages, provides proper incentives, and ensures the financing of desirable actions, including investments. To decide where the public sector should intervene in promoting or directly undertaking some projects, it needs to ask itself where the required resources, investment funds, implementation capacity, regulatory framework, would do most good − not what projects are the most important and beneficial to the country's development.

Such considerations should also guide ex post project evaluation. If all World Bank projects in a country turn out to have been excellent, in the sense that they made great contributions to development, this does not prove that they were well selected. World Bank projects may have done very well, but the same projects might have done just as well, or even better, without World Bank intervention. The choice of projects or the quality of their implementation may not have been much improved by World Bank involvement. Loans, of course, brought financial benefits; but these would have been the same no matter which projects had been selected. Conversely, finding that World Bank projects made only a small contribution to a country's development does not prove that they were either wrongly selected or badly implemented. It may be that all other projects, actually undertaken or

potentially feasible, were equally bad or worse.

It may even be that a few potential projects were better, made higher contributions to the country's development, but could be implemented by domestic agents (in either the private or the public sector) alone. Other actions were much more difficult, complex, risky; they needed World Bank help, and benefited from it. These were proper vehicles for World Bank intervention, even if the absolute value of their contribution was modest.

It may be proper, and even highly desirable, for the World Bank to select projects with relatively low ERRs (and more broadly, development contributions), while letting alone other projects, with higher returns. The private sector, domestic or foreign, or national or local governments, acting on their own or in cooperation with each other, may be perfectly capable and willing to implement some of the highest priority projects; they should be left to do so. The World Bank would then maximize its development contribution by reserving itself for the more difficult and riskier projects, with lower expected values of benefits, which would not be implemented at all without its intervention, or whose implementation it could much improve.

In principle, it may even be desirable for the World Bank to select inframarginal projects, projects with ERRs lower than the Bank's own lending charges (i.e. with negative net present values when discounted at the World Bank's lending rate), provided these projects would have been undertaken anyway, and that World Bank intervention helps to raise their benefits sufficiently. Mutatis mutandis, the same reasoning also applies to projects with non−quantifiable benefits: World Bank participation may be warranted even in some truly bad ones, if it turns an incipient disaster into a minor tragedy.break

However, this presumes that the Bank cannot actually stop the project, nor even radically transform it without contributing to its financing. To participate in such a project, the Bank would have to be utterly confident that by doing so, and only by doing so, it can greatly reduce the harm done. Such double confidence should be rare, given all that was said earlier about the fundamental uncertainty of the future. It is a brave man, and a braver institution, that can assert confidently that its participation in a disastrous action actually helped mitigate the disaster; and a very wise one indeed whose confident assertions to that effect also happen to be true. Also, such conditions would throw doubt on the very premises of Bank participation in assisting a country: what sort of a country is so determined to proceed with actions the World Bank knows, for sure, to be harmful? Transposing from the Bank to the government, it throws doubt on the adequacy of the overall policy framework. And even if all these questions are resolved, for political and ethical reasons the Bank may still wish to avoid truly bad projects, even if it could make them considerably less bad by participating in them. Notwithstanding G. K. Chesterton, if we disagree with the vivisection of children, it may be undesirable to participate in it, even to ensure that it is performed under sterile conditions.

World Bank Practice

Obviously, then, the issue for the World Bank is not to determine what constitutes good development projects, but what constitutes good World Bank interventions in favor of development. This key difference is given wide, though informal and confused, recognition in World Bank practice. A recent authoritative report about World Bank projects writes:

An average of 20% incidence of projects in difficulty may not be considered excessively high given the Bank's role as a development institution. Certainly, if the incidence were very low it could imply the Bank was not taking risks in a high−risk business. There is little doubt, however, that the high proportion of borrowing countries with poorly performing portfolios (39%) is a cause for concern.55

This passage does not really deal with "risk", but with projects that failed; it implies that it would not be appropriate for the World Bank to select all the "best" projects, those that make the highest economic impact on

the target country. World Bank staff members know and World Bank practice reflects this, as do the Bank's Articles of Agreement. Yet this vital point is recognized neither in the traditional methodological discussions nor in the actual economic analyses and ex−post evaluations of projects. The passage just quoted serves to spotlight the problem, but it also indicates that thinking about it remains unstructured.

The Bank does not seek systematically to finance those projects that are easiest to identify, simplest to implement, and yield the highest returns. It usually justifies its intervention partly in terms of the additional contributions this brings to the country. A large share of the efforts actually directed at identifying the vehicles for World Bank intervention (*"projects"*) is indeed aimed at the best ways in which the Bank can help the country, not at the best projects in the country. Bank practice certainly fully acknowledges that some projects may be excellent for the country, yet should be let alone by the Bank; conversely, difficult projects are sometimescontinue

55 *Portfolio Management Task Force, the World Bank, September 1992, para. 9.*

selected explicitly because of the Bank's contribution to them.

But these issues are given such an importance in the Bank's culture despite rather than because of formal methodological considerations. They are absent from the formal "*economic analysis of projects* " in appraisal reports. They are not presented in Operational Memoranda on the economic analysis of projects. Unavoidably, even in practice the importance given to the correct criterion, the World Bank's comparative advantage, is reduced by the formal subordination of project choices to the benefits the projects bring to the country. Meanwhile, a reverse effect has also taken place. The conflict between formal *"economic"* criteria and common sense has emptied of its substance most of the economic analysis, and eroded its role in project selection. This erosion was, of course, accelerated by the inadequacy of the traditional methodologies' analytical tools even for their own limited (and, for the Bank, largely irrelevant) purposes.

Much of the attention given to the Bank's contribution to projects is informal. Ex post evaluations unavoidably focus more on the formal *"economic analysis"* criteria, those that are most completely recorded in appraisal reports and other Board documents. OED therefore reviews projects in light of criteria that do not closely correspond to the criteria that, in practice, had guided their selection.

Project implementation does not come free. Most implementation costs form an integral part, of the project's capital costs. Each agency's practice determines the exact dividing line between implementation costs absorbed by the implementing agency and those included in the capital costs of the project. Private direct investors generally devote considerable resources to project implementation, but most of these are debited to the project's capital costs. Multilateral and bilateral aid agencies often allocate part of the implementation costs of the development projects they finance to separate, so−called *"technical assistance"* projects, but traditionally, the World Bank has been willing to absorb substantial implementation costs in its administrative budget. The difference is one of accounting practice rather than substance, but it means that such costs are deducted from the Bank's present profits, not from the investment project's and the investor's future profits (nor from the ERR that, in principle, justifies the project). Many of these costs born by the Bank are labelled "*supervision* " expenses, but the category also include various accounting and auditing services that ensure that loans disbursements (and reimbursements) are made for their designated objectives. This still leaves detailed engineering services, consultants to supervise construction, and of course construction itself, to be included in investment costs.

Investment costs and resources allocated to project implementation are subject to separate constraints, the *lending program* and the *administrative budget* . The exact location of the limit between these is arbitrary, but its existence is real; the administrative budget cannot be allowed to exceed some ratio of investment costs. Thus World Bank lending is subject to two separate budget constraints, neither absolutely fix nor quite country specific, but reasonably well stylized as annual country−specific administrative budget and lending program.

A double budget constraint prevents the unrestrained maximization of the objective function. If one of two projects allows the Bank to make a much larger impact on the target country's development, but takes more out of the administrative budget, the other project maycontinue

have to be preferred. Constrained project implementation budgets constrain the quality of projects that can be selected and implemented with any lending program. The constraint may be applied by reducing implementation expenditure on a given project, or by selecting altogether different projects, involving a lower ratio of implementation expenses to project costs.

Bank staff are acutely conscious of this relationship, though it has not been presented in quite such a formal manner. In the late 1960s and 1970s the Bank's lending program rose faster than its administrative budgets; and there was much complaining about the pressure to lend, and the impact this had on reducing project quality. Subsequently, though the importance of project supervision has been repeatedly reaffirmed, just as repeatedly inadequate project supervision has been blamed for insufficient project quality. The role of implementation costs and of the budget constraint on them will be left in the background over the remainder of this chapter. However, it should be kept well in mind. In reality, neither the implementation budget nor the resources affected to learning should be determined arbitrarily; allocations to them should be determined in function of the benefits they bring to development through project selection and implementation. Meanwhile, it is close enough to say that the World Bank should aim at selecting projects in each country so as to maximize the development impact of its intervention, subject to the two separate constraints of its lending program and of its administrative budget for project implementation expenses.

The World Bank's Comparative Advantages

In actual World Bank practice, the choice between highly concessional IDA and market−related IBRD financing is now independent from the nature of the project,56 but a variety of other financial considerations bring some influence to bear on the choice of projects. The practice of choosing projects according to their state of readiness, so as to fit them within a certain lending program, has been largely condemned but is largely practiced. It is quite rational as long as the annual lending program constitutes a budget constraint, but it may well cause the inclusion of projects that would be excluded if the lending constraints applied over different time periods.

Project selection decisions are also influenced by the speed of disbursements (the build−up or draw−down of the pipeline of committed undisbursed loans) and the capacity of a project to attract co−financing, or to be in competition with other funding unavailable for alternative uses. This is quite legitimate in transition periods, for instance when investible funds are scarce relative to the requirements of investment projects awaiting completion; or when creditworthiness is markedly underestimated by financial markets, say because a new commitment to policy reforms is not yet trusted. However, such transition periods are rare and, normally, short. Obviously, given a project, the faster it is completed the better. However, by itself faster utilization of World Bank funds does not normally constitute an advantage; nor does an additional inflow of capital on market terms, those being by definition terms on whichcontinue

56 *Actually, for a country that relies on both IBRD and IDA funds (and more generally, on foreign on both market and concessional terms), ceteris paribus it is advantageous to keep the IBRD and similar funds in the pipeline of committed undisbursed funds, and draw down the IDA and other concessional funds fastest. When a two loans relating to a fast−disbursing and a slow−disbursing project are approved simultaneously to an IBRD−IDA "blend"* country, it is in the country's interest to cover fast disbursements with low−cost IDA funds, and allocate IBRD funds to the *"pipeline".*

creditworthy countries should be able to obtain unlimited funds. The reference to a 10 percent (real) opportunity cost of capital may well tend to lead World Bank staff to attribute non−existent benefits to accelerated disbursements and to additional foreign financing obtained on market−related terms.

The main criterion for project selection is still the expected contribution of selected projects to development, and it is the only one formally reflected by the Bank's economic doctrine and project selection methodology. The skill requirements of a lending program so determined then tend to shape recruitment and training policies and, through them, the composition of staff. Priority areas for investment in the wide range of developing countries cover most sectors and most related skills. By having to deal with all those areas, Bank staff skills tend to become diffuse. This in turn attenuates their comparative advantages. The comparative advantages reflected in project selection tend to be those that emerge over the short run, and they are relatively modest. The Bank may be more competent than other investors and development agencies in most areas (a proposition that no longer attracts universal support), but this advantage is not particularly marked in any specific field.

Deliberate efforts have rarely been exerted to determine the areas where an institution like the World Bank could intervene most effectively, nor to shape the Bank's personnel policies to these comparative advantages. Nor has there been much effort to define lending programs which would maximize the impact of this double comparative advantage, derived from institutional characteristics and staff composition. Focus on what constitutes the World Bank's long−term comparative advantages has been remarkably absent from the project selection debate, which is of course a permanent component of the Bank's work. Even more surprisingly, it has also been absent from the debates about the Bank's organization and budgets, whose periodic re−emergence has also become a permanent feature. At neither level has there been much systematic thought given to what does and what should constitute the Bank's comparative advantages; through what types of projects could the Bank maximize its development impact. It cannot be demonstrated that this lack of focus, this partial confusion between country priorities and World Bank priorities has reduced the development impact of World Bank assistance relative to its potential; but it would be very surprising if it had not done so.

There are some exceptions to this general confusion between country priorities and Bank priorities. The Bank once had a Tourist Project Department. This was later abolished, and tourist projects were selectively eliminated from the Bank's arsenal. It was said that such projects retained high priority for developing countries, but they needed private investors to be successful, and these had the necessary skills for project formulation and implementation. Tourism projects were said often to be good projects, but the Bank was felt to have limited comparative advantages in them. Much of this argumentation was addressed to the lack of need for Bank financing (IFC has remained prepared to assist in the sector), but specialized technical competence was also at issue.

Whether or not the decision to specialize away from tourist projects was justified, it was based on an important and broadly valid principle. This sort of specialization should enhance the Bank's technical comparative advantages in the longer run. These need not be the same for all countries, but probably would contain a common core, and also exclude a common area (like the Bank now excludes tourism). A sharp turn towards such a comparative advantage focus iscontinue

the first necessary step towards actually reinforcing comparative advantages through specialized organization, recruitment and training. Such a turn should also enhance the efficacy of any budget control process. Even if everything the Bank does could be performed a little more efficiently, it stands to reason that the greatest possible gains in efficiency lie in reducing activities with low inherent comparative advantages, and increasing activities with high ones.

All this must start with abandonment of the World Bank's half−hearted attempts at justifying project selection in the light of traditional *"economic analysis"* methodology, including its newer non−quantified variants, and the

adoption, as formal project selection criterion, of the Bank's own comparative ability to help through certain projects and activities. This must lead to deliberate efforts to discover the projects, areas, and activities through which the Bank's own contribution to development can be maximized, and to strengthen these comparative advantages.

Policy Change through Project Involvement

Policy changes are the most effective way to improve the composition and implementation of a country's investment program, and the management of completed investment projects. The World Bank has great comparative advantages in helping bring about policy reforms. These rest on two legs, skill and clout. Its skills are derived from its long and extensive international experience, its ability to attract talented staff, its freedom from direct self−interest in the distribution of domestic income and in policies that help determine it. Its clout rests on its ability to make and withhold its own loans, and to influence capital inflows from other sources. That influence is in turn also largely related to its skills and to its reputation of impartiality (impugned but not destroyed). Thanks to these, its approval often facilitates access to other public capital flows, generated through aid groups; and its open disapproval can form a potent obstacle both to public aid and to private lending and equity inflows.

Policy influence is not inherently tied to project involvement. The Marshall Plan worked largely outside any project framework; the OECD continues to be an instrument of policy change without project links; so do the IMF and the Bank for International Settlements (BIS). The World Bank itself in principle will not lend at all unless country policies reach some minimal threshold of adequacy. It exerts policy influence by withholding lending or by varying its lending (but this departs from our stylized assumption that the size of the lending program is exogenously determined). The Bank has also frequently worked for major policy changes through "*program* " or "*structural adjustment* " loans not tied to specific projects. However, traditionally it has tied much of its detailed policy work to project involvement, and despite the growing frequency of policy−tied lending without concrete investment supports, the project tradition continues to exert a strong influence.

Though the Bank's project culture is partly the effect of lingering tradition, it also brings inherent benefits. Costly and lengthy learning processes are required to generate detailed policy reform proposals as well as projects. Within each sector, these learning processes largely overlap. The same studies abut to projects and to policy reform proposals. Indeed, a properly broad view of the economics of projects often helps to bring into focus the policy issues whose resolution may be needed to ensure the projects' profitability, or might, in other cases, destroy it (say for projects profitable only if given high customs protection).break

Moreover, there is often a wide gap between the identification of a policy problem and of the general nature of the desirable new policies, and the detailed formulation and implementation of sectoral policy reform. To help bridge the gap, such policy elements are often incorporated into projects. Generally in association with the financing of physical capital formation, but sometimes on their own, so−called *"projects"* center on the provision of consultants for institutional design and training, for the drafting of new detailed regulations and for other elements of policy reform. Such association has yet another advantage. Additional financing for physical investments (including financing that project and sectoral authorities just believe to be additional) does not merely provide incentives for policy reform (though that too is often necessary and always helpful), but also helps lay down the conditions of their success: reforms are always difficult and the political will to pursue them may easily wane if the desired increase in sectoral production and services lags too far behind because of the obsolescence and inadequacy of the capital stock.

The most difficult projects and policy reforms are often best suited to the Bank's comparative advantages. When one can easily identify simple policy measures that would greatly improve a country's or sector's economic performance, and when the country and sectoral authorities and the public share a strong desire to introduce such measures, their success is likely. As great skills were not needed to decide what was to be done, nor great

inducements for doing it, in such conditions the benefits of World Bank involvement would be, in reality, modest. Conversely, when reforms are complex and lengthy, when their impacts bear negatively on some segments of the population (at least for a time), when sectional interests strongly oppose the reforms while the responsible authorities and the public do not yet give it much understanding or support, the World Bank is then uniquely well placed to help support reforms and bring them about.

In such circumstances, however, the process is bound to be difficult and risky, and to be criticized even if successful. The success of reforms so conducted cannot be assured, and their cost is bound to be high, and to be judged excessive by many. Realistic presentation of the costs of reforms, of the magnitude of their favorable impacts, of the unavoidable lag from costs to benefits, and of the risks involved in the process, would strengthen opposition to reforms, perhaps decisively. However, if stress is therefore laid on the most favorable hypotheses concerning costs and outcomes, this strengthens the criticism and the opposition facing the whole process when reality turns out to be more protracted and less favorable than the projections. Moreover, no matter how much research and debate has prepared the process, one can never quite exclude the possibility that a better outcome could have been reached through easier paths if only different decisions had been taken. Critics will contend that alternatives had not been explored and that easier ones were available. As the opportunity for pursuing them has been definitely foregone, such allegations can never be disproved. When, as is usual, they are made while the process is still being pursued, they further enhance its difficulty, lengthen it and reduce its ultimate benefits. Such policy interventions always present a high possibility of failure and an even higher probability that, even if they are successful, their success will be deprecated and said to have been obtained at excessive cost. Yet such are the circumstances in which World Bank policy intervention is most necessary, because without it even the possibility of successful policy reform and investment would disappear.break

The influence of policies is widespread. They help determine not only the implementation of the investment program and the management of completed investment projects, but also the domestic savings effort and the country's attractiveness to foreign investment; not only the allocation, amounts and use of newly formed capital, but also the use of the existing capital stock. Assuming the Bank's influence on policies is favorable (i.e. that it knows what it is doing) it should bring it to bear as much as possible.

It may happen that past policies were pursued so consistently and have led to such an obvious dismal dead end that revulsion against them is generalized, and new policies receive broad support and minimal opposition, particularly in the early stages of reform, before the difficulties of reform itself are fully perceived. Ghana provides one example of such coincidence, where the Bank was supportively involved in reforms.

However, such events are rare. Generally, where there is a deep and widespread desire for reforms, they can take place without World Bank support, and are likely to have done so already. Viet−Nam provides an example of radical reforms undertaken without Bank involvement, which illustrates that in fact in such circumstances the Bank may not be needed, because its clout, at least, serves little purpose. Where extreme distortions persist, they generally have many advocates; at least, the necessary changes have many opponents. Heavily distorted country policies generally bring, or are thought to bring, benefits to specific groups, which are normally politically powerful. Heavy distortions also make it difficult to know where to start reforms and how to sequence them. The ultimate outcome, the policy framework of an undistorted economy, may be clear enough, and it follows a general prototype. The specific design of reforms, their time sequence and the appropriate degree of gradualness are, however, unique; they must be devised and designed and applied in unique circumstances. In thoroughly distorted economies, such design is particularly difficult. Neither knowledge nor political support may adequately underpin immediate, thorough, complete reform; yet distortions may be so pervasive that with partial reforms it may not even be clear whether they actually complicate or reduce distortions. All these circumstances often render the success of policy reforms difficult and elusive.

These are the circumstances in which there is the greatest need for the combination of clout and skills the Bank can bring to bear on policies. Resistance to the new policies (lack of general *"ownership"* , in current jargon) makes the process difficult and renders its outcome chancy, with failure a very distinct possibility, and success costly at best. The Bank is particularly well placed to endorse the reforms and help overcome local resistance to them, not least by mobilizing additional financing to support them.57 It should also be well placed to bring to them its knowledge of the country and its experience of comparable situations and of comparable reforms in other countries. In such situations its potential contribution is outstanding, its comparative advantage uniquely high; but the chances are that a significant large proportion of such involvements will fail, and even those that ultimately succeed will be perceived by critics as association with a protracted and painful process.break

57 *These are typically the transition circumstances when financial markets are most likely to underestimate creditworthyness. They are also time when the Bank's mandate and ability to disregard political risks is essential to its ability to support reforms.*

Mutatis mutandis, similar considerations apply to sectoral policy reforms tied to project choices. Sectoral findings must be viewed with great caution in an environment of generally distorted policies. Additionally, sectoral policy inputs share the dilemma of macroeconomic policy reform: the Bank is most useful where thorough reforms are needed, and there is resistance to the process; but these are also the sectors where involvement is most difficult and risky. They are also sectors where it is difficult to plan and design socially beneficial physical investment. Needs are not always evident: policy distortions influence demand, and when the distortions are thorough, it may even be unclear how demand will be affected when they start to unravel. Thus if transport services are generally underpriced, and actual use is constrained by physical shortages and bottlenecks, the overall level of demand following price increases may be unclear, and demand for specific services − this particular road, that commuter line − quite unpredictable. As relative prices and incomes change in accompaniment to reforms, and rationing mechanisms and other rules are modified, old bottlenecks may disappear and new ones emerge. Macroeconomic reforms also affect project costs, make it difficult to evaluate them. Yet, for the reasons already noted, if investment is postponed until the completion of reforms, both are likely to fail.

One can illustrate graphically this paradox and dilemma of policy interventions:

<−−− <−−− <−−−	−−−> −−−> −−−>
simple policies	complex policies
high "ownership"	low "ownership"
low risk	high risk
project success likely	project success endangered
low World Bank comparative advantage	high World Bank comparative advantage

At one end of a range lie projects and policy frameworks which would facilitate Bank intervention, but minimize the Bank's contribution: these projects and policy measures are assured of success precisely because their success does not require Bank intervention. They are good steps for the country; but they are not good ways for the World Bank to help the country. At the other end of the scale, the very qualities which render World Bank intervention desirable also render it risky.

Potential investment projects will be fully beneficial only in a radically improved policy environment, but they may have to be undertaken well before the success of reforms is assured. Thorough immediate policy reform may be desirable; but it may also be impossible, either for lack of adequate political support or because more time is needed for formulating and adopting detailed legislative and institutional steps and training (and possibly even recruiting) staff than for carrying out physical investments. If new investment awaits the completion of reforms, consequent stagnation may cause political support to falter and reforms to grind to a halt or slide back during the long wait for new investment benefits. Yet if investments are undertaken during the reform process while distortions persist, their exact nature and make−up, their costs and benefits cannot be calculated; great reliance must be placed on strategic comparisons rathercontinue

than detailed and spuriously precise calculations; on the flexibility derived from competent and dedicated management working in a policy environment with reasonable incentives, rather than on the precise design of projects.break

VI—
Tentative Conclusions

The World Bank has been practicing project selection, thoughtfully, for well−nigh half a century. One should not be surprised − and the author of this paper, for one, is not unduly disappointed − that a re−examination of project selection methodology has not led to the discovery of radical new methods whose easy application promises wonderfully to improve the development impact of all future World Bank actions. Were there such a method, the thinking men of the World Bank would have found it long ago.

Though no revolutionary new method emerges from this paper, it nevertheless unequivocally rejects the theories that supposedly govern the World Bank's project selection procedures. It finds them to be ill−adapted, and mostly just plain wrong. If this does not constitute a revolution, it is mostly because the Bank has long been paying only the most perfunctory formal lip service to the methodological theory that, supposedly, govern its project choices. A generation ago it was announced that *"the World Bank is gradually introducing consistent and systematic use of efficiency prices into its regular operational work [. . .] systematic use of social prices incorporating distributional aspects is being tested [. . .] does not constitute World Bank policy at this time but only a direction in which the Bank is moving . . . "* In reality, *"shadow"* prices have long been calculated perfunctorily, described in odd corners of reports only, and analysis based on them plays a truly minimal role in the choice of projects for World Bank financing. As for *"social"* prices that reflect income distributional and income−use preferences, little use has ever been made of them, and apparently none for many years.

But old and failed theories cling tenaciously, unused yet not rejected, no longer effective as guides to action, but still formidable obstacles to new and better theories. If the only impact of the present paper were to help clear away the rubble and open the door to future research, that would be benefit enough.

Bank practice has gradually evolved in directions which seem mostly to have been well justified. The Bank's own ability to contribute to development through projects has come to play an increasing role in its project selection, even though this plays no role in formal project selection methods. However, as always when theory and pragmatic rules conflict, both are weakened. The Bank should explicitly focus on its own comparative advantages, define and sharpen them, enhance them through re−defined management practices and personnel policies. That is a precondition for its continuing to serve development for a second half−century.

Main Findings

A summary was provided at the beginning of the paper, and it shall not be repeated here. Yet it may be useful to describe the kernel of major findings, preliminary to drawing their major implications.

a. *"Shadow"* prices and methods for the *"economic analysis of projects"* that incorporate them, including those prescribed in the World Bank, are built on stylized representations of the world that bear little relationship to reality. Because of this, they have no theoretical justification and are of no practical use. Had they been seriously applied to project selection, they wouldcontinue

have been more likely to worsen than to improve the overall use of investible funds and other resources. The saving grace of the World Bank's official endorsement of these methods is that it has largely disregarded them in practice. Their rubble must now be cleared away to enable the investigation of project selection methods to turn to a fruitful path.

b. Whenever applicable, project benefits and costs should be evaluated in the light of the market prices and legally valid rules deemed likely to prevail when the project is implemented and operates. Usually, these will not closely reflect the social optimum. However, there is much empirical evidence that socially desirable results ensue when decision−makers generally follow market signals, provided these and the distribution of initial advantages (wealth, education etc.) are not egregiously distorted. When these conditions do not prevail, and market signals or the distribution of initial advantages are highly distorted, they must be reformed. By−passing them through *"shadow"* prices does not improve results.

c. The World Bank has not attributed precise monetary values to the benefits of about two thirds of its recent projects (by value and by number). This is proper. Specific rates of return could be computed only if one could estimate the dollar value and the equivalent in automobiles and wheat of each human (or tiger) death postponed by health (or environmental) projects, as well as all tangible and intangible costs. Many such project benefits have no true unit value, because there is no social consensus over what the price should be, nor indeed that there should even be one. Social objectives and projects concerning *"public goods"* are adopted in their totality, not on the basis of the unit value of marginal improvements in child health, literacy, or similar conditions. It is not feasible to attribute unit values to the benefits of many projects; nor would doing so particularly improve decisions concerning them.

d. The provision of facilities for the training of philosophers, the vaccination of babies or the cleansing of water should not be justified on the ground that they will yield returns at least as high as the production of marshmallow; but the design of facilities, training programs, selection procedures should be based on detailed and, whenever possible, precise measurements and justification. Projects should be described as precisely as possible. Each of their elements should be related to the objective it aims to achieve; tangible costs should be given precisely and intangible costs should be described as closely as possible. Least cost solutions should be followed and higher costs paid only for known, though not necessarily quantified, benefits.

e. The interest or discount rate plays a key role in project design and selection. The World Bank has generally used an *"opportunity cost of capital estimated at 10 percent"* in real terms − meaning that all investment not associated with World Bank projects yield at least 10 percent. This is wrong. Capital has generally been available in international markets at real interest rates of not more than 4 percent, and there is no reason to assume that the marginal efficiency of investment anywhere lastingly exceeds that rate. Indeed, many developing countries have achieved much lower marginal returns to investment. The international rate (say 4 percent in real terms as a first approximation) is the appropriate first cut estimate of the discount rate to be used in project evealutaion and design. Market failures have more frequently led to overestimated than underestimated country creditworthiness, but sometimes markets may fail to supply capital at rates much lower than the marginal efficiency of investment,

notably in times of transition like reconstruction or structural adjustment. In such cases a higher discount rate may be justified,continue

but this must be demonstrated in each case. Once the proposed discount rate of about 4 percent is adopted, it will be appropriate to demand that most projects should also yield a profit, i.e. a postive discounted net present value.

f. Systematic use of a vastly excessive discount rate has probably somewhat biassed project selection against long−gestating projects, notably those with human resource development and environmental benefits. It might have impelled analysts to find creative methods that show high rates of return. Its use as a reference in ex post analysis has directed attention away from general conditions in the countries where project failures are concentrated, and towards project conditions; wrongly, because in many such countries general conditions must improve before project yields can be raised. It has contributed to substantial misinterpretation of the extent and causes of the failure of Bank projects in countries where low performance is concentrated: benefits with low or negative present values when discounted at the assumed 10 percent rate appear much better in light of a discount rate corresponding to the true marginal efficiency of investment. Most seriously, continued reference to *"the opportunity cost of capital estimated at ten percent"* has biassed the Bank's macroeconomic views and advice: in view of marginal investments deemed to yield 10 percent, foreign borrowing appeared highly desirable and debt seemed to present no solvency problems as long as real interest rates rarely exceeded 4 percent (and were long estimated by the World Bank at only 1 percent). Changing to a more realistic discount rate of reference is all the more urgent as the earlier macro−economic bias may otherwise again affect the Bank's analysis and advice related to capital flows.

g. Reality deviates from the projected operating environment, costs and benefits of projects, and the nature, magnitude and probability of most of these deviations are themselves unknown. World Bank dogma and practice tend to reduce the importance of uncertainty, on the grounds that it is symmetrical and is as likely to raise as to lower project returns. Project participants often need protection from downside deviations from projected benefits, not only to cushion extreme misery but also to ensure their full cooperation with projects they perceive as risky. The design and operation of country−wide safety nets is complex and costly, and is not a substitute to projects able to withstand risks. Specific technical choices (adaptable capital stock, high re−investible cash flows) may help in this. However, uncertainty is best faced by ensuring that managements are competent, well informed, well motivated and have wide latitude; that they can draw on well−educated labor; and that they operate in a reasonably undistorted market economy, in which social externalities tend to be internalized through prices or rules, which has widespread general education and where the distribution of wealth is generally well accepted by society.

h. What matters is whether an investment project works pretty well, yielding a significant long−lasting positive contribution to national income in return for the resources it first absorbed; the exact ratio of benefits to costs, the precise prices at which costs are incurred and benefits counted, matters little. Project selectors should seek to boost the reliability of the ultimate result, not the already spurious precision of the calculus, nor the proportion of projects covered by it.

i. According to times, fashions, and the state of animal spirits, examination of a specific project or action tends to bring into sharp focus either its benefits or its costs. In recent years, the rise of sectional interest groups and *development fatigue* seem to have caused interest tocontinue

focus mostly on the dangers and drawbacks of projects, and led to pressures to forego them, or at least to defer action until all its implications are fully worked out. Meanwhile, the implications of inaction, equally uncharted but potentially dangerous and costly, tend to be neglected. In charting a course, the implications of inaction must be paid as much attention as those of action.

j. All risks do not have equal implications for World Bank action. The Bank must accept many political risks properly refused by private lenders. These include the risk that governments may not carry out their undertakings. Mistrusting promises of good policies and depriving them of support would often effectively frustrate them. Trust should not lead to unlimited support to repeatedly broken promises: the Bank should take risks, not be repeatedly taken in (but it is easier to make the distinction than to detect the difference).

k. The World Bank should not aim at selecting those projects that best contribute to economic development: others may be able to implement them just as well. Subject to the double constraints of its lending program and administrative budget, the Bank should select the set of projects that give it the best opportunity to help the target country's overall development. This set may well leave out many good projects that can be well implemented without World Bank involvement; it includes projects that may not be the best or most important for the country, but those for which the Bank has the highest comparative advantages.

l. In the short run, comparative advantages are much influenced by the composition of staff, but in the long run they have a deeper institutional component. Pending a systematic evaluation, it seems that the World Bank's long−term comparative advantages depend on its international status, its reputation of competence, and its *"clout"* . These are best brought to bear on difficult, socially desirable but much resisted policies, which the Bank can study, define, often relate to projects, impose and implement. When such policies are needed, the Bank is uniquely qualified to help bring them about (they best suit its *"comparative advantages"*), and it should concentrate on them, leaving to others many easier projects with higher and more secure benefits. Beyond this generalization, specific sectors may also present positive or negative comparative advantages. Long ago, the Bank decided to opt out of tourism projects, on the grounds that these did not suit its comparative advantages. A systematic search may well identify other sectors in the same position; opting out of certain areas would allow comparative advantages in other areas to be strengthened.

m. Final decisions cannot be drawn from a careful comparison of all alternatives. Learning about projects takes time and resources, and no institution can afford to devote much of these to alternatives that are ultimately not followed. Studies are unavoidably targeted and designed on the basis of very limited knowledge; not studying a project is an implicit decision to reject it, without knowing anything about it. Studies absorb time and resources; those that do not lead to projects are spotlighted as wasteful mistakes. This impels managers to concentrate on studies thought most likely to lead to projects. If they therefore bypass innovative thinking that might lead to the best projects and most desirable development directions, that mistake remains implicit, rarely detected and almost never proven. There is also some institutional pressure to stifle new doubts concerning project proposals that finally result from costly studies. The most important decisions are those that lead to studying a specific project or other line of action. Once such studies have got under way, the pursuit of the project gets the overwhelming benefit ofcontinue

doubt. The process for taking such decisions should be formalized and strengthened, linked to a clear understanding of its reasons, including a systematic attempt to describe the potential courses of action implicitly rejected. The skills and personal qualities needed for such decisions should also be defined and promoted. They may be different from those needed for managing processes and decisions based on careful measured comparisons and other bureaucratic endeavors.

What Projects for the World Bank?

The fifty years of the World Bank coincide with the collapse of the Central Planning model in which many once saw a viable alternative development path; and with a great increase in private international capital flows. It is tempting to conclude that henceforth well−functioning countries with untrammelled private sectors can develop through private enterprise financed from private sources; while ill−functioning countries that hamper their private sectors cannot develop at all, and therefore should be brought to their senses, change their policies, and then fall

into the previous category. This would either leave no room at all for the World Bank, or leave it only the role of policy adviser and policeman that identifies bad policies, helps highlight them, and, when governments so request it, helps design and implement better policies. Requests for the services of the adviser would be prompted by the policeman, as the World Bank spotlight also informs outside financial sources, and turns off the private financing tap to those with bad policies, and turns it on to those with good ones.

Much in this paper supports such views. The condemnation of the *"shadow"* price methodology does not merely reject a technique for project evaluation; it signifies that truly bad policies cannot be by−passed, and good projects cannot be operated amongst bad policies. The stress on the need to rely on decentralized management and on providing it with consistent information underlines the key role of markets. The discussion of the applicable discount rate assumes the general willingness of international capital markets to provide financing to countries whose marginal efficiency of investment exceeds international interest rates. The Bank's comparative advantages were said to rest largely on its ability to influence policies, which is apparently consistent with the view that it should concentrate on them.

Yet in reality, there is continued high need for public sector involvement in project selection, and for World Bank assistance to it. There are a number of areas where, even in reasonably un−distorted economies, the profit motive alone would not provide adequate investment, or adequate operation of capital facilities. One can distinguish several broad reasons for this, each corresponding to a dimension where public intervention is necessary beyond the provision of a policy framework.

By and large corresponding to the projects whose benefits cannot be quantified, there are a number of areas in which social needs are not primarily described and met through the market. Education and health are prime examples; police and security are others; many environmental projects fall into the same category. It is obvious that if benefits are not even roughly measured by prices consumers pay, society must intervene through the government to ensure the appropriate supply and allocation of such products.break

Much economic infrastructure also falls into the area where investment must be publicly determined. By and large, it should be possible to attribute monetary values to the benefits of economic infrastructure. The category is quite large. Yet it is nevertheless impossible to rely on private initiative for the provision of infrastructure services, for several reasons. It is often difficult and costly to collect payments against the provision of infrastructure services: urban pavement and rural roads are good examples; so are drainage facilities, in some circumstances even water supply . . . Even when prices can be collected relatively easily, many such investments have a very long−term focus, and high initial investment requirements. Fixed costs therefore amount to a high proportion of total costs, and the original investment can often be recovered only over a long period. The looming uncertainty of the remote future tends to deter purely private investment, even if it is granted the necessary concessions and expropriation powers. Often, these uncertainties can be overcome only through government intervention. Little of that was needed for English railways; North American railways were heavily subsidized through land grants (and the first electric telegraph line through a cash subsidy); Continental European railways generally required more direct government financing of the infrastructure, and in many countries of all their investment costs).

Much infrastructure must be in specific locations, and often occupies large contiguous tracks: roads are a prime example. It is de facto impossible to construct such infrastructure without forced expropriation of earlier owners of land and other fixed facilities; the government alone has the authority to do so. Such projects therefore cannot come into being without government decisions and intervention. In many fields, natural circumstances tend to ensure monopoly (power and water distribution), or very limited oligopoly power (telephones); or government intervention is required to ensure some form of order (use of airwaves by radio and television, and of airspace by airplanes).

Government does not necessarily have to invest in all these areas; it may even be able to invest in none. There used to be a presumption in almost every country except the United States that in most areas of social and even economic infrastructure, government had to invest directly, and also operate directly the resulting infrastructure capital. There has been a change in attitudes about these questions, revolutionary in its speed and scope. From highways to hospitals, and even prisons, almost every area of infrastructure now seems open to private financing and operations in every country. There is nothing wrong with this, in principle, provided it is remembered that the basic responsibility remains the government's; and that this has implications with many ramifications, for social satisfaction, for prices, and for country creditworthiness. It is not necessarily worse, from the financial point of view, to have foreign private financing of infrastructure; nor is it necessarily better. But detailed government intervention and responsibility cannot be avoided. These are the areas where the World Bank involvement will keep project−related benefits as long as the World Bank itself retains project−related competence.

Where the Bank's comparative advantage lies within these areas will in large part be country−specific. It will depend on relative strengths and weaknesses in purely technical skills, and, much more, in ability to formulate and apply policies. Need for Bank involvement will be greatest, and remain longest, in the sectors with the weakest policy context. If indeed the Bank helps where it should, exploiting its comparative advantages, those who look back on those future projects should not be surprised that they appear to run less smoothly, and yield lowercontinue

rates of return or discounted profits (when these can be calculated) than other projects. This should be the result of the Bank's doing what it should, concentrating on areas where it can help best.break

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