

Social Organization and Development Anthropology

The 1995 Malinowski Award Lecture

Michael M. Cernca

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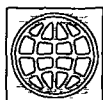
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Social Organization and Development Anthropology

The 1995 Malinowski Award Lecture

Michael M. Cernea

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The Malinowski Award Lecture

Michael M. Cernea is the recipient of the Bronislaw Malinowski Award, presented on March 31, 1995, at the fifty-fifth Annual Meeting of the international Society for Applied Anthropology, in Albuquerque, New Mexico.

The Malinowski Award is given by the society "in recognition of scholarly efforts to understand and serve the needs of the world through social science."

This monograph is the full text of the author's acceptance lecture.¹

Michael M. Cernea is senior adviser for social policy and sociology in the Vice Presidency for Environmentally Sustainable Development of the World Bank.

The views expressed in this lecture are those of the author and should not necessarily be attributed to the institutions with which he is affiliated.

The figure on the cover is a representation of the Bronislaw Malinowski Memorial Medallion, created in silver by the sculptor Emory Sekaquaptewa of the Hopi tribe in New Oraibi, Arizona. The design was influenced by a picture from Malinowski's book, *The Sexual Life of Savages*, representing a traditionally decorated Trobriand boat possibly carrying people and goods from one island to another.

Foreword

This monograph is the text of Michael Cernea's Malinowski Award lecture. The international Society for Applied Anthropology, in giving Michael Cernea the Award, said: "in recognition of scholarly efforts to understand and serve the needs of the world through social science."

The World Bank is remarkably well endowed in talent. The staff of the Bank are, by far, its most important resource. They bring exceptional professional skills to bear on the problems of the poor in this world. Yet, for the most part, these individuals labor in relative obscurity, deriving their reward and their satisfaction from a job well done.

But the Bank is also a place of innovation, where intellectual sparks fly and where the debate is forceful, professionally demanding, and characterized by a hard edge of intellectual rigor, tempered by pragmatism—by the need to get things done. The intellectual movers and shakers inside the Bank are seldom known and appreciated outside the institution. They derive their satisfaction from having pushed the institution's process of conceptualizing and practicing development along the lines they think are right.

So it is a very great pleasure to see one of our most distinguished colleagues recognized both inside and outside the Bank for the vision, the intellectual rigor, and the consistency with which he has carried his message to the development community and to the social science community over the past two decades.

Michael Cernea is a giant among the social scientists applying sociology and anthropology to the problems of development. He has stood firm for the need to address the social issues of development long before it became fashionable to do so. He has brought clarity of thinking and practical wisdom to the tasks at hand, both inside and outside the Bank. It is wonderful to see his special skills recognized through the distinguished Bronislaw Malinowski Award. His acceptance lecture is a powerful illustration of why he won the award and of why he has been so successful over so many years at the Bank in forging a community of sociologists and anthropologists and giving them voice and vision, organizational visibility, and intellectual weight.

I therefore feel that his lecture should be given wide public dissemination, and I am pleased to see it issued in our Studies and Monographs series.

*Ismail Serageldin
Vice President
Environmentally Sustainable Development
The World Bank*

Acknowledgments

This is a fitting time to express gratitude to my family for the strength and motivation they impart to my endeavors—in particular, thanks to my children and to my wife Ruth, a fellow anthropologist, for her support of my work, including her counsel in preparing this lecture.

My tribute goes also to my close colleagues, in the World Bank and outside it, who are committed to the kind of social science work discussed in this paper—in

particular to Irma Adelman, Cynthia Cook, Gloria Davis, Shelton Davis, Theodore Downing, Ashraf Ghani, Scott Guggenheim, Michael Horowitz, Maritta Koch-Weser, Ayse Kudat, William Partridge, Thayer Scudder, Ismail Serageldin, and Andrew Steer.

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Social Organization and Development Anthropology

In order to be of use, research must be inspired by courage and purpose. It must be briefed by that constructive statesmanship and wise foresight which establishes the relevant issues and have the courage to apply the necessary remedies....

Unfortunately, there is still a strong but erroneous opinion in some circles that practical anthropology is fundamentally different from theoretical or academic anthropology. The truth is that science begins with application.... What is application in science and when does "theory" become practical? When it first allows us a definite grip on empirical reality.

— Bronislaw Malinowski
The Dynamics of Culture Change

It is a great honor to receive the Bronislaw Malinowski Award from this scholarly community of development social scientists, and I am deeply grateful to the jury for this recognition. Being associated

through this Award with the name and legacy of Malinowski, and with the line of distinguished scholars who have preceded me as Award recipients, is a moving and stimulating experience.

A World on the Move

When Margaret Mead and her colleagues created the Society for Applied Anthropology some fifty-four years ago, few would have anticipated either the current expansion of applied social science or the recognition that issues of social and cultural development would receive.

Consider for a minute the gigantic wave of systemic socio-political changes that during the last decades have restructured the world and transformed beyond recognition the very societies we once studied—their polity, economy, culture, people, and national agendas. Anthropology itself

*To remain
relevant
to development,
social scientists
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think differently
of development
itself*

has always been rooted in reflections about community. Yet that has changed and must further change, in part because the notion of community itself began to erode, in part because of the dynamic of economic growth, decolonization, and ascent of scores of nations to statehood and self-construction. We live now in a newly integrated-and-fractured world system. During this decade, the very structures of our contemporary world were changed by the collapse of the former Soviet Union and Eastern European regimes, a triple collapse of political, economic and multinational state models. In turn, ethnicity and religions revive and reshape the planet's social map. To remain relevant to current developments, social scientists must learn to think differently of development itself.

Development has powerfully modified the individual's everyday life. Within our generation, since 1960, average life expectancy in the developing countries has increased by about twenty years, a change of still incalculable consequences. Adult literacy has risen from about 40 percent to over 65 percent, and average per capita incomes in the poor countries have doubled and in some nations have tripled or quadrupled. A child born today in the developing world is only half as likely to die before the age of five as a child born just a generation ago. The same child is twice as likely to learn to read, and can expect a standard of living two or three times higher.

Yet this is not the full picture.

The world still has about 1.3 billion people living in absolute poverty, defined as earnings of less than one dollar a day. More than 2 billion people still lack access to electricity and are forced to use sticks and dung for their energy needs. Roughly 1.7 billion lack sewage systems, and 1 billion lack access to clean piped water, resulting in the unnecessary death of some 3 million infants and children every year from diseases linked to dirty water. The inequality gap continues to widen: during the past 30 years, incomes in the countries with the richest 20 percent of the world's population grew nearly three times faster than in the countries with the poorest 20 percent. There are more refugees and displaced persons in today's world than at any time before, including the aftermath of the World War II. And 140 million of the world's adults are unemployed and cannot feed their families.

These and other daunting problems prescribe a formidable agenda for curing social ills—an agenda that our twentieth century will hand over to the next.

From a social development view point, however, we can say that the twenty-first century has already started. It started this very month, March 1995.

The first event of the new century took place when the governments of the world's countries met in Copenhagen in the first ever *World Summit for Social Development*. This Summit set its sights explicitly beyond *economic growth*

alone—toward *social* goals. As I was privileged to be present, I tried to anticipate what might be the Summit's likely effects on the discourse of social sciences in a world that inscribes *social* development on its banners.

The Summit produced a social charter—a social contract for the world at large—and this is unprecedented. All heads of states signed off on this Program of Action consisting of *nine major commitments*, each embodying a set of goals and actions regarding: poverty eradication; full employment; fighting social dis-integration; human rights; women and equality; enabling legal and institutional

frameworks; and other major social objectives. Global social development was spelled out in explicit goals, norms, and tasks.

True, there were also unconcluded agreements, contradictions, rhetoric, and confusion. Yet the historic significance of this event is inescapable. If there is any link between social research and thinking, on the one hand, and actual social development, on the other, the implications for our duties as engaged social scientists are equally inescapable.

This is an auspicious beginning for the twenty-first century. I see in it also a powerful call to our profession. ■

***Social development
is a powerful call
to our profession***

Anthropology at the World Bank

*Structural
difficulties and
sleep-robbing
questions confront
the introduction
of social science
knowledge within
an "economic
fortress"*

The citation for all Malinowski Award recipients states that it is offered in recognition of "efforts to understand and serve the needs of the world through social science." This is a tall order indeed; that my work is deemed by this Society to have done so is for me a morally and professionally rewarding judgment. Therefore, I feel it incumbent upon me, as it has been upon my scholarly predecessors, to briefly account for at least part of my intellectual biography, my work, and the ideas that inform it. For some of the structural difficulties and sleep-robbing questions that we have confronted in introducing anthropological knowledge within an "economic fortress" are not atypical: they are or will be faced by those of you, or of your students, struggling to accomplish the same kind of progress in other bureaucracies and settings.

The World Bank came rather late, and hesitantly, to the perception that it needed "some" anthropological skills in-house, as part of its regular staff. The World Health Organization (WHO), for instance, hired its first anthropologist in 1950, twenty four long years *before* the World Bank. George Foster, in his Malinowski address, highlighted the "perplexity" of both sides in that early encounter between anthropology and an international agency (1982:191).² I can add, and many in this room can confirm, that such perplexity is still widely present today also within

domestic private sector organizations. In turn US AID employed many anthropologists long before the World Bank created its first such staff position, mandating it to put anthropology on the Bank's skeptical intellectual map.³

I am therefore pleased to say that since that lone anthropological "slot" was allocated in 1974, the Bank's in-house corps of non-economic social scientists—sociologists, anthropologists, political scientists—has grown steadily in both number and institutional weight. This has been a major leap. The group assembled during these twenty years is today the world's *largest group of this kind working in one place*—about 50-60 social scientists, who actually practice development anthropology and sociology.

In addition, hundreds of social scientists from developing and developed countries are employed each year as short term consultants, largely due to the demand for social analysis legitimized by the core in-house group.

Beyond the change in numbers, there has also been change in the cultural substance of development projects and programs. The World Bank's manner of studying client countries during its early years was sarcastically described in a 1952 book review of three Bank country studies—about Turkey, Guatemala, and Cuba. The reviewer, C.P. Kindleberger (who was not an

*... the world's
largest group of
development-
oriented applied
social scientists*

anthropologist) characterized the Bank studies and the experts' approach to field work in this way:

Essentially, . . . these [studies] are essays in comparative statics. The [field] missions bring to the underdeveloped country a notion of what a developed country is like. They observe the underdeveloped country. They subtract the latter from the former. The difference is a program (1952:391).

Continuing, the reviewer commented on the Bank work teams of that time:

Most of the members of the missions came from developed countries with highly articulated institutions for achieving social, economic, and political ends. Ethnocentricity leads inevitably to the conclusion that the way to achieve the comparable levels of capital formation, productivity, and consumption is to duplicate these institutions.... (1952:392).

This image of the Bank is from a time long past. But please remember it as it helps measure the substantive changes over time in the institution's practices.

When I started work at the Bank in August 1974, I had no way of knowing that it would lead me, twenty-one years later, to this rostrum; in fact, I did not know then much simpler things, such as what I would have to do the very next day after my appointment. That "next day" proved to be a field trip to Tanzania, to help untangle the agricultural difficulties of a coun-

try that had just undergone villagization and "ujamaazation," a forcible grouping of peasants in state-established village-cooperatives.

Kigoma, the western region in which we worked on the magnificent shores of Lake Tanganyika, stood out among Tanzania's regions for having carried out ujamaazation in a particularly harsh manner. However, the grand lines of this statist approach to agricultural collectivization, and its dire consequences, were known to me from my previous studies on collectivization in Romania. Comparing what I knew about socialist collectivization in Eastern Europe with what I saw then in Tanzania about patterns of village organization and imposed change greatly facilitated my analysis. Field work was what I was comfortable with—and what gave me an edge over the other colleagues on the Kigoma team. This "edge" made them interested in my social analysis and findings relevant for the project at hand.

This was the first project field-work test—a starting point, if not exactly a road map. It opened for me a window into what was needed, from a sociological perspective, and how it could fit into the patterns of Bank work. It helped me perceive that changes in the Bank's culture and ethos were needed, to make this new kind of activity accepted in-house widely. And it also made it crystal clear to me that I would have to pay my way as a social scientist within the World Bank in the coin of know-

*As a social
scientist, I would
have to pay
my way in the coin
of knowledge
of recognizable
organizational
utility*

*I was cast
in the role
of an "ambassador
of the discipline"*

ledge of recognizable organizational utility.

I set to work with determination and, I confess, fear. To give you an idea of how it felt then, bringing anthropology's message to a rather agnostic and skeptical professional group, I should recall a story from the Vatican. After Vatican Two, Pope Paul decided to do something about spreading the faith in Eastern Bloc countries. He then created a new office in the Vatican, called ... the "Secretariat for Non-Believers." He appointed Cardinal Franz König of Austria (who years later told this story to the Washington Post), as Secretary to the Non-Believers. The poor Cardinal did not know what he was supposed to do. He went to the Pope and asked: "What shall I do?" The Pope, reportedly, shrugged and said "I don't know." Then he added in Latin "*Usus Docebit*": with God's help, "the use will teach you."

And so it was also with me, as I assumed the role of "Secretary to the Non-believers": the use taught me. That I faced a huge challenge and a tough personal test was intimidatingly obvious. Becoming the first incumbent of a new role within the Bank's organization, I stepped into an undefined and ambiguous situation. There was no formal status, no structure of expectations, not even a stereotype to live up to. My unit was itself new, a special experimental division just created by Robert McNamara to pilot the new policy for poverty alleviation that he had launched in Nairobi the previous year.⁴ From

the outset, I was cast in the role of an "ambassador of the discipline." In no uncertain terms, I was told that my work was to demonstrate to the institution whether the discipline I represented had a legitimate place in the Bank, is or is not compatible with it.

What the Bank only dimly realized at that time was that I had another very strong incentive to succeed—a personal, yet frightening incentive—a win-or-perish option: namely, my children were kept back as "hostages" by the then government of Romania, and were not permitted to join me in my resettlement to the U.S. The only way to successfully "extract" them was to succeed at my work and hold on to my job, legitimizing requests for their release. It took fifteen long months: I did hold on to my job, and I did get them here. I am happy to mention this tonight as my children are in this room with us—now, with their spouses and small children too! Anthropology is about real people, so I thought I would tell you this personal part as well.

My earlier pre-Bank training obviously had little to do with typical World Bank issues or conceptual vocabulary. I can confess now how suspended-in-the-air I felt then hearing the lingo of "credit disbursement curves," "shadow prices," or "economic rate of return calculation." But I was coming from a solid sociological and anthropological tradition of village studies, developed over decades and brought to maturity between the two World Wars by Romania's foremost sociologists and anthropolo-

gists, Dimitrie Gusti and his principal associate, Henri H. Stahl. Like Malinowski, Dimitrie Gusti also studied in Leipzig, with the same professors as Malinowski—the psychologist Wilhelm Wundt and the economist Karl Bücher—and at roughly the same time as Malinowski. Later, Dimitrie Gusti created, conceptually and organizationally, what came to be recognized as the “Romanian rural monographic school” (Gusti 1935, 1941), a thoroughly holistic, anthropological manner of studying village culture, customs, beliefs, natural context, economic activities, and political and social organization. Beyond mere ethnography, Gusti and Stahl (1934, 1939) also advocated action-orientation, and directed social research towards activities for improving the life of peasants under study (not a bad guidance for my own later work!). Many of Gusti’s huge research teams comprised 40, 50; or over 60 members, organized to fill with empirical data the eight-tiered conceptual framework of his village monographic method. After the war, when sociology and anthropology were ideologically and politically banned in Romania, surreptitiously studying Gusti’s and Stahl’s pre-war rural monographs was, for me and other young social researchers, a way of learning about empirical field investigation.

When the first opportunity appeared in the 1960s, I conducted my own village monographs, re-studying specific communities investigated thirty years before by

Gusti’s researchers to assess intervening change (Cernea, Kepes, Larionescu 1970). This is how I gradually gained my dual identity as a sociologist and anthropologist, field worker and academic.

In that period, rife with a distorting dominant ideology, the top-down prescription for research was to ascribe to reality the image of how it was supposed to be, but wasn’t. Genuine field work was ostracized, as it implied a threat to the establishment: the threat of deflating the ideological balloon with reported empirical evidence.

From those years, what I personally cherish most is the contribution I was able to make in the 1960s, with my research team, to resuming and legitimizing again empirical sociological field work in Romania, after an interruption of two decades. What I learned then about the iconoclastic power of facts for toppling falsehoods and inviting action served me then, and serves me now, in my current work. That empirical research was also what led, first, to my 1967 work in France, at the Centre d’Etudes Sociologiques, and then, most importantly, to the unforgettable year I spent in 1970-71 at the Center for Advanced Studies in the Behavioral Sciences at Stanford. That intellectually intense year profoundly restructured my thinking and rejuvenated my conviction in the power of social research—power to explain and power to guide action. In short, it changed my life, and I cannot miss this opportunity, a full quarter-century

...the iconoclastic power of facts for toppling falsehoods and inviting action

*To recognize
and research
"contemporary
variations among
existing cultures"*

— Margaret Mead

later, to again express my gratitude to the Center.

With this brief "pre-historical" account completed, I return to the issues of applying sociology and anthropology to purposive development programs.

Anthropological work has gradually but firmly gained ground inside the Bank. It has helped move the institution from its initial pristine ethnocentricity to an expanding recognition of the "contemporary variations among existing cultures" (Mead 1976) and of a broad spectrum of social variables in induced development. Of course, recognizing and understanding other cultures required that the Bank's own internal culture as a development agency underwent change. Anthropologists had to comprehend the Bank's culture to be able to hold their ground and function effectively, while acting to constantly modify the surrounding culture itself. I use culture here in a broad sense, referring to the Bank's properties as a multilevel organizational system, with an in-

ternational staff itself belonging to multiple national cultures, with its own norms, values, and goals, work rules and informal patterns of human interaction, turfs elegantly called areas of "intellectual jurisdiction," ways of accepting or rejecting criticism, incentives and penalties, rewards and conflicts.

Our most successful social scientists are those who have understood the culture best, yet didn't simply submit to it but rather took it into account and deliberately acted as insiders—sensitive and tenacious change agents.

Since our social scientists have broken important new grounds, promoting the use of sociological and anthropological knowledge in development—new grounds not only for the World Bank but, I submit, also for our profession at large—I will try further to distill a set of lessons of broader validity from our group's experiences and offer them for your critical reflection. □

Rationale for Social Analysis

Several basic premises and principles underscore my rationale for social analysis and my actual work as a development social scientist.

The first premise is that the kind of development I refer to is *induced* development. This is development purposively pursued, accelerated, and programmed, often guided by policy based on a mix of knowledge and assumptions, and therefore distinct from spontaneous development. Thus, while *spontaneous* development is only observed and passively described in anthropology, *induced* development is one which anthropologists can influence, if included.

The second premise is that financially-induced development interventions are planned programs for *social* development and *social* change, not just for economic growth. Social analysis, which brings to bear the conceptual and research techniques of non-economic social science, represents—and should be so employed—the methodology for clarifying the social and behavioral mechanisms of development and change. These social and behavioral mechanisms are intrinsic to development; they act sometimes as distinct mechanisms, but, more commonly, as fundamentally intertwined with the economic mechanisms of development. Therefore, these social mechanisms must be recognized and purposefully mobilized to achieve the objectives of development programs. Es-

entially, this is—and must be — similar to the ways in which the economic mechanisms are triggered and harnessed to attain set objectives.

To do this, the fundamental prerequisite for a successful development intervention is that the social fabric, the project's context, must be *explained* and *understood*. There is no substitute to social analysis for accomplishing this initial social mapping task.

Further, social analysis must be employed to also help *design the social goals and provisions* of the project. This includes designing the *social institutional scaffolding* within which the project-financed technology is embedded. The key design goals are increasing equity and reducing poverty through development. Such a design of social arrangements and of implementation steps, including needed collective action, is indispensable as a pro-active contribution to program design and program execution strategy. This kind of contribution must be regularly part of decision making, and always integral to the "development package" itself.

In addition to designing for, and mobilizing, the *constructive* social mechanisms of development, social analysis must also confront the sometimes *adverse social consequences of economic growth*. When decisions are made to implement social policy reforms or programs, economic and financial analyses are clearly neces-

***Social analysis must
be used to explain
the social fabric,
design the
social goals,
and chart the path
of collective action***

Social analysis is as necessary for inducing development as economic analysis is

Sociology and anthropology must combine their bodies of knowledge and methods

sary to assess incurred costs. Conversely, economic growth often has painful social costs. When decisions to invest in economic growth are made, their social risks and costs must be recognized in an equally unambiguous manner. I regard this as a moral imperative for carrying out social analysis *ex-ante*: to forecast the social costs, identify the at-risk groups, define the social mitigations to be pursued, and factor in these elements into the program itself.

For all these reasons—economic, social, moral, financial—social analysis is not only *instrumental* but, in my view, is indispensable. It directly increases the success of programs. Therefore, the sociological/anthropological knowledge required for and embodied in social research and analysis is *not a luxury or a marginal add-on* to inducing development, but is as necessary as the economic analysis is for designing and ascertaining the feasibility and adequate goal-directedness of development programs.

To carry out in practice this rationale for social research, forecasting, and design, the development sociologist or anthropologist possesses and contributes the store of professional knowledge about social organization and cultural systems necessary for inducing development with larger gains and fewer pains. *Applied* research is called upon to continuously expand the disciplinary store of knowledge of the social analyst, and refine it to answer specific practical questions at hand. The professional social analyst must also be ever aware of the imperfection of this knowledge,

and must formulate recommendations with requisite prudence and ethical responsibility.

The key ontological and methodological principles for using social knowledge are common to applied anthropologists working both in international programs (frequently named “development anthropologists”) and in domestic programs. Therefore, much of what I am saying about the former applies also to the latter. The cross-cultural nature of some development programs has spawned much misguided writing about the anthropologist’s role as “intercultural broker.” This is a concept that was developed in dignity (Wolf 1956, Wiedman 1976) but ended up frequently trivialized by practices that marginalized the utility of the anthropologist, miscast as a mere guide or translator of the local vernacular to his team co-members.⁵ In both domestic and international programs, anthropologists can and must do much *more* than “inter-cultural brokerage.”

Last, a critical premise is that development anthropology and development sociology have essential commonalities that far prevail over their differences.⁶ Therefore, it is beneficial to both professional communities to *bridge their traditional disciplinary divide*, combine their resources, and mutually empower their bodies of knowledge and methods. In the World Bank, our group of sociologists and anthropologists is harmoniously doing this kind of bridging. Today I will speak about both anthropology and sociology, and will often use one or another term interchangeably. □

Content in Project Analysis and Design

The main work bench for social anthropological endeavor in the World Bank is the development project.

Projects come in all forms, sites and sectors: from health care systems in Asia to urban infrastructure in Latin America, from irrigation in the Maghreb to reforestation in Pakistan, from education in Africa to reducing environmental pollution in Thailand, to combating AIDS in Uganda, to structural adjustment reforms, and to projects for building hydropower dams, curing cataract blindness or improving family planning and nutrition. More than 1,800 Bank-assisted projects are proceeding today, with Bank financing of \$150 billion and total investment costs of some \$500 billion.

Despite this enormous diversity, some basic common features exist. Every "project" is a social process, not just a commercial investment, and brings in play an array of different *social actors*. Yet for a long time, the conventional approach was to treat projects as only economic or technical interventions. How to craft projects as units of purposive and organized change intervention was not, and still is not for the most part, a science taught in the Academy. We had to invent, learn, and assert, in parallel with similar efforts of other colleagues elsewhere.

Noteworthy in this process of affirming the usefulness of social

analysis are several trends and shifts over the years. The key shifts we accomplished are: (1) moving away from ghettoizing the social scientist in tail-end project evaluations of limited consequence, to placing him up front, in project design and decision-making; and (2) moving from working on projects only, to crafting policies as well.

We have forcefully asserted that at issue is not the task entrusted to an individual sociologist but the overall call upon the full body of knowledge encapsulated by the discipline. An individual social expert can usefully perform a segmented role, such as an ex-post evaluation, but the non-economic social sciences should not be pigeonholed into one segment of the project cycle and excluded from others; nor should they be dispatched to work exclusively on projects, but be ostracized from policy formulation.

We have developed a matrix of "entrance points for social knowledge," tailored along all the key stages of the entire project timetable and including policy work as well (Cernea 1979, 1991). The main lesson of my entire experience is that the social scientist's *key contribution* is to be not only a data collector and offer passive "assessments" of the existing situation: he or she must also actively *design the content of induced change and chart the collective action path* toward accomplishing it.

... we had to discover and learn the "entrance points" for social knowledge into the project cycle

*... knowledge
for action
is a distinct body
of knowledge*

Does good social analysis lead to better projects? Has social analysis for projects made a difference to the countries where we are working and to their people?

Most emphatically, yes. Substantiating evidence has been constantly accumulating. Quantified proof was provided, for instance, by an independent secondary analysis carried out by Conrad Kottak (1985) on a set of 57 Bank-financed projects. Kottak hypothesized that if the projects' socio-cultural fit at appraisal is higher, these projects will be associated on average with a higher rate of return at completion. Conversely, initial socio-cultural misfit will be associated on average with lower rates of return.⁷ The overall findings showed that enhanced socio-cultural fit was associated with economic payoff: the average rate of return at audit time was 18.3 percent for projects found socio-culturally compatible, while for projects that were incompatible socio-culturally the returns were less than half, only 8.6 percent.

These findings are averages, and not every single project matched the overall trend. Convincing similar proof is emerging from a new, currently ongoing, larger comparative research. All available evidence, coming from project after project after project, substantiates the conclusion that the better the social analysis, the better the project.

However, it is also fair and responsible to acknowledge that social analysts have not been mistake-free. Some made erroneous

judgments or validated misguided projects. Others have misassessed and mispredicted the behavior of the populations involved. The tools for our analyses, and the methods for translating social knowledge into prescriptions for action, are only developing. Judgments often need to be made with far from perfect social data, and error has not graciously bypassed each one of us. Yet what is novel, despite such errors, is that new variables are taken into account—variables about social and cultural organization. These variables are factored in precisely because social analysts have started to "inhabit" the project-crafting process at its core, not just its periphery.

Two observations are in order: the first is about the *nature* of knowledge needed in applied work, the second about the *institutional* rules of using it.

Applied and development anthropologists need two categories of knowledge: "knowledge for understanding" and "knowledge for action" (Scott and Shore 1979), to explain and to prescribe. My experience confirms that knowledge for action is indeed a distinct body of knowledge, but one that taken alone can be utterly pedestrian and deceptive. Knowledge for action is valid only if it is incremental to, and relies on, knowledge for understanding, because otherwise, precious as it may be, it is rarely self-standing in the long haul.

These two distinct categories of knowledge, both indispensable,

*... social analysts
have not been
mistake-free*

result from different cognitive itineraries. Only segments of these itineraries pass through university halls. It is part and parcel of the applied anthropologists' job not just to apply knowledge, but *to create and recreate both types of knowledge* in each one of their assignments. This makes the applied job no less demanding than teaching or academic research.

The second observation, also from my World Bank home ground, is that inserting "more" social knowledge in projects cannot occur just by accretion of in-house individual anthropologists. Knowledge organizations have also formal bureaucratic rules and cultures. To create *systemic room* for social knowledge, we had to militate for *changing these rules*, in other words—for *institutional change* to mandate the use of this kind of knowledge.

Modifying organizational rules and procedures requires a tenacious effort. In anthropology we call this "change in the organization's culture." Although major advances have been made, there still is a long way to go in mainstreaming and generalizing social analysis in World Bank activities.

Important as formal rules are, the actors behind some of the new rules—those who caused rule-changes to happen—are even more important. May I take therefore a minute to give public credit to my colleagues, the anthropologists and sociologists of the World Bank, without whom the progress I am talking about wouldn't have happened. Some of these col-

leagues are tonight in this room, like Bill Partridge and others, but at this very moment most are spread throughout the world. I want to call your attention to the theoretical-cum-applied work of Gordon Appleby, Michael Bamberger, Doug Barnes, Lynn Bennett, our late David Butcher, Maria Clark, Michael Cohen, Cynthia Cook, Gloria Davis, Sandy Davis, Ashraf Ghani, Dan Gross, Scott Guggenheim, Steve Heyneman, Maritta Koch-Weser, Ayse Kudat, Marlaine Lockheed, Alice Morton, Shem Migot-Adholla, Raymond Noronha, William Partridge, Ellen Schaengold and many others. The lives of uncounted people across meridians have been significantly improved due to the committed and creative work of these social scientists. This work, which is by definition hands on, is also recorded in a number of scholarly and applied writings—a true thesaurus of social reflections on development experiences.⁸

Social analysis for development decisions is under exacting demands at the Bank, being expected to meet, in the words of a Bank manager, "three explicit characteristics: it must be based on a coherent analytical framework, it must be predictive, and it must be prescriptive as well" (Serageldin 1994:vi). My colleagues—responding to these challenges through social theorization or field work, through analytical studies, designing project components or even by calculating budget provisions for social components in many programs—have stimulated a more

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— Ismail Serageldin

sophisticated treatment by the Bank and many governments of development tasks. They helped produce better solutions to human problems.

There is a more general lesson in this: as year after year more social specialists have joined the Bank's staff, we have gained *critical mass* in-house. This has enhanced our impact, creating room for professional self-organization, networking, more refined strategies, informal and formal alliances in intellectual battles (see Kardam 1993). Conversely, the absence of a "critical mass" in other organizations also explains why the handful of social specialists are hampered and confined in their influence.

External factors have also converged in influencing and energizing this in-house process. First, the outside applied community has supported our work inside the

Bank in multiple ways. Eminent scholars and development anthropologists—may I highlight among them Scudder, Downing, Uphoff, Horowitz, Chambers, Kottak—have contributed so regularly throughout the years that they virtually are part of our in-house community. Second, and equally important, external criticism by NGOs and public interest groups has increasingly emphasized social issues in recent years. Significantly, the criticism from the environmental community (a lobby infinitely more vocal than the social science community) now concentrates not only on physical issues but on socio-cultural ones as well. Without taking into account the convergence of these (and other) factors, we could not understand what I described as a major progress of theoretical applied anthropology and sociology in World Bank-assisted activities. □

Fighting Biased Development Models

The kind of knowledge brought into the Bank by development sociologists and anthropologists did not land in an intellectual vacuum. It landed on territory long colonized by economic and technical thinking, both with entrenched tenure. It landed onto an in-house culture unfamiliar with and resistant to this new socio-cultural knowledge and expertise.

I know that this is the case in many other institutions.

The characteristics and biases of an institution's culture tend to put their imprint on the institution's products.

In large scale organizations, different bodies of knowledge compete for "intellectual jurisdiction" over tasks and over policy formulation. The encounter between the theories and practice of inducing development engenders "battlefields of knowledge" (Long and Long 1992). Therefore, how to carry out intellectual clashes with opposed conceptual paradigms is a tactical question that many of us in this room are facing, within different organizations. In our case, intellectual combat has been part of the history of anthropological work in the Bank,⁹ and it continues to be so—a creative struggle of ideas, interpretations, and models. Continuing this effort to help change the Bank's culture still remains high on today's agenda, not least as a premise for further improving the Bank's development programs.

Certain approaches to inducing development embody a common, vastly damaging, conceptual bias: they underestimate the socio-cultural structures of real societies. Such reductionist models of social change become painfully obvious in the design of some development projects. Most frequent among these reductionist perspectives are what I term the *econocentric* model of projects, the *technocentric* model, and the *commodocentric* model. (Comparable biases appear in other institutional contexts and cultures).

The *econocentric model* is visible in those approaches that one-sidedly focus on influencing the economic and financial variables, regarding them as the only ones that matter. Their presumption is that if you can "get the prices right," everything else will fall into place. This, however, is a reductionist econo-mythical belief, a mutilated image of reality. It wishes away the noneconomic variables from theory, but doesn't remove them from reality. The penalty for this simplification is severe. Most often, we have seen that when the social determinants of development are abstracted out by econocentric mind-sets, projects display an unrepressed and funny propensity—they fail.

The *technocentric model* is the approach that deals with the technological variables of development more or less "in vitro," dis-embed-

... we must oppose the biased paradigms in development work: the econocentric, technocentric, and commodocentric models in projects

*Non-economic
social scientists
must work hand
in hand with
economists and
technical experts*

ded and disembodied from their contextual social fabric. "Technology transfer" was once described as the ultimate development paradigm. Although this rage has been muted, there is still little understanding of the necessary proportionality between developing new physical infrastructure and creating the social scaffolding for it simultaneously. Technocentric models tend to underdesign and underfinance the social scaffolding.

My point is that it is not enough to "get the technology right" for the missing social infrastructures to spring up automatically overnight, by God's *fiat lux*. Overcoming technocentrism requires careful *social* engineering for institution building, to create and nurture the cultural arrangements in which the physical infrastructure is necessarily enveloped.

The *commodocentric model* is visible in scores of programs that focus on the commodity, the "thing," more than on the social actors that produce it. They focus on coffee production but less on coffee-growers, on "livestock development" but not enough on herders, on water conveyance but not on water users. "Putting people first" is not a familiar idea in these narrow approaches.

Development anthropology and sociology must militantly reject such reductionist models or

exaggerations, and provide integrated and actionable alternatives. Development is not about commodities. It is not even about new technologies or information highways. It is about people, their institutions, their knowledge, their forms of social organization. This is why non-economic social scientists must be present and work hand in hand with economists and technical experts in the core teams that *formulate development paradigms, policies, and the content of specific programs* (Cernea 1990a).

My personal conviction is that shying away from engaging in intellectual battle about the paradigms of development results not in more "friendly acceptance" of applied sociologists and their work, but in less.

By now I think that my answer about strategy in conceptual clashes on the battlefields of knowledge is clear. We must assert our conceptual differences, because they do make a difference. We must take firm positions without posturing, must be earnest without an offensively earnest tone, and must be opinionated while being free of fixed opinions. For applied social scientists, quibbling only for improving practical fixes is never enough. Asserting innovative ideas does require intellectual wrestling and theoretical engagement. □

Where Do Paradigm Biases Originate?

Some questions are inescapable at this point: Where do these distorting conceptual models originate? What should be done, and where should we do it, to correct or prevent them?

I would like to answer with a brief story. Not long ago I was invited to give a seminar for the social science faculty of an Ivy League university. During the discussions, some highly respected academic social scientists expressed hopelessness about the effectiveness of development anthropology or applied sociology. They voiced skepticism about the very legitimacy of applied development orientations in sociology and anthropology and complained about the domination of intellectually biased development models in World Bank programs and in other development agencies. The biases they complained about were, obviously, the neglect of the socio-cultural variables in economic development interventions and programs.

My response was, in turn, to question them: "Where do *you* see the roots, I asked, of these biases? Why do they persist? How do paradigm biases reproduce themselves?"

There was silence, or circuitous explanations.

I offered my own answer to these questions. Yes, there is a definite place where these models originate. "My Bank colleagues

with econocentric or technocentric biases, I said, came from this place: indeed, from among your own best and brightest graduates. They come to the World Bank from your own university, or from other universities of similar excellence. They are the former *magna cum laude* students in finance, economics, or technical specialties, who spent eight to ten years here next to your anthropology or sociology department doors, but were never invited to enter through these doors, and therefore were not touched intellectually or emotionally by your scholarship."

Indeed, I explained to my academic colleagues, "I work at the receiving end of your university's 'line of products'. Many of the former students in economics from your university became my colleagues, and they brought to the Bank—or to governments and the private sector elsewhere — biased, one-sided conceptual models. The models I am fighting in my colleagues' thinking and practical development work reflect nothing else, unfortunately, than the training received in your university's economic department, training that inculcated models that ignore social variables." Can we correct afterwards what the university has not done well at the right time?

As the seminar's chairman volunteered, there was "blood on the floor" after that seminar, but it was a discussion useful for all of us.

Where are the roots of biased intellectual models?

Look first into your university curricula

The models I am fighting in my colleagues' thinking reflect training received in your university

The way social sciences are taught goes sadly against the practical role social knowledge must exercise in modern society

This is a huge issue. I submit that the way social sciences are taught in most universities in the U.S., and across the developing world, goes sadly *against*, rather than *in support of*, the policy and practical role social science knowledge must exercise in modern societies. Trends and practices are at work, by commission or omission, that undermine the proactive role of noneconomic social sciences.

In my view, two major strategic errors in academic curricula and processes contribute to generating and reproducing such biases.

The first is the incorporation of only a minuscule dose, or even a "zero dose," of social science in the curricula of the vast majority of students majoring in non-social science fields. Even worse, the content of what students are taught about anthropology doesn't help either, because they are taught remote generalities, or exotic stories, rather than be introduced to the parts of anthropology or sociology that are directly relevant to contemporary issues and to their own specialization and future work.

The second strategic error is the absence of emphasis on the teaching of social science *for practice*, as opposed to teaching for general understanding, for *weltanschauung*.

The results of these anachronistic attitudes, to return to my Vatican metaphor, are that the armies of "non-believers" expand with every new cohort of undergraduates, while the "Secretaries to the Non-believers"—you, I, my colleagues—face a harder uphill battle.

The foremost battlefields of knowledge for conquering the minds of tomorrow's developers and policy-makers are in our universities—and this is where the battle should not be lost.

I leave it to my academic colleagues to draw the sober conclusions about the major restructurings indispensable, indeed imperative, in the teaching of social sciences. For how social sciences are being taught *today* prepares the terrain—fertile or unreceptive—for society's practical use of social sciences *tomorrow*. □

Conceptual Models for Development Anthropology

My next question at this point is: if we propose to put the biased models of development interventions on trial, how do we make the prosecution's case for anthropology?¹⁰ How do we argue anthropology's "can-do" claim to relevance?

The constructive argument is far more crucial than the critique. The way we legitimize to others or to ourselves the need for social analysis in development interventions creates a structure of expectations that becomes compelling.

In other words, if we argue just the pragmatic, short-term operational benefits' side of using anthropology, we will end up playing a very mundane fix-it-here-and-fix-it-there role. If, however, we convincingly construct the argument for a theoretical applied anthropology, we lay claim to having voice over the substance of development paradigms and policies.

The rigid dichotomy between applied and theoretical anthropology is a simplistic representation that must be rejected. The "practice" of anthropology can generate value-added for society only if it is practiced as *theoretical applied anthropology*. What has to be "applied" through applied anthropology is our theoretically generalized knowledge about societies and cultures. What else would anthropologists have to apply? They use the research methods. What they apply is the storehouse of

knowledge (van Willigen 1993, Angrosino 1976). Anthropologists bring to their work the knowledge about what is general in individual local societies ("cross cultural commonalities") and proceed to uncover what is unique in that individual society. I cannot imagine applied anthropology without this "theoretical understanding" lodged and carried along in the mental back-pack of the practicing anthropologist.

Upon scrutinizing much of the literature, I can see several types of arguments—models of rationalizing, or ways of "making the case"—for development anthropology. I'll refer to three such models. (Another, the "cultural brokerage" model, was mentioned earlier.)

Not all the models in circulation are either correct or equally powerful. I submit that some of them result only in a peripheral and diminished, even if real, role for anthropologists. Our discipline can do better than that. Because this year our meeting is devoted to "environment, development and health," I will define and discuss some of these models in the context of the anthropological variables of environmental programs.

The widest spread but least convincing model is what I'd call the *add-on model* (or argument) for anthropology's or sociology's case. This is the time-honored route of vaguely claiming that there are "some" cultural-social implica-

How do we argue anthropology's "can-do" claim to relevance?

*Focus your
applied inquiry
on structural
issues, on patterns
of social
organization*

tions to *all* environmental issues (or to health, or to whatever else the issue of the day is) and, therefore, one needs anthropology too, in addition to... and on, and on. You know the litany.

This weak way of making our case inherently begs for a marginal role, a glorified place at the periphery, a stereotypical add-on: "me too." Such an add-on is not even our sacred holism, because if holism is pleaded as an additive list of traits it becomes unholy, a messy eclectic mix. Consequently, the "add-on" model is neither compelling, nor apt to persuade people with opposed mind sets. In anthro vernacular, one could also call it the "hodgepodge model," because it contemplates reality syncretically as an amalgam of aspects, without identifying structures and grasping priorities and causalities within the belly of the social beast.

The core point, as I will stress further, is that the social-cultural variables are not just another "aspect," a minor side of a basically technical issue. These variables are essential to the structure of most major problems we encounter.

Another model is the *behavioral model*, so named because it focuses on the need for individuals to understand and amend their detrimental behaviors *vis-a-vis* the environment. This model is not invalid, because education and attitudes are significant for shaping individuals' behavior. It is merely insufficient. Indeed, it gives little weight to group structures and vested interests. It also places the

anthropological endeavor in the province of environmental education—a relevant but not central position. The logic of this argument pushes anthropologists toward a psychological and educational approach directed primarily to the individual's misconstrued perceptions and attitudes, but leaves out the structural societal dimensions.

An alternative model—which, in my view, represents the strongest way of arguing the case for, and actually practicing, applied anthropological and sociological analysis—is to focus on the *patterns of social organization* within which *social actors* act. Predicating the value added contribution of anthropological analysis primarily on revealing the models of social organization that underpin social processes and link social actors will together *best position applied research on the strongest theoretical ground*. This "locks" the laser of applied inquiry onto structural issues, giving it centrality.

This is the natural position that anthropological/sociological analysis should occupy, not because social scientists subjectively so desire it, but because of two indisputable facts. First is the centrality of social actors in development; second is the knowledge about patterns of social organization and their actors' motivations. This is the very core of the anthropological and sociological enterprise, the comparative advantage and special competence of our disciplines. □

Focusing on Patterns of Social Organization in Applied Research

To some, suggesting social organization as the underpinning conceptual matrix in applied anthropology may appear, at first sight, as impractical or remote.

We all know that the applied anthropologist is expected to be "pragmatic," "operational," quick on his/her problem-solving feet. Yet in my own research and field work, taking social organization as the starting point for analyzing, thinking through, and conceptualizing specific practical problems in very diverse cultural contexts, turned out every time to provide precisely the unexpected and original frame of reference that was absent in my economist or technical colleagues' perspective.

This was true in my field work on pastoralists in Senegal, on reforestation constraints in Azad Kashmir, and on irrigation and water-user societies in Thailand, Mexico, and India. Social organization provides a context and a launching pad for analysis, points out to linkages and dependencies, reveals encoded knowledge and meanings,¹¹ and helps identify all possible social actors, local and distant, with a stake in the problem under analysis.

Furthermore, "bringing social organization in" does not send the applied analyst always and necessarily to the *macro*-societal level. It gives the applied researcher, working at whatever social level, the

theoretical impetus to identify patterns of social organization in large social bodies, in remote rural communities, in inner city quarters, in service processes and sub-systems, or even in small "street corner societies." This is true also regardless of whether the problem at hand concerns environmental pollution, health services, crime in the neighborhood, resettlement of displaced people, or irrigation water supply systems.

The analyst should not be surprised—indeed, is rewarded—when such conceptualization redefines both the problem at hand and the conventional solutions. Robert Merton pointed out that "perhaps the most striking role of conceptualization in applied social research is its transformation of practical problems by introducing concepts which refer to variables overlooked... [and which may] lead to a statement of the problem that is dramatically opposed to that of the policymaker" or of whoever else is the user of applied research (1973:94).

Environmental management is a domain that compellingly illustrates, first, the centrality of anthropological analysis, and second, the analytical superiority of the "social organization of actors" model over the "add-on model" or the "behavioral" model. Anthropological knowledge—from Malinowski to Radcliff-Brown, from Raymond

"... the most striking role of conceptualization in applied social research is its transformation of practical problems by introducing concepts..."

— Robert K. Merton

*Focusing on
social organization
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programs to be
actor-oriented*

Firth (1969) to Fredrik Barth (1966, 1993)—is traditionally grounded in the study of the forms and patterns of social organization within which societies use the natural resources on which they depend.

This storehouse of knowledge and research methods is a major thesaurus for enriching current environmental policies and resource management programs in both developing and developed countries.

Yet the *centrality* of social organization issues to environmental problems and programs, however familiar it is to us, is NOT a self-evident truth. This is sadly proven by the abundance of one-sided technological eco-speak, or one-sided economythical “solutions,” and by the dearth of in-depth social understanding of these issues. Indeed, the intellectual debate about the conservation and management of natural resource domains is overwhelmed by the enormous diversity of the *technical* issues intrinsic to the nature of each resource. The overall picture becomes fragmented into technical resource-specific approaches, while the common social underpinnings of all these domains is not grasped, remains clouded, less visible.

The intellectual argument that I regard as the main entry point for social scientists into the environmental debate is that an improved and sustainable use of natural resources depends decisively on *improving the patterns of social organization for their management by the users themselves*. Who are these users? Primarily the world’s enormous mass of small farmers. My basic proposition is that *effective environ-*

mental policy must promote and rest on appropriate social organization. Neither technology unembedded in social organizational structures, nor free-market fundamentalism unable to control externalities, can alone tackle runaway resource abuses.

Anthropologists as social architects must help build practical models for collective action in resource management. And we have to recognize that we must also revisit and correct some of our own models, lovingly advocated in the anthropological literature but ineffective in practice—for instance, the rather romantic model of community-based tree-plot planting.

Communities are generally heterogeneous social entities and thus are seldom able to be the social agents of collective (unified) social actions. A case in point is the costly failure of most “village woodlot” and “community woodlot” schemes financed through hundreds of millions of wasted dollars. Although long praised uncritically by many social scientists, they have failed—and failed for social design reasons. As I demonstrated in my research on social forestry programs in Asia and Africa, inadequate social models have misled many investment strategies into financing approaches which, on social grounds alone, could not—and did not—succeed, wasting both goodwill and money (Cernea 1992b).

The focus on social organization compels development analysis to be actor-oriented. This is germane to both the explanatory and

prescriptive functions of applied research. We did not claim that people were totally out of sight in conventional approaches. But we showed that the characteristics of a given social organization were stripped of the flesh and blood of real life in what I termed econocentric or commodocentric models. We demonstrated that key social actors of development were dealt with as an afterthought, mostly as passive, nonparticipating recipients.

My core conceptual argument was and remains the following:

Putting people first in projects is not a goodwill appeal or mere ethical advocacy.... It is a theoretically grounded request to policy makers, planners, and technical experts to explicitly recognize the centrality of what is the primary factor in development processes.... It calls for changing the approach to planning. The requirement to admit the centrality of people in projects is... tantamount to asking for reversal of the conventional approach to project making.... The model adopted in projects that do not put people first clashes with the model intrinsic to the real social process of development, at the core of which are—simply—its actors. (Cernea 1985/1991:7,8)

Relying on theoretical and empirical knowledge about models of social organization provides development anthropologists with far-reaching inventories of analytical tools and social techniques. It is im-

portant not only to define social organization theoretically but also to “de-construct” social organization into its building blocks, such as: the social actors at the local level; the social contract governing relations (including conflicts) between users and stakeholders, local and distant; prevailing symbolic and cultural systems; rules of entitlements, for example, usufruct, ownership or custodianship rules; authority systems and enforcement mechanisms; an infinite range of producers’ organizations, from family based units to large corporate enterprises; macro-social factors that affect (undermine or solidify) local social organization; and others.

In turn, such conceptual deconstruction facilitates creative social engineering in practice. For instance, the social expert must be able to figure out which available building blocks can make up more adequate social arrangements and culturally-sound action strategies that s/he recommends for given circumstances.

As social architect, the anthropologist is called to define the needs for associational infrastructure, social capital, grassroots organizations or higher order institutions, and help design them. In her Malinowski lecture, Elizabeth Colson pointed out that our Society for Applied Anthropology was created to promote the use of “skills of social engineering” (1985:192). In turn, Raymond Firth emphasized the complexity of “analyzing the strength of relations in human engineering” (1981:196).

Putting people first in projects is not a mere ethical advocacy . . . it is a reversal of conventional project-making

*Forget your
"inhibitions about
transgressing the
boundaries of
separate social
sciences. . . when
you deal with
. . . causal relations
between facts"*

— Gunnar Myrdal

Social engineering skills are indispensable for designing improved social arrangements, better institutions, enabling legal frameworks, adequate incentive systems. What for policymakers and development managers may seem "elusive" sociological dimensions can and must be translated, with professional help from the social scientist, into *policy prescriptions* and pragmatic action-oriented strategies.

When they work as social architects, anthropologists regularly face economic variables. What they have to propose bears directly upon the economy. Yet applied anthropologists often skate rather lightly over the economic determinants of social organization and their implications. We have very much to learn (not just criticize) from our economists colleagues about economic analysis and measurement methods. When anthropologists bypass economic variables—and I have witnessed many such instances—the resulting recommendations are embarrassingly naive or directly erroneous. Conversely, when they consider the relevant economic dimensions, the results are powerful.

Take, for instance, the path-breaking research and policy prescriptions developed by the Binghamton Institute for Development anthropology in their Senegal River Basin studies. That study, as Michael Horowitz wrote, "was persuasive in large part because it provided hard field data on economic decision-making at

the level of the rural production unit" (1994:11), and considered variables that were absent in earlier economic analyses.

We must remind ourselves of Gunnar Myrdal's message to anthropologists, when he received the Malinowski Award exactly two decades ago, about how he lost his "inhibitions about transgressing the boundaries of separate social sciences," to delve into anthropology and sociology. He invited anthropologists to reverse his journey. "In dealing with a problem," Myrdal said, "it could never be a legitimate excuse that certain facts or causal relations between facts lay outside one's own field of knowledge" (1975:327).

Living daily inside an economic tribal culture, I can confirm that anthropology as practice can—indeed must—be strengthened by learning more from economic concepts and by internalizing quantifying methodologies.

This is not a ritualistic tribute to powerful neighbors: economic knowledge is intrinsically indispensable for understanding social organization patterns anywhere. Anthropologists cannot relegate the study of economic variables to the subdiscipline called "economic anthropology." The understanding and manipulation of economic variables through applied social engineering is essential for *all* development anthropologists who take the concept of social organization as their fundamental frame of reference. □

Can Theory Be Derived from Applied Research?

Applied anthropology is often deprecated by unfriendly voices and accused of being irrelevant to anthropological theory. Those who do so, however, have forgotten the classics and pay little attention to how scientific research advances. Half a century ago Malinowski firmly advocated a "practical anthropology" (1929) concerned with answering the issues of the day. He rejected the "erroneous opinion... that practical anthropology is fundamentally different from theoretical or academic anthropology. The truth is that science begins with application. What is application in science, and when does 'theory' become practical? When it first allows us a definite grip on empirical reality" (Malinowski 1961:5).

Applied anthropology facilitates such a "grip" on empirical reality. Even more—it is able to help change social reality.

The view that applied research is atheoretical—either doesn't use theory, or doesn't lead to theory—disempowers the discipline of anthropology. For some members of the academic community, this opinion justifies disengagement and less concern with the public issues of the day. But this view has also instilled some resigned defensiveness among a segment of the practicing anthropological community.

This charge is not only mis-

placed, it is epistemologically unwarranted. First, the research objects of applied anthropology generally have no less intrinsic potential to generate theory than the research objects of academic anthropology. Purposive programs for development and change are complex processes no less theory-worthy than, say, kinship systems, or reciprocal gift-giving, or funerary rituals. Second, the methods of data generation are not necessarily different—many are similar—in applied and academic research. The overall research designs may be different, but neither holds the monopoly on correctness in method. Individual researchers can choose either to distill from their data a course for future action or to pursue theoretical proposition building. Both are valid endeavors.

Sol Tax, in his Malinowski lecture, expressed forcefully his conviction that:

... we deal not with a distinction between pure and applied anthropology but rather an amalgam or continuum of the two, a differing mixture of models in all of us who are anthropologists.... At different times one of us can be doing much that is theoretical and general; at other times much more than is particular and applied, and at still other times engage in activities that are inextricably intertwined. (Tax 1977:277)

"The theory becomes practical... when it allows a grip on empirical reality"

— Bronislaw Malinowski

There is no "distinction between pure and applied anthropology but rather a... continuum of the two"

— Sol Tax

*...little time is
given. . . to
comparison,
thinking, synthesis,
and generalization*

*All human behavior
is grist for our mill. . .
and all good research
data. . . have
theoretical
potential*

— George Foster

Whatever the personal inclinations, the general state of our art, which ultimately is the cumulative result of each individual's work, does reveal difficulties of growth and unresolved problems. Although work in applying anthropology has expanded significantly during the last decade, the progress appears mostly as a vertical piling up of primary case accounts—with too little horizontal cross-synthesis of comparable cases. Fragmentation results also from the sheer mass of what is published. Methodologically, the over-estimation of "rapid appraisal methods" has resulted in all-too-ready excuses for sloppy assessments, for weakening longitudinal research, and for neglecting the collection of long time-series data. Comparative research is little practiced. It seems that most practitioners are so driven by their case-focused pursuits that little time is given to the essential task of looking back and around—for comparison, thinking, synthesis, and generalization. These weaknesses should be of concern to all of us.

I submit, however, that the task of *generalizing* empirical data resulting from applied research is not the charge of only those who define themselves as applied anthropologists. It is equally a task of those working in academic and theoretical anthropology and sociology. A vast volume of factual material is laid out in countless applied reports and studies and is readily available to those interested in extracting theory from em-

pirical findings. No tribal taboo forbids the access of non-applied academic anthropologists to these empirical treasures reported in applied studies.

An outstanding example of what can be done for theory with data from applied research and case studies was given by Goodenough in his "cooperation and change" (1963) study; Spicer termed Goodenough's approach to using applied findings for theory building as an "exciting discovery in anthropology" (1976:134).¹² Yet it is rather sad to realize, three decades later, how little this promising breakthrough has been replicated.

I urge all colleagues in anthropology—academic as well as applied—to join in the effort of distilling theoretical propositions and methodological lessons from good applied research findings and experiences. "All human behavior," noted George Foster in his Malinowski lecture, "is grist for our mill, and all good research data—whatever the context in which they are gathered—have theoretical potential" (Foster 1982:194).

To sum up: first, data from good applied research do have theoretical potential. Second, exploiting that potential is a *collective* (professional community) task, rather than a segregated subgroup task. Third, a broad spectrum of "theoretical products" can be extracted from applied work: concepts, propositions, methodologies for purposive action, hypotheses, and models. □

The "Third Leg" of the Theory-Practice Dichotomy: Policy Development

The part of development anthropology that perhaps best demonstrates the infertility of a dichotomy between applied and theoretical anthropology is the work in policy formulation. Such work can be neither claimed nor performed by an a-theoretical applied anthropology. To combine and convert knowledge and field findings into predictive and prescriptive policy propositions is intrinsically a theorizing operation.

Tom Weaver has written a passionate argument in support of anthropology's potential as a policy science, showing how it engages "the very basis of this field, its goals, its subject matter, research techniques, theory, methodology, its very future" (Weaver 1985:203). In this vein, our experiences practicing anthropology in the Bank have taught us a crucial lesson: however effective our anthropological inputs have been in various individual development projects, *the most important successes, those with the farthest reaching influence, have been in policy formulation.*

Several categories of Bank policies have incorporated substantive anthropological/sociological contributions, yet such contributions are little known. My point is not to list just what we have done, but to show what can be done along policy lines.

- *Cross-sectoral social policies.* Among the essential policies

of this kind, written virtually in full by social scientists are the World Bank's policy on involuntary population resettlement entailed by development programs (World Bank 1980, 1986, 1991c, 1994; see also Cernea 1988, 1993, 1995; Guggenheim 1992; Partridge 1989), the policy on indigenous peoples affected by development projects (World Bank 1982, 1990; Davis and Wali, 1993), and the policy regarding non-governmental organizations in Bank-supported activities (World Bank 1989; Cernea 1989).

- *Sectoral policies.* Non-economic social scientists have made very substantial contributions in the formulations of several of the Bank's major sector development policy statements in cooperation with technical specialties, such as the urban growth policy (World Bank 1991a), the policy on investments in primary education (World Bank 1990); the forestry and reforestation policy (World Bank 1991b; Cernea 1992b), and the water resources policy (World Bank 1993).
- *Socioeconomic and environmental policies.* The poverty alleviation policy, the environmental policy guidelines,

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and others have benefited significantly from similar inputs. Vast efforts are being invested now in codifying participatory approaches and preparing policy guidelines for other social policy domains.

For all these macro-processes, the multiplier effect from investing the knowledge and efforts of social scientists in policy formulations is enormous. Consider the case of involuntary population resettlement. I have written too much on resettlement lately (1995a, 1995b, 1990b, 1988) to repeat this here, so I call your attention to one fact only: At least 10,000,000 people each year are subjected to forced displacement by dams and urban infrastructure, and good policy work in this area stands to benefit vast numbers of affected people.

Resettlement policies are, perhaps, the most telling case of *breaking the isolation and disinterest* in which much good anthropology used to be held. As you know, a valuable body of knowledge was generated in the 1960s and 1970s (Gans, Colson, Scudder, Chambers) on the disasters of forced displacement, yet to no avail. That research was gathering dust on library shelves, largely ignored by planners and policy makers. In the late 1970s, we took that knowledge as a theoretical basis for writing a policy on resettlement for the Bank. But rather than only repeat descriptions of development's disasters, we proposed policy and operational solutions to solve

them. The Bank then adopted the policy in 1980.

What happened next? This is most significant: over these last 15 years, in addition to improvements in resettlement practice, a "cascade" of policy advances occurred, all with social scientists' involvement:

- The Bank's resettlement policy itself was improved in four subsequent rounds (1986, 1988, 1991, 1994).
- Promoting the resettlement policy outside the Bank itself, we have helped draft a policy statement on resettlement (essentially identical with the Bank's policy) for all twenty five OECD countries' bilateral donor agencies, to be applied in their aid programs. Formal adoption of this policy by OECD ministers took place in 1991 (see OECD 1992).
- The multilateral development Banks for Asia, Africa, and Latin America have adopted or are now vetting their resettlement policy.
- Some countries (including Brazil, China, Colombia, and Uganda) have developed domestic social policies and legal frameworks on resettlement, borrowing more than a page from the policy written by social scientists for the World Bank.
- Further, and unexpectedly, some anthropologists in the United States have proposed that the U.S. Government adopt the World Bank's re-

settlement policy for resolving the Navajo-Hopi dispute (Brugge 1993)—a long shot, though, we all agree!

- And at the Social Summit in Copenhagen in March 1995, as a result of an explicit initiative of Bank social scientists supported by other anthropologists (particularly Downing), and backed by several governments (Uganda, Canada, Switzerland) and by NGOs, involuntary resettlement issues have been incorporated in the Summit's Program of Action (see Cernea 1995b).

So this is an indisputable fact: we are today a long, long way from the point when anthropological studies on resettlement were languishing forgotten on library shelves and no resettlement policy whatsoever existed, neither in the Bank, nor most anywhere else in the developing world.

For our "state-of-the-art house-keeping," we should also note that these policies are not the whole story. During the past fifteen years the body of social science knowledge on resettlement itself has been greatly expanded, enriched, tested, corrected and recreated due primarily to operational applied research. A series of seminal papers on the anthropology of displacement and resettlement have resulted from on-the-ground association with resettlement operations, and have stimulated creative contributions in legal thinking and other related fields (Bartolome 1993; Cook 1993; Shihata 1993;

Davis and Garrison 1988; Downing 1995; Guggenheim 1990, 1991, 1993; Partridge 1989; Partridge and Painter 1989; Cernea 1986, 1988, 1990a, 1993, 1995a; Scudder 1991; McMillan, Painter and Scudder 1992; Mathur 1994). This knowledge accumulation is bound to yield further progress in the years ahead.

Most important is that the social science contribution has resulted in major changes in the practice of involuntary resettlement throughout the world—changes in resettlers' entitlements, in planning, in financing, in turning around insensitive bureaucracies. The overarching meaning of all these changes is that the lives and fates of many people worldwide are improved through better protection and added opportunities. We know that resettlement remains painful, and much does not yet happen along ideal policy lines. But in improving this development process we are now further ahead than any development anthropologist would have dared to dream just a decade ago.

For me personally, this kind of progress is the most gratifying reward. I feel privileged indeed to have had the chance to be part of this process, to be a development applied social scientist. My message tonight is that our profession is consequential. It makes a difference. I would want this message to reach the students who engage in the study of anthropology and sociology, and who consider dedicating their life and work to applying this body of

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knowledge. It is not an easy profession, but it is a generous and useful one.

On this very point, it is appropriate to conclude. In its great wisdom, the Talmud teaches that "One who saves a single life is as one who saves an entire world." From this view we can derive courage and motivation for each single one of our projects, big or small, be it a major policy or a small inner city

health project. Each of you here tonight, each applied anthropologist, has probably experienced being the local "Secretary to the Non-believers" in one place or another. But we are gaining converts. We "win" when we make lasting professional contributions that benefit many people.

Let us broaden our knowledge, refine our tools, and embolden our moral commitment to do this beautiful work. □

Notes

1. An abbreviated and not fully edited version of the lecture was published in *Human Organization* 54 (3) (Fall 1995).
2. Commenting on the difficulties of the first American anthropologist (Cora DuBois) who in 1950 joined the World Health Organization for about one year in Geneva, George Foster wrote:
"She did not plan to recommend specific courses of action. (Her) assignment was plagued by the problem that has affected many subsequent anthropologists: neither she, nor the hiring organization, really knew what she should do. Shortly after joining WHO she left for India and Southeast Asia where, in the regional office, her reception was unenthusiastic" (1982:191). And Foster quoted further the personal description given him by Cora DuBois: "I saw my role [as that] of an observer and consultant. I was left to make my own plans and schedules and I was more than a little perplexed as to what was expected of me. Much the same perplexity about my role obtained when I returned to Geneva." (Foster, 1982:191).
3. Two years prior to my joining the Bank, two consultants were commissioned to carry out an in-house study to assess whether or not anthropology could contribute—and if yes, how—to Bank activities. After six months, a long—and in my view good—report was submitted; nonetheless, that report did not convince anybody. Neither of the two consultant authors was retained in the World Bank to apply what they advocated.
4. The special Division for "experimental" poverty projects was created by Robert McNamara with the explicit purpose of testing out new approaches and generalizing them throughout the Bank. McNamara appointed as Division head his own personal assistant, Leif Christoffersen, an innovation-oriented development enthusiast. This Division was located in the Bank's central Agriculture and Rural Development Department, directed by an iconoclastic economist, Montague Yudelman, also close adviser to McNamara. For years, this Department was the braintrust of the Bank's strategic reorientation toward poverty reduction and the avant-garde of many innovative approaches.
5. In a broad sense, what an anthropologist typically does is understanding and describing a culture. In this broad sense "cultural brokerage" is non-controversial as a concept and role. What is objectionable—and I witnessed numerous instances of this practice—is the limitation of the anthropologist's role to the minor aspects of language intermediation or other mechanics of "development tourism," to use Robert Chambers' expression, while his/her competence on essential issues of social organization, stratification, ethnicity, and local insti-

tutions is not treated as indispensable to the job at hand.

6. I note, as a testimony to the intimate relationship between these two disciplines, that Malinowski himself did not hesitate to frequently term his analyses of the Trobriands "sociological," not only "anthropological" (Malinowski: 1984; initial edition 1922). Among many others, Radcliff-Brown has also used the two terms interchangeably in describing his own work.
7. Kottak's analysis (1985) was "blind," in that the coding of sociocultural and socioeconomic variables (including variables of social organization, stratification, ethnicity, gender-based divisions of labor, and others) was completed without knowledge of the project's economic performance; only after the social coding had been done were the rates of return introduced in the analysis.
8. The many publications that have resulted so far from the work of Bank sociologists and anthropologists are described and summarized in our 1994 annotated bibliographic volume: *Sociology, Anthropology and Development. An Annotated Bibliography of World Bank Publications 1975-1993*, by M. M. Cernea, with the assistance of A. Adams. ESD Studies and Monograph Series 3.
9. Of enormous help in this continuous and not-always-smooth theoretical engagement in-house has been our long and tenacious program of sociological seminars and training courses, delivered by Bank sociologists and anthropologists to the rest of our colleagues—hundreds and hun-
- dreds of such seminars over the years (see also Kardam 1993). Many outside social scientists have joined us in this intellectual process of reconstructing the Bank's culture. There is a lesson in this as well—about the need and ways to shape and carry out strategies of gradual cultural and institutional change over time.
10. This is a deliberate reversal of Polly Hill's book title, *Development Economics on Trial: The Anthropological Case for the Prosecution* (1986). Unfortunately, despite many valid observations about statistics and surveys, this book, in my view, fell short of fulfilling the promise of its provocative title. Nor did it convincingly construct the positive case for the role of anthropology in development.
11. What accounts for the forcefulness of Malinowski's analysis of one of the Trobrianders' activities—economic exchange? The answer is the context—social and cultural—which Malinowski reconstructed around the economic exchange processes. As Malinowski wrote, in "my monograph the reader will clearly see that, though its main theme is economic—for it deals with commercial enterprise, exchange and trade—constant reference has to be made to social organization, the power of magic, to mythology and folklore." (1992:34).
12. Praising Goodenough's "Cooperation in Change" for its success in developing "pure theory" by interpreting descriptive case studies of administrative action, Edward Spicer wrote: "In a sense [Goodenough's book] represented

a swing back full circle from the excitement of anthropologists' discovery that they could analyze cases as ethnographic data in a framework useful to administrators, to excitement over discover-

ing that the processes of administration could be brought into the context of pure theory in anthropology with resulting illumination of concept and new generalization" (1976:134).

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