The 1995 Malinowski Award Lecture: Social Organization and Development Anthropology

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> *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 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 its recipients, is a moving and stimulating experience.

A World on the Move

When the Society for Applied Anthropology was founded in 1941, few would have anticipated either the current expansion of development anthropology and sociology as applied social science disciplines, or the recognition that social and cultural issues would receive in international and national development.

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 societies anthropologists traditionally studied. Anthropology itself has

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changed and must further change, largely due to the dynamic of economic and political world changes, and the ascent of many nations to statehood and self-construction.

We live now in a new world system, one that is both integrated and fractured. During the 1980s/1990s, the structures of our contemporary world were changed by the collapse of the former Soviet Union and Eastern European dictatorial regimes, a triple collapse of political, economic, and multinational state models. In turn, ethnicity and religious revivals also are reshaping the planet's social map.

To remain relevant in development, social scientists must learn to think differently about development itself. Development has powerfully changed the individual's everyday life. Since 1960, average life expectancy in the developing countries has increased by about 20 years, a change of incalculable consequences. Adult literacy has risen from about 40% to over 65% by now; 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 only part of the 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 two 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 one billion lack access to clean piped water, resulting in the unnecessary death of some three 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% of the world's population grew nearly three times faster than in the countries with the poorest 20%. There are more refugees and displaced persons in today's world than at any time before, even in the aftermath of the World War II. One last, ominous fact: at this very moment, 140 million of the world's adults are unemployed and cannot feed their families.

The Social Development Summit: A Powerful Call to Our Profession

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

From the view point of social development, however, we can say that the twenty-first century has already started, Figuratively, I'd say that the new twenty-first century started when the governments of all 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. I was privileged to be present and I attempted to grasp what might be the Summit's likely

consequences for the role and necessary contributions 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. This is unprecedented. All heads of states signed off on a Program of Action consisting of nine major commitments, each embodying a set of goals and actions regarding: poverty eradication; full employment; fighting social disintegration; 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 major issues omitted, empty rhetoric, agreements not-reached. Yet the historic meaning 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. And this is why I see and hear in it also a powerful call to our profession for knowledge usable for social knowledge.

Anthropology's Debut in an Unexpected Place: The World Bank

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, and a strong encouragement for reaching further and deeper.

I feel it incumbent on me today, as it was upon my predecessors, to briefly account for at least part of my intellectual biography, my past and current work, and the ideas that inform it. For some of the structural difficulties and sleep-robbing questions I've faced in bringing social science knowledge into a heavyweight financial fortress, are not fully atypical: many will be confronted by other anthropologists, struggling to accomplish comparable tasks in other bureaucratic and development settings.

The World Bank came rather late, in the early1970s, to recognizing that it needed to bring and incorporate a voice for sociological/anthropological knowledge among the inhabitants of its house, as part of its regular staff. In fact, it wasn't the first international agency to arrive to this recognition. The World Health Organization (WHO), for instance, hired its first anthropologist much earlier, in 1950, i.e., 24 long years *before* the World Bank. Yet that first experience was not a success. In his Malinowski Lecture George Foster noted, tongue in cheek, that in that early encounter between anthropology and an international agency there was "perplexity" on both

sides (1982:191): neither part quite understood what the other one could do and needs, to achieve synergy and success.[1]

I can add that such 'perplexity' remains present today within the vast majority of domestic and government organizations that I met during my work on many meridians. One exception, the US Agency for International Development (AID) did attempt to employ anthropologists and sociologists before the World Bank created its first such staff position, to put anthropology on the Bank's skeptical intellectual map.[2] And fortunately, I'm happy to report that since that lone socio-logical "slot" was allocated in 1974, the World Bank's in-house corps of non-economic social scientists—sociologists, anthropologists, political scientists—has grown steadily in number and also—essentially—in institutional weight. This has been a major leap.

The group of social scientists assembled at the World Bank during these twenty years is today the world's largest group of this kind working in one place—close to 150 professionally trained social scientists who actually practice development anthropology and sociology. In addition, hundreds of social scientists from devel-oping and developed countries are employed each year as short-term consultants, largely due to the demand for social research and analysis legitimized by the Bank's new, pioneering social development policies and by the effectiveness of the ini-tial core in-house group.

Beyond the change in numbers, there has also been change in substance. Regard-ing cultural substance, the World Bank's original manner of treating its client countries was sarcastically described in a 1952 review of three Bank country studies—for Turkey, Guatemala, and Cuba. The reviewer, C. P. Kindleberger (by the way, he wasn't an anthropologist!) characterized the Bank studies and the experts' fieldwork in this way:

> Essentially,. . . these [three studies] are essays in comparative statistics. The [field] missions bring to the under-developed 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:

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[1]Commenting on the unsuccessful experience of the first anthropologist who joined the World Health Organization in Geneva in 1950, 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 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 WHO office her reception was unenthusiastic"* (1982:191). And Foster quoted further the personal description given him by that colleague: *"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).

[2]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 they submitted a long—and in my view, good—report with their conclusions and proposals: nonetheless, that report did not convince anybody. Neither of the two consultant authors was retained in the institution.

> 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.

Producing Knowledge of Recognized Organizational Utility

When I started work at the Bank in September 1974, I had no way of knowing that it would lead me, twenty-one years later, to this noble rostrum; in fact, I didn't know then many more pressing and vital things, such as what I'd 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 country that had just undergone villagization and "ujamaazation," a forcible grouping of peasants in state-imposed 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 *ujamaaization* 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 forced collectivization in the 1960s in Romania. Comparing what I knew with what I now observed and learned about patterns of village organization and change greatly facilitated my understanding. Fieldwork in rural communities was what I was comfortable with and what gave me an edge over my other colleagues on the Bank's Kigoma team. This was my first development project test. It opened for me a window into what was needed, from an anthropological perspective, and how my contributions could fit into the patterns of Bank work. It also made clear to me that I would have to pay my way in the Bank in the coin of knowledge of recognizable organizational utility.

I set to work with determination and, I confess, fear. To give you an idea of how it felt then, while trying to bring anthropology's message to a rather agnostic and skeptical professional group, I should recall a story from the Vatican. After Vatican II, 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 Konig 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 policy experimental division just created by Robert McNamara to pilot the new policy for poverty alleviation that he had launched in Nairobi the previous year.[3] From the outset, I was cast in the role of an "Ambassador of the discipline." I was told in no uncertain terms, that my work was to demonstrate to the institution whether or not the discipline I represented had a legitimate and compatible place in the Bank.

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 US. The only way to successfully "extract" them was to succeed at my work and hold on to my job, and thereby legitimize my request 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 this well worth mentioning.

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 anthropologists, Dimitrie Gusti, Henri H. Stahl, Anton Golopentia. Like Malinowski, Dimitrie Gusti also had studied in Leipzig, with the same professors as Malinowski—the psychologist Wilhelm Wundt and the economist Karl Bucher—and at roughly the same time as Malinowski. Later, Dimitrie Gusti created, conceptually and organizationally, what came to be recognized internationally as the "Bucharest rural sociological school", a thoroughly holistic, anthropological manner of studying village culture, customs, beliefs, natural context, economic activities, and political and social organization. The way rural sociology was conceived, taught, and practiced in Romania was largely akin with social anthropology. Early twentieth century Romanian social researchers such as Gusti and Stahl also advocated an orientation to action, and directed rural research towards social reform activities for bettering the peasants' life—not bad guidance at all for my own later work at the Bank! After WWII, when the teaching and practice of sociology were politically banned in Romania, surreptitiously studying Gusti's, Stahl's, and Golopentia's pre-war writings was, for me and other young researchers, a way of learning about empirical investigation.

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[3]The special Division for "experimental" poverty projects was headed by Leif Christoffersen, appointed to that new position from his previous assignment as Assistant to the President of the World Bank, Robert McNamara. and was located in the Bank's Central Agriculture and Rural Development, directed by Montague Yudelman.

When the first opportunity appeared in the 1960s, I conducted my own village monographs, restudying communities investigated 35 years before by Gusti's researchers to assess intervening change (Cernea et al. 1970). This is how I gradually gained my dual identity as a sociologist and anthropologist, fieldworker 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 fieldwork was ostracized, as it implied a grave threat to the establishment: the threat of deflating the ideological balloon with 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 re-legitimizing empirical 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 1970–1971 at the Center for Advanced Studies in the Behavioral Sciences at Stanford (CASBS). 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 later, to again express my gratitude to the Center.

With my personal brief "pre-historical" account completed, I return to the central issue of employing social science knowledge throughout the range of activities involved in conceiving, preparing and implementing development projects and programs. The anthropological work carried out at the World Bank has helped the institution itself to evolve from its initial ethnocentricity to the deliberate consideration of differences in social variables between and within developing countries and the *"contemporary variations among existing cultures"* (Mead 1976) that are consequential for development processes and programs. My community of practicing anthropologists/sociologists has broken some new grounds not only for the World Bank but, I submit, for our profession as well. I will try further to derive some lessons of broader validity from our group's experiences and propose them for your reflection.

The Rationale for Social Analysis in Financially Induced Development

Several premises underpin my comments. The first is that the type of development I'm referring to is *financially induced development*. This is development that is purposively pursued, accelerated, and programmed, often guided by policies that are based on a mix of technical knowledge, assumptions, and economic doctrines. Financially induced development is significantly different from *spontaneous*

development. Thus, while spontaneous development is by and large only observed and passively described in anthropology, a financially induced development interventions (e.g, "projects") is one which trained social specialists can influence if they participate in their crafting and implementation.

The second premise is that anthropologists do possess a body of professional knowledge about social organization and cultural systems that is sorely needed for inducing development deliberately, with larger gains and fewer pains.

The third premise is that the key ontological and methodological principles for using social knowledge are common to applied anthropologists working either in international programs or in domestic programs. So, 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 1973) but ended up frequently trivialized by practices that marginalized the utility of anthropologists, miscast as mere guides or translators of the local vernacular to their team co-members.[4] In both domestic and international programs, anthropologists can and must be more than "inter-cultural brokers."

My fourth premise is that development anthropology and development sociology have essential commonalities that prevail over their differences.[5] Therefore, it is beneficial to both professional communities *to bridge their traditional disciplinary divide* and mutually empower their bodies of knowledge and methods, as our Bank group of sociologists and anthropologists has done harmoniously. In what follows, I will refer to both anthropology and sociology and will often use one or the other term interchangeably.

Shifting Social Analysis from Projects' Tail-end to Upfront

The main work bench for 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

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[4]In a broad sense, what anthropologists typically do is try to understand and explain culture. In this perspecgive, "cultural brokerage" is non- controversial as concept and role. What is objectionable—and I witnessed numerous instances of this practice—is the limitation of anthropologists' roles to the minor aspects of language intermediation or other mechanics of "development tourism," to use Robert Chambers' expression, while their competence on essential issues of social organization, stratification, ethnicity, and local institutions is not treated as indispensable to the job at hand.

[5]I note, as a testimony to the intimate relationship between these two disciplines, that Malinowski himself did not hesitate to term his analyses of the Trobriands "sociological," not just "anthropological" (Malinowski 1922).

blindness, or improving family planning and nutrition. More than 1800 Bank assisted projects are proceeding today, with Bank financing $150 billion and total investment costs of some $500 billion. Despite this enormous diversity, some common features exist. Every "project" is a social process, not just a commercial investment, and brings into 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 and learn, in parallel with similar efforts of other colleagues elsewhere.

Noteworthy in this learning process are several shifts and trends over the years. The key shifts we accomplished are: (I) moving away from ghettoizing social scientists in tail-end project evaluations of limited consequence by placing them 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 input made by the body of knowledge encapsulated by the discipline. An individual social expert can usefully perform a segmented role, such as an expost 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, while being ostracized from policy for mulation. I have developed a matrix of "entrance points for social knowledge" tailored along all the key stages of the entire project cycle and including policy work as well (Cem (Cernea 1979, 1985/1991). The main lesson of our entire experience is that the key contribution of anthropologists is not to be only data collectors or make static "assessments," but to actively *design* the content of induced change and chart the social action path toward accomplishing it.

Does this work make a difference to the countries where we are working and to their people? Affirmative evidence is 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' sociocultural fit at appraisal is higher, these projects will be associated on average with a higher rate of return at completion. Conversely, initial sociocultural misfit will be associated on average with lower rates of return.[6] The overall findings showed that enhanced sociocultural fit was associated with economic payoff: the average rate of return at audit time was 18.3% for projects found socio-culturally compatible, while for projects that were incompatible socio-culturally the returns were less than half that at only 8.6%. These findings are averages, and not every

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[6]Kottak's secondary analysis 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.

single project matched the overall trend. Better proof may be forthcoming about more recent projects.

However, it is also fair and responsible to say that social analysts have not been mistake-free. Some made erroneous judgments or validated misguided projects. Others have mis-assessed and mis-predicted 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 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 specialists 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-sustaining in the long haul. These two distinct categories of knowledge, both indispensable, result from different cognitive itineraries, and only segments of these itineraries pass through university halls. It is part and parcel of applied anthropologists' jobs not just to apply knowledge, but *to create and recreate both types of knowledge* in each of their assignments. This makes the applied job no less demanding than teaching or academic research. The second observation, also from my Bank home ground, is that inserting social knowledge in projects cannot occur on a significant scale just by simple accretion of in-house individual anthropologists. Knowledge organizations also have formal bureaucratic rules. To create systemic room for new social knowledge, we had to militate for changing these rules. In other words for institutional change to mandate the use for this kind of knowledge. Modifying rules is an arduous effort in itself. In anthropology we call this "change in the organization's culture." Although advances have been made, there still is a way to go for mainstreaming and generalizing social analysis in the World Bank.

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 special public credit to my colleagues, the anthropologists and sociologists of the World Bank, without whom the progress I am talking about would not have happened. I want to call your attention to the theoretical-cum-applied work of Gordon Appleby, Michael Bamberger, Doug Barnes, Lynn Bennett, the 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 their committed and creative work, which exceeds by far the published record.[7]

Social analysis for development investment 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, must be predictive, and it must be prescriptive as well" (Serageldin 1994:vi). My colleagues—responding to these challenges through social theorization or fieldwork, through analytical studies, designing tangible project components or even through earmarking budgetary provisions for social components in many programs—have stimulated and contributed to a more sophisticated treatment by the Bank and many governments of development tasks. They have helped to 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, and informal and formal alliances in intellectual battles (see Kardam 1993). The absence of a "critical mass" in many organizations also explains why the handful of social specialists are hampered and confined in their influence. External factors have also converged in influencing this in- house institutional process. First, the outside applied community has supported our work inside the Bank in multiple ways. Eminent scholars and development anthropologists—may I highlight especially among them Thayer Scudder, Theodore Downing, Norman Uphoff, Michael Horowitz, Robert Chambers, Conrad 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 sociocultural 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.

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[7]The written record of their published work is described in our an-notated bibliographic volume: *Sociology, Anthropology and Development. An Annotated Bibliography of World Bank Publications 1975–1993.* ESD Studies and Monograph Series No. 3.

Fighting Econo-centric, Techno-centric, and Commodo-centric Development Models

The kind of knowledge brought into the Bank by development anthropologists did not land in a vacuum of knowledge. It landed on territory long colonized by economic or technical thinking, both with entrenched tenure. I know that this is the case in many other institutions. In large-scale organizations, different bodies of knowledge compete for jurisdiction over tasks and over policy formulation. The interlocking of theories and practice in inducing development creates "battlefields of knowledge". Therefore, how to carry out intellectual clashes with opposed conceptual paradigms is a pragmatic, tactical question that many of us must face. In our case., intellectual combat has been part of the history of anthropological work in the Bank, and it continues to be so—a creative struggle of ideas, interpretations, and models.

Several different approaches to inducing development display a similar and profoundly damaging conceptual bias. They underestimate the sociocultural structures in the development process. This distortion is often and painfully visible in the design of development projects. Most widespread among these biased perspectives are what I term the *econocentric* model of projects, the *technocentric* model, and the *commodocentric* model. (Comparable biases appear in other institutional contexts).

By the *econocentric model* I have in mind 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 widespread econo-mythical belief remains a mutilated representation of reality. It simply wishes away the noneconomic variables from theory, but does not remove them from reality. But we have seen that when the social determinants of development are left out by econocentric mindsets, projects display an unrepressed and not at all funny propensity—they fail.

By the *technocentric model,* I have in mind the approach that caters to the technological variables of development more or less "in vitro," dis-embedded 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 considerable disregard of the necessary proportionality between developing new physical infrastructure and creating the social scaffolding for it simultaneously. Technocentric models under-design 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 techno-centrism requires careful social engineering for institution building, to induce and nurture the cultural arrangements in which the physical infrastructure is necessarily enveloped.

By the *commodocentric model* I have in mind 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 approaches.

Development anthropology and sociology must militantly reject such fallacious models or exaggerations and provide integrated, convincing, 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 I think that 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.

My personal conviction is that shying away from engaging in intellectual battles about the paradigms of development results not in more "friendly acceptance" of applied anthropological or sociological work, but in less. By now, you have heard my answer about strategy in conceptual clashes on the battlefields of knowledge. We must assert our conceptual differences, because they 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. Winning requires intellectual wrestling and theoretical engagement.[8]

Where Do Biases Originate?

A question is 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?

My answer is 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 anthropologists expressed their well-worn hopelessness and skepticism about the legitimacy and effectiveness of development anthropology or applied sociology, and in general about development dominated by biased models.

My response was, in turn, a question: "Where do you see the roots, I asked, of these biases? Why do they persist and reproduce themselves?"There was silence, or circuitous explanations.

I gave my own answer to these questions. Yes, there is a definite place where these models originate. "My Bank colleagues with econocentric or technocentric

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[8]Of enormous impact in this—not-always-smooth—in-house theoretical engagement 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 hundreds of such seminars over the years (see also Kardam 1993). Many outside social scientists have joined us in this intellectual reconstruction process. There is a lesson in this respect as well—about shaping and carrying out strategies of gradual cultural and institutional change over time.

biases, I said, came from this place, from among your own best and brightest graduates. From your 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 never entered, and were not touched intellectually 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 of your university bring to the Bank, or to governments and the private sector, biased, one-sided conceptual models. The models I am fighting reflect nothing else, unfortunately, than the training received in your university's economics department, training that inculcated models that ignore social variables." Can we correct afterwards what the university has not done 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.

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

In my view, there are two major strategic errors in academia in this respect.

The first common strategic error is the small dose, or the "zero dose," of social science taught to the vast majority of students majoring in non-social fields. Quite often, what they are told about social sciences does not greatly help either when they are taught generalities instead of being taught the parts of anthropology or sociology that are directly relevant to their own specialization and future work.

The second strategic error is the scarce or often nonexistent curriculum emphasis on the teaching of social science for practice, as opposed to teaching for general understanding or for, so to say, just *weltanshauung*. 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 and, I, my colleagues face a harder uphill battle.

The first battlefield of knowledge for the minds of tomorrow's developers and policymakers is 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 they prepare the terrain—fertile or infertile—for society's practical use of social science.

The Rationale 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?[9] How do we argue anthropology's "can do" claim to relevance?

The constructive argument is far more crucial than the critique, for several reasons, not least because 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. Eventually, this turns out to be the way we end up practicing social analysis. In other words, if we argue just the pragmatic, short-term operational benefits' side of using anthropology, we will end up playing a 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 paradigm 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 can- not imagine applied anthropology without this "theoretical understanding" lodged and carried along in the mental back-pack of practicing anthropologists.

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 of them. (Another, the "cultural brokerage" model, was mentioned earlier.) Not all the models in circulation are correct or equally powerful. I submit that some of them would result only in a peripheral and diminished, even if real, role for anthropologists. Our discipline can do better than that.

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[9]This is a deliberate reversal of Polly Hill's title to her 1986 book "Development Economics on Trial: The Anthropological Case for a Prosecution." Unfortunately, despite many valid observations about statistics, surveys, and so on, this book, in my view, did not fulfill the promise of its provocative title. Nor did it construct the *positive* case for anthropology.

The widest spread but weakest model is what I'd call the *add-on* model (or argument) for anthropology sociology's case. This is the time-honored route of vaguely claiming that there are "some" cultural-social implications to all environmental issues (or to health, or to that whatever else may be the issue of the moment) and, therefore, one needs anthropology too, in addition to... and on, and on. You know the litany.

This 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 un-holy, a messy eclectic mix. Consequently, the "add-on" model is neither compelling nor apt to change opposed mind sets. In our vernacular, one could also call it the "hodgepodge model," because it sees reality syncretically as an amalgam of aspects, without grasping structures, 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 mainly 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 either. The logic of this argument pushes anthropologists toward an educational approach unduly limited to the individual's misconstrued attitudes, but leaves out the structural economic and societal dimensions.

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

The Focus on 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 applied anthropologists are expected to be "pragmatic," "operational," quick on their problem-solving feet. Yet, in my own fieldwork, taking social organization as the starting point for conceptualizing, thinking through, and analyzing specific practical problems in very diverse cultural contexts turned out every time to provide precisely the unexpected and original frame of reference absent in my economists' or technical colleagues' perspectives. This was true in my 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,[10] 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 applied analysts always and necessarily to the *macro-societal* level. It gives applied researchers, 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 (1973:94) 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.

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 Radcliffe-Brown, from Raymond Firth to Fredrik Barth—is traditionally grounded in the study of the forms and patterns of social organization within which societies use the natural resources

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[10]What accounts for the forcefulness and path-breaking quality of Malinowski's selective analysis of one of the Trobrianders' activities economic exchange? The answer is the context: social and cultural. As Malinowski wrote, in this 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".

on which they depend. This storehouse of knowledge and research methods is a major thesaurus for framing 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 econo-mythical "solutions," and by the dearth of in-depth social understanding of these issues. Indeed, the intellectual debate about resource domains is overwhelmed by the enormous diversity of the technical issues intrinsic to each resource. The overall picture becomes fragmented into technical resource-specific approaches, while the common social underpinnings of all these domains 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 environmental 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 some of our own models, lovingly advocated in the anthropological literature but ineffective—for instance, the rather romantic model of community-based tree-lot 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. Inadequate social models have misled many investment strategies into financing approaches which, on social grounds alone, could not—and did not— succeed, thus wasting both goodwill and money (Cemea 1992a, 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. Our argument was, and is, this: putting people first in projects is not a goodwill appeal or a 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 tested analytical tools and social techniques. It is important not only to define social organization theoretically but also to "deconstruct" 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, e.g. 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 undermine or solidify local social organization; etc. In turn, this facilitates creative social engineering work. 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 in given circumstances.

As social architects, anthropologists are 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), and Raymond Firth, in his Malinowski address, emphasized the complexity of "analyzing the strength of relations in human engineering" (1981:196). Social engineering skills are indispensable for designing better social arrangements, improving institutions, enabling legal frameworks, and constructing adequate incentive systems. What for policymakers and development managers may seem "elusive" sociological elements can be translated, with 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 colleagues the economists 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 Institute for Development Anthropology (IDA) 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 absent in earlier economic analyses, such as yields per unit capital, per unit labor, and per unit land.

We must remind ourselves of Gunnar Myrdal's words, when he received the Malinowski Award, 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 the 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 enriched and strengthened by learning more from economic concepts and by internalizing quantifying methodologies. This is not a ritualstic 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 basic guide.

Can Theory be Derived from Applied Research?

Applied anthropology is often deprecated by unfriendly voices and accused of being intrinsically incapable of benefiting theory. Long 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 does not use theory, or does not lead to theory—disempowers the discipline of anthropology. For some in the academic community, this opinion justifies disengagement and less concern with the public issues of the day. But this view has also induced some resigned defensiveness among a segment of the practicing anthropological community.

This charge is not only misplaced 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. Development programs are complex social processes no less theory- worthy than 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 are different, but neither holds the monopoly on 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 was correct when, in his Malinowski lecture, he expressed the 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).

Whatever our personal inclinations, the general state of our art—which ultimately is more than the sum 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, progress appears mostly as a vertical accumulation 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 overestimation 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 left for 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 only of 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 development reports and studies and is readily available to those interested in extracting theory from empirical findings. No tribal taboo forbids access to these empirical treasures to non-applied academic anthropologists. An outstanding example of what can be done for theory with data from applied research and case studies was given by Goodenough (1963). Spicer termed Goodenough's approach to using applied findings for theory building as an "exciting discovery in anthropology" (1976: 134).[11] Yet it is rather sad to realize 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"

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[11]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 form anthropologists' 'discovery' that the processes of administration can be brought into the context of pure theory in anthropolohy".

can be extracted from applied work—concepts, propositions, methodologies for purposive action, hypotheses, models, etc.

The "Third Leg" of the 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 atheoretical 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 1985a, 1985b:203). In this vein, our experiences in practicing anthropology at the World 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 impact, have been in policy formulation.

Several categories of World Bank policies have incorporated substantive anthropological/sociological contributions, yet such contributions are little known. My point is to show not just what we have done, but 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; 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 Social 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 co- operation 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 1992a), and the water resources policy (World Bank 1993).

Socioeconomic and Environmental Policies The poverty alleviation policy, the environmental policy guidelines, 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 domains, 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

to repeat this here, so I call your attention to one fact only: At least ten million people each year are subjected to forced displacement by the construction of dams and urban infrastructure. And 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 on the disasters of forced displacement, yet to no avail. That research was gathering dust on library shelves, ignored by planners and policy makers. In the late 1970s, we took that knowledge as an empirical and theoretical basis for writing a policy on resettlement for the Bank. But rather than 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 fifteen years, a "cascade" of policy advances occurred in this area, all with the participation of social scientists.

The Bank's resettlement policy itself was improved in four subsequent rounds (1986, 1988, 1991, 1994).

Promoting the resettlement policy beyond the Bank itself, we helped to draft a policy statement on resettlement (essentially identical with the Bank's policy) for all 25 OECD countries, to be applied by the bilateral donor agencies of OECD countries 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 (Brazil, China, Colombia, and others) 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 US Government adopt the World Bank's resettlement 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 Cemea 1995).

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 anywhere else in the developing world.

For our "state-of-the-art housekeeping," we should also note that these policies are not the whole story. During the past fif-teen 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; Davis and Garrison 1988; Downing 1995; Guggenheim 1990, 1993; Partridge 1989; Partridge and Painter 1989; Cernea 1988, 1990a, 1990b, 1993, 1995; Scudder 1991; McMillan et al. 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, even 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 farther 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, and am excited to practice this vocation. 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 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 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 better and better.

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